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Does identity matter?
Three empirical essays on the dynamics of public-private contractual relationships

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Summary

This dissertation examines the role that identity of contracting parties play in public-private relationships. It contains three empirical essays. The first chapter examines whether the identity of managers in the contracting firm plays a role in shaping the way procurement contracts are written, through the theoretical lens of relational contracting. Specifically, using contracts signed between French municipalities and the largest parking lot operator, it relies on text analysis methods to proxy contract rigidity, i.e. the degree to which parties rely on formal clauses. Results provide some evidence that contracts signed with newly appointed managers are more rigid in some aspects, potentially indicating that relational dynamics enter into play.

The second chapter investigates how repeated interactions between a public buyer and a seller shape renegotiations of public contracts. Repeated interactions can be seen through different theoretical lenses. In line with transaction cost economics (TCE), it may be argued that repeated interactions are indicative of a "lock-in" situation due to specific assets. Relational contract theory (RCT), on the other hand, argues that repeated interactions foster cooperation through the development of informal agreements. Each theoretical framework has different consequences regarding renegotiation. TCE posits that parties will attempt to use their bargaining power to extract rents through renegotiation. RCT would support the idea of cooperative renegotiations, as showcased in Beuve and Saussier (2021). I investigate this question using a panel version of the parking lot data used in the first chapter, and using a precise coding of renegotiation outcomes. Results suggest that repeated interactions increase the bargaining power of the supplier, who is able to obtain additional rents through renegotiation.

The third chapter studies the effects of mayor changes on procurement outcomes, using a database covering all municipal procurement contracts awarded between 2015 and 2023 in France. The hypothesis tested is that a change in mayor should reduce the risk of favoritism, thus increasing competition, and reducing preference for local or known companies. It relies on two quasi-experiments. A first quasi-experiment studies the effect of mayor deaths, but provides little conclusive evidence, potentially due to the small number of deaths which occurred throughout the observation period. In the second part, we use close electoral races

to obtain quasi-random municipal turnover. We find some evidence that municipal turnover improves procurement outcomes.

Keywords: Public procurement, identity, relational contract, transaction costs, favoritism, renegotiation, municipal, rigidity, competition

Résumé

Cette thèse étudie le rôle que joue l'identité des parties contractantes dans les relations public-privé. Elle comprend trois essais empiriques. Le premier chapitre étudie comment l'identité des dirigeants de l'entreprise contractante influence la manière dont les contrats de commande publique sont rédigés, à travers le prisme théorique du contrat relationnel. Plus précisément, en utilisant des contrats signés entre des municipalités françaises et le principal opérateur du secteur du stationnement, elle s'appuie sur des méthodes d'analyse textuelle pour construire un indicateur de rigidité contractuelle, c'est-à-dire du degré auquel les parties s'appuient sur des clauses formelles. Les résultats suggèrent que les contrats signés avec des dirigeants récemment nommés sont plus rigides sur certains aspects, ce qui pourrait indiquer que des dynamiques relationnelles entrent en jeu.

Le deuxième chapitre étudie comment les interactions répétées entre un acheteur public et un contractant privé influencent le processus de renégociation des contrats. L'effet des interactions répétées peut être interprété selon différents cadres théoriques. Conformément à la théorie des coûts de transaction (TCT), on peut considérer que les interactions répétées signalent une situation de dépendance due à la spécificité des actifs. La théorie des contrats relationnels (TCR), à l'inverse, soutient que les interactions répétées favorisent la coopération grâce au développement d'accords informels. Ces deux cadres théoriques impliquent des conséquences différentes sur la renégociation : la TCT suppose que les parties chercheront à utiliser leur pouvoir de négociation pour extraire des rentes, tandis que la TCR soutient l'idée de renégociations coopératives, comme illustré dans Beuve et Saussier (2021). Ce chapitre s'attache à résoudre cette opposition à l'aide d'une version en panel des données de stationnement utilisées au premier chapitre, et grâce à un codage précis des différents types de renégociations. Les résultats suggèrent que les interactions répétées augmentent le pouvoir de négociation du contractant privé, qui parvient à obtenir des rentes supplémentaires lors des renégociations.

Le troisième chapitre étudie les effets des changements de maire sur la conduite des appels d'offre de commande publique, en s'appuyant sur une base de données couvrant tous les contrats de commande publique municipale attribués entre 2015 et 2023 en France. L'hypothèse testée est que les changements de maire devraient réduire le risque de favoritisme, augmentant

ainsi la concurrence et diminuant la préférence pour les entreprises locales ou déjà connues. Deux quasi-expériences sont mobilisées. Une première, fondée sur les décès de maires en exercice, ne fournit que peu de résultats concluants, probablement en raison du faible nombre de décès survenus pendant la période d'observation. Dans une seconde partie, nous exploitons des élections municipales très disputées pour obtenir une variation quasi-aléatoire de l'identité du maire. Nos résultats suggèrent que les changements de maire liés aux élections municipales améliorent la qualité des appels d'offre.

Mots-clés: Commande publique, identité, contrat relationnel, coûts de transaction, favoritisme, renégociation, municipal, rigidité, concurrence

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Foreword

This dissertation, entitled "Does identity matter? Three empirical essays on the dynamics of public-private relationships" contains three chapters, related by the common theme of identity in public procurement. Each chapter constitute a standalone study, and thus can be read independently from the others. This implies potentially redundant information from one chapter to another.

Contents

General Introduction

Contracts between public authorities and private companies are an essential topic for economic research. Two main reasons for this can be identified, the first being the financial weight of the sector: public procurement amounts to 13% of GDP in the OECD [OECD, 2023], and around 8% of GDP in France. Within public procurement, goods and services purchased by French municipalities, which are the focus of this work, take up a significant share: municipal procurement represent 49% of total procurement expenditures in France [Intercommunalités de France, 2024]. A second essential dimension of public procurement is that public contracts are a way to provide public services, either directly (when operation of a public service is outsourced to a private firm) or indirectly (when private firms provide intermediate goods and services necessary to the operation of public services by authorities). Public services, although generally complex to include in the measurement of welfare, are progressively being accounted for in measurements of global wealth and poverty (see for instance Gethin [2024a,b]). Privately produced public goods and services play a great role in most of French citizens' daily life, including most of urban constructions, the management of water, public transportation, highways, parking lots and waste management. The proper governance of public contracting, and through it the proper governance of public service provision, are therefore essential contributors to economic well-being as a whole. It is then paramount, both for the sake of rational public spending, and for the sake of improving the quality of public services, that appropriate policies pertaining to public procurement be designed and evaluated by empirical research.

The goal of this research is first and foremost to contribute to policy making in the field of public procurement. Procurement policies include a wide array of tools. Before a public contract is signed, rules may be designed to govern the publicity and attribution of the contract, in order to avoid favoritism and to foster competition, but also to grant flexibility

and the possibility of negotiation when necessary. Procurement policies may also concern the content of contracts. Policymakers may wish to restrict the duration of contracts and introduce incentives for parties to write clauses designed to fulfill environmental or social goals. Finally, procurement policies include rules governing the life of the contract after it is signed and may concern the conditions for its modification, termination and prolongation. Various aspects of these policies have been examined by recent empirical research, testifying of economists' interest in the matter: publicity rules [Coviello and Mariniello, 2014], strictness of awarding procedures [Decarolis et al., 2020a; Carril, 2022; Szucs, 2023; Celis Galvez et al., 2025], renegotiation rules [de Jaegher et al., 2023]. Optimal policies have been to shown to be contingent both on characteristics of the good or service awarded[1] and on the institutional environment in which the contract is signed [Coviello et al., 2018b; Bosio et al., 2022].

The evaluation of public procurement policies cannot solely rely on empirical assessments of country-specific, and sometimes industry-specific regulations. General theories of public-private relationships must be crafted, and have been so for several decades. The research on public contracting was first approached through the question of efficient regulations of firms operating utilities or natural monopolies, such as railways or electricity networks. Pioneering works include Averch and Johnson [1962]; Stigler and Friedland [1962], who highlighted the inefficiency of rate-of-return regulation, and Demsetz [1968], who emphasized the necessity to award the rights to operate utilities through competition. Explicitly integrating asymmetric information in procurement relationships, McCall [1970] introduced the adverse selection problem in regulation as well as the now classical distinction between fixed-price and cost-plus regulation. This stream of literature has later expanded to a general theory of agency relationships applied to regulated industries and procurement [Laffont and Tirole, 1993].

A second historical stream of the research has focused on whether or not public services should be contractually delegated to private firms. In the aftermath of Williamson [1979]'s trailblazing work, the "make-or-buy" question was subsequently applied to the case of public services [Schmidt, 1996; Hart et al., 1997]. The tradeoff is generally presented as such: outsourcing allows to benefit from lower costs, but reduces quality because of contractual

[1]For instance, the widely studied "rules versus discretion" tradeoff has traditionally emphasized the necessity of high discretion in the case of complex projects and high contractual incompleteness [Bajari and Tadelis, 2001; Bajari et al., 2009], although evidence that discretion is efficient for small contracts also exists [Chever et al., 2017]

incompleteness and operator opportunism. On the other hand, in-house provision increases production costs but internalizes incentives to provide high quality. The research on make-or-buy decisions has also greatly expanded since, moving away from purely economic determinants to include political and institutional variables such as ideological leaning of the public authority [López-de-Silanes et al., 1997; Picazo-Tadeo et al., 2012], whether or not the administration is governed by an appointed manager or an elected official [Levin and Tadelis, 2010], size of the authority [Bel and Fageda, 2007], and culture [Athias and Wicht, 2025].

The latter strand of the research brings us closer to the subject of this dissertation. While, traditionally, the make-or-buy decision was the focal point of transaction cost theory, a more precise question has emerged: within the continuum of bilateral arrangements that can be used to outsource a service, which should be used? Indeed, the question of contracting out poses several subquestions: How should the contractor be selected? How long should the contract be made to last? How precisely should the written provisions prescribe tasks to the public service operator? Should the renegotiation process be framed *ex ante* by the contract? How intensively should the public authority monitor the contractor's activity, and using which instruments? These considerations serve as an illustration of the fact that "make-or-buy" is not a dichotomy but a continuum, within which lie several types of bilateral arrangements, spanning from spot contracting to vertically integrated hybrid structures (in the language of Williamson [1985]), or from classical to neoclassical to relational contracts (in the vocabulary of MacNeil [1978]).

Within this continuum, an essential question to distinguish between institutional arrangements is the following: does the identity of my partner matter? While neoclassical theory insists that price is the only relevant variable to make an economic decision, this view does not hold in the presence of long term relationships where contracts are incomplete and information is imperfect. Identity of the partner may serve as a basis for reputation and the development of trust through relational contracts. The focal role of identity in real-life economic transactions is described by Goldberg [1976]:

> *The pure discrete transaction of economic theory involves the contemporaneous exchange of claims or rights between the contracting parties. The identity of the parties and the social milieu within which the contract is consummated are irrelevant. The exchange is*

> *cloaked in anonymity with one party selling to the market and the other buying from the market. This is an extreme caricature of contract and in its purest form it has no real world counterpart. Contract typically involves the projection of exchange into the future, with contemporaneous exchange as a special case. Entering into a contract will generally entail placing restrictions on the contracting parties' future options. Freedom of contract is the freedom to impose restrictions on one's future behavior.*

The goal of this dissertation is to follow up on this statement and assess the role of identity in public-private relationships. Topical questions are the following: How does repeatedly dealing with the same partner affect the quality of an outsourcing relationship? How do changes within contracting organizations (both public and private) disrupt those relationships? The remainder of this introduction sets the background for the empirical research performed throughout this dissertation. Section 1 discusses the role given to identity in French public contract law through the concept of *intuitu personae*, which is used by legal scholars and practitioners to describe cases where identity of parties in a contract is particularly important. Section 2 gives an overview of the main economic theories regarding the role of identity in public-private relationship, including topics such as reputation, relational contracting and favoritism. Section 3 assesses the state of current empirical research on the matter, discussing the identification of relational contracts in public contracting, the role of reputation and the role of discretion in public procurement awarding procedures. It also points out gaps in the literature that this dissertation will, in part, attempt to fill. Finally, section 4 summarizes the three chapters of the dissertation.

Intuitu personae: when identity matters in French public contract law

The concept of *intuitu personae* has long been used by French-speaking legal scholars and practitioners to describe a class of contracts in which identity of one or both of parties is of particular relevance[2]. More specifically, *intuitu personae* encompasses all transactions "in which the identity of one of the parties is considered essential [...] due to their specific

[2]Occurrences of the term can be found in sources as early as Delvincourt [1825].

aptitudes, to the nature of the service expected from them, etc." [Cornu, 2016][3]. In private contracting, *intuitu personae* entails a series of legal consequences: the inability for judges to force performance of the contract by a third party when the contractor fails to do so, automatic termination of the contract in case performance is made impossible for the chosen contractor, and the ability to unilaterally terminate the contract for the party who has contracted *intuitu personae* [Krajeski, 1998]. The spirit of these particularities is to ensure performance by the chosen partner only, underlying the importance of identity in such contracts. *Intuitu personae* is not limited to private contracting. Regarding concession contracts, or *délégations de service public* (DSPs)[4], French administrative case law has traditionally been adamant in asserting that public authorities should be able to freely choose their contractors based on their individual characteristics[5]. Alibert [1990] relates this discretionary power with the notion of *intuitu personae*:

> *intuitus personae [...] means that the public authority possesses discretionary power in choosing its concessionnaire, and that this ability is not restricted by the obligation of a prior call for competition.*

The emphasis placed by French public law on *intuitu personae* in the case of concession contracts contrasts with the legal monitoring of the awarding of *marchés publics*, that is, standard procurement contracts. As soon as 1836, a decree regulating public buying was passed, introducing the necessity of publicly issued calls for tenders in State procurement. The *Code des marchés publics* (Code of public procurement) was introduced in 1964. The organization of call for tenders in standard procurement is subject to strict procedural requirements and to the application of principles of competition, freedom of access, equal treatment and equal access.

Although initially submitted to opposing regulatory frameworks (strict awarding rules for standard procurement, total discretion for concessions), the two main categories of public con-

[3] All translations from French are the author's.

[4] The French notion of *délégation de service public* roughly corresponds with the EU notion of a concession, i.e. a long term contract where risk is transferred to the operator. For the remainder of this introduction, I will use the word concession to refer to these contracts.

[5] In a ruling dated from July 23rd, 1909, the *Conseil d'Etat* (the French supreme administrative court) states: "The principle of competitive bidding is incompatible with the nature of a concession contract, as the concessionaire must be selected based on their person, their qualities, and the guarantees they provide." Several decades later, the principle remains intact: "As a rule, the public authority freely chooses the concessionnaire" (Conseil d'Etat, April 16th, 1986, "*Compagnie luxembourgeoise de télédiffusion*".)

tracts have undergone somewhat of a legal convergence during the late twentieth and early twenty-first centuries. On one hand, lighter awarding procedures for *marchés publics* have been introduced in the case of low value goods and service (an "adapted" procedure was introduced in 2004), and negotiation-based awarding procedures have been promoted (the "competitive dialogue" procedure was also introduced in 2004). On the other, legal requirements concerning the award of concession contracts have progressively been tightened. In 1993, the "Sapin" Act introduced publicity and open competition requirements for concessions. The 2014/23 EU directive (transposed in French law in 2016) continued the push towards more competition, imposing that public authorities to publish a "concession notice", set a list of awarding criteria, fix a delay for the reception of tenders, and restricting the duration of concessions in order to submit operation of public services to more frequent competition. Although concessions remain subject to more lenient requirements than standard procurement contracts, this convergence has driven some scholars to believe that "concession contracts are no longer signed *intuitu personae*, that is to say freely, but in compliance with the general principles of public procurement." [Capitant, 2018]. Capitant [2018] goes as far as stating:

> *The current state of the law is probably only a step in the fusion of rules governing public procurement and concessions. Indeed, no solid basis can be found for the subsisting difference in regime [...] between the two types of contracts.*

The very foundations of the difference in regime between concessions and standard procurement contracts thus appear contested, both by scholars and by law. Indeed, the definitional distinction between concessions and standard procurement does not directly appear to call for separate awarding procedures: *Délégations de service publics* (concessions) are defined by the fact that the private operator bears the financial risk from operating the public service and collects revenue from said operation. They are generally used for the long-term provision of a service (e.g. water supply, highways). *Marchés publics* (standard procurement) are contracts by which a public administration purchases a specific, well-defined good or service and immediately pays the price. They generally serve short-duration or immediate purposes (provision of physical goods or short-lasting services such as construction and maintenance works)[6]. There is *a priori* no direct reason for concession contracts to be exempted from

[6]The fundamental criterion to distinguish between *marchés publics* and *délégations de service public* is the mode of compensation of the contracting firm: a *délégations de service public* (concession) is defined by the fact that a substantial share of the contractor's compensation is composed of revenues from operating

competitive procedures.

There is however, a correlation between the usual characteristics of concessions (long duration, less oversight from the public administration) and some features which render competition less potent according to economic theory: contractual incompleteness, asymmetric information, investment of specific assets. Theory has also shown that non-market mechanisms, where identity of the partner enters into play (reputation, relational contracts) may be optimal in the presence of the aforementioned market imperfections. The fundamental question asked here is thus the following: what are the economic justifications for allowing public contracts to be awarded *intuitu personae*? In other words, **in which cases can individual characteristics of the partner observed by the public authority be considered better of a selection mechanism than competitive bidding?**

The following section reviews the theoretical economic literature on the matter, and the potential justifications for why identity of the partner may be of relevance in the awarding of public contracts.

Why should identity matter? Answers from economic theory

Competitive bidding may be inefficient

Awarding procedures can be ranked from the most anonymous (price-based, sealed bid auctions) to the most identity-based (perfect discretion with no legal oversight). In between lies a continuum of mechanisms such as scoring rule auctions, competitive dialogue, and the current awarding mechanism for French concessions (the public authority selects the winner but must justify that the chosen offer was the most competitive for objective reasons).

While price-based competition is closest to the spirit of neoclassical economic theory, and has been promoted as the efficient way of choosing partners for public administrations [Demsetz, 1968; Stigler, 1968; Posner, 1972], various economic frameworks have demonstrated the

the service (*Conseil d'Etat*, April 15th 1996, "*Préfet des Bouches-du-Rhône*.")

inefficiency of market mechanisms under some conditions. Incentive theory, relying on the fundamental assumption of asymmetric information, has shown auction mechanisms to generate an informational rent for the contractor [Laffont and Tirole, 1986, 1987], and to reduce incentives to invest in the case of repeated auctions [Laffont and Tirole, 1988]. More importantly, competitive bidding becomes inefficient in the presence of noncontractible aspects of performance, where competing firms may bid below their expected costs and renegotiate *ex post* to more advantageous conditions [Hart and Moore, 1988; Bajari et al., 2014; Herweg and Schmidt, 2017].

Reputation and relational contracts as substitutes to competitive tendering

In the presence of incomplete contracts, theory has demonstrated the efficiency of nonmarket mechanisms relying strongly on identity of the parties. The foundational work in this stream of research is generally considered to be Macaulay [1963], who shows that written contracts are merely a secondary tool in managing economic relationships, whereas informal agreements supported by social norms, reputation and threats of being refused future deals are predominant. Further qualitative studies [Bernstein, 1992, 2016] have confirmed the generality of such finding in a variety of industries. A concurrent research avenue was initiated by MacNeil [1978], who also insisted on the necessity of noncontractual adaptation mechanisms in long-term relationships:

> *Two common characteristics of long-term contracts are the existence of gaps in their planning and the presence of a range of processes and techniques used by contract planners to create flexibility in lieu of either leaving gaps or trying to plan rigidly.*

This line of thought has later been transposed to microeconomics, giving birth to Relational Contract Theory (RCT), with seminal works such as Telser [1980] and Bull [1987] instilling the concept of implicit agreements supported by the present discounted value of remaining in the relationship. Precisely, a relational contract refers to an agreement wherein outcomes are not necessarily observable by third parties, but where parties comply because the benefits from deviating are smaller than the discounted gains from cooperating and remaining in business.

The element of repetition at the ability to credibly commit to remaining in a relationship are thus crucial to the existence of relational contracts. Additionally, the transaction cost economics (TCE) framework has emphasized the need for nonmarket, hybrid structures in the presence of specific assets and incomplete contracts: Williamson [1976] underlines the inefficiency of repeated auctions in such circumstances, and Williamson [1983] recommends parties to commit to long term cooperation through the exchange of "hostages", which are incompatible with the periodical re-awarding of contracts. The concept of hybrid structure was later introduced in Williamson [1985] to describe a class of long term, highly integrated relationships, distinct from price-based spot contracting. While the concepts of hybrid forms and relational contracts have been developed in distinct avenues of the literature, it is worth noting that they do not necessarily refer to distinct empirical structures: Bernstein and Peterson [2023] argue that long term supplier-manufacturer relationships supported -in part- by relational contracts constitute an "economic hybrid".

Both relational contracts and hybrid structures appear incompatible with the idea of periodically re-awarding public contracts through formal auctions: they require the ability for a public buyer to freely select contractors based on potentially unverifiable criteria and prospects of future business. As such, industries where formal contracting is inefficient appear to require high discretion awarding procedures. It should be noted however that relational contracting is not in complete opposition with auction mechanisms: "invited" auction mechanisms, where bidders can discretionally be excluded (such as studied in Calzolari and Spagnolo [2020]) mix aspects of competition with relational contracting.

Favoritism and undesirable effects of identity-based procurement

To foster the development of relational contracts and reliance on reputation in public contracting, some amount of buyer discretion appears essential. Discretion however runs into the risk of favoritism, which is one of the primary reasons for regulating procurement[7]. Indeed, if one lifts the assumption of public organizations as "black boxes" acting for the general interests, and admits that they are before all constituted of individuals seeking private benefits [Buchanan and Tullock, 1962], discretionary procurement generates strong inefficiencies.

[7]The "Sapin" Act of 1993 was explicitly directed at reducing corrupt practices in procurement.

Public officials may have incentives to award contracts to a given firm in exchange for bribes [Burguet and Che, 2004; Compte et al., 2005]. Rather than being fueled by corruption, favoritism in the presence of discretion can also simply stem from switching and search costs associated finding new partners, and may be in this case associated with insufficient incentives for public administrations to lower their costs.

The implementation of favoritism is straightforward in purely discretionary settings such as the former procedure for French concessions. However, even market-based mechanisms may be distorted to implement favoritism: Arozamena et al. [2023] show that *ex post* renegotiation allows the favored company to underbid during the auction stage. Laffont and Tirole [1991] show that in scoring rule auctions, public buyers may set the weights of awarding criteria in order to favor a preferred candidate, making price-based auctions more socially valuable. All in all, it remains that stronger regulations on awarding procedures should increase the cost of favoritism for public buyers.

The general conclusion of this section is that the role of identity in procurement is ambiguous from a theoretical perspective. While it may be optimal to build relationships of trust and mutual dependency in the presence of incomplete contracts, favoritism and corruption may undermine the efficiency of identity-based awarding procedures. The following section provides an overview of empirical evidence, and highlights some of the remaining gaps to be filled.

State of the art in empirical research and further avenues

The role of identity in public procurement: existing empirical evidence

Relational contracts and reputation in public procurement. A strand of the empirical literature has attempted to identify relational contracts and reputational effects through various proxies for relationship value. Few papers have attempted to identify relational contracts directly, measuring the present discounted value (PDV) of future business. In private contracting, such studies are a blossoming topic [Macchiavello and Morjaria, 2015, 2020]. To my knowledge, the only study in a procurement setting using a proxy for future business oppor-

tunities is Gil and Marion [2013], who find that relational contracting significantly decreases bid levels in highway procurement subcontracting. Indirect proxies for the existence of a relational contract include the stock of past interactions, which may be extrapolated as a proxy for future interactions [Corts and Singh, 2004] or the history of litigiousness between parties [Crocker and Reynolds, 1993]. Beuve and Saussier [2021] proxy cooperative behavior by the likelihood of contract renewal and estimate the "relational" value of contract renegotiations. Desrieux et al. [2013] use bundling of public service (i.e. delegating multiple public services to a single firm) as a proxy for the existence of a relational contract. Concerning reputational incentives, Andreyanov et al. [2023] show that the ability to award contracts based on subjective measures of past performance may improve procurement outcomes.

Rules *versus* discretion: the effect of awarding procedures on procurement outcomes. Another important field of the empirical literature has studied the effects of buyer discretion on procurement outcomes, leveraging regulatory framework where under a certain value threshold, a higher discretion procurement procedure is available. Some studies suggest that high discretion is associated with relational contracts through improved procurement outcomes [Coviello et al., 2018a] and adaptation to exogenous shocks [Bafundi et al., 2023]. Other studies are more pessimistic on the effects of such policies, documenting manipulation of contract value through "bunching" below the regulatory threshold [Carril, 2022] and higher chances of contracts being attributed to politically connected firms [Baltrunaite et al., 2020; Szucs, 2023; Celis Galvez et al., 2025]. Some studies provide mixed evidence: Decarolis et al. [2020a] show that high discretion may both improve procurement auctions and foster corruption. Fazio [2025] show that discretion increases the price of goods purchased, but also their quality. Studies such as Bosio et al. [2022] and Hoekman and Onur Taş [2024] find that the benefits of discretion are conditioned by institutional quality.

Favoritism in public contracting. A distinct but closely related strand of the literature focuses on identifying favoritism *per se* using connections between firms and officials in charge of public procurement. There is overlap between the favoritism literature and the "rules *versus* discretion" literature in cases where high discretion is related with political connectedness of winning firms [Szucs, 2023; Celis Galvez et al., 2025] and on corruption of public buyers [Decarolis et al., 2020a]. Other recent empirical studies generally revolve around relating political connections of firms (or corporate connections of elected officials) with procurement

outcomes. Schoenherr [2019] identifies political connectedness of Korean State owned firm CEOs through shared networks with the President. Titl and Geys [2019] use firm donations to political parties to show that politically connected firms are more likely to win procurement auctions when their preferred party gains access to power, and Baránek and Titl [2024] show that such favoritism is socially inefficient. Titl et al. [2024] use machine learning techniques to detect political connections. A subset of the empirical studies on favoritism use disruptions in buyer-seller relationships through exogenous deaths and resignations [Brogaard et al., 2020] or electoral turnover in close races [Coviello and Gagliarducci, 2017].

Open questions for empirical research

Many general debates on the role of identity remain to be settled. These transversal topics are approached throughout the dissertation, sometimes by several chapters. The following subsection reviews these overarching matters.

The existence of informal contracting in the public sector. An important aspect of relational contracting identified in the private sector is the ability to rely on informal provisions [Macaulay, 1963; Blouin and Macchiavello, 2019; Macchiavello and Morjaria, 2015; Gil et al., 2021; Barron et al., 2020]. As a consequence, the existence of a relational contract may reduce the optimal level of reliance on written clauses, assuming that formal and informal provisions are substitutes [Baker et al., 1994; Poppo and Zenger, 2002; Kvaloy and Olsen, 2009; Desrieux and Beuve, 2011]. Studies have shown that in many industries, written provisions serve as a space in which informal agreements exist, rather than a full specification of each party's obligations [Bernstein, 1992, 2016][8]. There is evidence of relational contracting in public procurement affecting variables such as bid levels and incentive provision mechanisms [Corts and Singh, 2004; Gil and Marion, 2013; Bafundi et al., 2023]. However, there is hardly any evidence on the way repeated interactions affect the degree to which public contracts rely on (in)formal provisions. A recent strand of the literature has stressed the fact that public contract writing is subjected to specific constraints pertaining to public scrutiny and third

[8]Other motives for "voluntary" incompleteness include the cost of specifying contingencies [Dye, 1985], strategic ambiguity [Bernheim and Whinston, 1998], signaling [Spier, 1992], noisy measurement of performance [Allen and Gale, 1992], varying degrees of verifiability between tasks in multi-task contracts [Milgrom and Holmstrom, 1991], reciprocity and trust [Fehr and Gachter, 2000; Ghoshal and Moran, 1996], reference points and the constraints the pose on renegotiation [Halonen-Akatwijuka and Hart, 2013].

party challenges [Spiller, 2009; Moszoro and Spiller, 2019], thus reducing the ability for public entities to rely on informal contracting. Beuve et al. [2019] and Beuve et al. [2021] show that municipalities where public scrutiny, proxied by political contestability, is strong, do write more rigid contracts in attempts to thwart third party challenges. Another feature of public contracts is that they are, by law, subjected to higher rigidity, in that some provisions (such as price and technical specifications) must be made verifiable for the contract to be valid[9]. An open question for empirical research is the following: despite legal and institutional constraints leading to higher rigidity in public contracts, do relational contracts reduce reliance on formal clauses? This is the main topic of Chapter 1, where I use text analysis methods to identify how repeated interactions shape the writing of contracts.

The role of individuals in public-private relationships. Whether the identity of individuals matters in inter-organization relationships remains open to empirical investigation. The literature on favoritism relies heavily on identifying individual connections between elected officials and company managers. However, the literature on relational contracts has yet to determine the role of intermediaries (managers, elected officials, appointed bureaucrats) between organizations. From a theoretical standpoint, this topic has been investigated by Troya-Martinez and Wren-Lewis [2022], who present a relational contracting model where the manager of the supplying firm may collude with the buyer to obtain private benefits. In this framework, manager collusion may generate inefficiencies but may also increase the manager's commitment to the expected level of quality. This dissertation provides novel evidence on the role of individuals in public-private relationships. Chapter 1 questions whether repeatedly interacting with known managers affects the design of public contracts, assuming that individual ties help develop relational contracts, and that, conversely, manager changes constitute disruptions in relational contracts. The question of individual identity also appears in Chapter 3, which studies the effect of mayor changes on municipal procurement outcomes. Using two quasi-experiments (mayor deaths and municipal elections), it helps disentangle the effects of a change in mayor only (which happens following mayor death) and the effects of a change in municipal council (which happens in case of electoral turnover). Doing so, it contributes to determining whether favoritism is attached to the identity of the mayor or to the municipal council as a whole.

[9] Aghion and Hermalin [1990] study the question of legal restrictions on contractual provisions in an asymmetric information setting.

How does past experience shape bilateral contractual relationships? While some works have used the stock of past experience as a proxy for relational contracting, this variable remains a very imperfect indicator of how parties value future business. Indeed, a large stock of past interactions between parties can be viewed as two things: a symptom of the inability for parties to leave the relationship, in the spirit of the "lock-in" situation [Williamson, 1979], or a sign that parties are willing to sustain a long-lasting relationship due to mutual interest. Chapter 2 attempts to settle this dichotomy by studying how past experience affects renegotiation of long term contracts. More specifically, I examine whether repeated interactions increase the bargaining power of one of the parties (which a Williamsonian hold-up situation would lead to), or if on the contrary repeated interactions decrease the prevalence of self-interested renegotiation (which a relational theory of past interactions would support).

Summary of chapters

Chapter 1: Manager identity, relational contracting and the design of public-private agreements

This chapter examines how the identity of managers in a private firm affects the development of relational contracts in a public-private setting. Our theoretical framework proceeds in two steps. First, a large literature has provided evidence of the efficiency and feasibility of flexible, low rigidity written arrangements in the presence of relational contracts. Second, I hypothesize that dealing with an unknown manager increases the municipality's uncertainty with regards to the supplier's valuation of the relationship, thus undermining the feasibility of relational contracts. I combine both steps to form a proposition relating manager identity to contract rigidity through the intermediary of relational contracting. Specifically, I examine whether, when a municipality has not dealt with a manager (or set of managers) in the past, contracts between both parties are more rigid. I also test whether some mediating factors, such as contract type and public scrutiny, which are expected to affect the scope of relational contracting, influence the relationship between manager identity and contract rigidity.

I use a data base from the parking lot sector to examine whether changes in management

do affect the way contracts are written. I build on Beuve et al. [2019] and construct a new set of text-based measures of rigidity, which attempt to correct some shortcomings of the previous approach. Results suggest that contracts signed with unknown managers are more likely to contain clauses pertaining to penalties and monitoring of the contractor, potentially indicating distrust and a lesser extent of relational contracts. When interacting the identity of the manager with contract type, I find that the effect is driven by DSP (concession) contracts, which are more subject to incompleteness, and may thus be more prone to relational contracting. Finally, interacting manager identity with proxies for public scrutiny, I find no evidence that public scrutiny plays a mediating role.

Overall, this chapter present some evidence that relational contracting may occur in public procurement settings, despite the institutional constraints making them less pervasive than in the private sector.

Chapter 2: Bargaining power, renegotiation and repeated interactions: an empirical study of long term public contracts

In this chapter, I study how long term history between parties to a public contract affects renegotiation. I propose two competing theoretical frameworks to analyze the role of past experience. Under a transaction cost inspired theory, repeated interactions may signify a lock-in situation wherein one (or both) of parties have invested specific assets in the relationship and are unable to leave the relationship due to excessive switching costs. In this context, parties will attempt to capture quasi rents through opportunistic renegotiation. On the contrary, one may analyze repeated interactions as evidence that a relational contract is ongoing, wherein opportunistic behavior is deterred by prospects of future business. Under this framework, parties may refrain from engaging in self interested renegotiations in order to preserve the relationship.

I use the data from the parking lot sector used in the first chapter to examine which of these theoretical frameworks appears more likely to enter into play. I use hand-coded renegotiation outcomes to precisely determine the content of each amendment. I focus on long term concession contracts and transform the data set into a panel in order to control for contract

heterogeneity with contract fixed effects. Because of this data structure, it is impossible to directly estimate the effect of past experience. Thus, I use variables destined to capture demand for renegotiation on the municipality side and on the company side, which I then interact with the uninterrupted time in business between parties. To proxy municipality demand for renegotiation, I use debt variations, which are expected to lead the municipality to push for financial renegotiation of the contract. On the company side, I leverage the multi contract structure of the data, i.e. the fact that within a city-company history, multiple contracts may be ongoing simultaneously. I posit that the company is more likely to attempt self-interested renegotiation quickly after being awarded a new contract than before, and thus use these "side" contracts to proxy company demand for renegotiation.

Preliminary results indicate that the variables used to proxy demand for renegotiation on each side are positively correlated with renegotiation outcomes that can plausibly be interpreted as self-interested. When interacting these proxies with past experience, two main findings emerge. First, in longstanding relationships, municipalities are less likely to obtain financial renegotiation in times of debt increase. Second, past experience increases the likelihood that contractors obtain favorable renegotiations after being awarded side contracts. These results thus seem to indicate that contractor bargaining power increases with time in business, potentially indicating dependence of municipalities to their operator when parties have been contracting for extended periods of time.

Chapter 3: Mayor changes and competition: and empirical study of French municipal procurement (2015-2023)

In the final chapter, we[10] examine how exogenous changes in identity of French mayors affect public procurement calls for tenders. We use a novel data set of public procurement contracts awarded by French municipalities between 2015 and 2023, which provides information on the type of good or service procured, the number of bidders who answered the call, the price paid, the type of procedure used, the type of publicity threshold as well as information on the identity of the winning firm. Our main prediction is that changes in mayor should disrupt previously established connections, thus fostering competition, and reducing reliance on local

[10] This chapter is co-authored with Adrien Deschamps (University of Avignon).

firms or firms which won previous contracts.

In the first part of the empirical analysis, we consider the effect of mayor deaths on procurement outcomes, using a staggered diff-in-diff design. Data on mayor deaths was collected thanks to the recently constituted BREF [Labatut et al., 2020] dataset. Results are overall inconclusive, potentially due to the small size of the treated sample, and do not allow us to make definitive conclusions on the procurement effects of mayor deaths.

In the second part of the empirical analysis, we follow Coviello and Gagliarducci [2017] and implement a regression discontinuity (RD) design using results from the 2014 and 2020 municipal elections. The goal of this design is to rely on close races, for which the result of the election is as good as random. We estimate the effect both parametrically at the contract level, and nonparametrically at the city-term level, using modern nonparametric estimators. In both cases, we find robust evidence that electoral turnovers increase competition in calls for tender and reduce the likelihood that local bidders win contracts. We also provide evidence that this effect is stronger in municipalities where the defeated mayor was in office for extended periods of time. These results confirm the prediction that incumbent mayors, especially longstanding ones, may develop connections with firms and potential relations of favoritism.

Table 1: Summary of chapters

Chapter and research question	Data and methodology	Main results
▪ Chapter 1: Manager identity, relational contracting and the design of public-private agreements ▪ Research question: do changes in the identity of private company managers affect the rigidity contracts signed with municipalities?	▪ Database from the French parking lot industry (N = 398) ▪ Text based measures of contract rigidity ▪ OLS with control variables and fixed effects on cross sectional data	▪ Contracts signed with known managers are less likely to regulate penalties and strictly monitor the contractor's behavior ▪ This effect is driven by DSP (concession) contracts, which are characterized by longer duration and higher incompleteness ▪ Public scrutiny does not significantly affect this relational effect
▪ Chapter 2: Bargaining power, renegotiation and repeated interactions: an empirical study of long-term public contracts ▪ Research question: how do repeated interactions between a public entity and a private firm affect parties' bargaining powers in contract renegotiations?	▪ Data: data from parking lot sector in panel format with hand-coded renegotiation outcomes (N = 281, T = 46) ▪ TWFE models, interacting past experience with proxies for buyer initiated renegotiations (municipal debt increases) and supplier initiated renegotiation (side contracts)	▪ Both proxies (municipal debt and side contracts) are positively correlated with renegotiation likelihood ▪ Past experience between parties reduces the prevalence of financial renegotiations in reaction to municipal debt increases ▪ Past experience between parties increases the likelihood of contractor-favorable renegotiation following a side contract

Summary of chapters (continued)

Chapter and research question	Data and methodology	Main results
▪ Chapter 3: Mayor changes and competition: and empirical study of French municipal procurement (2015-2023) ▪ Research question: do exogenous changes in mayor identity increase the competitiveness of public procurement calls for tender?	▪ Dataset containing all public procurement contract notices published by French municipalities between 2015 and 2023 (N = 44,020), complemented with electoral data as well as city and mayor characteristics. ▪ Diff-in-diff using mayor death as an exogenous source of change in mayor ▪ Regression discontinuity design using 2014 and 2020 municipal election results	▪ The effects of mayor deaths are heterogeneous, potentially due to small sample size ▪ Robust evidence that electoral turnovers in close races increase the average number of bidders and decrease the likelihood that local firms win procurement contracts

1 Manager identity, relational contracting and the design of public-private agreements[1]

1.1 Introduction

The tradeoff between buyer discretion and strict awarding procedures is an essential debate in the design of public procurement policies. Recent contributions include Carril [2022], who studies the effect of a change in the threshold for high discretion procedures on various procurement outcomes and shows that increased discretion may increase quality. Bosio et al. [2022] show in a cross-country study that the effect of increased procurement discretion varies according to institutional parameters: high discretion may improve efficiency, but only in countries with high public sector capacity. On the other hand, Szucs [2023] documents a spike in favoritism following the introduction of a high discretion procurement procedure in Hungary, leading to worsened procurement outcomes.

The rationale behind high discretion procurement policies stems from various theoretical arguments, of which two main examples can be given: the inefficiency of highly formalized auctions in the presence of incomplete contracts [Bajari et al., 2014; Herweg and Schmidt, 2017] and the ability to rely on self-enforcing, relational contracts when repeated interactions are possible [Levin, 2003; Malcomson, 2012]. The latter will be the focus of this paper. The role of relational contracts as a disciplining tool in public procurement has already been the

[1]I am grateful to participants of IOEA 2023, JMA 2024, AFED 2024 and the Government and Regulation Seminar at University Paris-Dauphine for comments on this work.

subject of several studies: existing works have documented the effect of relational contracts on adaptation to unforeseen events [Bafundi et al., 2023], on the choice between cost-plus and fixed price arrangements [Corts and Singh, 2004], on bid levels in procurement auctions [Gil and Marion, 2013] and on the choice of an optimal awarding procedure [Albano et al., 2017; Calzolari and Spagnolo, 2020].

This paper contributes to the literature on relational contracting in public procurement by documenting the extent to which relational contracts may help reduce transaction costs through the development of flexible, low rigidity contracts, where some aspects of performance remain unspecified in the written contract. Studies on the private sector have indeed stressed that repeated interaction and reputational threats may lead to highly informal contracts being viable [Bernstein, 1992; Gil, 2011; Halonen-Akatwijuka and Hart, 2013]. The literature on public contracts has, however, highlighted that, because of their specific nature, public contracts are subject to public scrutiny which may generate incentives to invest in high rigidity, independently from the existence or nonexistence of a relational contract [Spiller, 2009]. Empirical studies confirm that public contracts are indeed more rigid than their equivalent private counterpart [Moszoro et al., 2016], and that said rigidity is increasing in the expected level of public scrutiny [Beuve et al., 2019]. From a policy standpoint, reduced rigidity may be associated with several positive outcomes: higher powered incentives [Spiller, 2009], a decrease in "ink" costs, in bargaining costs, in legal enforcement costs, and most importantly, in renegotiation costs [Beuve et al., 2021]. High reliance on formal clauses, which implies litigation costs in case of breach, monitoring costs to collect proof of said breaches, and contract modification costs, may be a determining factor in the tradeoff between "make-or-buy" for public administrations: the role of transaction costs in deciding between in-house production and delegation is perhaps the oldest finding in organizational economics [Coase, 1937]. It should however be noted that rigidity is not systematically considered to penalize efficiency: Aghion and Hermalin [1990] develop a model in which legal requirements on contracting (e.g. making it compulsory to have a written provision on a specific matter) can force parties to reveal their information and thus improve contractual outcomes.

The goal of this paper is to examine whether relational contracts still play a role in reducing reliance on formal provisions despite the specific institutional constraints associated with public contracting. Because relational contracts are by design unobservable, I use a proxy

variable, manager changes, which is expected to affect the extent of relational contracting. Indeed, relational contracts rely on knowledge of the other party's valuation of the relationship. I posit that dealing with an unknown manager introduces uncertainty with regards to the contractor's valuation of the relationship, and thus deters the municipality from extensively relying on relational contracting. The first proposition tested is that manager changes reduce reliance on relational contracting, and thus increase reliance on formal clauses, leading to more rigidly written contracts. I test this proposition on a data set from the French parking lot sector. To proxy investment in written, verifiable clauses, I construct a text-based measure of contract rigidity. I leverage changes in the identity of company managers to empirically identify disruptions in relational contracts, and I find some evidence that contracts are more rigidly written when public buyers are dealing with managers they have not dealt with in the past, although this effect is restricted to a subset of rigidity dimensions.

A second set of propositions relates manager changes with the institutional conditions for feasibility of relational contracts in the public sector. Indeed, both strict awarding rules and public scrutiny [Spiller, 2009] prevent reliance on informal contracting in public procurement. The former acts on the credibility of the relationship continuity promise, which is crucial to relational contracts, by potentially allowing competitors to be awarded contracts when the buyer is deprived from discretion. The latter acts on the ability for parties, even if engaged in a continued relationship, to rely on flexible agreements, as gaps in contracts present opportunities for judicial and political challenges by interested third parties. I thus test the hypothesis that the effect of manager changes is less intense in settings where awarding procedures are highly restrictive, and where public scrutiny (which reinforces the likelihood of third party challenges) are more likely. Results show that the class of contracts which are awarded with high discretion (*délégations de service public* or DSPs, which roughly correspond with concessions) drive the main effect, while *marchés publics* (MP, standard procurement contracts), which are awarded through more formal procedures, appear insensitive to manager changes. Although confounding factors enter into play (MP and DSP contracts differ vastly in many characteristics such as duration, incentive provision and average level of rigidity), these result provide some support to the hypothesis. On the other hand, I do not find a significant mediating effect of public scrutiny on the relationship between manager changes and rigidity.

From a policy perspective, this study provides partial support to the idea that high discretion

awarding procedures allow parties to economize on transaction costs, through the development of low-rigidity contracts with a relational component. Systematic studies on the matter should however balance out the expected benefits of high discretion (relational contracting, reduction of transaction costs) with its drawbacks (lower competition, corruption). Recent works on the matter seem to indicate that increased discretion, in general, is not necessarily associated with lower performance [Coviello et al., 2018a; Decarolis et al., 2020a; Carril, 2022], especially when it comes to developed countries [Bosio et al., 2022]. This paper also supports the idea that public administrations should form partnerships with companies characterized by stable management, allowing for the development of relational contracts. While managerial stability has been identified as a source of efficiency within public organizations [Meier and O'Toole, 1999, 2007, 2011], little evidence exists on the effect of managerial stability within private partners of public organizations.

The welfare implications of reduced rigidity remain to be identified precisely by empirical research. From a theoretical standpoint, Spiller [2009] underlines that rigidity leads to "difficulties in adapting to shocks and [...] to low-powered incentives". Concerning the adaptation issues, there is evidence of increased renegotiation likelihood in the presence of high rigidity [Beuve et al., 2021]. Regarding the incentive aspect, there is to this day no empirical evidence on the fact that rigidity generates productive inefficiencies. Future work should attempt to relate rigidity with outcomes indicating the quality of the procurement relationship, such as realized costs compared with an expected costs. Additionally, it should be recalled that the focal point of Spiller [2009]'s analysis is in fact to justify that high rigidity is rational in the presence of government and third party opportunism. A fully fledged analysis of relational contracting and rigidity should thus measure both the productive benefits of low rigidity and its costs in terms opportunism.

The paper is structured as follows. Section 2 presents the theoretical framework relating repeated interactions with lower rigidity. Section 3 describes the data collection process. Section 4 presents the empirical evidence for the role of repeated interactions with managers and decreased rigidity, as well as the interplay of relational contracting with institutional factors. Section 5 concludes.

1.2 Literature and testable propositions

The theoretical framework tested in this article can be summarized simply in two questions: do manager changes disrupt relational contracts? Do disruptions in relational contracts translate into less rigidly written contracts? Relational contracts are by essence, unobservable, thus we test whether manager changes affect contract design in a way to simultaneously answer both of these question. The underlying assumption is that manager changes do not affect contract design through other channels than relational contracts, conditional on time fixed effects which should account for general trends in the way parties draft contracts, and other observable contract characteristics.

1.2.1 Relational contracting, manager identity and contract rigidity

Relational contracts and reliance on formal clauses. An essential question in this work is whether the existence of relational contracts reduces investment in written clauses. In the presence of a relational contract, rigidly planning out performance generates transaction costs that may be unnecessary if the appropriate level of performance can be enforced through a self-enforcing agreement, explaining Macaulay [1963]'s fundamental finding that business relationships rely only marginally on written contracts. These transaction costs include "ink" costs of writing the contract *ex ante*, but also the costs associated with renegotiating the provisions if the initial clauses are not adapted to the state of the world. Indeed, under a flexible, relational agreement, performance details can be specified in real time and do not require modifications of the formal contract. From a microeconomic perspective, Kvaloy and Olsen [2009] present a relational contracting model where the principal may invest in written clauses which allow performance to be verifiable in court with some probability. When considering a quadratic transaction cost function, they show that trust (proxied by a high discount factor) decreases the optimal level of investment in written clauses, thus showing a substitution effect between written clauses and the extent of relational contracting. The third party opportunism (TPO) literature makes a similar assumption, although approached negatively: works such as Beuve et al. [2019, 2021] consider that rigidity in public contracts are indicative of the absence of implicit contracts, and that informal adjustment mechanisms are replaced by formal renegotiations. Additionally, a literature relying on behavioral assumptions

supports the idea that rigid contracts may generate distrust [Ghoshal and Moran, 1996; Fehr and Gachter, 2000], thus going in the direction of a substitution effect between written contract and informal agreements.

However, the relationship between formal and relational contracts is not purely monotonic, i.e. relational contracts are not systematically associated with less reliance on formal contracts[2]. From a theoretical perspective, a strand of the literature has insisted on the necessity of some formal provisions to make relational contracts enforceable: this is the case in Klein [2000], where the formal contract acts on the cooperation constraint of the relational contract by modifying the profits obtained from deviation. From an empirical perspective, various works show that written provisions may remain important in the presence of relational contracts. Frydlinger et al. [2019] and Frydlinger and Hart [2023] provide evidence that guiding principles are essential to support relational contracts based on reference points. Bernstein and Peterson [2023] argue that in supplier-manufacturer relationships, strict written clauses remain necessary despite some provisions being enforced informally. Specifically, they show the importance of "managerial" provisions, that is "contract administration mechanisms that support them have the potential to increase the likelihood that cooperative contracting relationships [...] arise and endure." Some studies find that repeated interactions increase the level of contractual detail [Poppo and Zenger, 2002; Vanneste and Puranam, 2010]. It should be noted that the effect of repeated interactions in Vanneste and Puranam [2010] concerns the level of technical detail: legal detail, on the other hand, decreases as party repeatedly interact, a finding consistent with the idea that legal monitoring decreases with relational contracting.

Despite evidence of complementarity between formal and informal contract, this work builds essentially on the hypothesis of substitutability, assuming that a reduction in the ability to rely on informal clauses is met with an increase in rigidity.

Manager identity and relational contracting. The second stage of the theoretical framework in this paper is to consider the effect of manager identity on the feasibility of relational contracts. An essential feature in relational contracts is the fact that parties value the relationship sufficiently to comply with the unwritten terms of the agreement. Theoretical models show that certainty about the other party's valuation of the relationship is crucial: Halac [2011] writes that "uncertainty about a firm's prospects can render relational contracts

[2] A survey of the formal *versus* informal contracting literature is provided in Gil and Zanarone [2017]

ineffective." This paper questions whether changes in the identity of the manager a municipality is dealing with may disrupt relational contracts. The study of the role of managers in relational contracts is a rather novel topic in research. It has been investigated theoretically by Troya-Martinez and Wren-Lewis [2022]. One of the key findings of this study is that when engaged in a relational contract, the manager of the supplying company may collude with the buyer, and that this collusion may enhance the incentives of the seller to provide adequate performance. Although I do not assume necessary collusion between the manager and the municipality for the relationship, I argue that time may be necessary to build trust and learn about the manager's valuation of the relationship, and their potential will to comply with a relational contract. One can thus assume municipalities to be more willing to rely on relational terms when dealing with managers they have already interacted with in the past. The phenomenon of gradual learning about preferences is described in studies such as Watson [1999], where parties progressively increase the stakes of the relationship as they learn about each other's valuation of this relationship.

Two theoretical intuitions are hence the starting point of our work: first, relational contracts reduce reliance on written clauses. Second, dealing with an unknown manager reduces reliance on relational contracts. Combining both propositions leads to an operational, testable proposition:

Proposition 1. *Contract rigidity is higher when a municipality has not dealt in the past with current company managers.*

This reduced form proposition imposes an exclusion restriction, i.e. that the effect of manager changes on rigidity stems only from relational contracting. An alternative explanation to the fact that rigidity varies with manager identity could be that certain managers are prone to writing more/less rigid contracts. To answer this remark, it should first be stressed that managers in large companies are rarely in charge of directly drafting contracts, which reduces the plausibility of such an effect. I assume that the effect of manager changes on rigidity comes not from the company side, but primarily from reduced trust on the municipality side, who demands more guarantees, or "safety nets" through rigid contractual clauses. Indeed, a procurement contract is primarily an agency relationship where the public authority is the principal, and has more interest in strictly monitoring the contractor's behavior. In addition,

the use of time fixed effects allows to control for general firm level trends in the way contracts are written, thus controlling for any general change in the contracting strategy of the company associated with the identity of managers. The idea that rigidity is primarily demanded by the municipality also explains why the proposition is framed subjectively: I consider that trust in the company stems from the bilateral history with manager(s), but not from total manager history. In other words, I do not consider reputational spillovers from the manager's behavior in contracts with other municipalities.

1.2.2 Institutional determinants of relational contracting

In public procurement, the feasibility of relational contracting is highly restricted. Pure relational contracting is not available, and Beuve and Saussier [2025] write that "to rely on relational contracts in the public domain is more complicated than in the private one, if not unfeasible". There is however evidence of relational dynamics in public procurement, occurring through the choice of high discretion awarding procedures when available [Bafundi et al., 2023], through renegotiation as a means of preserving the continuation of the relationship [Beuve and Saussier, 2021], or through the bundled award of various public services to a single operator [Desrieux et al., 2013]. A fundamental assumption made in this paper is that public procurement still leaves space for informality, as some types of clauses may be included or not at the discretion of the contracting parties. As will be exhibited in the empirical part, there is variability in the amount of legal monitoring occurring, be it on the planning of judicial procedures, the planning of penalties and remedies in case of breaches, the amount of reporting that operators must perform, etc. Moreover, despite papers such as Beuve et al. [2019] arguing against relational contracting in the public sector, the variability that their rigidity scores exhibit also testify of the existence of a "relational space", i.e. parts of agreements that may be left unwritten at will.

This study tests whether the institutional factors which are expected to relational contracting play a mediating role in the relationship between manager changes and rigidity. Indeed, the feasibility of relational contracting in the public sector is conditioned by several elements. First, some freedom of choice in the identity of the contracting firm is necessary. Because the awarding procedure for DSP contracts is more lenient than than for MP contracts, and

generally more negotiation based, it is expected that the "relational" effect of manager changes is driven by DSP contracts rather than standard procurement deals (MP contracts):

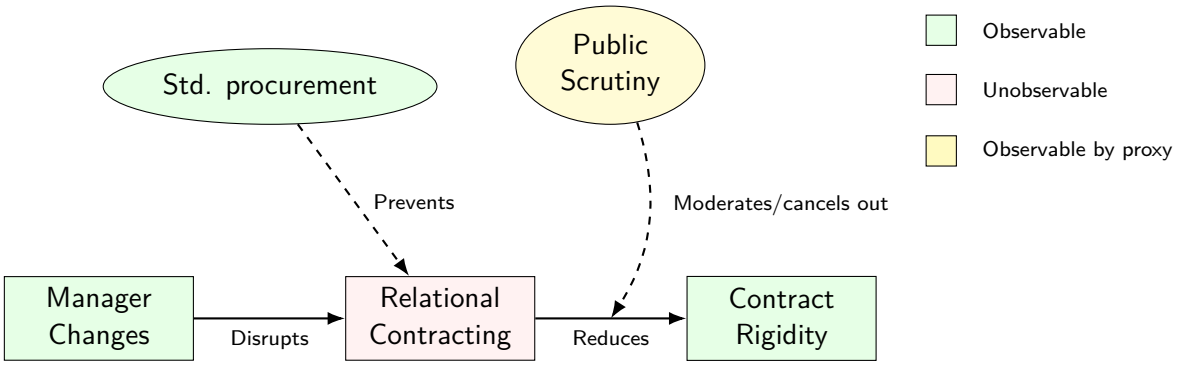
Proposition 2. *The effect of manager identity on contract rigidity is stronger for DSP contracts than for MP contracts.*

Additionally, the third party opportunism (TPO) framework has stressed the fact that public contract rigidity is not only driven by characteristics of the bilateral relationship (such as the existence of an implicit agreement), but also by the likelihood of third party challenge. Following Beuve et al. [2019], political contestability is used to proxy the risk of third party challenge, and is thus expected to reduce the effect of manager identity on rigidity:

Proposition 3. *The effect of manager identity on contract rigidity is stronger for cities undergoing low political contestability.*

The theoretical framework tested in this study is summarized in figure 1.1.

Figure 1.1: Theoretical framework: visual summary



Source: Author.

1.3 Data

1.3.1 General presentation of the data

The initial database is comprised of 471 contracts between French municipalities and a single firm, for the construction and/or operation of parking lots. Two main types of contracts are to be distinguished. "*Marchés publics*" (MP) contracts, are usually short term contracts concerned only with the operation of parking lots, where the demand risk is borne by the buyer (the municipality). "*Délégations de service public*" (DSP) are long term contracts that may govern both the construction and operation of the parking lot (in which case they are called concessions), usually long term and where the operator bears all or part of the demand risk. We argue that MP contracts are less prone to the identification of a relational effect since they are much shorter, and regulated by stricter procedures. DSP contracts last on average 26.12 years while MP contracts last on average 2.89 years: it is clear that DSP contracts are more prone to the emergence of relational contracts. Finally, and most importantly, MP contracts are governed by strict procurement regulations which generate (at least in theory) much less discretion in the awarding procedure.

1.3.2 Rigidity scores

Construction of the dictionary. The set of rigidity variables is based on a dictionary of search terms, which was constructed upon reading a set of contracts and identifying recurring terms which indicated strict monitoring of quality, delays, as well as legal remedies in case of breach of the contract. In total, eleven rigidity categories are defined. "Penalties" deals with the financial sanctions imposed by the municipality in case of contractual failure on the contractor side. "Faults" contains terms pertaining to noncompliance of the contractor to provisions. "Termination" deals with the instances where the contract is terminated before its planned term. "Delays" concerns delays in construction. "Monitoring" contains a list of terms indicating that the municipality exercises oversight over the contractor's behavior. "State of infrastructure" contains terms relative to the physical qualities of the infrastructure. "Litigation" concerns words which pertain to judicial procedures. "Revision" contains clauses

dealing with the renegotiation process, indicating formal tools to adapt the contract (thus excluding informal adjustments). "Authorizations and permits" deals with instances where the contractor requires administrative approval to conduct a specific task, indicating strict oversight. "Records and stats" contains terms pertaining to reporting that must be performed by the contractor, also indicating strong oversight. Overall, these search terms are designed to capture lack of trust, reliance on written tools and on court enforcement of strictly monitored provisions. This dictionary and the categories differs from the one in Beuve et al. [2019] but remains close to its general spirit. Table 1.1 presents the dictionary of search terms used.

Table 1.1: Search terms for rigidity (translated from French)

Category	**Search terms**
Penalties	Penalties, penalty, fine, sanctions, sanction, penalization, coercive measure, coercive measures
Faults	Failure, failures, defaulting, default, misconduct, serious misconduct, neglect, negligence, complying, comply, seriousness, harm, harms, non-compliance
Termination	termination for fault, terminated for fault, formal notice, temporary suspension of contract
Delays	delay, delays, late
Control	control, controls, guarantee, guarantees, checks, check, quality, safety
State of infrastructure	*état des lieux*, deteriorate, deteriorated, deterioration, perfect state, repair, repairs
Caretaking	visit, cleanliness, hygiene, caretaking, waste, sweeping, cleaning
Dispute	challenge, challenges, dispute, disputes, court, jurisdiction, litigation
Revision	revision of conditions, revision of provisions, revised, renegotiation, renegotiate, renegotiated, contract modification
Authorizations and permits	authorization, authorizations, approbation, permit, permits
Records and stats	statement, statements, report, statistics, information

Note: Search terms which share the same semantic root (i.e. plurals, variation in tense or gender, adjectives/adverbs/verbs drawn from the same noun) are considered as a single term for the calculation of frequencies.

Computation of rigidity scores. The rigidity score built here attempts to correct some of the shortcomings of the Beuve et al. [2019] approach, which relies on the normalized frequency of search terms[3]. Two main issues can be put forth. First, the use of term frequencies leads to rigidity scores being decreasing in contract length: a 1-page contract containing one rigidity

[3] Beuve et al. [2019] also use a different dictionary and set of categories, although both dictionaries are closely related.

term is considered equivalent to a 10-page document containing 10 rigidity terms. This correction fails to capture the fact that contractual rigidity is likely to be correlated with contract length. Another shortcoming is that all occurrences words in a dictionary category have the same contribution to rigidity. I attempt to adopt a more granular approach. First, I seek to build a rigidity score which gives more weight to rare terms, i.e. terms that do not appear in many documents. Intuitively, the idea is that if a term is present in every document, it is unlikely to be a powerful way to discriminate between rigid and flexible contracts. A second criteria is that repeated occurrences of a term within a contract should not have the same weight: I assume that the marginal contribution of a word to total rigidity is decreasing in the number of occurrences of this word. I thus seek to build a rigidity score satisfying the following properties:

- Rigidity scores are not mechanically decreasing in contract length
- Rarer terms contribute to rigidity more than frequently used terms
- The marginal contribution of each occurrence of a term is decreasing in the number of occurrences of the term within a document

TF-IDF (term frequency - inverse document frequency) [Jones, 2004] is a text analysis tool designed to attribute scores to words according to their salience, i.e. how distinctively they characterize a document compared to others. For a given term, the TF-IDF score is computed by multiplying the frequency of the term within the document (TF, text frequency) by the logarithm of the inverse document frequency (IDF, the document frequency being the share of documents which contain a given word). The TF part gives a premium to words that are intensely used within a document, while the IDF part rewards words that are used in a small number of documents. Thus, a document has a high TF-IDF score for a given term if the term is mentioned frequently within the document, and appears in a small number of documents. Conversely, a search term has a low score in a document if it seldom appears in this document and/or if it appears in a large number of documents.

TF-IDF captures some dimensions that satisfy the criteria mentioned above. The IDF dimension allows to put a premium on distinctive terms which do not appear in many documents, while terms which appear in every documents do not contribute to the final score, allowing to

discriminate between contracts. The TF part on the other hand poses similar problems to the Beuve et al. [2019] normalized frequency score: it imposes a penalty on longer documents, as the number of occurrences of a term within a document is divided by the total number of words within the document. I thus modify the TF part to make a term count (TC), meaning that for a given number of occurrences, a search term will contribute to rigidity with the same intensity regardless of the length of the document. I however assume that the marginal contribution of each occurrence of a given term to rigidity is decreasing (i.e. the "additional" rigidity that a contracts get by shifting from 0 to 1 occurrence of a word is larger that the additional rigidity that it gets by shifting from 19 to 20 occurrences of the word). Thus I transform the term count by taking its natural logarithm and adding one (here the $log(x+1)$ function is simply an arbitrary function which is increasing and concave over $[0, +\infty]$). The score attributed to a search term in a given document is now a "log of (term count + 1) - inverse document frequency" (LTC-IDF) score which is computed as such:

$$LTC\_IDF_{i,t} = log(TC_{i,t} + 1) \cdot IDF_t$$

where TC is the number of occurrences of term t in document i, and IDF is the log of the inverse of the share of documents which contain the given term. The rigidity score that a contract i gets for a given category c is then the sum of the LTC_IDF scores of the terms t (with T the number of terms in the category) for this given contract:

$$RigidCat_{i,c} = \sum_{t=1}^{T} LTC\_IDF_{i,c,t}$$

Finally, for a given contract, the category-specific rigidity scores are aggregated into a general rigidity score. The category scores are first normalized using a z-transformation (subtracting the mean and dividing by the standard deviation) so that category-specific scores use the same scale. Then I use the sum of each of the 11 category-specific rigidity score to obtain a general rigidity score for each contract:

$$RigidGen_i = \sum_{c=1}^{11} \left(\frac{RigidCat_{i,c} - \overline{RigidCat_c}}{StdDev(RigidCat_c)} \right)$$

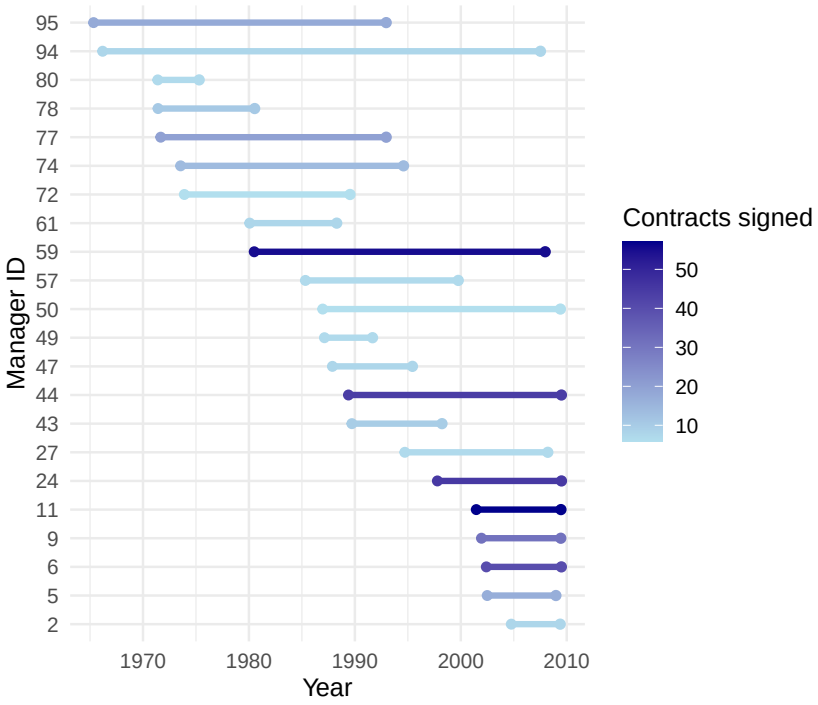
where $\overline{RigidCat_c}$ is the mean of $RigidCat$ across contracts and $StdDev(RigidCat_c)$ is the standard deviation of this same variable across contracts.

1.3.3 Manager identity

In order to identify changes in management of the operating company, I collected the names of the individuals who signed each contract on behalf of the company. These were available for all contracts, excepting very few exceptions which I eliminated from the data. In general, according to the title associated with their names, they are either the national CEO of the company, or the head of a regional branch when the firm has been divided in several subnational entities. Figure 1.2 displays the time span between the first and last contracts signed by each manager who signed over 5 contracts. It appears that there may be multiple signers at the same time, which can be explained by two factors. The first reason for this is that a single contract may be signed by multiple managers, including the regional branch manager and the national CEO. Moreover, different managers may be in charge of different types of contracts. Another reason is that the company we observe in 2009 has undergone many structural changes, and is the result of mergers between various companies whose managers were the signers of the initial contracts.

I then derive a categorical variable $Manager$ from the data collected. This variable takes three values: $FirstContract$ if the contract is the first in the city-company history, $Unknown$ if the municipality has already signed a contract with the company, but never with the current set of signing managers and $Known$ if the municipality has signed a contract with at least one of the current signing managers. I distinguish $FirstContract$ from $Unknown$ because $FirstContract$ is correlated with total experience between parties, and may induce confusion between the effects of knowing a manager and the effect of the city-company relationship length overall. One of the shortcomings of this variable is that the data collection process has not allowed the systematic identification of different manager types. While sometime, information is available about the exact position of the manager, in many cases it is not. I thus rely on a simplified variable, which considers all manager to have equivalent statuses.

Figure 1.2: Manager tenure and number of contracts signed



Note: This figure plots the timespan over which each manager name is observed in the data. Colors indicate the number of contracts signed over this timespan. Source: Author.

1.3.4 Public scrutiny

I use proxies for public scrutiny in order to obtain a measure of how likely third party challenges of the contract are. I use two primary measures. First, I use the number of effective parties (NEP) [Beuve et al., 2019], which is designed to capture the number of sizable competing parties in the first round of municipal elections. It is computed by taking the inverse of the Herfindahl-Hirschmann Index (HHI), which captures political concentration (i.e. the monopoly-like feature of an election) in the latest municipal election before the contract was signed :

$$NEP = \left(\sum_{i=1}^{N} s_i^2 \right)^{-1}$$

where s_i denotes the vote shares of party i and N is the total number of parties in the first round. For each contract, the NEP is calculated with the results of the first round of the most recent municipal election. Because Paris, Lyon and Marseille have specific administrative

features (the mayor is elected indirectly, and these cities are divided in districts, (*arrondissements*), which all have their individual mayors), they are excluded from the sample when the NEP is used.

A secondary proxy for public scrutiny is the timing of the contract with respect to elections. Municipal elections in France are held nationally every 6 years around March and April. I posit that contracts signed prior to a municipal election are subject to more risks of politically motivated challenges, given that the returns from destabilizing the mayor in office for political opponents are higher during an electoral campaign. I use a pre-election dummy variable which takes value 1 if the contract was signed within 12 months before the first round of a municipal election.

1.3.5 Control variables and fixed effects

The main empirical challenge of this work is to control for confounding factors which may affect rigidity, independently from manager identity. Most importantly, I control for the type of contract (MP *versus* DSP) as well as for whether the contract involves construction, operation or both. At the contract level, I also control for duration of the contract, number of slots in the the parking lot, total experience between parties prior to the contract (both expressed in years and number of contracts), whether the contract has ended or not, whether the contract was renewed or not, and whether the parking lot is underground or not.

Concerning time and city level heterogeneity, the size of the sample limits the use of fixed effects. Regarding time, I use half-decade fixed effects to control for the long term dynamics of contract writing style rather than year fixed effects, as it is not infrequent that there are one single contract for a given year, or a very small number. Because time variables may also capture heterogeneity in the type of contracts (there are much more MP contracts in the 2000s), I interact the time fixed effects with type of contract. In order to control for heterogeneity between cities, I use a set of city level control variables: municipal population at the closest census, political leaning of the city, a Transparency International corruption perception index for French municipalities.

I also control for manager characteristics using total manager tenure, which is proxied through

the first appearance of the manager in the data. In the case where multiple managers signed the contract, I use the tenure of the longest standing one.

Table 1.2: Descriptive Statistics

Variable	N	Min	Max	Mean	SD
Rigidity scores					
General	415	-13.58	19.87	0.62	6.66
Revision	415	0	3.81	0.20	0.46
Caretaking	415	0	9.95	2.68	1.88
Faults	415	0	8.87	2.56	1.52
Disputes	415	0	6.86	1.90	1.44
Penalties	415	0	5.88	1.95	1.43
Authorizations and permits	415	0	3.96	1.36	0.99
Quality	415	0	2.28	0.93	0.41
Delays	415	0	0.79	0.35	0.17
State of infra.	415	0	1.55	0.59	0.38
Records and stats	415	0	6.51	1.15	1.08
Explanatory variables					
Manager: Known	421	0	1	0.31	0.46
Manager: Unknown	421	0	1	0.30	0.46
Manager: First contract	421	0	1	0.39	0.49
Type: DSP	421	0	1	0.64	0.48
Type: MP	421	0	1	0.36	0.48
NEP	351	1	4.67	2.83	0.90
PreElection	421	0	1	0.18	0.39
Control variables					
Manager tenure	419	0	41	7.96	7.38
Duration (years)	421	0.15	65	17.89	15.49
Year	421	1963	2009	1996.14	12.20
Construction	421	0	1	0.29	0.45
Slots	412	20	23481	1020.05	1904.21
Experience (years)	421	0	46	9.49	11.95
Experience (contracts)	421	0	74	5.71	13.08
Renewed	421	0	1	0.14	0.35
Ended	421	0	1	0.24	0.43
Underground	421	1	3	1.93	0.63
Population	414	363	2790091	412320.73	799978.53
Left	421	0	1	0.11	0.31
Right	421	0	1	0.23	0.42
Corruption	421	0	14.41	2.04	1.44

1.4 Empirical strategy and results

1.4.1 Empirical strategy

The first proposition concerns the general effect of manager changes on contract rigidity To test Proposition 1, the following equation is estimated:

$$Rigid_{i,c,t} = \beta_1 Manager_{c,t} + \beta_2 X_{i,c,t} + \beta_3 Z_{c,t} + \gamma_{t,k} + \varepsilon_{i,c,t} \tag{1.1}$$

Subscripts i, c, t, k denote respectively contract, city, time and type (i.e. concession or standard procurement) of the contract. $Rigid$ is a rigidity score indicating reliance on written clauses. $Manager$ is a categorical variable taking 3 values: $FirstContract$ if the contract is the first in the city-company history, $Unknown$ if the contract is not the first in the city-company history but is signed only by managers which the city has not dealt with in the past, and $Known$ if the contract is signed with at least one manager that the city has interacted with in the past. Vector X contains contract level control variables: the number of lots in the parking lot, the duration of the contract, rank of the contract in the city-municipality relationship, previous years of experience between parties, tenure of the longest standing manager among those signing the contract, whether the contract was renewed or not, whether the contract has ended before or after the observation span, whether the parking lot involves underground parking, and whether the contract involves construction and operation or only operation (in the case of concession contracts). Vector Z contains city $\times$ time specific control variables: log of municipal population (at the closest census in time) the political leaning of the mayor in office, the Transparency International municipal corruption index. Finally, type-specific time fixed effects $\gamma_{t,k}$ are included. The reason for the interaction of time fixed effects is that the distribution of types is strongly biased in time (the gist of standard procurement contracts were signed in the 2000s), meaning that simple time fixed effects would capture variation in the type of contract, which are strong predictors of contract rigidity. City fixed effects are not included in the main specification but are included as robustness checks in the appendix. Estimation is performed via OLS. Given that the treatment variable $Manager$ is clustered at the time $\times$ city level (there is potential geographical heterogeneity in the identity

of managers for two contracts signed in the same year), I use cluster robust standard errors at the time $\times$ city level.

Propositions 2 and 3 aim at identifying moderating factors in the effect of manager changes on contract rigidity. To test propositions 2 and 3, the $Manager$ treatment is interacted successively with contract type and with political contestability, which indicates the likelihood of third party challenges:

$$Rigid_{i,c,t} = \beta_1 Manager_{c,t} + \beta_2(Manager_{c,t} \times Type_{i,c,t}) + \beta_3 X_{i,c,t} + \beta_4 Z_{c,t} + \gamma_{t,k} + \varepsilon_{i,c,t} \quad (1.2)$$

$$Rigid_{i,c,t} = \beta_1 Manager_{c,t} + \beta_2(Manager_{c,t} \times Contest_{c,t}) + \beta_3 X_{i,c,t} + \beta_4 Z_{c,t} + \gamma_{t,k} + \varepsilon_{i,c,t} \quad (1.3)$$

Apart for the inclusion of interaction terms, the specifications are identical as the one used to test proposition 1.

Two main concerns regarding endogeneity emerge. First, manager changes may be endogenous to relational contracts: misconduct of a manager through violation of a relational agreement may lead to manager dismissal. On the contrary, the potentially collusive behavior associated with relational contracting could also lead the company board to remove the manager. Second, manager changes can also be assumed to be endogenous to public scrutiny: a manager engaging in relational contracting may be sanctioned through judicial challenges by interested third parties, and forced to resign due to public pressure. In this case, public scrutiny as an confounding factor would only lead to more conservative estimates. Indeed, assuming that public scrutiny is positively correlated with the likelihood of manager change, then the coefficient associated with manager changes in the main equation is necessarily smaller than the one associated with only "exogenous" manager changes.

An additional criticism concerns the exogeneity of contract type assumption. This critique can be relatively easily discarded given that standard procurement and concession contracts generally answer very different needs, as is showcased by the difference in average duration between both types of contracts (26.5 years for DSP contracts against 2.7 years for MP contracts).

1.4.2 Results: Manager changes and contractual provisions

Main specification. Table 1.3 presents the results for proposition 1 using the dictionary based proxies for rigidity. The main coefficient of interest is the one associated with variable $Manager\_Unknown$. The general effect on rigidity is nonsignificant. However, we observe a significant effect, in the expected positive direction, concerning clauses dealing with penalties, monitoring and state of the infrastructure. These results points towards a relational effect, wherein unknown managers are monitored more strongly, and formal remedies are more likely to be planned out in case of breach, consistent with a theory that manager changes represent disruptions in relational contracts.

Robustness checks. A set of robustness checks are performed in the Appendix section. When removing Paris from the sample (table 1.7), the result on the "State" rigidity score remains significant while the result on "Penalties" loses significance. Other coefficients are not affected. When including city fixed effects rather than city level controls, our coefficients lose significance, potentially due to oversaturation of the model given the small size of the sample considered. When using the Beuve et al. [2019] scores we do not observe significant effects (table 1.12), despite the strong correlation between both variables (see figure 1.5). Although these robustness checks do not contradict our initial results, they show that coefficients generally lose significance when imposing additional constraints on the specification or on the sample.

Endogeneity concerns. Manager changes may be endogenous to relational contracts, if one considers the case where misconduct of a manager in a relational contract leads to this manager being dismissed. I assume that such effects would be more likely to occur in large cities: a small municipality is unlikely to dictate strategic changes in the operating company. Thus, to check whether such effects enter into play, I interact the "Manager" variable with a dummy indicating whether the city contains less than 60,000 inhabitants (which roughly corresponds with the median population). Results do not indicate that our main effect is significantly smaller in low population cities.

Table 1.3: Results for Proposition 1: Manager changes and rigidity

Dependent Variables:	Rigidity	Adaptation	Caretaking	Termination	Fault	Disputes	Penalties	Permits	Quality	Delays	State	Monitoring
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
Manager_Unknown	0.822	-0.016	0.195	-0.022	-0.145	-0.102	0.311**	-0.030	-0.009	0.038*	0.153***	0.282**
	(0.871)	(0.088)	(0.261)	(0.016)	(0.190)	(0.160)	(0.149)	(0.094)	(0.053)	(0.022)	(0.053)	(0.136)
Manager_FirstContract	-0.114	0.040	-0.066	0.003	0.084	-0.039	-0.053	-0.054	-0.043	-0.008	-0.030	0.125
	(1.37)	(0.043)	(0.388)	(0.030)	(0.320)	(0.270)	(0.250)	(0.176)	(0.083)	(0.035)	(0.074)	(0.213)
ManagerTenure	-0.214	-0.011	0.018	-0.002	-0.115**	-0.054	0.040	-0.004	-0.015	0.003	-0.017	-0.035
	(0.244)	(0.019)	(0.083)	(0.006)	(0.055)	(0.049)	(0.052)	(0.037)	(0.014)	(0.007)	(0.014)	(0.043)
PastYears	-0.459	0.038	-0.034	0.011	0.0009	-0.100	-0.227***	0.024	-0.004	-0.020*	-0.072***	-0.076
	(0.459)	(0.055)	(0.116)	(0.009)	(0.111)	(0.094)	(0.069)	(0.053)	(0.027)	(0.011)	(0.027)	(0.081)
PastContracts	0.504	-0.014	0.132	-0.022*	0.147	0.222*	0.424***	-0.151*	-0.031	0.021	0.035	0.086
	(0.571)	(0.034)	(0.231)	(0.013)	(0.141)	(0.128)	(0.097)	(0.085)	(0.034)	(0.014)	(0.031)	(0.090)
Fixed-effects												
HalfDecade-ContractType	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	398	398	398	398	398	398	398	398	398	398	398	398
R^2	0.381	0.087	0.265	0.420	0.177	0.392	0.429	0.474	0.319	0.251	0.371	0.188
Within R^2	0.108	0.036	0.097	0.112	0.084	0.077	0.125	0.146	0.061	0.092	0.129	0.092

Note: This table presents the results of a set of linear models estimated via OLS. Dependent variables are rigidity scores based on the LTC-IDF method described in Section 3.2. The main variable of interest is "Manager_Unknown", which is equal to one if the company and the municipality have interacted in the past, but never with this current set of managers. "Manager_FirstContract" is a dummy equal to one if the contract signed was the first in the city-company relationship. "ManagerTenure" is the natural log of the number of years since the first appearance of the manager(s) in the data. "PastYears" and "PastContracts" indicate the number of years and contracts respectively since the municipality and the company have first interacted. Additional control variables for which coefficients are not included are: a dummy indicating whether the contract was renewed, a dummy indicating whether the contract was ended at the end of the observation period (2009), the log of the city population at the closest census, a categorical variable indicating whether the parking lot was built under, on or above ground, political ideology of the mayor dummy variables, a Transparency International corruption index, and a dummy indicating whether the contract involved construction. All specifications contain type specific time fixed effects. Std errors clustered at the city $\times$ time level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

1.4.3 The differentiated effect of manager changes according to contract type

Proposition 2 posits that the effect from proposition 1 is driven by DSP (or concession) contracts, which provide a large amount of discretion to the buyer, who is thus able to credibly commit to repeated interactions. Although there are many confounding factors that could explain why DSP contracts are more affected by manager changes than MP (standard procurement) contracts other than high discretion and relational contracts, it is worth checking whether this prediction is verified empirically. In the main specification, I interact the "Manager" variable with a "type of contract" dummy variable. Results (table 1.4) indicate that the non interacted "Unknown manager" coefficient (i.e. the one corresponding with DSP contracts) is positively and significantly associated with "state of infrastructure" and "monitoring" categories of rigidity, confirming the general effect of manager changes on rigidity. On the other hand, manager changes appear to affect MP contracts through an increase in adaptation clauses, designed to plan out future renegotiations (although this effect is only significant at the 10 % level). This effect is surprising, given that renegotiation clauses tend to be more frequent in DSP contracts overall.

Overall, these results support the intuition that the general effect of manager changes is driven by DSP contracts rather than by MP contracts. To comfort this intuition, I run the basic set of regressions for proposition 1 on the subsample of DSP contracts. Results are stable and show significant evidence that manager changes affect the "state" and "monitoring" rigidity categories for the subset of DSP contracts (table 1.9). When adding city fixed effects to this specification (table 1.10), the coefficient for "monitoring" remains significant at the 10% level, potentially indicating that the inclusion of MP contracts generated some noise in the initial sample.

Table 1.4: Manager changes and rigidity according to contract type

Dependent Variables:	Rigidity	Adaptation	Caretaking	Termination	Fault	Disputes	Penalties	Permits	Quality	Delays	State	Monitoring
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
Manager_Unknown	1.16	-0.106	0.436	-0.016	-0.043	-0.008	0.307	0.034	0.020	0.035	0.155**	0.382**
	(1.07)	(0.098)	(0.309)	(0.019)	(0.211)	(0.220)	(0.205)	(0.124)	(0.056)	(0.027)	(0.065)	(0.152)
Manager_FirstContract	0.965	-0.010	0.124	0.036	0.322	0.163	-0.092	0.143	-0.012	0.015	-0.014	0.280
	(1.41)	(0.059)	(0.429)	(0.032)	(0.339)	(0.318)	(0.298)	(0.188)	(0.089)	(0.036)	(0.084)	(0.214)
ManagerTenure	-0.158	-0.016	0.033	-0.0002	-0.102*	-0.043	0.039	0.007	-0.013	0.003	-0.017	-0.025
	(0.255)	(0.019)	(0.086)	(0.006)	(0.057)	(0.051)	(0.053)	(0.037)	(0.015)	(0.007)	(0.014)	(0.044)
PastYears	-0.495	0.042	-0.046	0.010	-0.008	-0.108	-0.226***	0.017	-0.006	-0.020*	-0.073***	-0.084
	(0.466)	(0.055)	(0.115)	(0.009)	(0.110)	(0.097)	(0.069)	(0.054)	(0.027)	(0.011)	(0.027)	(0.081)
PastContracts	0.612	-0.022	0.156	-0.019	0.172	0.244*	0.420***	-0.132	-0.028	0.023	0.036	0.103
	(0.582)	(0.035)	(0.231)	(0.013)	(0.141)	(0.135)	(0.098)	(0.084)	(0.035)	(0.014)	(0.032)	(0.090)
Manager_Unknown × TypeMP	-0.557	0.221*	-0.573	-0.008	-0.196	-0.184	-0.0006	-0.110	-0.066	0.015	0.0003	-0.215
	(1.80)	(0.131)	(0.471)	(0.035)	(0.415)	(0.312)	(0.264)	(0.192)	(0.112)	(0.044)	(0.104)	(0.274)
Manager_FirstContract × TypeMP	-2.55	0.106	-0.419	-0.078**	-0.557	-0.474	0.093	-0.466**	-0.071	-0.057	-0.037	-0.356
	(1.99)	(0.085)	(0.466)	(0.038)	(0.472)	(0.409)	(0.305)	(0.217)	(0.119)	(0.050)	(0.096)	(0.288)
Fixed-effects												
HalfDecade-ContractType	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	398	398	398	398	398	398	398	398	398	398	398	398
R^2	0.385	0.096	0.268	0.430	0.182	0.396	0.430	0.481	0.320	0.258	0.371	0.192
Within R^2	0.115	0.045	0.101	0.127	0.089	0.082	0.125	0.158	0.063	0.100	0.130	0.096

Note: This table presents the results of a set of linear models estimated via OLS. Dependent variables are rigidity scores based on the LTC-IDF method described in Section 3.2. The main variable of interest is "Manager_Unknown", which is equal to one if the company and the municipality have interacted in the past, but never with this current set of managers. It is interacted with a "Type" dummy variable: if "Type_MP" is equal to one, this indicates a standard procurement contract. The non interacted coefficient for "Manager_Unknown" thus reflects the effect for units which are not MP contracts, i.e. DSP contracts. "Manager_FirstContract" is a dummy equal to one if the contract signed was the first in the city-company relationship. "ManagerTenure" is the natural log of the number of years since the first appearance of the manager(s) in the data. "PastYears" and "PastContracts" indicate the number of years and contracts respectively since the municipality and the company have first interacted. Additional control variables for which coefficients are not included are: a dummy indicating whether the contract was renewed, a dummy indicating whether the contract was ended at the end of the observation period (2009), a type of contract (MP or DSP) dummy variable, the log of the city population at the closest census, a categorical variable indicating whether the parking lot was built under, on or above ground, political ideology of the mayor dummy variables, a Transparency International corruption index, and a dummy indicating whether the contract involved construction. All specifications contain type specific time fixed effects. Std errors clustered at the city × time level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

1.4.4 The mediating effect of public scrutiny

This subsection discusses the results for the tests on the mediating effect of public scrutiny. While the main effect of public scrutiny variables (NEP and closeness to elections) on rigidity is expected to be positive, the interaction with $Manager\_Unkown$ is expected to have a negative coefficient, as public scrutiny should reduce the relevance of manager identity in determining rigidity. Table 1.5 presents the results for the regressions using the number of effective parties (NEP) as a proxy for political contestability. I split the NEP at the integer above the median, i.e. 3, and interact this dummy variable with the "Manager" treatment. Results do not confirm our theoretical predictions, as we observe positive and significant coefficients of $Manager\_Unknown \times (NEP > 3)$ for two rigidity variables, and no negative and significant effect. Next, I use the $PreElection$ dummy variable to proxy public scrutiny (table 1.6). In this case, evidence is mixed. We observe the expected effects for the "Delays" rigidity category, but the opposite effect for the "Quality" category. Again, these results do not allow us to confirm Proposition 3. Overall, results from the interaction of manager changes with public scrutiny do not support the idea that public scrutiny plays a mediating role in the relationship between manager identity and contract rigidity.

1.5 Conclusion

This paper examines whether relational contracts play a role in shaping public-private agreements. Using a negative proxy for relational contracting, i.e. the fact the municipality is dealing with managers that it has not previously contracted with. I find evidence that public contracts are more rigidly written when the municipality has not previously interacted with the current set of managers. Specifically, clauses concerning penalties and monitoring of the contractor, which one can relate to distrust, are significantly affected by manager changes. Evidence is stronger when focusing on DSP (concession-like) contracts, which given their long duration and high discretion awarding procedures appear more prone to the development of relational contracts, and flexible agreements. When turning to the role of third party opportunism, I do not find evidence that public scrutiny plays a significant mediating role on the relationship between manager identity and relational contracting.

This work remains subjected to several critiques. The diversity in nature of managers is not taken into account in the current state of our data. Indeed, manager changes encompass changes at the global head of the company, but also changes in the identity of regional branch managers. Moreover, it is likely that the managers considered in this study do not directly negotiate contracts: while my preferred explanatory channel is that manager changes generate distrust on the municipality side, and that the additional rigidity is demanded by the municipality, information on the precise mechanics of contract negotiation would shed light on the drivers of our results. In addition, the endogeneity of manager changes is not fully taken into account. Although I do not find that the effect is driven by larger cities, which may have sufficient bargaining power to lead to manager changes, fully accounting for endogeneity by instrumenting manager changes could provide robustness to the results. Finally, due to the small size of the sample, two-way fixed effects often fails to yield significant coefficients, and the specifications without city fixed effects may fail to fully account for unobserved heterogeneity.

Potential follow-ups to this study include the use of data on judicial procedures to obtain additional information on the relational quality of the contract: one may expect that judicial disputes represent a failure of the relational contract, as parties must revert to relying on written clauses. Future works relating changes in management with judicial procedures would shed additional light on the relational dimension of those manager changes. Additionally, this study focuses on changes in management on the company side. I do not examine whether bureaucratic or political turnovers within municipalities have an effect on contract rigidity. The study of public official identity, coupled with the literature on public scrutiny, may provide an interesting research avenue for the political economy of how public contracts are written. An additional avenue for research concerns reputational spillovers of company behavior: in this study, I consider bilateral relationships between several municipalities and a single company to be independent from one another, and distrust to stem only from the municipality not having interacted with current manager(s) previously. One could assume that opportunistic behavior on the company side may harm its reputation towards other cities, potentially affecting the way other cities write contracts, and whether other cities award contracts to this firm.

Table 1.5: Effect of manager changes according to political competition

Dependent Variables:	Rigidity	Adaptation	Caretaking	Termination	Fault	Disputes	Penalties	Permits	Quality	Delays	State	Monitoring
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
Manager_Unknown	-0.230	0.031	-0.060	-0.041*	-0.220	-0.171	0.070	-0.051	-0.084	-0.004	0.100	0.239
	(1.27)	(0.159)	(0.308)	(0.024)	(0.271)	(0.233)	(0.187)	(0.144)	(0.077)	(0.030)	(0.072)	(0.195)
Manager_FirstContract	-0.259	0.115	0.043	0.009	-0.020	0.032	-0.379	0.026	0.016	-0.020	-0.107	0.013
	(1.93)	(0.084)	(0.445)	(0.037)	(0.464)	(0.315)	(0.274)	(0.230)	(0.116)	(0.047)	(0.101)	(0.315)
(NEP $>$ 3)	-2.16	-0.004	-0.430	-0.050	0.141	-0.364	-0.182	-0.256	-0.187	-0.064	-0.211**	0.360
	(2.35)	(0.190)	(0.529)	(0.037)	(0.559)	(0.449)	(0.257)	(0.206)	(0.135)	(0.052)	(0.106)	(0.315)
ManagerTenure	0.116	-0.011	0.068	0.002	-0.050	0.008	0.066	0.023	-0.008	0.012	0.001	-0.004
	(0.336)	(0.025)	(0.081)	(0.008)	(0.078)	(0.067)	(0.061)	(0.050)	(0.018)	(0.009)	(0.016)	(0.057)
PastYears	-0.109	0.032	0.001	0.012	0.089	-0.084	-0.099	0.031	0.002	-0.015	-0.044	-0.014
	(0.506)	(0.056)	(0.117)	(0.010)	(0.128)	(0.097)	(0.069)	(0.063)	(0.029)	(0.012)	(0.029)	(0.091)
PastContracts	0.295	0.011	0.252	-0.013	-0.020	0.403*	0.039	-0.056	0.029	0.020	-0.054	-0.059
	(1.12)	(0.047)	(0.306)	(0.022)	(0.298)	(0.220)	(0.134)	(0.118)	(0.068)	(0.025)	(0.062)	(0.184)
Manager_Unknown $\times$ (NEP $>$ 3)	1.70	-0.093	0.623	0.043	-0.243	0.044	0.269	-0.093	0.292*	0.093*	0.138	-0.338
	(2.53)	(0.175)	(0.655)	(0.047)	(0.628)	(0.452)	(0.305)	(0.270)	(0.159)	(0.056)	(0.134)	(0.365)
Manager_FirstContract $\times$ (NEP $>$ 3)	2.61	-0.132	0.645	0.030	0.047	0.604	0.426	0.163	0.228	0.097	0.155	-0.121
	(2.66)	(0.189)	(0.598)	(0.048)	(0.626)	(0.526)	(0.339)	(0.271)	(0.158)	(0.062)	(0.117)	(0.377)
Fixed-effects												
HalfDecade-ContractType	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	291	291	291	291	291	291	291	291	291	291	291	291
R^2	0.400	0.105	0.350	0.319	0.176	0.372	0.416	0.495	0.304	0.285	0.383	0.170
Within R^2	0.140	0.044	0.148	0.153	0.102	0.086	0.148	0.187	0.095	0.136	0.179	0.125

Note: This table presents the results of a set of linear models estimated via OLS. Dependent variables are rigidity scores based on the LTC-IDF method described in Section 3.2. The main variable of interest is "Manager_Unknown", which is equal to one if the company and the municipality have interacted in the past, but never with this current set of managers. It is interacted with "NEP $>$ 3", a dummy variable whether or not the number of effective parties (see Section 3.4.) in the most recent municipal election was greater than 3. "Manager_FirstContract" is a dummy equal to one if the contract signed was the first in the city-company relationship. "ManagerTenure" is the natural log of the number of years since the first appearance of the manager(s) in the data. "PastYears" and "PastContracts" indicate the number of years and contracts respectively since the municipality and the company have first interacted. Additional control variables for which coefficients are not included are: a dummy indicating whether the contract was renewed, a dummy indicating whether the contract was ended at the end of the observation period (2009), the log of the city population at the closest census, a categorical variable indicating whether the parking lot was built under, on or above ground, political ideology of the mayor dummy variables, a Transparency International corruption index, and a dummy indicating whether the contract involved construction. All specifications contain type specific time fixed effects. Std errors clustered at the city $\times$ time level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 1.6: Effect of manager changes on contract rigidity in pre-election periods

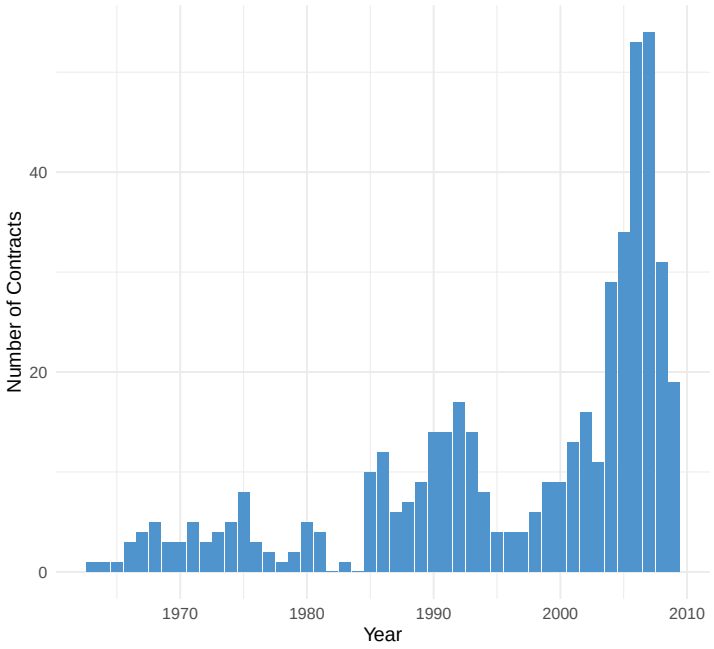
Dependent Variables:	Rigidity	Adaptation	Caretaking	Termination	Fault	Disputes	Penalties	Permits	Quality	Delays	State	Monitoring
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
Manager_Unknown	0.630	-0.017	0.246	-0.022	-0.201	-0.047	0.234	0.075	-0.019	0.018	0.140**	0.182
	(0.975)	(0.074)	(0.292)	(0.017)	(0.221)	(0.156)	(0.176)	(0.093)	(0.061)	(0.024)	(0.061)	(0.149)
Manager_FirstContract	-0.384	0.080	-0.117	0.001	-0.032	0.032	-0.156	-0.056	-0.069	-0.032	-0.017	0.069
	(1.44)	(0.061)	(0.383)	(0.031)	(0.339)	(0.279)	(0.257)	(0.177)	(0.086)	(0.037)	(0.077)	(0.207)
PreElec	0.350	0.097	0.179	0.003	-0.160	0.346	-0.214	0.058	-0.022	-0.040	0.094	0.026
	(1.42)	(0.095)	(0.375)	(0.024)	(0.296)	(0.386)	(0.204)	(0.125)	(0.085)	(0.032)	(0.070)	(0.195)
ManagerTenure	-0.183	-0.013	0.033	-0.001	-0.110*	-0.048	0.041	0.006	-0.014	0.003	-0.017	-0.034
	(0.247)	(0.019)	(0.083)	(0.006)	(0.057)	(0.049)	(0.053)	(0.036)	(0.014)	(0.007)	(0.014)	(0.043)
PastYears	-0.389	0.045	-0.022	0.011	-0.001	-0.079	-0.231***	0.015	-0.003	-0.020*	-0.062**	-0.058
	(0.476)	(0.055)	(0.121)	(0.009)	(0.115)	(0.093)	(0.070)	(0.052)	(0.028)	(0.011)	(0.028)	(0.079)
PastContracts	0.469	-0.018	0.129	-0.022*	0.147	0.215	0.424***	-0.144*	-0.032	0.020	0.030	0.075
	(0.595)	(0.033)	(0.237)	(0.013)	(0.145)	(0.132)	(0.098)	(0.084)	(0.035)	(0.014)	(0.033)	(0.091)
Manager_Unknown × PreElec	0.569	-0.015	-0.236	-0.002	0.228	-0.277	0.319	-0.394**	0.040	0.082**	0.025	0.349
	(1.66)	(0.155)	(0.479)	(0.038)	(0.369)	(0.403)	(0.276)	(0.186)	(0.112)	(0.040)	(0.099)	(0.297)
Manager_FirstContract × PreElec	2.82	-0.170	0.748	0.019	0.682	0.010	0.480	0.135	0.173	0.121**	0.052	0.495
	(2.11)	(0.121)	(0.507)	(0.045)	(0.504)	(0.584)	(0.355)	(0.206)	(0.111)	(0.050)	(0.109)	(0.380)
Fixed-effects												
HalfDecade-ContractType	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	398	398	398	398	398	398	398	398	398	398	398	398
R^2	0.388	0.092	0.274	0.421	0.182	0.397	0.432	0.481	0.324	0.266	0.384	0.203
Within R^2	0.119	0.042	0.108	0.113	0.089	0.085	0.129	0.157	0.068	0.110	0.147	0.108

Note: This table presents the results of a set of linear models estimated via OLS. Dependent variables are rigidity scores based on the LTC-IDF method described in Section 3.2. The main variable of interest is "Manager_Unknown", which is equal to one if the company and the municipality have interacted in the past, but never with this current set of managers. It is interacted with a PreElection variable indicating whether the contract was signed within 12 months preceding a municipal election. "Manager_FirstContract" is a dummy equal to one if the contract signed was the first in the city-company relationship. "ManagerTenure" is the natural log of the number of years since the first appearance of the manager(s) in the data. "PastYears" and "PastContracts" indicate the number of years and contracts respectively since the municipality and the company have first interacted. Additional control variables for which coefficients are not included are: a dummy indicating whether the contract was renewed, a dummy indicating whether the contract was ended at the end of the observation period (2009), the log of the city population at the closest census, a categorical variable indicating whether the parking lot was built under, on or above ground, political ideology of the mayor dummy variables, a Transparency International corruption index, and a dummy indicating whether the contract involved construction. All specifications contain type specific time fixed effects. Std errors clustered at the city × time level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Appendix A

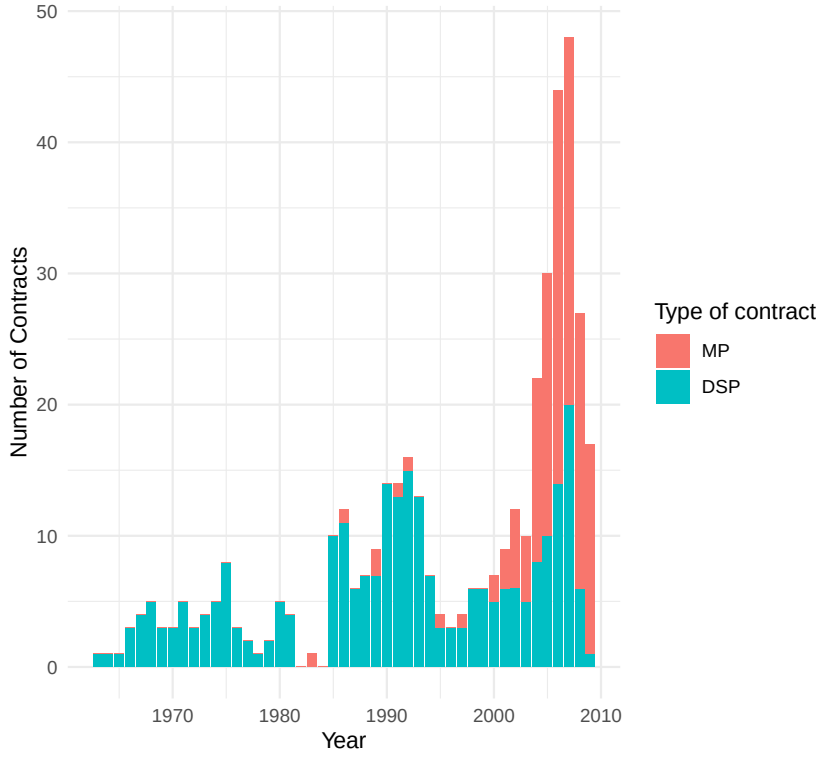
A1: Additional descriptive statistics

Figure 1.3: Number of contracts signed per year



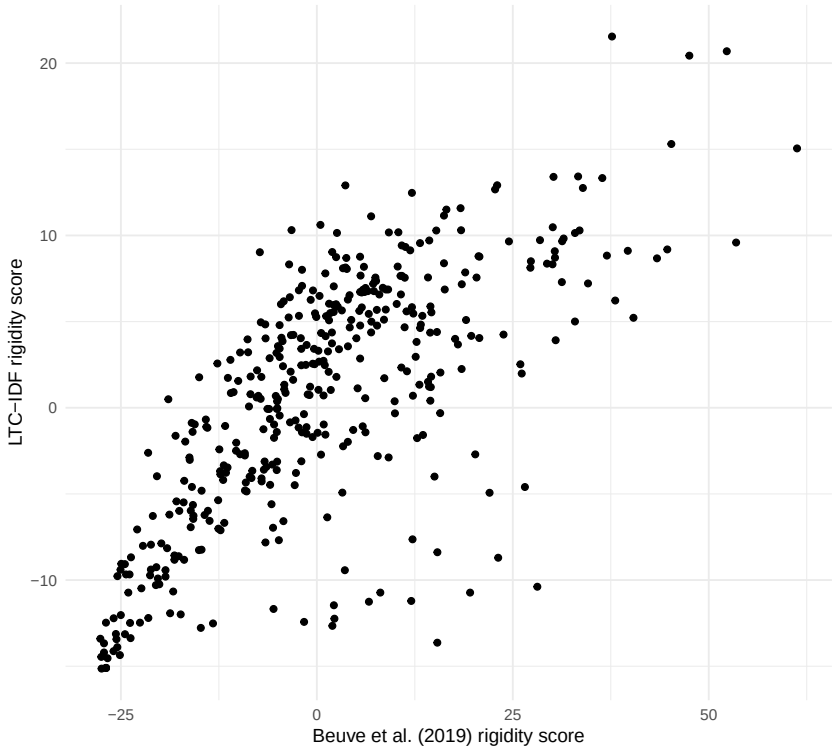
Note: This figure plots the number of contracts signed each year in our data. Source: Author.

Figure 1.4: Number of contracts signed per year (according to contract type)



Note: This figure plots the number of contracts signed each year in our data, separating between main contract types. MP indicates standard procurement contracts, while DSP refers to concession contracts. Source: Author.

Figure 1.5: Comparison of LTC-IDF rigidity score with the Beuve et al. [2019] score



This figure plots the LTC-IDF rigidity score, constructed in this study, against the rigidity score developed by Beuve et al. [2019]. Source: Author.

A2: Robustness checks

Table 1.7: Test for proposition 1, Paris excluded from the sample

Dependent Variables:	Rigidity	Adaptation	Caretaking	Termination	Fault	Disputes	Penalties	Permits	Quality	Delays	State	Monitoring
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
Manager_Unknown	0.353	-0.0001	0.138	-0.025	-0.260	-0.124	0.227	-0.034	-0.011	0.021	0.144**	0.096
	(1.05)	(0.100)	(0.278)	(0.020)	(0.224)	(0.186)	(0.181)	(0.107)	(0.068)	(0.026)	(0.062)	(0.159)
Manager_FirstContract	0.068	0.063	0.057	0.016	0.055	0.266	-0.277	-0.013	0.018	-0.012	-0.056	-0.038
	(1.64)	(0.063)	(0.398)	(0.033)	(0.416)	(0.279)	(0.281)	(0.201)	(0.103)	(0.042)	(0.089)	(0.273)
ManagerTenure	-0.165	-0.010	-0.031	0.0008	-0.102	-0.050	0.032	0.016	-0.011	0.007	-0.025	-0.018
	(0.299)	(0.020)	(0.074)	(0.007)	(0.069)	(0.059)	(0.062)	(0.044)	(0.016)	(0.008)	(0.015)	(0.050)
PastYears	-0.319	0.037	-0.043	0.008	0.050	-0.117	-0.152**	0.001	-0.007	-0.018	-0.053*	-0.009
	(0.489)	(0.055)	(0.118)	(0.010)	(0.115)	(0.104)	(0.073)	(0.060)	(0.027)	(0.011)	(0.029)	(0.084)
PastContracts	0.567	-0.0002	0.243	-0.002	0.087	0.482**	0.144	-0.064	0.018	0.021	-0.020	-0.091
	(1.00)	(0.040)	(0.275)	(0.020)	(0.276)	(0.226)	(0.144)	(0.106)	(0.058)	(0.024)	(0.059)	(0.163)
Fixed-effects												
HalfDecade-ContractType	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	339	339	339	339	339	339	339	339	339	339	339	339
R^2	0.404	0.098	0.338	0.399	0.177	0.388	0.366	0.507	0.347	0.250	0.406	0.228
Within R^2	0.120	0.035	0.123	0.143	0.088	0.069	0.094	0.175	0.072	0.095	0.139	0.110

Note: This table presents the results of a set of linear models estimated via OLS. Contracts signed in Paris are excluded from the sample. Dependent variables are rigidity scores based on the LTC-IDF method described in Section 3.2. The main variable of interest is "Manager_Unknown", which is equal to one if the company and the municipality have interacted in the past, but never with this current set of managers. "Manager_FirstContract" is a dummy equal to one if the contract signed was the first in the city-company relationship. "ManagerTenure" is the natural log of the number of years since the first appearance of the manager(s) in the data. "PastYears" and "PastContracts" indicate the number of years and contracts respectively since the municipality and the company have first interacted. Additional control variables for which coefficients are not included are: a dummy indicating whether the contract was renewed, a dummy indicating whether the contract was ended at the end of the observation period (2009), a type of contract (MP or DSP) dummy variable, the log of the city population at the closest census, a categorical variable indicating whether the parking lot was built under, on or above ground, political ideology of the mayor dummy variables, a Transparency International corruption index, and a dummy indicating whether the contract involved construction. All specifications contain type specific time fixed effects. Std errors clustered at the city $\times$ time level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 1.8: Results for proposition1, including city fixed effects

Dependent Variables:	Rigidity	Adaptation	Caretaking	Termination	Fault	Disputes	Penalties	Permits	Quality	Delays	State	Monitoring
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
Manager_Unknown	0.157	-0.033	0.004	-0.019	-0.190	-0.201	0.214	0.026	-0.046	0.016	0.092	0.225
	(1.11)	(0.062)	(0.370)	(0.022)	(0.271)	(0.179)	(0.218)	(0.142)	(0.066)	(0.026)	(0.071)	(0.163)
Manager_FirstContract	-0.237	-0.094	0.200	0.009	-0.203	0.002	0.019	0.002	-0.007	-0.016	0.021	-0.054
	(2.13)	(0.080)	(0.616)	(0.041)	(0.480)	(0.353)	(0.397)	(0.301)	(0.141)	(0.048)	(0.125)	(0.327)
ManagerTenure	0.106	-0.033	0.001	0.005	-0.062	-0.015	0.141*	0.030	0.005	0.007	0.0001	0.012
	(0.348)	(0.024)	(0.153)	(0.007)	(0.083)	(0.076)	(0.076)	(0.056)	(0.027)	(0.009)	(0.023)	(0.061)
PastYears	-0.575	-0.036	0.348	-0.021	-0.106	-0.068	-0.117	-0.159	0.014	-0.023	-0.019	-0.052
	(0.906)	(0.045)	(0.292)	(0.018)	(0.201)	(0.185)	(0.165)	(0.124)	(0.058)	(0.022)	(0.053)	(0.121)
PastContracts	1.05	0.008	0.224	0.029	0.080	0.132	0.381	0.021	0.015	0.026	0.064	-0.057
	(1.29)	(0.056)	(0.403)	(0.027)	(0.274)	(0.294)	(0.234)	(0.175)	(0.084)	(0.035)	(0.079)	(0.216)
Fixed-effects												
HalfDecade-ContractType	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	398	398	398	398	398	398	398	398	398	398	398	398
R^2	0.648	0.683	0.495	0.754	0.571	0.714	0.649	0.725	0.596	0.620	0.649	0.586
Within R^2	0.045	0.053	0.099	0.070	0.056	0.065	0.094	0.210	0.051	0.045	0.047	0.057

Note: This table presents the results of a set of linear models estimated via OLS. Dependent variables are rigidity scores based on the LTC-IDF method described in Section 3.2. The main variable of interest is "Manager_Unknown", which is equal to one if the company and the municipality have interacted in the past, but never with this current set of managers. "Manager_FirstContract" is a dummy equal to one if the contract signed was the first in the city-company relationship. "ManagerTenure" is the natural log of the number of years since the first appearance of the manager(s) in the data. "PastYears" and "PastContracts" indicate the number of years and contracts respectively since the municipality and the company have first interacted. Additional control variables for which coefficients are not included are: a dummy indicating whether the contract was renewed, a dummy indicating whether the contract was ended at the end of the observation period (2009), a type of contract (MP or DSP) dummy variable, the log of the city population at the closest census, a categorical variable indicating whether the parking lot was built under, on or above ground, political ideology of the mayor dummy variables, a Transparency International corruption index, and a dummy indicating whether the contract involved construction. All specifications contain type specific time fixed effects and city fixed effects. Std errors clustered at the city $\times$ time level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 1.9: Results for Proposition 1, focusing only on DSP contracts (no city fixed effects)

Dependent Variables:	Rigidity	Adaptation	Caretaking	Termination	Fault	Disputes	Penalties	Permits	Quality	Delays	State	Monitoring
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
Manager_Unknown	1.27	-0.159	0.479	-0.012	0.041	-0.090	0.323	0.023	0.035	0.036	0.170**	0.475***
	(1.14)	(0.114)	(0.322)	(0.021)	(0.209)	(0.242)	(0.214)	(0.138)	(0.056)	(0.027)	(0.066)	(0.151)
Manager_FirstContract	1.33	0.031	-0.044	0.044	0.486	0.238	0.188	0.194	-0.053	0.048	-0.004	0.071
	(1.56)	(0.056)	(0.508)	(0.038)	(0.346)	(0.349)	(0.365)	(0.204)	(0.094)	(0.038)	(0.096)	(0.217)
ManagerTenure	-0.169	-0.037	0.012	-0.002	-0.060	-0.064	0.052	0.006	-0.012	0.005	-0.009	-0.028
	(0.254)	(0.023)	(0.102)	(0.006)	(0.053)	(0.057)	(0.059)	(0.043)	(0.016)	(0.007)	(0.015)	(0.042)
PastYears	-0.463	0.085	-0.146	0.006	-0.013	-0.068	-0.134	0.064	-0.026	-0.011	-0.084**	-0.170**
	(0.577)	(0.064)	(0.172)	(0.012)	(0.122)	(0.121)	(0.112)	(0.079)	(0.030)	(0.011)	(0.033)	(0.086)
PastContracts	0.724	-0.027	0.195	-0.003	0.241*	0.173	0.452***	-0.214**	-0.019	0.032**	0.043	0.081
	(0.570)	(0.034)	(0.278)	(0.014)	(0.136)	(0.154)	(0.132)	(0.103)	(0.034)	(0.016)	(0.038)	(0.098)
Fixed-effects												
HalfDecade-ContractType	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	255	255	255	255	255	255	255	255	255	255	255	255
R^2	0.341	0.116	0.185	0.530	0.185	0.338	0.226	0.243	0.428	0.166	0.429	0.242
Within R^2	0.070	0.065	0.112	0.069	0.063	0.096	0.118	0.215	0.067	0.063	0.114	0.102

Note: This table presents the results of a set of linear models estimated via OLS. Only DSP contracts are included in the sample. Dependent variables are rigidity scores based on the LTC-IDF method described in Section 3.2. The main variable of interest is "Manager_Unknown", which is equal to one if the company and the municipality have interacted in the past, but never with this current set of managers. "Manager_FirstContract" is a dummy equal to one if the contract signed was the first in the city-company relationship. "ManagerTenure" is the natural log of the number of years since the first appearance of the manager(s) in the data. "PastYears" and "PastContracts" indicate the number of years and contracts respectively since the municipality and the company have first interacted. Additional control variables for which coefficients are not included are: a dummy indicating whether the contract was renewed, a dummy indicating whether the contract was ended at the end of the observation period (2009), a type of contract (MP or DSP) dummy variable, the log of the city population at the closest census, a categorical variable indicating whether the parking lot was built under, on or above ground, political ideology of the mayor dummy variables, a Transparency International municipal corruption index, and a dummy indicating whether the contract involved construction. All specifications contain type specific time fixed effects. Std errors clustered at the city $\times$ time level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 1.10: Results for proposition 1, focusing on DSP contracts, including city fixed effects

Dependent Variables:	Rigidity	Adaptation	Caretaking	Termination	Fault	Disputes	Penalties	Permits	Quality	Delays	State	Monitoring
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
Manager_Unknown	1.31	-0.113	-0.065	0.021	0.138	-0.107	0.382	0.145	0.028	0.040	0.138	0.370*
	(1.46)	(0.091)	(0.526)	(0.026)	(0.290)	(0.244)	(0.346)	(0.213)	(0.085)	(0.041)	(0.108)	(0.199)
Manager_FirstContract	0.870	-0.039	0.265	0.010	0.086	0.364	0.088	-0.092	-0.033	0.050	0.021	0.209
	(2.80)	(0.098)	(0.797)	(0.048)	(0.579)	(0.509)	(0.665)	(0.459)	(0.180)	(0.067)	(0.192)	(0.451)
ManagerTenure	-0.208	-0.050*	-0.092	0.003	-0.115	-0.049	0.121	-0.015	-0.007	0.004	-0.012	-0.016
	(0.466)	(0.028)	(0.198)	(0.009)	(0.090)	(0.091)	(0.112)	(0.079)	(0.031)	(0.011)	(0.033)	(0.080)
PastYears	-1.43	0.012	0.474	-0.064**	-0.281	-0.103	-0.280	-0.384*	-0.068	-0.015	-0.077	-0.002
	(1.29)	(0.049)	(0.375)	(0.029)	(0.253)	(0.235)	(0.301)	(0.225)	(0.077)	(0.032)	(0.089)	(0.171)
PastContracts	2.46	-0.027	-0.018	0.080**	0.446	0.387	0.722**	0.279	0.056	0.039	0.106	0.036
	(1.67)	(0.061)	(0.558)	(0.032)	(0.370)	(0.412)	(0.338)	(0.286)	(0.107)	(0.049)	(0.112)	(0.314)
Fixed-effects												
HalfDecade-ContractType	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	255	255	255	255	255	255	255	255	255	255	255	255
R^2	0.602	0.693	0.448	0.795	0.531	0.734	0.537	0.641	0.606	0.514	0.639	0.595
Within R^2	0.093	0.110	0.154	0.158	0.085	0.077	0.108	0.309	0.075	0.066	0.074	0.098

Note: This table presents the results of a set of linear models estimated via OLS. Only DSP contracts are included in the sample. Dependent variables are rigidity scores based on the LTC-IDF method described in Section 3.2. The main variable of interest is "Manager_Unknown", which is equal to one if the company and the municipality have interacted in the past, but never with this current set of managers. "Manager_FirstContract" is a dummy equal to one if the contract signed was the first in the city-company relationship. "ManagerTenure" is the natural log of the number of years since the first appearance of the manager(s) in the data. "PastYears" and "PastContracts" indicate the number of years and contracts respectively since the municipality and the company have first interacted. Additional control variables for which coefficients are not included are: a dummy indicating whether the contract was renewed, a dummy indicating whether the contract was ended at the end of the observation period (2009), a type of contract (MP or DSP) dummy variable, the log of the city population at the closest census, a categorical variable indicating whether the parking lot was built under, on or above ground, political ideology of the mayor dummy variables, a Transparency International municipal corruption index, and a dummy indicating whether the contract involved construction. All specifications contain type specific time fixed effects and city fixed effects. Std errors clustered at the city $\times$ time level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 1.11: Results for proposition 1 (interacted with dummy for city size)

Dependent Variables:	Rigidity	Adaptation	Caretaking	Termination	Fault	Disputes	Penalties	Permits	Quality	Delays	State	Monitoring
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
Manager_Unknown	0.429	-0.046	0.232	-0.029	-0.326	-0.104	0.287	-0.044	-0.025	0.037	0.131**	0.217
	(1.03)	(0.050)	(0.329)	(0.021)	(0.238)	(0.197)	(0.187)	(0.127)	(0.062)	(0.026)	(0.061)	(0.158)
Manager_FirstContract	-0.407	0.172	-0.296	0.008	-0.010	0.027	0.002	0.084	-0.121	-0.020	-0.099	-0.067
	(1.81)	(0.175)	(0.543)	(0.040)	(0.387)	(0.419)	(0.333)	(0.226)	(0.095)	(0.042)	(0.101)	(0.249)
ManagerTenure	-0.237	-0.011	0.014	-0.002	-0.124**	-0.052	0.035	0.0008	-0.017	0.002	-0.020	-0.042
	(0.246)	(0.018)	(0.083)	(0.006)	(0.057)	(0.049)	(0.052)	(0.037)	(0.014)	(0.007)	(0.014)	(0.043)
PastYears	-0.428	0.045	-0.030	0.009	0.012	-0.102	-0.190***	0.009	-0.007	-0.021*	-0.066**	-0.069
	(0.481)	(0.064)	(0.116)	(0.010)	(0.114)	(0.103)	(0.066)	(0.055)	(0.027)	(0.011)	(0.027)	(0.081)
PastContracts	0.413	-0.007	0.093	-0.017	0.117	0.235	0.371***	-0.107	-0.038	0.020	0.014	0.047
	(0.656)	(0.036)	(0.243)	(0.013)	(0.166)	(0.151)	(0.104)	(0.085)	(0.038)	(0.016)	(0.034)	(0.099)
Manager_Unknown × SmallCity	0.909	0.058	-0.112	0.023	0.431	0.013	-0.056	0.085	0.049	0.004	0.035	0.140
	(1.78)	(0.200)	(0.465)	(0.034)	(0.381)	(0.347)	(0.283)	(0.193)	(0.100)	(0.042)	(0.102)	(0.250)
Manager_FirstContract × SmallCity	0.512	-0.178	0.285	0.002	0.188	-0.084	-0.139	-0.148	0.117	0.018	0.090	0.274
	(2.14)	(0.266)	(0.470)	(0.040)	(0.451)	(0.481)	(0.339)	(0.222)	(0.103)	(0.046)	(0.108)	(0.249)
Fixed-effects												
HalfDecade-ContractType	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	398	398	398	398	398	398	398	398	398	398	398	398
R^2	0.382	0.101	0.267	0.424	0.183	0.392	0.439	0.478	0.321	0.252	0.379	0.192
Within R^2	0.110	0.051	0.099	0.117	0.089	0.077	0.139	0.154	0.065	0.093	0.140	0.096

Note: This table presents the results of a set of linear models estimated via OLS. Dependent variables are rigidity scores based on the LTC-IDF method described in Section 3.2. The main variable of interest is "Manager_Unknown", which is equal to one if the company and the municipality have interacted in the past, but never with this current set of managers. It is interacted with a dummy variable indicating whether the city has less than 60,000 inhabitants. "Manager_FirstContract" is a dummy equal to one if the contract signed was the first in the city-company relationship. "ManagerTenure" is the natural log of the number of years since the first appearance of the manager(s) in the data. "PastYears" and "PastContracts" indicate the number of years and contracts respectively since the municipality and the company have first interacted. Additional control variables for which coefficients are not included are: a dummy indicating whether the contract was renewed, a dummy indicating whether the contract was ended at the end of the observation period (2009), a type of contract (MP or DSP) dummy variable, the log of the city population at the closest census, a categorical variable indicating whether the parking lot was built under, on or above ground, political ideology of the mayor dummy variables, a Transparency International corruption index, and a dummy indicating whether the contract involved construction. All specifications contain type specific time fixed effects. Std errors clustered at the city × time level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 1.12: Results for Proposition 1 using Beuve et al. [2019] rigidity scores

Dependent Variables:	Rigidity	Termination	Arbitration	Penalties	Certifications	Evaluation	Litigation	Contingencies
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Manager_Unknown	-1.15	-0.721	0.390	-0.086	-0.352	-0.325	0.051	0.172
	(1.82)	(0.448)	(0.431)	(0.442)	(0.347)	(0.512)	(0.517)	(0.357)
Manager_FirstContract	2.63	0.590	-0.365	0.756	0.931*	0.316	-0.564	0.829
	(3.00)	(0.707)	(1.02)	(0.682)	(0.500)	(0.830)	(0.700)	(0.740)
ManagerTenure	-0.444	0.228	-0.208	-0.012	-0.171	-0.101	-0.141	-0.020
	(0.578)	(0.141)	(0.129)	(0.148)	(0.111)	(0.174)	(0.154)	(0.112)
PastYears	0.483	0.305	-0.495	-0.085	0.378**	0.283	0.119	-0.140
	(1.18)	(0.236)	(0.402)	(0.236)	(0.170)	(0.345)	(0.234)	(0.263)
PastContracts	-1.09	0.001	0.133	0.296	-0.635**	-0.729*	-0.599	0.623**
	(1.43)	(0.297)	(0.280)	(0.251)	(0.266)	(0.413)	(0.462)	(0.311)
Fixed-effects								
HalfDecade-ContractType	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics								
Observations	403	403	403	403	403	403	403	403
R^2	0.346	0.167	0.240	0.350	0.210	0.298	0.149	0.173
Within R^2	0.162	0.080	0.120	0.089	0.107	0.142	0.074	0.109

Note: This table presents the results of a set of linear models estimated via OLS. Dependent variables are rigidity scores based on Beuve et al. [2019]. The main variable of interest is "Manager_Unknown", which is equal to one if the company and the municipality have interacted in the past, but never with this current set of managers. "Manager_FirstContract" is a dummy equal to one if the contract signed was the first in the city-company relationship. "ManagerTenure" is the natural log of the number of years since the first appearance of the manager(s) in the data. "PastYears" and "PastContracts" indicate the number of years and contracts respectively since the municipality and the company have first interacted. Additional control variables for which coefficients are not included are: a dummy indicating whether the contract was renewed, a dummy indicating whether the contract was ended at the end of the observation period (2009), a type of contract (MP or DSP) dummy variable, the log of the city population at the closest census, a categorical variable indicating whether the parking lot was built under, on or above ground, political ideology of the mayor dummy variables, a Transparency International corruption index, and a dummy indicating whether the contract involved construction. All specifications contain type specific time fixed effects. Std errors clustered at the city $\times$ time level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

2 Bargaining power, renegotiation and repeated interactions: an empirical study of long term public contracts[1,2]

2.1 Introduction

How does time in business affect the balance of bargaining power in long term contractual relationships? This question is particularly relevant to public contracting, where expropriation through renegotiation both by the private party [Iossa and Martimort, 2012] and the public one [Howell and Sadowski, 2018; Valero, 2015] are recurring concerns. The present paper analyses how, in a set of long term contracts from the French parking lot sector, the ability for each party to initiate and and obtain favorable renegotiation varies according to the stock of past interactions parties have together. Renegotiation is an essential topic in procurement policy. The fundamental tradeoff it generates between *ex ante* efficiency and *ex post* adaptation is summarized by Saussier and Tirole [2015]:

> *the renegotiation of contracts [...] tends to limit or even eliminate the benefits of competitive tendering procedures. Renegotiations are, however, useful when then enable the contract to reflect new circumstances, notably in the case of complex and long term contracts. The aim is therefore to encourage beneficial changes whilst fighting opportunistic*

[1]This chapter is the latest version of a work which has greatly evolved since its beginnings. I am grateful to participants at IOEA 2022, SIOE 2022, EARIE 2023 and the Paris workshop celebrating Oliver Williamon's legacy in 2024. I am especially grateful to Marta Troya-Martinez and Sasha Rodivilov for their comments on a very early version of this work.

[2]This chapter is derived from a working paper, currently under review at *International Review of Law and Economics*.

renegotiations [...]

This statement showcases how complex it is to devise appropriate renegotiation rules. It is generally impossible for a third party such as a judge or a lawmaker to determine what constitutes "opportunistic renegotiations" and "beneficial changes". The EU has restricted renegotiations of public contracts to a maximum 50 % of the initial value of the contract, in an attempt to limit excessive renegotiation which would completely undermine efficiency of the awarding procedure, and to guarantee a minimal level of commitment to the initial terms of the contract. However, this threshold remains arbitrary, and insufficient to discriminate between socially improving renegotiations and opportunistic ones. A well known solution to deter opportunistic behavior in the presence of unverifiable outcomes (which we could argue "opportunistic renegotiation" and "beneficial changes" are) is to rely on self-enforcing agreements supported by repeated interactions [Macaulay, 1963; Baker et al., 2002]. While there is evidence, in the private sector, that relational contracts support cooperative adaptation mechanisms to exogenous change [Gil et al., 2021; Barron et al., 2020], research on relational contracts as disciplining tools for renegotiation remains incomplete, especially in the public sector. Studies on the matter include Bafundi et al. [2023], who use high discretion awarding procedures as a proxy for relational contracting, and Domingos et al. [2025], who show that contracts awarded under high discretion undergo more cooperative renegotiation than others. To my knowledge however, there is no study explicitly considering the effect of repeated interactions on renegotiation in a public procurement setting.

In this study, I attempt to cover this gap by analyzing how repeated interactions shape renegotiation processes in long term public contracts from the French parking lot sector, signed between French municipalities and a single operator. Having access to all the amendments made for these contracts, I consider that renegotiation is constituted by any amendment which substantially affects the terms of the contract. My empirically strategy revolves around using proxies for buyer-initiated renegotiations and seller initiated renegotiations, and interacting them with the stock of past experience between parties. The goal of this test is to assess whether one or both of the parties see their ability to obtain renegotiations decrease or increase with time in business. I use two variables designed to capture demand for renegotiations respectively from the contractor and from the municipality. On the contractor side, I leverage the fact that parties may have several simultaneous contracts: I consider that in the

period immediately after being awarded a new contract, the company is more likely to attempt self-interested renegotiations of pre-existing contracts, compared with the period immediately before, where the company has high incentives to behave cooperatively. On the municipality side, I posit that municipalities are more likely to attempt self-interested renegotiations when they are in financial stress (which I proxy with debt increases), in order to obtain revenues from public service operation[3]. Concerning the first variable, I observe that following the awarding of a new contract, price reneogotiations as well as expansions of the parking lots are more likely. On the municipality side, I find that renegotiations of the transfer scheme between the city and the company are more likely when municipal debt increases strongly, potentially indicating self-interested renegotiation from the municipality. I then interact these proxies for renegotiation demand with the stock of past interactions between parties to examine how repeated interactions affect self-interested behavior.

Past interactions do not only allow to capture cooperative, relational dynamics. Indeed, the transaction cost economics (TCE) tradition has emphasized the issue of bilateral dependence in the presence of specific assets [Williamson, 1975; Riordan and Williamson, 1985]. Repeated interactions may potentially be indicative of a "lock-in" situation where opportunism is all but deterred, once sunk investments have made all threats to leave the relationship ineffective. The ambiguous effect of repeated interactions is at display in Oliver E. Williamson's work itself. In his essay on credible commitment, Williamson [1983] supports the relational contracting argument, underlining the idea that increasing stakes in a relationships and broadening its scope helps reduce opportunism:

> *An alternative way by which to protect contracts against expropriation is to expand the contractual relation. One way of accomplishing this is for buyer and seller to devise a mutual reliance relation.*

On another note however, Williamson [1979] also cites frequency of interactions as increasing the risk of opportunism in the presence of specific assets:

> *whereas recurrent spot contracting is feasible for standardized transactions (because large-numbers competition is continuously self- policing in these circumstances), such*

[3] In most of the contracts considered in the empirical analysis, a transfer mechanism exists wherein the operator pays back some amount of the revenue collected to the municipality. The increase of this transfer can thus be a source of revenue for the municipality

contracting has seriously defective investment incentives where idiosyncratic activities are involved. (...)

This work thus seeks to determine whether relational or Coasian dynamics prevail in the parking lot industry, where asset specificity is considered relatively low, but not completely absent [Brown and Potoski, 2003]. Overall, results indicate that the operator's ability to obtain favorable renegotiations (decrease in transfers to the municipality, contract prolongations) increases with time in business, whereas financial renegotiations in reaction to municipal debt increases get less likely. Combined, these results appear to indicate that repeated interactions increase the bargaining power of the private contractor. However, it not impossible that the favorable renegotiations that the contractor obtains in long-standing relationships are parts of an implicit agreement as counterparts for unverifiable performance outcomes.

This paper contributes to two main strands of the literature. First, it relates to the effects of past experience on public-private relationships. Two main conflicting views exist. On one hand, the ability for a public entity to remain in business with the same firm is necessary for the development of relational contracts and the enforcement of unverifiable outcomes [Calzolari and Spagnolo, 2020; Corts and Singh, 2004; Gil and Marion, 2013]. In the presence of incomplete contracts, these relational mechanisms may outperform market mechanisms, such as periodically re-awarding the contract through an auction. On the other, repeatedly awarding a contract to the same firm may indicate that competition is not effective due to the incumbent firm having too strong of an advantage over others [Zupan, 1989; Yvrande-Billon, 2009; Rodrigues et al., 2012; Albalate et al., 2022], allowing it to extract rents from the public party. Incumbent advantage may also stem from favoritism, which also reduces the efficient spending of public monies.

This article also contributes to a rich literature on the determinants of public contract renegotiation. Determinants of renegotiation include contract incompleteness, both involuntary [Hart and Moore, 1988; Bajari et al., 2014; Gagnepain et al., 2013; Carril et al., 2024] and voluntary [Arozamena et al., 2023; Brogaard et al., 2020; Schoenherr, 2019], rigidity and third party opportunism [Beuve et al., 2019, 2021], reforms in renegotiation rules [de Jaegher et al., 2023], incentive provision mechanism [Bajari and Tadelis, 2001] and the decision to bundle design and construction or not in the case of infrastructure projects [Decarolis and Palumbo,

2015]. Studies explicitly relating renegotiation to repeated interactions remain scarce. A closely related study by Beuve and Saussier [2021] captures the relational dimension of renegotiation by showing how it affects renewal probability. Taking the reverse logic, I investigate how repeatedly contracting with a partner shapes the renegotiation process. Another related study is the one by Domingos et al. [2025], who show that contracts awarded with high discretion awarding procedures are renegotiated more cooperatively, potentially indicating a "relational" component to renegotiation. From a theoretical perspective, several papers have investigated renegotiation dynamics in long-term contractual relationships, showing that renegotiation may be used as a mechanism to sustain relational contracts [Klein, 2000; Watson et al., 2020; Kostadinov, 2021].

The remainder of this work is structured as follows. Section 2 develops the theoretical propositions to be tested. Section 3 presents the data used in the empirical part. Section 4 presents the preliminary empirical analysis relating proxies for renegotiation demand with renegotiation outcomes. Section 5 presents the main part of the empirical analysis, where proxies for renegotiation demand are interacted with time in business. Section 6 concludes.

2.2 Propositions

2.2.1 Theoretical hypotheses on past experience and bargaining power

Three general hypotheses can be formed with respect to the effect of repeated interactions on renegotiation. The first two hypothesis originate from TCE and consider the role of repeated interactions as consolidating a bilateral monopoly relationship. The third hypothesis stems from relational contracting, and considers repeated interactions as a disciplining factor.

A standard issue raised by TCE in outsourcing public services concerns incumbent advantage and the dependency of public authorities on a given supplier [Williamson, 1976; Zupan, 1989; Chong et al., 2015]. This incumbent advantage can stem from various theoretical channels: better knowledge of the infrastructure (and the ability to "sabotage" it in case of change in partner), economies of scale from multi-contract relationships [Desrieux et al., 2013], sunk

investments from the public buyer (searching for the right partner, organizing an awarding procedure, negotiating the terms of the contract...). Under such conditions, it should be expected that the supplier gains additional bargaining power through time:

Hypothesis 1a. *The stock of past interactions between parties increases the supplier's relative bargaining power.*

On the other hand, when specific investments are made by the supplier, the balance of bargaining power may shift in favor of the buyer. One may assume that repeated interactions increase the stock of relationship-specific investments made by the contractor, and thus the quasi-rent [Klein et al., 1978] which may be appropriated by the government party. Government side expropriation has been an essential preoccupation in public service management, especially in developing countries [Guasch, 2004; Guasch et al., 2007] Although parking lots are generally considered to be a low asset specificity industry [Brown and Potoski, 2003], long term contracts, especially those involving construction of the infrastructure, may not be entirely devoid of idiosyncratic investments. In this context, one can expect that a long standing partner is made vulnerable by such investments, increasing the bargaining power of the public party:

Hypothesis 1b. *The stock of past interactions between parties increases the municipality's relative bargaining power.*

Finally, repeated interactions may indicate the existence of a relational contract. Although properly identifying relational contracts requires finding proxies for the present discounted value (PDV) of future interactions [Macchiavello and Morjaria, 2015, 2022; Gil and Marion, 2013], past interactions may be an indicator that relational dynamics are at play and have been used to proxy relational contracts [Corts and Singh, 2004]. Sufficiently high stakes on both sides may actually enhance cooperation rather than garner the opportunism typically considered in hold-up situations. Empirical studies have shown that repeated interactions affect the choice of an incentive mechanism in the direction of higher powered incentives [Corts and Singh, 2004] and reduce winning bid levels in procurement auctions [Gil and Marion, 2013].

Hypothesis 2. *The stock of past interactions between both parties reduces self-interested*

renegotiations on both sides.

2.2.2 Testable propositions

It is empirically challenging to isolate the effect of past experience on bargaining power. Directly relating past experience to renegotiation is impossible due to the panel data structure used: the presence of contract fixed effects prevents the estimation for any contract-constant variable. The stock of interactions prior to the present contract being contract-constant, using total past experience between parties would simply amount to a linear time trend over the observation period. The empirical strategy I use in this paper instead revolves around using external proxies for the likelihood of renegotiation and interacting these proxies with variables indicative of time in business between parties. Precisely, I use one proxy (municipal debt) for the likelihood of municipality induced renegotiation, and a second proxy (side contracts) for the likelihood of contractor initiated renegotiation. This allows me to separately estimate the magnitude of buyer and contractor bargaining power, and to relate this bargaining power to the stock of past interactions.

Preliminary propositions

A first set of preliminary propositions concerns the effect of two proxies for likelihood of renegotiation on actual renegotiation outcomes. The first proxy I use concerns contractor initiated renegotiations. It is what I call "side contracts", i.e. the fact that, aside to the main contract being studies, parties sign another deal for the management or construction of a supplementary parking lot. The idea behind this variable is the following: private contractors are unlikely to attempt self-interested renegotiations before the awarding of a new contract, as the municipality has strong leverage and can decide not to award the contract to the opportunistic partner. On the other hand, once the contract is secured, an opportunistic contractor may attempt self-interested renegotiation without fearing retaliation in the short run. Thus, I posit that the signing of a side contract generates a sharp, short-term increase in the ability for the contractor to enforce interested renegotiation:

Proposition 1. *Contractor-favorable renegotiations are more likely immediately following the*

award of a side contract.

The second preliminary proposition I test concerns government initiated renegotiations. Renegotiation can be used by a municipality to improve its financial situation given that most DSP contracts in our data contain a transfer of some share of the revenue generated by the contract back to the municipality. Increasing the amount transferred back to the municipality can be a way for it to obtain financial gains. I hypothesize that in times of financial stress, proxies by high increases in municipal debt, municipalities are more likely to attempt renegotiation of the contract in order to improve their financial situation:

Proposition 2. *Government-favorable renegotiations are more likely in times of municipal financial stress.*

Interacting proxies for likelihood of renegotiation with past experience

Conditional on preliminary propositions 1 and 2 being validated, the empirical strategy aims at indirectly estimating the effect of past experience by interacting side contracts and municipal debt increases with time in business between parties. The combined effect of those interactions may help shed light on the consequences of repeated contracting on renegotiation.

Under Hypothesis 1a, the bargaining power of the supplier increases with time in business. Thus supplier-initiated renegotiations are more likely on longstanding relationships, while municipality-initiated renegotiations in reaction to debt increases are less so, leading to the following pair of propositions:

Proposition 3a. *The interaction of past experience with side contracts increases the likelihood of supplier-favorable renegotiations.*

Proposition 3b. *The interaction of past experience with debt variations decreases the likelihood of buyer-favorable renegotiations.*

Under the symmetrical Hypothesis 1b, the municipality gains bargaining power as parties repeatedly interact. Thus, the ability for the municipality to obtain renegotiations in case

of financial stress increases, while the ability for the contractor to leverage side contracts to obtain self-interest renegotiations weakens:

Proposition 4a. *The interaction of past experience with side contracts decreases the likelihood of supplier-favorable renegotiations.*

Proposition 4b. *The interaction of past experience with debt variations increases the likelihood of buyer-favorable renegotiations.*

Finally, the Hypothesis 2 suggests that repeated interactions reduce opportunistic behavior on both sides, through the development of relational contracts. In this perspective, one may expect that both parties avoid using renegotiation to obtain benefits, in order to preserve the relational contracts. This leads to the following set of propositions:

Proposition 5a. *The interaction of past experience with side contracts decreases the likelihood of supplier-favorable renegotiations.*

Proposition 5b. *The interaction of past experience with debt variations decreases the likelihood of buyer-favorable renegotiations.*

The following table summarizes the hypotheses, variables and expected effects from the empirical analysis.

Table 2.1: Expected effect of variables in the analysis

Variable	**Definition**	**Expected effect on renegotiation**
Relationship	Total number of years parties have continuously been in business together for	[No direct effect of the "Relationship" variable is estimated]
SideContract	$=1$ if a side contract between parties has been signed during the current or previous year of observation (0 if not)	▪ **P1:** Higher likelihood of renegotiations in favor of the supplier
DebtVariations	Percentage of municipal debt variation between year $t-1$ and year t	▪ **P2:** Higher likelihood of renegotiation in favor of the municipality
SideContract $\times$ Relationship		▪ **P4a/P5a:** Negative effect on renegotiation in favor of the contractor ▪ **P3a:** Positive effect on renegotiation in favor of the contractor
DebtVariation $\times$ Relationship		▪ **P3b/P5b:** Negative effect on renegotiation in favor of the municipality ▪ **P4b:** positive effect on renegotiation in favor of the municipality

2.3 Data

2.3.1 General presentation of the data

The initial data set consists in the universe of contracts signed by the largest French parking lot operator and French municipalities between 1963 and 2009. This data set contains 471 contracts and 165 municipalities. For each contract, I have access to the PDF file of the contract itself as well as PDF files for each amendment that occurred before 2010. The initial set of contracts contains both "marché publics" (MP) contracts, which are generally short-term contracts where the contractor bears no financial risks, and "délégations de service public" (DSP) contracts, which are long term contracts where the contractor bears a substantial share of the financial risk. DSP contracts are themselves shared between concessions (involving both construction and operation) and "affermage" (only operation) contracts. In this paper, I focus of the subset of the 281 DSP contracts, because they are much more subject to renegotiation due to their long duration, and to being more prone to incompleteness. Including MP contracts as observations, which are very rarely renegotiated, may generate some noise. MP contracts are however used when measuring side contracts.

For the empirical analysis, I re-organize the data as a panel with a yearly time scale, using each DSP contract as an individual unit. This allows to increase the number of observations, and to control for contract heterogeneity by including contract fixed effects. Because contracts are signed in different years, the observation timespan for each contract is different, leading to an imbalanced panel. Another important feature of this panel is its clustered structure: there may be several simultaneous DSP contracts within a single city, leading to within-city correlation of variables. Specifically, the city of Paris composes a very large part of the data set (around 25% of contracts in the data are signed by the Paris city council).

2.3.2 Renegotiation data

There is debate concerning the operationalization of renegotiation. The theoretical concept of renegotiation is primarily associated with incomplete contract theory [Grossman and Hart, 1986; Hart and Moore, 1988], wherein parties must renegotiate due to some aspects of per-

formance being contractible only *ex post*. Studies such as Gagnepain et al. [2013] aim at empirically capturing this specific notion of renegotiation. This study considers a more general definition of renegotiation, where any modification of the contract substantial enough that it involves an element of bargaining is considered to be renegotiation. This work does not assume that renegotiation is systematically associated with incompleteness: it only seeks at deducing the relative bargaining powers of parties through renegotiation, whether modifications of the contract are provoked by unforeseen events or not. Thus, I associate all amendments, except the ones explicitly classified as minor with renegotiation.

The coding of renegotiation outcomes was performed by manually reading each of the amendments (which were made available to me as PDF documents). I then transformed these renegotiation outcomes into dummy variables adapted for the panel format of the data: Each renegotiation outcome is a dummy variable equal to 1 if a type of outcome is realized for a unit during a given year. These variables are then used as dependent variables in the econometric analysis. The following paragraph describes the coding stage with more precision.

During the initial coding stage, I constituted a database with one row per amendment, and a series of dummy variables indicating whether an outcome was realized. I coded 3 price variables, indicating whether the amendment increased user prices, decreased user prices of modified user prices in a way that was not clearly positive or negative. The latter category was labeled "qualitative" price modifications. Theses qualitative price modifications encompass a large array of situations. Because in general, the pricing system of parking lots is not linear (i.e. the hourly rate is not constant), there are occasions where one price bracket increased while another bracket decreased, which I labeled a qualitative modification of the pricing system. Qualitative price modification also include the creation of special pricing schemes for certain categories of the population (e.g. local residents), or the introduction of price zones for on-street parking. It must be noted that all contracts possess clauses to adjust user prices to inflation without using amendments.

I also coded specific variables to indicate how transfer mechanisms were renegotiated. Typically, in "délégation de service public" contracts, the contractor directly collects revenue from user fees and transfers some amount back to the municipality. This amount is usually a nonlinear function of revenue destined to guarantee higher levels of rent extraction for the municipality when the contractor collects high amounts of user fees. However, financial

arrangements come in quite a large variety: in some cases, the contract plans no transfer (generally, this means that the contractor has constructed the infrastructure and retains all revenue from user fees during a certain period of time in order to compensate the initial investments), and in some cases the transfer is simply a linear function of revenue. Symmetrically with the user price renegotiations, I coded 3 transfer renegotiation dummy variables, one indicating whether the amount transferred to the municipality increases, another indicating if it decreases and a qualitative modification of the transfer scheme variable. Qualitative modifications generally amount to changing the functional form relating revenue to transfer (in a way that is not clearly positive nor negative).

Aside from these financial variables, I coded renegotiation outcomes pertaining to the scope or the magnitude of the service. These come in two main forms: additional investments, and increase of the number of parking lot slots (which does not necessarily come with additional investments, in the case for instance of on-street parking). Another category concerns renegotiation of the duration of the contract. Two main types of contract prolongation appear in the data. A specific category of contract prolongations is designed to give additional time to the municipality to organize a call for tenders when an existing contract about to be terminated. These are generally short-term prolongation, explicitly designed to organize the next call for tenders, and do not entail substantial benefits for the contractor, and I thus do not consider them to be actual prolongations. The remainder of prolongations are classified as "Prolongation", in the sense that they are deliberate decisions to expand the time period for which the contractor may collect revenue from operating the parking lot.

Aside from these categories, a residual category of amendments exist, which do not affect the substantial dimensions of the contract discussed above. I label these amendments "minor". Minor amendments generally deal with administrative or mundane management issues, and do not have long-term consequences on contractual life. They are a testimony of the importance of written provisions in public contracts [Beuve et al., 2019], in contrast with the public sector where reliance on written clauses is generally considered minimal[4]. A list of all specific minor amendment cases is contained in the appendix section.

The following table indicates the expected beneficiaries of each renegotiation outcome. Concerning price renegotiations, a few simplifying assumption must be made. The first one is to

[4]See for instance Bernstein [1992, 2016] on the matter.

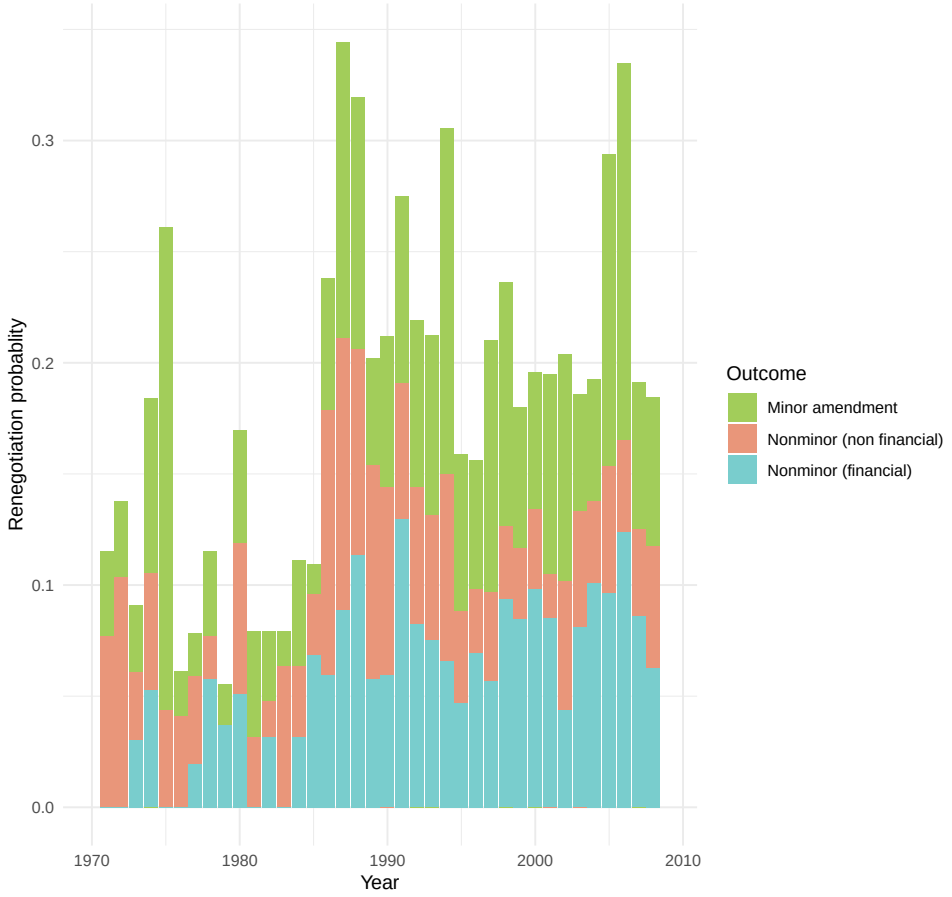
Table 2.2: Renegotiation outcomes and who benefits from them

Renegotiation outcome	**Benefits to:**
Price (Increase)	Company
Price (Decrease)	Uncertain/municipality
Price (Qualitative)	Uncertain
Transfer (Increase)	Company
Transfer (Decrease)	Municipality
Transfer (Qualitative)	Uncertain
Expansion	Company and municipality
Investments	Uncertain/municipality
Prolongation	Company
Minor	Neutral

assume that the initial price charged by the operator is always smaller than monopoly price, such that an increase in price always increases operator profits. I thus assume that price increases primarily benefit to the company. Price decreases on the other hand are assumed to mainly benefit the municipality, who may gain political support from them. Qualitative modifications of the pricing system have uncertain effects. Concerning renegotiations of the transfer scheme, matters are more straightforward as these renegotiations are the result of zero-sum bargaining, where either the company (in case of increase) or the municipality (in case of decrease) collect a greater share of revenue. Expansion renegotiations are assumed to benefit both parties, who may collect additional revenue while also providing political gains by expanding the quantity of parking slots produced. The effect of additional investments being planned out is uncertain: investments may increase quality but may also put a financial burden on the operator. Because this financial burden can be shared between parties, it is unclear which party benefits from the additional investments. However, because additional investments are almost systematically initiated by the municipality, on average I expect new investments to benefit the municipality more than the operator.

The final data set is aggregated at the year level to fit the panel structure. I converted all renegotiation variables to dummy variables, meaning that if, for a given contract, user prices were renegotiated twice during the year, the price renegotiation variable will still be coded as 1. Figure 2.1 describes the yearly frequency of renegotiation, divided into three main subtypes: "Amendments" contains all contract modifications, "NonMinor" excludes minor modifications

Figure 2.1: Evolution of renegotiation likelihood through time



Note: This figure displays empirical probabilities of renegotiation outcomes per year. 3 types of outcomes are considered: "Minor" indicates all contract modifications classified as substantial, "Nonminor (non financial)" indicates substantial modifications which are not classified as financial, "Nonminor (financial)" indicates substantial modifications dealing with prices and/or transfers. For each outcome, probabilities were computed by taking the total occurrence of the outcome over the year and dividing it by the number of ongoing contracts during this year. Calculations by the author.

and "Financial" is the subset of "NonMinor" dealing with prices and/or financial transfers.

2.3.3 Explanatory variables: side contracts, debt increases and past experience

Side contracts. Side contracts are additional contracts signed between parties for another project. Generally, parties sign separate contracts for the operation of parking lots located in different areas, as they are subject to different financial conditions. I assume for the empirical analysis that being awarded a side contract generates a strong increase in bargaining power for the contractor, who can behave opportunistically without fearing retaliation in the short run. I thus created a binary variable "Side contract" indicating whether an additional contract

was signed in the city during the year considered. I also use a dummy variable "Post side contract" indicating whether the observation lies within a two-year span after the award of a side contract.

Municipal debt variations. To proxy demand for renegotiation on the municipality side, I use public finance data. Specifically, I proxy municipal demand for renegotiation with municipal debt increases. The data on municipal public finances is available for all French municipalities from 2000 on, and is issued by the French ministry of public finances[5]. The complete dataset provides several variables on yearly municipal budgets including debt, spendings, investments and taxes. Each variable is expressed in euros, in euros per capita, and as a ratio of the category it belongs to. The variable chosen to proxy financial stress, and thus municipal demand for renegotiation is annual evolution of municipal debt. The idea behind this variable is that municipalities in financial stress attempt to obtain additional revenues from the public service, which may translate in renegotiating the financial terms of parking lot management contracts. This phenomenon appears and the data, as well as in the text of the amendments. Indeed, some renegotiations are explicitly motivated by the fact that the municipality was audited by the *Cour des comptes*[6], who concluded that the municipality could improve its financial situation by renegotiating this contract.

Past experience. Past experience can be expressed both in time or in number of contracts. When looking at past experience expressed in years, it should be noted that all relationships in this data base are uninterrupted, i.e. that there is no municipality for which we observe "breaks" during which no contract is ongoing (in other words, if the relationship terminates, it never starts again). Past experience expressed as a number of contracts suffers from correlation with other relevant variables such as city size, which may be correlated with renegotiation outcomes (through bargaining power for instance). I thus only use it in robustness checks.

Table 2.3 presents descriptive statistics for the variables I use throughout the empirical analysis.

[5] https://www.impots.gouv.fr/cll/zf1/accueil/flux.ex;jsessionid=6B7880D4AFF7DBE0BE17D9A9A81F2FB4?_flowId=accueilcclloc-flow

[6] The Cour des Comptes ("Court of Accounts") is France's supreme audit institution, and is considered an administrative court in French law.

Table 2.3: Summary statistics

Full panel					
	N	Mean	SD	Min	Max
All Amendments	4978	0.17	0.37	0	1
Minor	4978	0.06	0.24	0	1
NonMinor	4978	0.12	0.32	0	1
Price	4978	0.04	0.21	0	1
Price_Up	4978	0.02	0.14	0	1
Price_Down	4978	0.01	0.08	0	1
Expansion	4978	0.03	0.18	0	1
Investments	4978	0.05	0.21	0	1
Transfers	4978	0.03	0.18	0	1
Transfers_Down	4978	0.01	0.10	0	1
Transfers_Up	4978	0.01	0.10	0	1
Prolongation	4978	0.01	0.12	0	1
Side contract	4978	0.24	0.42	0	1
Relationship (years)	4978	20.05	12.85	0	46
Relationship (contracts)	4978	14.57	21.51	0	74
Ongoing contracts in city	4978	15.04	21.92	1	70
Restricted panel (2000-2009)					
	N	Mean	SD	Min	Max
All Amendments	2180	0.17	0.38	0	1
Minor	2180	0.06	0.24	0	1
NonMinor	2180	0.12	0.32	0	1
Price	2180	0.05	0.23	0	1
Price_Up	2180	0.02	0.14	0	1
Price_Down	2180	0.01	0.11	0	1
Expansion	2180	0.03	0.16	0	1
Investments	2180	0.04	0.19	0	1
Transfers	2180	0.03	0.18	0	1
Transfers_Down	2180	0.01	0.11	0	1
Transfers_Up	2180	0.01	0.11	0	1
Prolongation	2180	0.01	0.09	0	1
Ongoing contracts in city	2180	16.81	23.69	1	60
Debt variation	1914	0.03	0.16	0	1
Relationship (Years)	2180	20.88	13.14	0	37
Relationship (Contracts)	2180	15.53	22.58	0	54

2.4 Preliminary evidence: side contracts and municipal debt variations as determinants of renegotiation

2.4.1 Side contracts and renegotiation

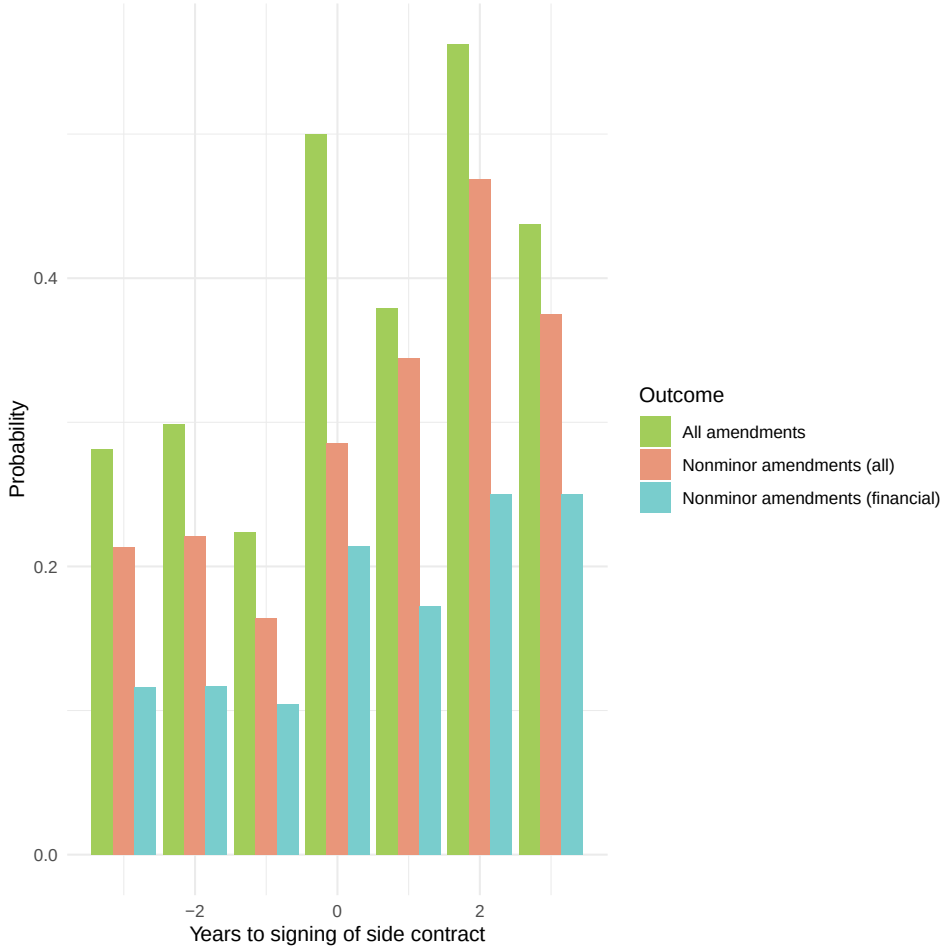
In this subsection, I test the validity of side contracts as a proxy for contractor-initiated renegotiations. I expect contractor favorable renegotiations to be more likely immediately after the contractor is awarded a new deal. Figure 2.5 is a descriptive representation of renegotiation likelihood around side contracts. A significant increase in the likelihood of renegotiation immediately following the awarding of a side contract can be noticed for all kinds of amendments, including financial ones. This finding rather supports the intuition that the contractor would attempt renegotiation after being awarded a new contract. However, other reasons could exist for this effect: for example, coordination between contracts may require a modification of the initial deal.

I further investigate this phenomenon by running an event study relating time to award of a side contract with renegotiation outcomes, using a 3 year pre and post observation span. The "time to event" variable was coded as such: any contracts within 3 years of a side contracts are considered treated. When a contract is within 3 years of several side contracts, the smallest time interval is considered "time to side contract" (for example, if there is a side contract 3 years prior to the observation and a side contract 1 year after the current observation, time to side contract is -1). In the event where an observation is equidistant between two side contracts (i.e. it is as close in the past and in the future to a side contract), it is removed from the data. The baseline value for "time to side contract" is the "Control", was attributed to all treated contracts outside of the 3 year span, as well as to all never treated contracts. The specification is the following:

$$Reneg_{i,c,t} = \beta_1 \sum_{k=t-3}^{t+3} Side_{c,k} + \beta_2 Reneg\_Lags_{i,c,t} + \gamma_i + \delta_t + \varepsilon_{i,c,t} \tag{2.1}$$

where $Reneg$ indicates the set of renegotiation outcomes, $Side$ is a variable indicating whether a side contract was signed in city c at year k, and i, t denote contract and time indicators.

Figure 2.2: Likelihood of renegotiation around new contract signings



Note: This figure presents the renegotiation likelihood for contracts around the signing of a side contract. The data was filtered to keep only observations with 3 years before/after the signing of a side contract. In the case where an observation was both within 3 years before and after a side contract, the closest value from 0 (i.e. the year of the side contract) was selected. In the cases where an observation was equally distant before and after a side contract, it was removed from the data. The data was then aggregated by taking the mean of each outcome dummy per value of the "Years to side contracts" variables. "All amendments" indicates any modification of the contract. "NonMinor amendments (all)" indicates any modification of the contract apart from those classified as minor. "NonMinor amendments (financial)" indicates modifications of the contract dealing with prices or financial transfers. Calculations by the author.

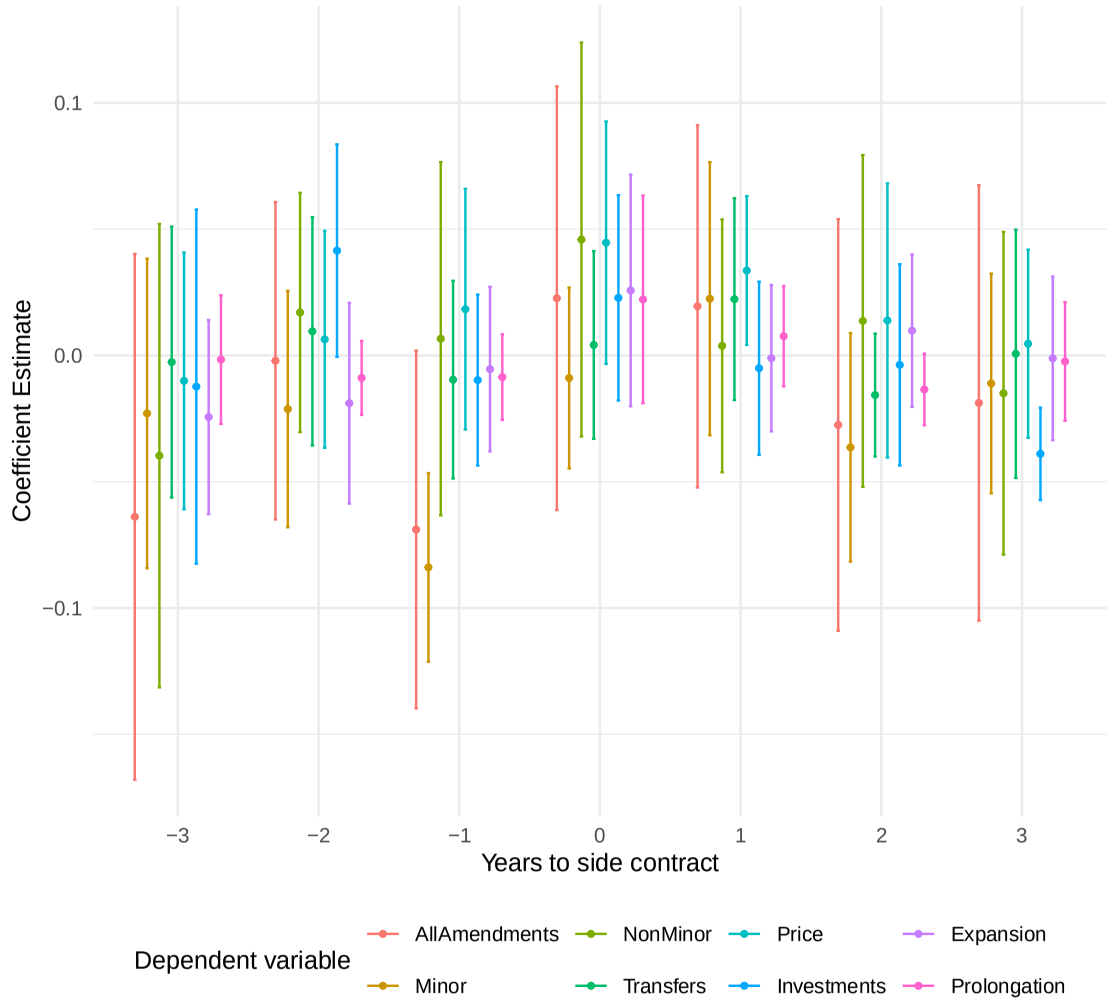
Results for the event study regressions are presented visually in figure 2.3. A striking observation we can first make is the drop in renegotiation probability during the year preceding a side contract. During the pre-side contract period, it is expected that government side bargaining power is at its maximum. Event study results do not seem to indicate that governments use this bargaining power to enforce opportunistic renegotiation. This effect is almost entirely driven by minor amendments, as non minor renegotiations remain roughly at their baseline level. The interpretation of this phenomenon is not entirely clear. Assuming that minor amendments represent a relational cost (i.e. they involve haggling and transaction costs for both parties), then it may be that the drop in minor amendments is due to the private operator refraining from initiating those in order to preserve the quality of the relationship. This interpretation suffers from two main shortcomings. First, upon reading the amendments, it is clear that minor amendments are generally initiated by the public party. Second, given the content of those minor amendments, it is also very unlikely that they harm the relationship to the point where one party refuses to participate in a follow-up contract. A second interpretation for the drop in minor amendments would be that the negotiation sessions for the minor amendments are carried out simultaneously with the negotiations for the follow-up contracts, to economize on transaction costs by scaling on negotiation time. However, we do not observe a spike in minor amendments during side contract awarding years. Upon examining the coefficients for non minor renegotiations, results seem to indicate overall that there are no pre trends concerning substantial renegotiations, supporting the parallel trends assumption. A spike in the likelihood of price renegotiation is to be noted in the time period following the awarding of a side contract. This potentially indicates that increased bargaining power on the contractor side translates into more likely price renegotiations.

I further investigate the effects of side contracts on renegotiation run a simple diff-in-diff regression on all renegotiation outcomes. Because the dynamics appear to be mainly short run, I consider a post treatment period spanning from the year of the side contract to second year following the side contract. The control group is composed of never treated units and units outside of a 3-year treatment window (as is the case in the event study above). To deal with potential reverse causality issues (where past renegotiations affect the probability of side contracts), I control for a set of lagged renegotiation outcomes ($Reneg\_Lags$). The model specification is the following, where $Post\_SideContract$ is a variable indicating that the contract is observed in the two years following the award of a side contract:

$$Reneg_{i,c,t} = \beta_1 Post\_SideContract_{c,k} + \beta_2 Reneg\_Lags_{i,c,t} + \gamma_i + \delta_t + \varepsilon_{i,c,t} \quad (2.2)$$

where subscripts i, c, t denote contract, city and year respectively. Observations are weighted by the inverse of the total number of ongoing contracts in the city for the given year the observation is, to ensure that estimates are not biased by larger cities which have more ongoing contracts. Results (table 2.4) confirm the observation from the event study that price renegotiations are more likely following the award of a side contract. Due to lack of power however, it is impossible to determine whether this effect is driven by price decreases or increases. Were this effect driven by price increases, it would potentially indicate contractor opportunism in the aftermath of a side contract being awarded. However, opportunism would not be the only explanation to such a phenomenon: price increases may be justified by the necessity to generate additional revenue in order to finance works in the new contract. Other renegotiation outcomes are *a priori* not affected by side contracts. Additionally, one should keep in mind that this absence of result may mask heterogeneous effects of side contracts according to the stock of past interactions, which I examine in the following subsection.

Figure 2.3: Event study: renegotiation and time to side contract



Note: This figure presents the results of a set of event study regressions relating time to signature of a side contract with renegotiation outcomes. Estimation is performed with OLS. Confidence bands are at the 95% level. Renegotiation outcomes are a set of 8 binary variables indicating whether the outcome has occurred for a contract during a given year. Each regression contains year and country fixed effects, as well as 1-year lags for each of the renegotiation dummy variables. Standard errors are clustered at the year × city level. Observations are weighted by the inverse number of ongoing contracts in the city of the contract during the year of the observation. The control group is the set of contracts which have no side contracts during their lifespan, i.e. "never treated" units, as well as treated units outside of the 3 year treatment window.

Table 2.4: Side contracts and renegotiation: diff-in-diff estimates

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
PostSideContract	0.031	0.013	0.028	0.030**	0.016	0.003	0.019	0.006	0.010	-0.005	0.009	0.013
	(0.022)	(0.014)	(0.020)	(0.015)	(0.010)	(0.004)	(0.013)	(0.013)	(0.010)	(0.006)	(0.009)	(0.011)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443
R^2	0.242	0.145	0.206	0.222	0.191	0.258	0.187	0.162	0.132	0.161	0.112	0.116
Within R^2	0.007	0.004	0.007	0.007	0.006	0.009	0.011	0.015	0.017	0.011	0.005	0.012

Note: This table presents the result of a diff-in-diff model estimated with OLS. *PostSideContract* is a dummy variable indicating whether the contract is observed in a 2 year period starting at treatment year, the treatment being that parties have signed a side contract. The control group consists of not-yet-treated observations, never treated observations and no longer treated observations (over 3 year after treatment, units are no longer considered treated). Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city $\times$ year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

2.4.2 Debt increases and municipality led renegotiation

The idea tested in this subsection is that when municipalities face high financial stress, they may attempt renegotiation of their contracts to obtain additional revenue from the public service. I estimate a simple model relating municipality financial stress, proxied with municipal debt variations, with renegotiation probability, to check the preliminary hypothesis that financial stress is associated with more frequent renegotiation. The proxy for financial stress is the ratio of debt variation between years t and t-1 labeled $DebtVar$. The estimated model takes the following form:

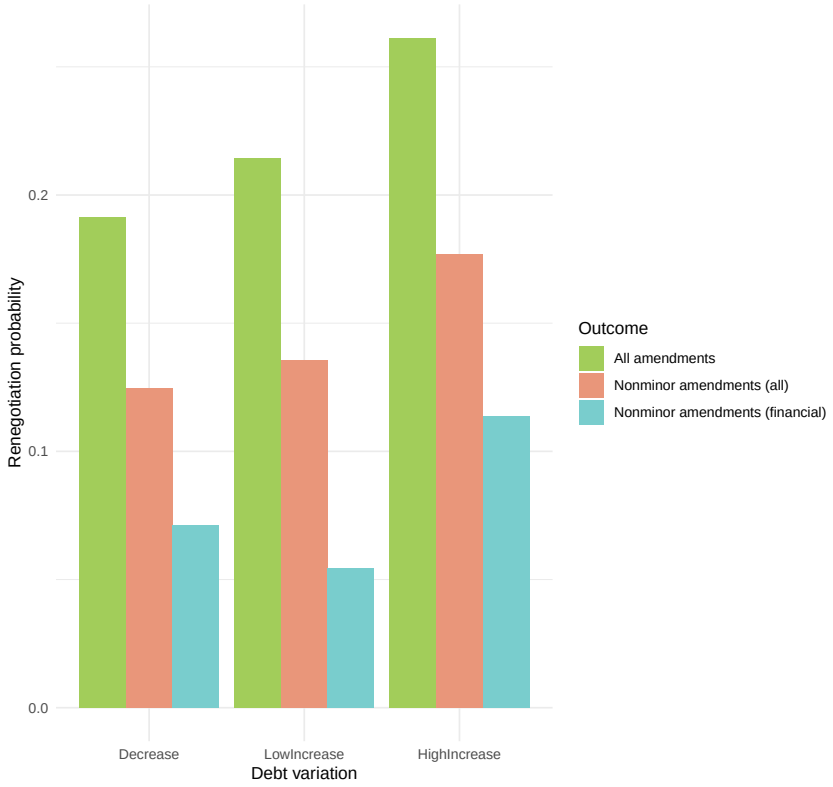
$$Reneg_{i,c,t} = \beta_1 DebtVar_{c,t} + \beta_2 X_{i,c,t} + \delta_t + \gamma_k + \varepsilon_{c,t}$$

The vector of controls X contains lagged renegotiation variables as well a variable indicating tax revenue collected during the year, in order to eliminate the effect of tax revenue on financial stress (i.e. increases in public municipal debt are more sustainable if met with corresponding increases in tax revenue). Models are estimated using OLS, because of the sensitivity of logistic regression to the inclusion of fixed effects [Katz, 2001]. Every model contains year and contract fixed effects. Standard errors are clustered at the city $\times$ year level, given that the treatment occurs at this level. Because I use debt variations and not debt stocks, there is no particular issue of serial correlation in the explanatory variables. Observations are weighted by the inverse of the total number of ongoing contracts in the city for the given year the observation is, to ensure that estimates are not biased by larger cities which have more ongoing contracts. Concerning the endogeneity of municipal debt to renegotiation, the only renegotiation type for which municipalities are realistically likely to contract additional debt is when planning out additional investments. Thus I conjecture that in the general case, municipal debt is exogenous to renegotiation outcomes.

Descriptive evidence. Figure 2.4 shows descriptive evidence of how renegotiation probabilities change according to municipal debt variations. It appears that renegotiation in general is more likely as debt increases, and this effect is driven mainly by nonminor renegotiations. However, upon focusing on the financial subset of renegotiations, the relationship does not appear linear. Indeed, financial renegotiations (dealing with user fees or monetary transfers

between municipality and concessionnaire) are more likely in years of debt decrease that in year of low increase. On the other hand, there is clearly a higher likelihood of financial renegotiation when debt increases strongly than when it increases mildly (the probability of financial renegotiation more than doubles). Overall, descriptive evidence comforts the intuition that in times of high financial stress, financial renegotiation is particularly likely to occur.

Figure 2.4: Debt variations and renegotiation outcomes



Note: this figure presents the empirical probability of renegotiation outcomes as functions of debt categories. The debt variation variable takes 3 modalities : "Decrease" if debt decreases, "LowIncrease" if debt increases less than the median percentage for the municipality, and "HighIncrease" if the increase is larger than the median municipality debt increase. "All amendments" indicates any modification of the contract. "NonMinor amendments (all)" indicates any modification of the contract apart from those classicied as minor. "NonMinor amendments (financial)" indicates modifications of the contract dealing with prices or financial transfers. Renegotiation probabilities are computed as such: I first computed city $\times$ year average probability of each outcome, then took the mean of these average renegotiation probabilities across categories of the Debt variation variable. Calculations by the author.

Econometric results. Results for the effect of $DebtVar$ on renegotiation are presented in in table 2.5. Consistent with the graphical analysis, they indicate that financial stress increases the likelihood of financial renegotiations, specifically pertaining to monetary transfers between the concessionnaire and the municipality, although the coefficient is only significant at the 10% level. This effect is strongly significant (at the 1% level) when focusing on debt increases by interacting debt variations with a debt increase/decrease dummy variable (see Appendix, table 2.9). This result consistent with the graphical observation that there is potentially a

nonlinear effect of debt variations on financial renegotiation, and confirms that strong financial stress is associated with more likely financial renegotiation. Similarly, financial stress increases the likelihood of expansion renegotiations, suggesting that the municipality wishes to collect additional revenue by expanding the number of slots operated. While this result is only significant at the 10% level, when I interacting debt variations with an increase/decrease dummy (table 2.9) the effect of debt increases is positive and significant at the 5% level.

2.5 Past experience and the balance of bargaining powers

In this section, I test the main set of propositions by interacting side contracts and debt variations with (uninterrupted) time in business between parties. The goal of these tests is to assess how repeated interactions shift the balance of bargaining powers and incentives to attempt self-interested renegotiations.

2.5.1 Effect of time in business on contractor-initiated renegotiations

I first interact variable $Post\_Side$, indicative of increased contractor bargaining power, with past experience. The primary variable for past experience I use is the number of years in business, labeled transformed with the inverse hyperbolic sine (IHS)[7] function. The specification is the following:

$$\begin{aligned} Reneg_{i,c,t} = {} & \beta_1 Post\_SideContract_{c,k} \\ & + \beta_2 \left(Post\_SideContract_{c,k} \times Relationship_{c,k}\right) \\ & + \beta_3 Reneg\_Lags_{i,c,t} + \gamma_i + \delta_t + \varepsilon_{i,c,t} \end{aligned} \tag{2.3}$$

where $Relationship$ is the number of years parties have been in business transformed with

[7] The inverse hyperbolic sine (IHS) provides an equivalent transformation to the natural logarithm, except that it is defined over 0.

the IHS function.

Main specification results. Results for the interacted effect of side contracts and past experience are presented in table 2.6. There is no significant evidence of an effect of repeated interactions on the likelihood of price renegotiations, which were positively affected in the initial specification with no interaction terms. However, upon examining coefficients for the non interacted $PostSide$ variable, it appears that, in recent relationships, price increases are more likely following side contracts, while price decreases are less likely. This supports the idea behind proposition 1, i.e. that the contractor has more bargaining power after being awarded a new contract.

Regarding other renegotiation outcomes, two main results emerge upon considering the effect of the interaction term. First, transfer decreases are significantly more likely as parties garner experience: this signifies that the contractor is able to obtain favorable financial renegotiations after being awarded side contracts when parties have a large stock of prior interaction. This effect, which is significant at the 5% level strongly supports the hypothesis that the contractor's bargaining power increases in time. Second, prolongation of the contract is also significantly more likely in older relationships, also indicating that the the contractor is able to obtain favorable renegotiations in longstanding relationships. This evidence should however be tempered by the fact that there is weak evidence (significant at the 10% level) that price decreases are also more likely in older contractual relationships.

Robustness checks. As robustness checks, I use different specifications for the "relationship" variable: using a nontransformed "age of relationship" variable (table 2.11), measuring past experience in number of contracts (table 2.12), and splitting the "age of relationship" variable into dummy variables at the 10, 20 and 30 year thresholds (tables 2.13, 2.14 and 2.15). Overall, the most consistent result concerns the decrease in transfers to the municipality, i.e. the fact that in longstanding relationships, the contractor is able to obtain more favorable financial terms quickly after being awarded a new contract. This result is significant in most specifications.

Table 2.5: Debt increases and renegotiation

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
DebtVariation	0.070	0.059	0.058	0.016	0.044	-0.005	0.090*	-0.005	0.061*	0.016	0.054	-0.011
	(0.086)	(0.048)	(0.106)	(0.045)	(0.046)	(0.012)	(0.048)	(0.037)	(0.033)	(0.027)	(0.038)	(0.010)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914
R^2	0.316	0.242	0.298	0.319	0.406	0.337	0.283	0.285	0.204	0.243	0.152	0.214
Within R^2	0.040	0.024	0.046	0.035	0.026	0.029	0.038	0.049	0.032	0.024	0.011	0.007

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *DebtVariation* is a continuous variable indicating the growth rate of municipal debt between year $t-1$ and year t. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city $\times$ year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

2.5.2 Effect of time in business on municipality-initiated renegotiation

In this subsection, I interact debt increases with variables indicating past experience, in order to capture the effect of repeated interactions on municipality initiated renegotiation. Similarly to the previous subsection, I run a set of robustness checks with various specifications for the "Relationship variable". I also separate the debt variation variable into debt increases and debt decreases by interacting it with a decrease/increase dummy variable, in order to capture the potentially nonlinear effect of debt variations. The general specification is the following:

$$Reneg_{i,c,t} = \alpha_1 DebtVar_{c,t} + \alpha_2 (DebtVar_{c,t} \times Relationship_c) + \beta X_{i,c,t} + \delta_t + \gamma_k + \varepsilon_{c,t} \quad (2.4)$$

Main specifications. Results for the main specification are presented in table 2.7. Although weakly significant, there is evidence that repeated interactions reduce the prevalence of transfer renegotiations during years of financial stress. When accounting for the nonlinear effect of debt variations, by interacting the debt variation variable with a debt increase/decrease dummy, both the main effect and the interacted effects of debt increases become significant at the 5% level (table 2.10). This result indicates that in periods of strong financial stress, the likelihood of financial renegotiations decreases significantly with the the stock of past experience. An intuitive interpretation for this phenomenon is the following: in time of financial stress, municipalities attempt renegotiations of the financial terms. In long standing relationships, the company has garnered sufficient bargaining power to refuse renegotiation without fearing retaliation, as the municipality is locked in the relationship, thus leading to the observed effect. Overall, similarly with the results from the previous subsection, this interpretation would support the idea that the contracting firm gains bargaining power as parties repeatedly interact.

Alternative specifications for past experience. Tables 2.16, 2.17,2.18, 2.19 and 2.20 display results for alternative versions of the past experience variable. Overall, the effect of debt variations and of debt variations interacted with past experience on transfer renegotiations is consistent. It is no longer significant when using the number of contracts as a proxy for

past experience, and when splitting the "age of relationship" variable at the 30 year threshold.

2.6 Conclusion

This study empirically analyses how bargaining power evolves with time in business in public-private relationships, using renegotiation as a source of information on bargaining powers. Results suggest that overall, the operating company is more likely to obtain profitable renegotiations when the relationship has been going on for an extended period of time, while the municipality is less able to obtain renegotiations of financial clauses in reaction to debt increases. Taken together, these results suggest that time in business increases the relative bargaining power of the operating firm and decreases that of the municipality. This is consistent with the fact that parking lots are an industry involving little asset specificity, thus allowing the contractor to easily switch to another relationship. On the other hand, the administrative and search costs incurred by the municipality, as well as the synergies from granting multiple contracts to the same firm, may render the municipality captive and weaken its position in renegotiation. Nevertheless, an explanation through the lens of relational contracting remains possible, if one imagines that firm-favorable renegotiations are performed in exchange for the performance of some unverifiable task by the company. In this case, repeated interactions would be necessary for the relational contract to enter into play. This interpretation of results can be related to works such as Kostadinov [2021]; Watson et al. [2020], where renegotiation of the formal contract is part of an implicit agreement.

Another blind spot concerns the multi-service nature of the company. Despite the parking lot branch being relatively autonomous, the parking lot company considered in this study belongs to a larger conglomerate, which is active in construction in general. It is uncertain whether the contractual relationship considered for parking lots is fully independent from contractual relationships with other branched of the conglomerate. There is evidence of bundling the provision of several goods in service with a single company participating in the development of relational contracts [Desrieux et al., 2013]. Because no data on such multi-service situations is available, this study is unable to take them into account.

Additionally, the proxies for renegotiation demand used in this study (side contracts and

debt variations) can also be subjected to criticism. Concerning debt variations, it should be stressed that they remain an imperfect proxy for financial stress: debt increases may be sustainable for a municipality depending on its fiscal revenue and on the interest rates it faces. Concerning side contracts, as mentioned in the empirical analysis, there is a risk of endogeneity: the signature of side contracts may be conditioned to previous cooperative behavior in renegotiations. By controlling for lagged renegotiation variables, I attempt to take into account the risk of reverse causality at best. Proper identification of the effect of side contracts would require to use instrumental variables. Various instruments were considered (population increases, development of public transportation) but they fail to satisfy the criteria for being valid instrumental variables.

Despite these shortcomings, this study remains a novel source of evidence on the determinants of renegotiations in public contracts. Future works should seek to test whether the propensity of long term relationships to become dominated by one party, as our results suggest, is conditioned by asset specificity, which would support a hold-up theory of renegotiation. Doing so would require obtaining data with similar contracts exhibiting varying degrees of asset specificity. Additionally, evidence is needed on the implications of relational contracting on formal renegotiation in the public sector. While our work does not seem to support a relational theory of renegotiation, future studies should seek to obtain reliable proxies for the existence of a relational contract and study how they shape the renegotiation process of public contracts.

Table 2.6: Side contracts, renegotiations and time in business

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
PostSideContract	0.100	0.027	0.095	0.063	0.077*	-0.026*	0.094	0.035	-0.030	-0.068**	0.020	-0.035*
	(0.115)	(0.070)	(0.102)	(0.056)	(0.045)	(0.014)	(0.080)	(0.043)	(0.057)	(0.026)	(0.047)	(0.018)
PostSideContract × Relationship	-0.019	-0.003	-0.019	-0.010	-0.019	0.009*	-0.023	-0.008	0.012	0.019**	-0.004	0.015***
	(0.033)	(0.021)	(0.031)	(0.018)	(0.013)	(0.005)	(0.022)	(0.012)	(0.016)	(0.008)	(0.013)	(0.005)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443
R^2	0.253	0.159	0.208	0.223	0.191	0.259	0.189	0.164	0.132	0.162	0.112	0.118
Within R^2	0.021	0.020	0.010	0.008	0.007	0.011	0.013	0.016	0.017	0.012	0.006	0.013

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *PostSideContract* is a dummy variable indicating whether the contract is observed in a 2 year period starting at treatment year, the treatment being that parties have signed a side contract. The control group consists of not-yet-treated observations, never treated observations and no longer treated observations (over 3 year after treatment, units are no longer considered treated). *Relationship* is a continuous variable indicating the total number of years since the parties have signed their first contract with each other, transformed using the inverse hyperbolic sine function. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city × year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 2.7: Debt variations, past experience and renegotiation

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
DebtVariation	0.208	-0.104	0.279	-0.002	-0.056	-0.004	0.156	-0.041	0.285*	0.177	0.109	0.001
	(0.177)	(0.110)	(0.200)	(0.122)	(0.119)	(0.039)	(0.117)	(0.086)	(0.136)	(0.135)	(0.095)	(0.027)
DebtVariation × Relationship	-0.051	0.060	-0.082	0.007	0.037	-0.0006	-0.024	0.013	-0.083*	-0.060	-0.020	-0.004
	(0.046)	(0.041)	(0.050)	(0.034)	(0.040)	(0.012)	(0.036)	(0.028)	(0.042)	(0.041)	(0.022)	(0.007)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914
R^2	0.316	0.244	0.300	0.320	0.407	0.337	0.283	0.285	0.209	0.248	0.153	0.214
Within R^2	0.041	0.026	0.048	0.035	0.029	0.029	0.038	0.049	0.038	0.030	0.012	0.007

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *DebtVariation* is a continuous variable indicating the growth rate of municipal debt between year $t-1$ and year t. *Relationship* is a continuous variable indicating the total number of years since the parties have signed their first contract with eachother, transformed using the inverse hyperbolic sine function. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city × year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

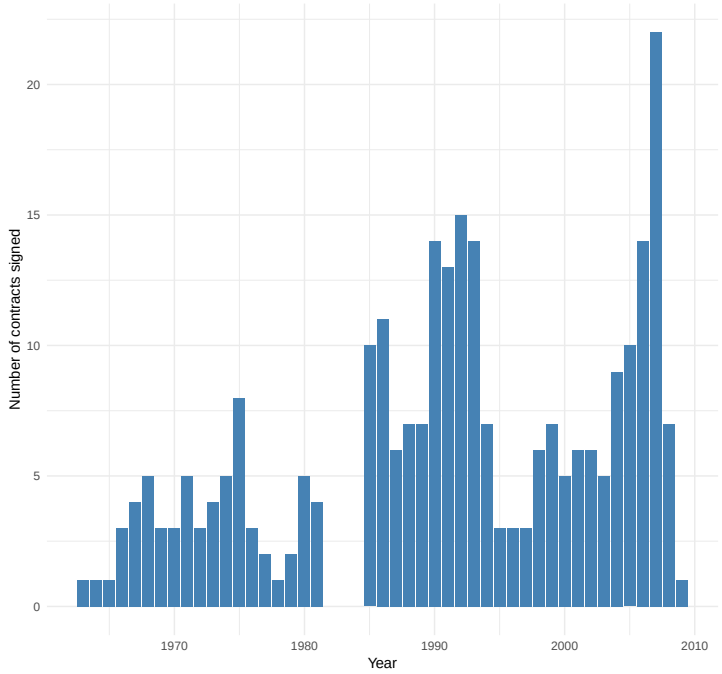
Appendix B

B1: Descriptive statistics

Table 2.8: Contract characteristics

Statistic	N	Min	Max	Mean
Starting year	274	1963	2009	1991.23
Duration	274	1	65	26.204
Number of slots	272	30	23,481	971
Construction	274	0	1	0.445
Number of amendments	274	0	30	3.85
Ended	274	0	1	0.10

Figure 2.5: DSP contract signings per year



Note: This figure presents the number of DSP contracts, which serve as units in our panel, signed per year. Source: Author.

B2: List of minor amendment cases

Below is a list summarizing the content of amendments which have not been classified in the set of substantial renegotiation outcomes:

- Re-counting of slots in the parking lot;
- Revision of the price-revision indices after public authorities have published new recommendations;
- Marginal changes in the wording of some provisions;
- Changes in the rules applicable to the security personnel;
- Minor changes in the way the car park is operated (opening hours, number of slots);
- Introduction of a yearly price revision clause as a consequence of a prolongation amendment (such a clause was not needed before as the contract was expected to last only one year);
- Marginal changes in the goods to be provided (time stamps);
- Change in the way the VAT is collected after a change of concessionnaire;
- Municipality certifies that works were conducted properly;
- Minor changes as a consequence of a previous amendment;
- Clarifications on how the breakdown of the price (with no financial consequence);
- Precision onthe payment of the "régisseur" after the collection of fees was delegated to a "régisseur";
- Two security guards are hired
- Concessionnaire yields old materials to the municipality (small amount)
- Change in the indices used for the yearly performance report

- Introduces the possibility to divide the hourly rate into ten-minute time slots
- Change in the name of the concessionnaire
- After specific payment card was introduced, change in the way such card is attributed (can only be sold to customers by a bank)
- Change on the way fees are collected on request of State officials
- Change from Francs to Euros
- Rectification of an error made in a previous amendment
- Merging two contract documents
- Precision on the cost of works
- Definition of the opening date of the car park

B3: Nonlinear effect of debt variations

Table 2.9: Debt variations and renegotiation (Debt variations separated in decreases and increases)

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
DebtVariation × DebtDecrease	-0.116	0.274**	-0.302**	-0.163*	-0.019	-0.007	-0.036	-0.085	-0.071	-0.073	0.002	0.033
	(0.158)	(0.096)	(0.130)	(0.086)	(0.062)	(0.042)	(0.067)	(0.078)	(0.063)	(0.063)	(0.032)	(0.034)
DebtVariation × DebtIncrease	0.147**	-0.030	0.207**	0.090	0.070	-0.004	0.143**	0.028	0.116***	0.053	0.075	-0.029
	(0.054)	(0.067)	(0.085)	(0.055)	(0.065)	(0.008)	(0.059)	(0.061)	(0.019)	(0.038)	(0.044)	(0.026)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914
R^2	0.316	0.245	0.302	0.321	0.406	0.337	0.284	0.285	0.205	0.244	0.152	0.215
Within R^2	0.041	0.027	0.051	0.038	0.027	0.029	0.040	0.050	0.034	0.025	0.012	0.008

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *DebtDecrease* and *DebtIncrease* are the two values of a binary variable indicating whether municipal debt has decreased or increased between years $t-1$ and t. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city × year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 2.10: Past experience, debt variations and renegotiation (debt variations separated in decreases and increases)

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
DebtVariation × DebtDecrease	-0.345	-0.029	-0.270	-0.397	-0.219	-0.033	-0.132	0.039	-0.265	-0.273	-0.001	0.097
	(0.445)	(0.202)	(0.274)	(0.295)	(0.148)	(0.131)	(0.271)	(0.065)	(0.269)	(0.224)	(0.091)	(0.087)
DebtVariation × DebtIncrease	0.434*	-0.065	0.433	0.126	-0.008	0.011	0.259*	-0.123	0.542**	0.394	0.149	-0.029
	(0.215)	(0.099)	(0.246)	(0.133)	(0.164)	(0.031)	(0.129)	(0.108)	(0.181)	(0.219)	(0.098)	(0.051)
DebtVariation × DebtDecrease × Relationship (years)	0.086	0.098	-0.002	0.078	0.062	0.009	0.036	-0.046*	0.080	0.078	0.004	-0.021
	(0.134)	(0.081)	(0.080)	(0.076)	(0.039)	(0.032)	(0.081)	(0.020)	(0.081)	(0.071)	(0.024)	(0.020)
DebtVariation × DebtIncrease × Relationship (years)	-0.115	0.015	-0.091	-0.014	0.032	-0.006	-0.047	0.060	-0.171**	-0.137	-0.030	-0.0004
	(0.076)	(0.045)	(0.087)	(0.044)	(0.060)	(0.014)	(0.054)	(0.042)	(0.068)	(0.074)	(0.025)	(0.013)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914
R^2	0.317	0.246	0.303	0.322	0.409	0.337	0.285	0.286	0.214	0.254	0.153	0.216
Within R^2	0.042	0.028	0.052	0.038	0.030	0.029	0.041	0.051	0.044	0.038	0.013	0.009

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *DebtVariation* is a continuous variable indicating the growth rate of municipal debt between year $t-1$ and year t. *DebtDecrease* and *DebtIncrease* are the two values of a binary variable indicating whether municipal debt has decreased or increased between years $t-1$ and t. *Relationship* is a continuous variable indicating the total number of years since the parties have signed their first contract with eachother, transformed using the inverse hyperbolic sine function. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city × year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

B4: Alternative specifications for past experience

This section presents tests with different specifications for the past experience variable. They include models with non-IHS transformed "Relationship" variable, various splits of the "Relationship" variable into binary variables, and specifications with the number of contracts instead of the number of years as a proxy for past interactions.

Table 2.11: Past experience, Side contracts and renegotiation (past experience not IHS transformed)

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
PostSideContract	-0.023	-0.038	0.021	0.018	0.032	-0.016**	0.031	-0.0009	-0.008	-0.034**	0.021	0.003
	(0.051)	(0.030)	(0.038)	(0.026)	(0.020)	(0.006)	(0.029)	(0.024)	(0.023)	(0.013)	(0.018)	(0.015)
PostSideContract × Relationship	0.003	0.003*	0.0004	0.0007	-0.0010	0.001**	-0.0008	0.0004	0.001	0.002**	-0.0007	0.0006
	(0.002)	(0.002)	(0.002)	(0.001)	(0.0008)	(0.0005)	(0.001)	(0.0008)	(0.001)	(0.0007)	(0.0008)	(0.0004)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443
R^2	0.243	0.146	0.206	0.223	0.191	0.259	0.187	0.162	0.132	0.163	0.113	0.116
Within R^2	0.008	0.006	0.007	0.008	0.007	0.010	0.011	0.015	0.017	0.013	0.006	0.012

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *PostSideContract* is a dummy variable indicating whether the contract is observed in a 2 year period starting at treatment year, the treatment being that parties have signed a side contract. The control group consists of not-yet-treated observations, never treated observations and no longer treated observations (over 3 year after treatment, units are no longer considered treated). *Relationship* is a continuous variable indicating the total number of years since the parties have signed their first contract with each other. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city × year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 2.12: Past experience, Side contracts and renegotiation (past experience expressed in number of contracts)

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
PostSideContract	0.049	0.018	0.065	0.019	0.019	-0.015	0.037	0.023	0.002	-0.038**	0.035	0.022
	(0.052)	(0.029)	(0.056)	(0.034)	(0.019)	(0.009)	(0.033)	(0.033)	(0.034)	(0.019)	(0.030)	(0.026)
PostSideContract × PastContracts	-0.006	-0.0010	-0.017	0.005	-0.002	0.009	-0.009	-0.004	0.005	0.019*	-0.014	-0.004
	(0.021)	(0.011)	(0.028)	(0.016)	(0.009)	(0.007)	(0.015)	(0.015)	(0.014)	(0.011)	(0.014)	(0.011)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443
R^2	0.243	0.145	0.207	0.223	0.191	0.258	0.188	0.164	0.132	0.162	0.114	0.117
Within R^2	0.007	0.004	0.008	0.008	0.006	0.009	0.011	0.017	0.017	0.012	0.007	0.012

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *PostSideContract* is a dummy variable indicating whether the contract is observed in a 2 year period starting at treatment year, the treatment being that parties have signed a side contract. The control group consists of not-yet-treated observations, never treated observations and no longer treated observations (over 3 year after treatment, units are no longer considered treated). *PastContracts* is a continuous variable indicating the total number of contracts parties have signed together in the past, transformed using the inverse hyperbolic sine function. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city × year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 2.13: Past experience, Side contracts and renegotiation (Dummy variable for age of relationship, 10 year split)

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
PostSideContract	0.007	-0.012	0.019	0.040*	0.034	-0.001	0.018	-0.006	-0.003	-0.018**	0.007	-0.009
	(0.045)	(0.026)	(0.038)	(0.023)	(0.023)	(0.001)	(0.028)	(0.016)	(0.024)	(0.008)	(0.021)	(0.008)
PostSideContract × Relationship_10	0.035	0.035	0.013	-0.014	-0.026	0.006	0.001	0.017	0.018	0.019**	0.002	0.031**
	(0.054)	(0.029)	(0.051)	(0.030)	(0.026)	(0.005)	(0.029)	(0.018)	(0.030)	(0.009)	(0.026)	(0.013)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443
R^2	0.242	0.145	0.206	0.223	0.191	0.258	0.187	0.162	0.132	0.162	0.112	0.118
Within R^2	0.007	0.005	0.007	0.007	0.007	0.009	0.011	0.015	0.017	0.011	0.005	0.013

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *PostSideContract* is a dummy variable indicating whether the contract is observed in a 2 year period starting at treatment year, the treatment being that parties have signed a side contract. The control group consists of not-yet-treated observations, never treated observations and no longer treated observations (over 3 year after treatment, units are no longer considered treated). *Relationship_10* is a dummy variable indicating whether parties have been in business for over 10 years or not. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city × year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 2.14: Past experience, Side contracts and renegotiation (Dummy variable for age of relationship, 20 year split)

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
PostSideContract	0.003	-0.008	0.017	0.022	0.020*	-0.004	0.017	-0.002	0.0003	-0.017***	0.016	0.010
	(0.030)	(0.017)	(0.024)	(0.017)	(0.011)	(0.002)	(0.016)	(0.016)	(0.014)	(0.006)	(0.011)	(0.013)
PostSideContract × Relationship_20	0.103**	0.073	0.038	0.029	-0.014	0.025**	0.007	0.028	0.033*	0.043**	-0.027**	0.010
	(0.051)	(0.049)	(0.031)	(0.027)	(0.013)	(0.011)	(0.022)	(0.020)	(0.019)	(0.017)	(0.011)	(0.015)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443
R^2	0.243	0.146	0.206	0.223	0.191	0.259	0.187	0.163	0.132	0.164	0.114	0.116
Within R^2	0.009	0.006	0.008	0.008	0.006	0.010	0.011	0.015	0.017	0.013	0.007	0.012

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *PostSideContract* is a dummy variable indicating whether the contract is observed in a 2 year period starting at treatment year, the treatment being that parties have signed a side contract. The control group consists of not-yet-treated observations, never treated observations and no longer treated observations (over 3 year after treatment, units are no longer considered treated). *Relationship_20* is a dummy variable indicating whether parties have been in business for over 20 years or not. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city × year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 2.15: Past experience, Side contracts and renegotiation (Dummy variable for age of relationship, 30 year split)

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
PostSideContract	0.019	0.001	0.025	0.023	0.016	-0.002	0.021	0.004	0.006	-0.011*	0.011	0.014
	(0.025)	(0.013)	(0.021)	(0.015)	(0.010)	(0.002)	(0.014)	(0.016)	(0.011)	(0.007)	(0.009)	(0.012)
PostSideContract × Relationship_30	0.104*	0.093**	0.022	0.059*	0.0006	0.039*	-0.020	0.017	0.032	0.050*	-0.019**	-0.006
	(0.054)	(0.044)	(0.047)	(0.032)	(0.019)	(0.021)	(0.024)	(0.035)	(0.033)	(0.029)	(0.009)	(0.014)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443	4,443
R^2	0.243	0.147	0.206	0.223	0.191	0.260	0.187	0.162	0.132	0.163	0.113	0.116
Within R^2	0.008	0.006	0.007	0.008	0.006	0.011	0.011	0.015	0.017	0.013	0.006	0.012

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *PostSideContract* is a dummy variable indicating whether the contract is observed in a 2 year period starting at treatment year, the treatment being that parties have signed a side contract. The control group consists of not-yet-treated observations, never treated observations and no longer treated observations (over 3 year after treatment, units are no longer considered treated). *Relationship_30* is a dummy variable indicating whether parties have been in business for over 30 years or not. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city × year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 2.16: Debt variations, past experience and renegotiation (non transformed past experience variable)

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
DebtVariation	0.177	-0.007	0.201	0.016	-0.008	-0.007	0.145*	0.001	0.157**	0.065	0.096	-0.012
	(0.109)	(0.058)	(0.132)	(0.077)	(0.087)	(0.018)	(0.077)	(0.059)	(0.060)	(0.059)	(0.066)	(0.017)
DebtVariation × Relationship	-0.008**	0.005	-0.011**	-5.47×10^{-5}	0.004	0.0001	-0.004	-0.0005	-0.007**	-0.004	-0.003	7.91×10^{-5}
	(0.003)	(0.004)	(0.004)	(0.003)	(0.005)	(0.0007)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.0006)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914
R^2	0.317	0.243	0.301	0.319	0.407	0.337	0.284	0.285	0.207	0.244	0.154	0.214
Within R^2	0.041	0.025	0.050	0.035	0.029	0.029	0.039	0.049	0.036	0.026	0.014	0.007

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *DebtVariation* is a continuous variable indicating the growth rate of municipal debt between year $t-1$ and year t. *Relationship* is a continuous variable indicating the total number of years since the parties have signed their first contract with eachother. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city × year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 2.17: Past experience, debt variations and renegotiation (Past experience expressed in number of contracts)

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
DebtVariation	0.106	0.111	0.056	-0.128	-0.055	-0.022	0.140	0.085	0.144	0.102	0.030	-0.014
	(0.177)	(0.078)	(0.189)	(0.072)	(0.052)	(0.037)	(0.101)	(0.096)	(0.125)	(0.116)	(0.047)	(0.029)
DebtVariation × PastContracts	-0.030	-0.043	0.002	0.119**	0.082*	0.014	-0.042	-0.075	-0.069	-0.072	0.020	0.003
	(0.078)	(0.052)	(0.085)	(0.038)	(0.042)	(0.027)	(0.052)	(0.054)	(0.083)	(0.079)	(0.017)	(0.017)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914
R^2	0.316	0.243	0.298	0.321	0.408	0.337	0.283	0.286	0.205	0.245	0.152	0.214
Within R^2	0.040	0.024	0.046	0.037	0.029	0.029	0.038	0.050	0.033	0.026	0.011	0.007

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *DebtVariation* is a continuous variable indicating the growth rate of municipal debt between year $t-1$ and year t. *PastContracts* is a continuous variable indicating the total number of contracts parties have signed together in the past, transformed using the inverse hyperbolic sine function. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city × year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 2.18: Past experience, debt variations and renegotiation (Dummy variable for past experience, 10 year split)

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
DebtVariation	0.133	0.0002	0.162	0.018	0.018	-0.008	0.122*	0.003	0.125**	0.043	0.087	-0.014
	(0.090)	(0.051)	(0.107)	(0.066)	(0.071)	(0.015)	(0.062)	(0.052)	(0.047)	(0.044)	(0.055)	(0.016)
DebtVariation × Relationship_10	-0.160	0.148	-0.263**	-0.007	0.065	0.008	-0.080	-0.021	-0.160**	-0.069	-0.084	0.008
	(0.106)	(0.121)	(0.099)	(0.072)	(0.111)	(0.012)	(0.077)	(0.070)	(0.061)	(0.050)	(0.057)	(0.017)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914
R^2	0.316	0.244	0.301	0.319	0.407	0.337	0.283	0.285	0.207	0.244	0.154	0.214
Within R^2	0.041	0.026	0.050	0.035	0.027	0.029	0.039	0.049	0.036	0.025	0.014	0.007

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *DebtVariation* is a continuous variable indicating the growth rate of municipal debt between year $t-1$ and year t. *Relationship_10* is a dummy variable indicating whether parties have been in business for over 10 years or not. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city × year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 2.19: Past experience, debt variations and renegotiation (Dummy variable for past experience, 20 year split)

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
DebtVariation	0.162	0.055	0.155	0.012	0.011	-0.007	0.136*	0.024	0.110**	0.038	0.077	-0.016
	(0.090)	(0.062)	(0.116)	(0.058)	(0.062)	(0.014)	(0.070)	(0.054)	(0.042)	(0.038)	(0.051)	(0.015)
DebtVariation × Relationship_20	-0.278***	0.011	-0.293**	0.012	0.098	0.006	-0.139	-0.088	-0.146**	-0.066	-0.070	0.015
	(0.074)	(0.099)	(0.114)	(0.057)	(0.114)	(0.013)	(0.086)	(0.072)	(0.052)	(0.044)	(0.050)	(0.015)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914
R^2	0.318	0.242	0.302	0.319	0.408	0.337	0.285	0.286	0.206	0.244	0.153	0.215
Within R^2	0.043	0.024	0.050	0.035	0.029	0.029	0.041	0.050	0.035	0.025	0.013	0.007

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *DebtVariation* is a continuous variable indicating the growth rate of municipal debt between year $t-1$ and year t. *Relationship_20* is a dummy variable indicating whether parties have been in business for over 20 years or not. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city × year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 2.20: Past experience, debt variations and renegotiation (Dummy variable for past experience, 30 year split)

Dependent Variables:	Renegotiation	Minor	NonMinor	Price	Price_Up	Price_Down	Expansion	Investments	Transfers	Transfers_Down	Transfers_Up	Prolongation
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
DebtVariation	0.075	0.062	0.061	0.010	0.042	-0.007	0.096*	2.97×10^{-5}	0.060	0.016	0.053	-0.011
	(0.087)	(0.051)	(0.109)	(0.047)	(0.047)	(0.012)	(0.049)	(0.038)	(0.034)	(0.028)	(0.038)	(0.011)
DebtVariation × Relationship_30	-0.393	-0.246	-0.204	0.414	0.089	0.142	-0.458*	-0.386	0.117	0.038	0.036	0.034
	(0.388)	(0.337)	(0.361)	(0.252)	(0.161)	(0.102)	(0.231)	(0.272)	(0.222)	(0.085)	(0.103)	(0.036)
Fixed-effects												
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contract	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics												
Observations	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914	1,914
R^2	0.316	0.243	0.298	0.320	0.406	0.338	0.284	0.286	0.204	0.243	0.152	0.214
Within R^2	0.040	0.024	0.046	0.036	0.026	0.029	0.040	0.050	0.032	0.024	0.011	0.007

Note: This table presents the result of a two-way fixed effects model estimated with OLS. *DebtVariation* is a continuous variable indicating the growth rate of municipal debt between year $t-1$ and year t. *Relationship_30* is a dummy variable indicating whether parties have been in business for over 30 years or not. Dependent variables are a set of binary variables indicating whether a renegotiation outcome has occurred during the year in the observed contract. Observations are weighted by the inverse number of ongoing contract in the city × year of the observation. Std errors are clustered at the city and year level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

3 Mayor changes and competition: an empirical study of French municipal procurement (2015-2023)[1, 2]

3.1 Introduction

Due to the absence of term limits, mayors of French municipalities often remain in office for extended periods of time, sometimes spanning over several decades[3]. Such extended mandates may lead to the development of connections with local firms, biasing the outcome of procurement auctions and potentially fueling favoritism. In a closely related study on Italian municipal procurement, Coviello and Gagliarducci [2017] show that extended tenure in office does lead to deteriorated procurement outcome, and document the improving effects of the introduction of a term limit in 1993. The effects of politician tenure are a long debated topic in economics and politics (e.g. Adams and Kenny [1986]). The argument that politician tenure increases corruption has found support in the public procurement literature, but also in a study dealing with media influence: Besley and Prat [2006] show that longstanding officials are more likely to develop media capture, potentially leading to biased reporting. On the other hand, term limits may induce short-sightedness of elected officials and suppress the disciplining effect of having to sustain a durable political reputation [Besley and Case, 1995], especially

[1] This chapter co-authored with Adrien Deschamps, currently a PhD student at university of Avignon.

[2] I thank Lucas Eustache for research assistance on a preliminary version of this work.

[3] Examples are numerous. Laurent Cathala, the current mayor of Créteil, in the suburbs of Paris, was first elected in 1977 and was reelected for the eighth time in 2020. Famous anticolonial poet and politician Aimé Césaire remained mayor of Fort-de-France, the largest city (74,000 inhabitants) in the island of Martinique for 56 years (1945-2001). The record belongs to Paul Girod, who died after 63 years at the head of Droizy, a small hamlet (74 inhabitants) in Northern France.

during the official's final term, when electoral threats are no longer a viable incentive [Barro, 1973]. Moreover, it has been argued that extended tenure in office allows for learning [Miquel and Snyder, 2004] and increased politician effort [Dal Bó and Rossi, 2011].

This paper questions whether longstanding mayors reduce the competitiveness of procurement auctions in the case of French municipal procurement. Using an dataset comprised of all municipal contract award notices published over the 2015-2023 period, we examine how exogenous endings of municipal office affect a set of procurement outcomes including prices, number of bidders, geographical origin of the winner and identity of the winner. Our empirical strategy relies on two quasi-experiments. First, we study the effect of mayor deaths using a difference-in-difference strategy. Second, we use close electoral races in the 2014 and 2020 municipal elections to obtain causal estimates for the effect of electoral turnover on procurement outcomes. In the case of mayor deaths, we do not find overall evidence of an increase in competition, potentially due to small number (50) of deaths which occurred over the observation period. However, results show evidence of increased competition following changes in mayor identity associated with electoral turnovers. A particularly robust finding is that local firms are less likely to win procurement auctions following an electoral turnover. Additionally, when varying the intensity of the treatment by including tenure of the previous mayor, we find that the effect of turnovers is stronger in elections where the incumbent mayor was in office for a long period of time. This result suggests that extended periods in office allow for the development of low competition procurement auctions, potentially biased in favor of local and/or known suppliers. To our knowledge, this study is the first to estimate the causal effect of changes in mayor identity on procurement outcomes in the French case.

Testing two causes of mayor change allows us to distinguish between a mayor effect and a municipal council effect. Indeed, in the case of mayor death, the composition of the municipal council does not change, and the new mayor is elected among the remaining members. On the other hand, electoral turnovers are associated with an recomposition of the municipal council as a whole. Whether connections of the municipality with specific firms are linked to the person of the mayor, or to the municipal council as a whole remains uncertain. Our results indicate clearer and stronger effects for changes in the municipal council associated with electoral turnover. However, the difference in sample size between both quasi experiments is likely to be the main explanatory channel for the observed discrepancy in statistical power.

Procurement contracts in France are highly regulated by law, such that their attribution should be dictated solely by economic efficiency concerns, and so that favoritism should not be able to occur. However, even in highly regulated contexts, evidence exists of procurement rules being manipulated to achieve favoritism: in the presence of thresholds below which high discretion procurement is possible, it is a well documented phenomenon that contracts tend to "bunch" below these thresholds [Carril, 2022; Szucs, 2023]. *Ex post* renegotiation may also allow favored firms to bid below their expected costs, knowing that they will be granted advantageous financial conditions through modification of the contract [Arozamena et al., 2023]. Additionally, scoring rule auctions can be manipulated to place more weight on the criteria that will favor the preferred firm [Laffont and Tirole, 1991]. Evidence of reduced competition in the presence of longstanding mayors does not *per se* constitute a diagnosis of favoritism. Indeed, the observed results (higher number of bidders and lower likelihood that a local firm wins an auction following a change in mayor) could simply be driven by firm expectations: companies face transaction costs in bidding for a procurement contract, and will only place a bid if the expected gains from the contract exceed the cost of bidding. Expectations on the supply side of potential favoritism (and of reduced favoritism following a change in mayor) are sufficient to obtain the results we observe. Our study thus calls for follow-up works identifying explicit mayor-firm connections, as is performed in the literature on favoritism.

The welfare implications of our work also remain up for debate. Whether "cozy" [Calzolari and Spagnolo, 2020] procurement policies are necessarily welfare reducing is not certain. Increased competition is generally associated with improved procurement outcomes, among which lower prices [İlke Onur et al., 2012; Iimi, 2006; Bulow and Klemperer, 1996] and low costs of operation [Amaral et al., 2013]. However, low competition in itself is not a problem if relational contracts provide appropriate incentives to private contractors, and the existence of relational contracts may deter competitors from entering auctions. Relational contracts may even prove superior to competition in the presence of incomplete contracts [Calzolari and Spagnolo, 2020; Carril, 2022; Coviello et al., 2018a]. Restricted auctions and negotiated procedures have been shown to be more efficient in settings where incomplete contracts are a significant issue [Bajari and Tadelis, 2001; Bajari et al., 2009]. Thus, our work also calls for expansions on the welfare consequences of reduced competition in municipalities where mayors have been in office for extended periods. Other studies have used various proxies to

identify *ex post* (in) efficiency of procurement contracts, such cost as time and cost overruns or renegotiations [Decarolis et al., 2020b]. Such variables are to this day not available for the procurement data we are considering.

This chapter contributes to a large and growing literature on the identification of favoritism in public procurement. A number of papers use changes in thresholds below which high discretion awarding procedures exist, to identify whether public authorities manipulate the value of awarded contracts to benefit from higher discretion [Carril, 2022; Szucs, 2023; Celis Galvez et al., 2025]. Decarolis et al. [2020a] show that corrupt officials tend to disproportionately use discretionary procedures. Other studies rely on finding direct links between elected officials and corporations, through political donations from firms [Brogaard et al., 2020; Titl and Geys, 2019] or participation of firm management to political parties [Baránek and Titl, 2024]. Similar to the second part in our study, Coviello and Gagliarducci [2017] use municipal election results to proxy exogenous changes of mayor.

The second part of the empirical analysis participates in a literature on the effects of electoral turnover on public policy outcomes using regression discontinuity designs. Outside of the field of public procurement, Akhtari et al. [2022] document the effects political turnover on bureaucratic turnover study in the case of Brazilian municipalities. They show that bureaucratic upheavals following political turnovers reduce the efficiency of public service provision. Bazzi et al. [2025] study the case of political turnover in Indonesian villages and show significant improvement of policy outcomes following municipal turnover. Marx et al. [2024] study a set of national (parliamentary and presidential) elections over the 1946-2018 period, and show that at the country level, turnovers improve macroeconomic outcomes such as trade intensity and inflation reduction.

The remainder of this chapter is structured as follows. Section 2 presents the institutional and legal background for this study. Section 3 presents the data used. Section 3 constitutes the first part of our empirical analysis, where we study the effect of mayor deaths on procurement outcomes. Section 4 is the second part of the empirical analysis, where we use municipal electoral results to study the effect of mayor turnover on procurement. Section 5 discusses our results and concludes.

3.2 Institutional background

3.2.1 Municipalities, mayors and municipal elections in France

Municipalities (*communes*) are the smallest scale of government in France. There are over 36,000 municipalities in France, with over 50% of them having less than 500 inhabitants, making France a very fragmented country in terms of local government.

Municipalities are governed by a municipal council, which is elected through a list-based municipal election. Municipal elections are held every 6 years in France according to a nationwide schedule (the past municipal elections were held in 2014 and 2020, although the 2020 election was partly re-scheduled due to COVID-19). Voters vote for a list of candidates to the municipal council, and the final council is a combination of the lists with the most votes. In the majority of municipalities, the system is a two round list-based system. In the first round, all lists of candidates are admitted. The election is won directly if one of the lists gets an absolute majority of expressed votes. If not, all lists with more than 10% of votes can participate in the second round. Seats at the municipal council are awarded according to the second round results, but there is a very strong premium for the first place list: the winning list is guaranteed to have at least 50% of total seats and can thus appoint the mayor (who is generally the "head" of the winning list). The remainder of seats is awarded proportionally between all lists which have had at least 5% of votes in the second round (including the winning list). This system thus grants an absolute majority to the winning list, even in the event of a close race. There are two exceptions to the aforementioned system large cities where elections are held at the *arrondissement* level and small municipalities (less than 1000 inhabitants). We exclude those from our analysis of elections in the second part.

In case the mayor dies or resigns, the remaining members of the municipal council elect a new mayor among themselves, within three weeks following the death/resignation (the deputy mayor acts as mayor for the interim period). Public elections are not held in this case, hence the composition municipal council remains intact.

3.2.2 Regulations in French public procurement

French municipal procurement is subject to the general public procurement law, which is governed by a dedicated code, the *Code de la commande publique*. More generally, it is subject to European law, specifically the 2014 directive on public procurement[4]. The fundamental principles of European Union law - non-discrimination and free movement of people and goods - apply to public procurement. In addition, European case law has deduced a principle of transparency specific to public procurement law[5]. Article L3 of the *Code de la commande publique* states that in order to ensure the efficiency of public procurement and the proper use of public funds, contracting authorities must respect the principle of equal treatment of candidates for the award of a public procurement contract, the principle of open access, and the principle of transparency of procedures.

Within those general principles, several different awarding procedures are available to public buyers. be The main awarding procedure in French procurement is the open procedure. It concerns over 70 % of contracts awarded in our data. The general principle of the open procedure is the following. First, a call for tenders is issued by the contracting authority. Depending on the contract value, certain forms of advertising may be imposed. Any firm can submit a bid during the bidding period. After a certain advertising period, the potential bids are assessed by the contracting authority. If several offers are satisfactory, the best one is selected by the contracting authority in accordance with the award criteria it previously announced. The adapted procedure is a more flexible mode of procurement, introduced in 2004, and designed to allow for reduce procedural requirements for low value contracts. The negotiated procedure allows the public buyer to select a subset of bidders and to negotiate the terms of the contract individually for them. The restricted procedure allows the public buyer to invite a set of firms to bid. The open design contest and restricted design contest are specialized procedures used for highly technical and specific projects involving architecture or urban planning. Finally, in emergency cases, the contracting authority can award a public contract without competition. The share of awarding procedures in our data is displayed in table 3.10.

[4]Directive 2014/24/EU of the European Parliament and of the Council of 26 February 2014 on public procurement and repealing Directive 2004/18/EC Text with EEA relevance

[5]Judgment of the Court (Sixth Chamber) of 7 December 2000. Telaustria Verlags GmbH and Telefonadress GmbH v Telekom Austria AG, joined party: Herold Business Data AG.

Any failure to comply with the rules of transparency, accessibility, or equal treatment, either at the competitive bidding stage or in the award decision, may be appealed to the administrative courts by the aggrieved companies, which would annul the award procedure. Moreover, favoritism and corruption in the award of public contracts are criminal offenses, which may lead to fines and prison sentences.

3.3 Data

3.3.1 Procurement data and dependent variables

The procurement data used in this paper is the *BeauAMP*[6] [Deschamps and Potin, 2025] database. It contains the information published by contracting authorities in France from 2015 to 2023 in online award notices. It provides accurate information on both public contracts and the contracting parties, including their national identifiers (the SIREN code). Awarding notices contain one or several lots, which correspond to specific tasks to be performed or goods to be provided, and which may be attributed to different companies. The procurement dataset contains information on each lot awarded by French public entities since 2015. Variables in this dataset include the CPV code for a given lot, i.e. the type of good or service provided, the date of publication for the call for tenders (and for the awarding of the contract), the number of participants in the auction, the publicity threshold (if the contract value is large enough to make publication in the Official Journal of the E.U. mandatory), the awarding procedure used, the ZIP code for the winner, and their SIRET and SIREN codes, which are national identifiers for organizations.

An issue in the data concerns a potential selection bias for contracts awarded in 2015-2016. Indeed, although the publication of award notices was made compulsory by the 2014 EU directive, its implementation in French law was performed through a decree from March 25, 2016. As it appears in figure 3.5, there is a notable increase in the number of contracts over the period 2015-2017, potentially indicating that some municipalities were not publishing award notices during the years 2015 and 2016. To deal with this issue, we conduct robustness

[6] *Base Étendue, Améliorée et Unifiée des Annonces des Marchés Publics*

checks where we only keep cities which have been publishing award notices since 2015.

Structure of the data and aggregation at the award notice level. The initial data uses lots as individual observations. Lots themselves are nested within award notices, which group the different goods/services procured simultaneously by the municipality for a given purpose. Because there is strong correlation for most variables withing lots of a given award notice, and to avoid biasing estimation in favor of multi-lot award notices, we aggregate the data at the award notice scale. We use the mean for quantitative variables (such as price, number of bidders) and the modal value for categorical value (procedure, localization of winner, industry). Data is also clustered within cities, and as is expected, there is a clearly increasing trend relating city population and number of contracts awarded.

Dependent variables used in the analysis. The dependent variables used in this study are proxies for the existence of effective competition in procurement auctions and for potential favoritism. Concerning the intensity of competition, we use variables based on the number of bidders and on price. Because the number of bidders distribution is highly skewed to the right, we use the log of the number of bidders, as well as a series of dummy variables splitting the number of bidders into contracts below and above the threshold (we use thresholds of 1, 3 and 5 bidders). Concerning the price variable, we use the log of the awarded price. Price rebates cannot be computed as price estimates (e.g. engineer estimates made before the award in the case of construction contracts) are very rarely present in our data. A second set of dependent variables deals with the identity of winning bidders. First, we construct three variables based on the geographic origin of the winner, using the post code associated with the SIRET identifier of the firm. We create dummy variables based on three levels of geographical proximity: city (if the winner is in the city awarding the contract), department (if the winner is in the *département* where the buyer city is located), and neighbor (if the winner is located either in the *département* of the city, or a neighboring *département*[7]). We also use variables designed to capture preference for firms that the municipality has already dealt with in the past.

[7]Neighboring *département* are those that share a border with the *département* of the contract.

3.3.2 Data on mayor changes and mayor tenure

Mayor deaths. To study the effects of mayor deaths on procurement outcomes, we use a database containing dates of death for all deceased mayors in France between 2015 and 2020, *BREF*[8] [Labatut et al., 2020]. We restrict the procurement sample to contracts awarded between 2015 and 2019 (we remove 2020 from the sample in order to avoid Covid-19 related effects, and also because no mayors died in 2020). Over the period 2015-2020, 60 mayors died, 53 of which were mayors of municipalities present in our procurement data. The list of deceased mayors can be found in the appendix (table 3.12.) However, only a few cities have substantial numbers of contracts awarded pre and post treatment, which is necessary to appropriately estimate within city treatment effects. Table 3.11 displays the number of contracts/lots awarded before and after treatment for cities in the treated group. There are only 3 cities for which we have a over 10 award notices pre-treatment and post-treatment.[9]

Electoral turnovers and win/loss margins. In the second part of the empirical analysis, we identify exogenous changes in mayor using electoral data. Data on municipal elections is made available by the ministry of the Interior. We focus on cities for which the standard mode of election was used (municipalities over 1,000 inhabitants, excluding Paris, Lyon and Marseille). We use data from the 2014 and 2020 elections. Because our empirical strategy revolves around identifying close electoral races, only a subset of municipalities can be considered in the analysis. First, we restrict the analysis to election where the incumbent mayor was a candidate. Second, we eliminate elections where the incumbent mayor did not get enough votes to participate in the second round, or was not among the first two candidates with the most votes in the second round. Finally, we exclude elections where no second round was held (which occurs when one candidate obtains more than 50% of votes in the first round). We then compute vote margins using the ratio of second round vote shares between the incumbent mayor and the main challenger (i.e. the candidate other than the incumbent mayor with the most votes of the second round). For simplicity of presentation, we express the vote margin such that a positive vote margin indicates that the challenger won the election over the incumbent mayor (thus, a positive vote margin indicates an electoral turnover).

[8] *Base révisée des Elu.es de France*

[9] We remove the city of Dijon from the analysis because the context is very specific: François Rebsamen was mayor until 2014, when he was nominated as Minister of Labor, and was replaced with Alain Millot, who died in 2015. Upon Millot's death, Rebsamen regained his seat as mayor of Dijon. We also remove from the sample municipalities where the mayor resigned during the time period studied.

Mayor tenure. In the regression discontinuity analysis of election results, we interact mayor changes with tenure of the previous mayor in office, in order to obtain a proxy for the expected intensity of the effect of a mayor change. Mayor tenure data is not available in the BREF data set. Thus we collected it by scrapping the Wikipedia page of the commune, using the Wikipedia API, as most Wikipedia pages for French municipalities are reasonably well standardized and contain a "list of mayors" table. When the algorithm was unable to find the data, we hand collected the data on the Wikipedia page of the municipality and on the website of the municipality if needed.

3.3.3 Control variables and fixed effects

Contract level controls. An essential empirical challenge in this work is to appropriately control for heterogeneity between contracts. The dependent variables used in the analysis are indeed strongly correlated with characteristics of the good or service provided. We thus control for nature of the good/service using the CPV (common procurement vocabulary) code, which can be decomposed in several levels of precision (each digit in the CPV code describes the good/service with a supplementary degree of precision). We also control for the size of the contract using the publicity threshold (either French or European for larger contracts). We also control for the type of procedure to award the contract, and include a dummy indicating whether the awarded contract is a framework agreement or not. Finally, we control for the number of lots in the contract, as well as for whether multiple winners were among the winners of the contract. All specifications at the contract level use year $\times$ month fixed effects.

City level controls. In the case of the diff-in-diff analysis of mayor deaths, we control for city heterogeneity using city fixed effects. In the case of the baseline RD analysis, city fixed effects cannot be included as election results are time invariant. We thus use a set of contract level control variables. These include population, population density, unemployment rate and median income. These city characteristics are made publicly available by INSEE (the French national statistics agency).

Mayor characteristics. In the analysis of electoral turnover, we also control for a set of mayor characteristics, which are available in the *Répertoire national des élus*, a French database of

elected officials in France. The available controls are gender, birth year and socio-professional category. Socio-professional status is a codification created by the INSEE which contains 8 categories of employment: farmers (1), merchants and business owners (2), managing positions and intellectual professions (3), intermediate professions (4), office employees (5), manual workers (6), pensioners (7), other unemployed (8). We also control for political leaning of the mayor, which is available in electoral data through political "nuance" of the elected mayor. We simplify the variable into three categories: far right (National Rally and other mayors labeled "far right" by the INSEE data), right and center (includes all moderate right-wing and centrist parties) and left (Socialist Party, Communist Party, the Greens and other parties labeled "left" by the INSEE data). A residual category exists for mayors who are not openly aligned with national political parties.

Descriptive statistics on the variables used throughout the analysis are presented in tables 3.1 (contract level data), 3.2 (mayor and municipal term level variables) and 3.3 (city characteristics). In the appendix section, we also provide contract level summary statistics according to treatment status for the specific samples used in the diff-in-diff (table 3.13) analysis and in the RD analysis (table 3.32).

Table 3.1: Summary Statistics: Contract level characteristics

Statistic	N	Mean	Median	Min	Max
Number of bidders	44,020	3.76	3.00	0.00	215.00
Price	44,020	477,299.50	195,680.40	7,136.00	7,988,968.00
City winner	44,020	0.07	0.00	0.00	1.00
Dept winner	44,020	0.39	0.00	0.00	1.00
Neighbor winner	40,547	0.64	1.00	0.00	1.00
Known Winner	44,020	0.30	0.00	0.00	1.00
Number of lots	44,020	2.95	1	1	224
Multiple winners	44,020	0.42	0	0	1
Open	44,020	0.80	1	0	1
EU threhold	44,020	0.84	1	0	1
Year	44,020	2019.30	2019	2015	2023
Industry: TransportEquipment	44,020	0.05	0	0	1
Industry: Furniture	44,020	0.05	0	0	1
Industry: Materials	44,020	0.04	0	0	1
Industry: Construction	44,020	0.17	0	0	1
Industry: Maintenance	44,020	0.06	0	0	1
Industry: Finance	44,020	0.06	0	0	1
Industry: CivilEngineering	44,020	0.09	0	0	1
Industry: Agricultural	44,020	0.04	0	0	1
Industry: Services	44,020	0.05	0	0	1
Industry: Waste	44,020	0.06	0	0	1
Industry: Other	44,020	0.34	0	0	1

Table 3.2: Summary Statistics: municipal term and mayor characteristics

Statistic	N	Mean	Median	Min	Max
Challenger vote margin	453	−3.71	−4.30	−39.65	47.86
Turnover	453	0.38	0	0	1
Previous mayor tenure	442	10.44	6	1	43
Gender (male)	452	0.80	1	0	1
Birth year	453	1953.43	1953	1937	1985
Employment: Farmer	452	0.06	0	0	1
Employment: Business owner	452	0.11	0	0	1
Employment: Mgt./intellectual profession	452	0.36	0	0	1
Employment: Intermediate profession	452	0.10	0	0	1
Employment: Office employee	452	0.07	0	0	1
Employment: Manual worker	452	0.08	0	0	1
Empployment: Pensioner	452	0.19	0	0	1
Employment: Other unemployed	452	0.03	0	0	1
Left wing	453	0.33	0	0	1
Right wing/center	453	0.49	0	0	1
Far right	453	0.01	0	0	1

Table 3.3: Summary Statistics: city characteristics

Statistic	N	Mean	Median	Min	Max
Population (voters)	338	17,716.74	9,365.5	918	245,018
Population density	385	2,063.54	973.90	27.40	19,597.10
Median income	385	21,001.03	20,430	11,266	40,198
Unemployment rate	385	8.71	8.50	4.50	17.60
Number of contracts	385	31.95	12	1	598
Latitude	385	46.80	47.44	42.60	50.78
Longitude	385	2.73	2.54	−4.50	7.96

3.4 Mayor death and procurement outcomes: Evidence from a staggered diff-in-diff

In this section, we seek to identify whether changes in the identity of the mayor, subsequent to mayor death, affect procurement outcomes in a way such that favoritism appears less prominent. Because the municipal council altogether does not change its composition after mayor death, this empirical strategy allows us to separate mayor identity from political variables.

3.4.1 Preliminary evidence: effect of mayor death on the amount and structure of procurement

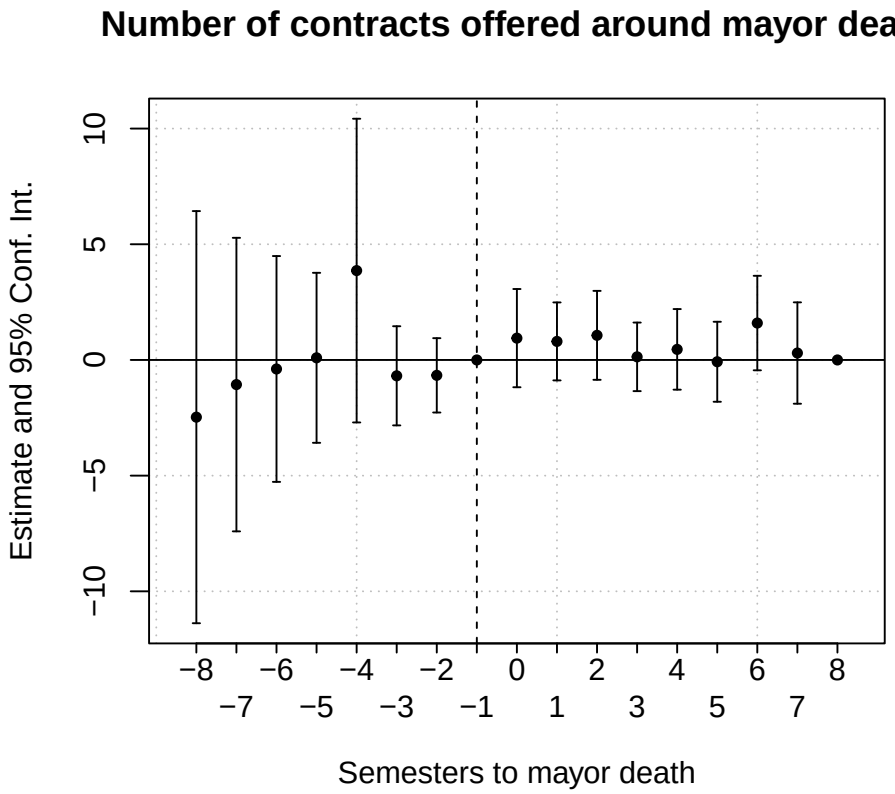
A preliminary question in this study is whether or not mayor death affects the amount of procurement carried out by the municipality, which case estimation of the effect of mayor death on competition could be biased by variations in procurement structure. Figure 3.1 plots the average number of calls for tenders issued by municipalities in the treatment group according to time to mayor death (expressed in semesters), demeaned from city and contract year averages. It shows that there is no significant effect of mayor death on the number of call for tenders issued by municipalities, using a 4 year time window. The large standard errors in early semesters are due to a small numbers of observations.

3.4.2 Empirical strategy

We use a difference-in-differences (DiD) strategy, exploiting the fact that mayor death serves as a random treatment and that municipalities where mayors did not die may be used as a control group. For each outcome, we first estimate a basic two-way (city and time) fixed effects model including contract level controls as well as industry (measured using the first 5 digits of the CPV) fixed effects:

$$Y_{i,c,s,t} = \alpha POST\_DEATHc,t + \beta X_i + \theta_t + \delta_c + \gamma_s + \varepsilon_{i,c,s,t}$$

Figure 3.1



Note: this figure plots the average number of contracts signed before and after mayor death in treated cities, controlling for semester and city fixed effects, and using -1 as a reference year. Source: Author.

Y indicates the procurement outcome considered in the model, $POST\_DEATH$ is a dummy indicating whether the contract was treated at time t. X is a vector of contract level-controls, while θ_t, δ_c and γ_s indicate respectively time, city and industry fixed effects.

Selection bias. As mentioned in the previous section, the fact that publishing award notices was made compulsory only in 2016 may introduce some selection. To handle this, we run our set of two-way fixed effects while keeping only the set of cities which have published award notices in 2015 in the data. It is indeed unlikely that cities which were publishing award notices before it was a legal requirement stopped doing so after.

Treatment heterogeneity. It is likely that the treatment is heterogeneous across different variables. Specifically, we focus on the industry and size of contract dimensions. Concerning industry heterogeneity, we believe that some industries are more prone to favoritism than others. The construction industry for instance is known to be prone to circumventions of awarding rules. Concerning contract value, it is likely that favoritism is more prominent in smaller contracts where judicial overview and public scrutiny in general are lower. To identify such effects, we first interact the treatment variable with a set of industry dummies. We take the first 2 digits of the CPV code (i.e. a gross definition of industries) and determine the 10 most represented industries in the dataset, while other industries are bunched in an "other" category. Concerning value heterogeneity, we divide the price variable into 4 quartiles to obtain a categorical variable with low, medium, high and very high value categories. We interact the treatment dummy with these categories, to identify non linearities in treatment effect with respect to contract size.

Staggered rollout of the treatment. An important issue with our design is the staggered rollout of the treatment. It is now well known that standard two-way fixed effects incorrectly estimates the average treatment effect (ATE) in staggered DID settings [Goodman-Bacon, 2018]. Several methods have been developed in recent years to correctly derive treatment effects in such settings [Sun and Abraham, 2021; de Chaisemartin and D'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Borusyak et al., 2024]. These methods generally revolve around estimating group-time specific effects, with groups being defined as treatment cohorts (i.e. units treated in the same period). Our data is however unsuited for the estimation of a full set of group-time effects, given that for many cities, there aren't contracts for every time period (assuming that we divide time in years). Our data requires us to stick with standard

"2x2" models rather that event studies, if looking at cohort or group specific effects. We use two methods to account for heterogeneous treatment effects with respect to treatment timing. First, we estimate a separate diff-in-diff for every treatment cohort (we artificially divide treatment effects by year of treatment). Next, we estimate city-specific DID model for each of the three cities that have a substantial amount of pre and post observations (Le Mans, Thionville and Champigny-sur-Marne). This is akin to "stacked" diff in diff methods [Cengiz et al., 2019; Baker et al., 2022] where event-specific, "2x2" data sets are created.

3.4.3 Results

Naive TWFE. The standard diff-in-diff estimates with the full sample are presented in tables 3.4 and 3.5. Results on the intensity of competition are ambiguous: following mayor death, contracts with over 1 bidder are significantly more likely, but contracts with over 3 bidders are significantly less likely. We do not find evidence of an effect of mayor death on other variables. When restricting the analysis to municipalities which have been publishing award notices since 2015 (tables 3.14 and 3.15), we observe similarly ambiguous effects of mayor deaths concerning the number of bidders. Additionally, we observe a positive an significant effect of prices and on the likelihood of local winners. Overall, these preliminary results do not support the prediction that competition increases following mayor death.

Treatment heterogeneity. We investigate heterogeneous treatment effects in a series of regressions where the post-treatment dummy is interacted with industry dummies and size of contract dummies. The results for the interaction of the treatment with industry dummies is presented in tables 3.16 and 3.17. Results concerning the interaction of the treatment with industry dummies are strongly heterogeneous, with some industries exhibiting increases in competition while other industries are associated with decreases in competition. Similarly, the interaction of the treatment with price quartiles is rather inconclusive: while mayor death is associated with a highly significant and positive effect on the likelihood that more that one bidder participates, other variables associated with the number of bidders are unaffected. The effect of mayor death on prices is heterogeneous according to contract size: it appears negative for smaller contracts but positive for large ones. Overall, these results do not particularly support our predictions.

Table 3.4: Mayor death and the intensity of competition: TWFE

Dependent Variables:	log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Post_Mayor_Death	0.005	0.075**	-0.082**	-0.022	0.261
	(0.061)	(0.032)	(0.038)	(0.036)	(0.262)
Fixed-effects					
year-month	Yes	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	25,376	25,377	25,377	25,377	19,000
R^2	0.390	0.295	0.322	0.313	0.457
Within R^2	0.014	0.003	0.014	0.019	0.106

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. >1/3/5 Bidders indicates the share of lots in the contract for which over 1/3/5 bidders responded. Log(Price) is the mean of the log of the award price. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.5: Mayor death and the nature of competition: TWFE

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Post_Mayor_Death	0.005	0.041	-0.022	-0.032
	(0.041)	(0.076)	(0.112)	(0.029)
Fixed-effects				
year-month	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes
Fit statistics				
Observations	25,377	25,377	23,490	25,377
R^2	0.275	0.441	0.489	0.358
Within R^2	0.001	0.003	0.003	0.118

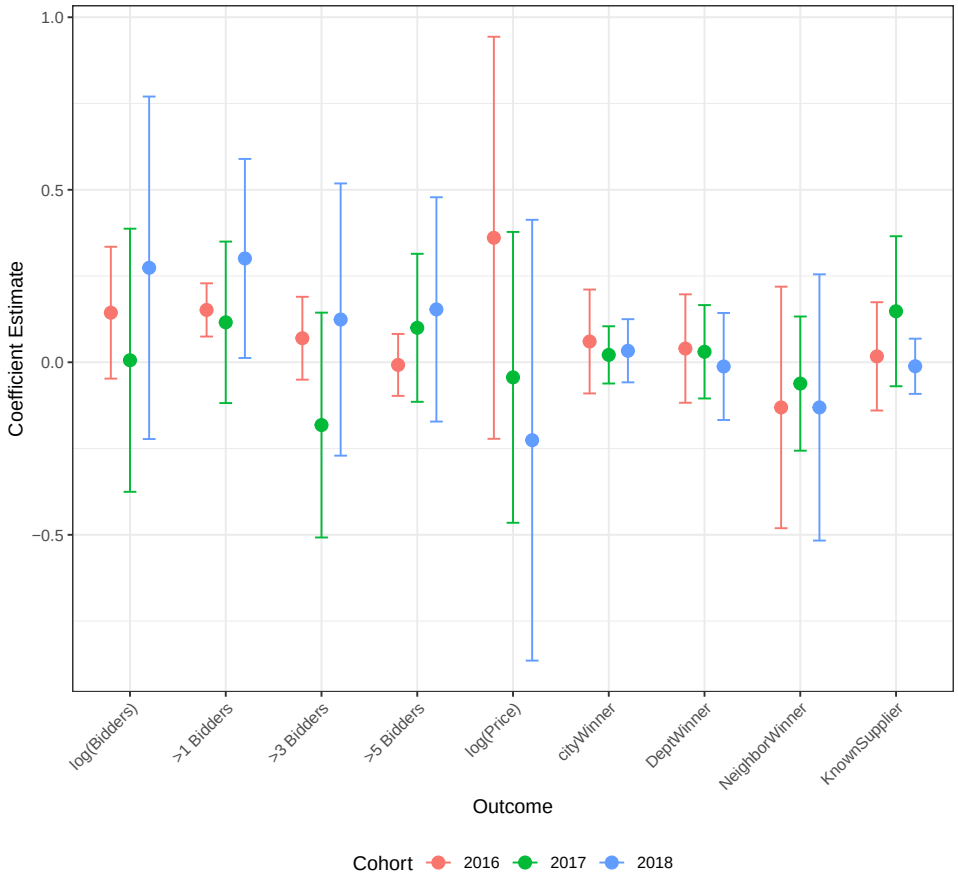
Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Cohort specific effects. In order to appropriately deal with the staggered design of the treatment, and potential heterogeneity of treatment through time, we estimate cohort specific effects. We define cohorts as years of treatment, and perform regressions on cohort specific data sets. For a treatment year t, we remove from the data set all contracts in cities which were treated in other years that t, then we remove year t contracts, and we compare competition outcomes in years before t with competition outcomes in years prior to t. Because we need at least $t-1$ and $t+1$ observations, we do not estimate the effect of being treated in 2015 or 2019 (the first and last years of our sample). Thus we estimate cohort specific effects for units treated in 2016, 2017 and 2018 respectively. Figure 3.2 summarizes the cohort specific effects of mayor death on procurement outcomes (regression tables are in the appendix). Concerning cities treated in 2016, we observe a positive effect of mayor death on the likelihood that over one bidder participates in calls for tenders, but no other significant effect. Concerning cities treated in 2017, we find no significant effect of mayor death at all. Finally, in cities treated in 2018, we also observe a significant increase in the likelihood that more than 1 bidder participates, and no other effect. The only significant results thus go in the expected direction, as mayor death appears to be associated with a reduction in single-bidder auctions in different instances.

City specific effects. A more precise approach to dealing with the staggered design is to compute city specific effects. Thus, we build city-specific datasets and estimate city specific effects for the 3 cities which have a substantial number (greater than 15) of pre and post treatment contracts (Le Mans, Thionville and Champigny-sur-Marne). Results are plotted in figure 3.3, and regression tables are in the appendix section. For the city of Thionville, results concerning the nature of competition generally follow our predictions, with a significant increase in the likelihood local bidders winning contracts. However, the effect of the number of bidders is ambiguous. Concerning Le Mans, the outcomes following mayor death appear to indicate more of a decrease in competition: the number of bidders is negatively affected, while prices and local preference are significantly and positively affected. Finally, coefficients concerning Champigny-sur-Marne are significant but highly volatile. The effects of mayor death are very heterogeneous from city to city, and do not allow to identify a unidirectional effect of mayor death. Overall, it is clear that our results suffer from the small size of the treated sample.

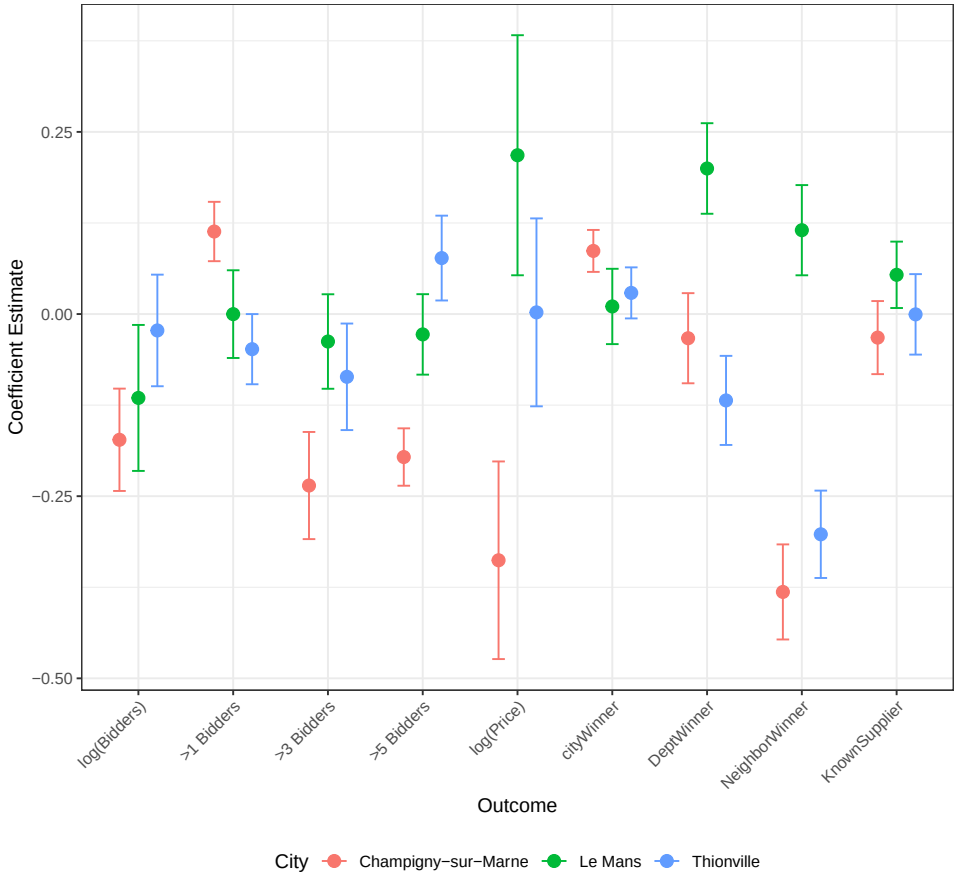
In conclusion, the effects of mayor death on procurement outcomes do not clearly support the prediction of increased competition. The main drawback of our study is the small number of treated municipalities, and the overall small size of those municipalities, leading to overall volatility in our estimates.

Figure 3.2: Cohort specific effects of mayor death



Note: This figure presents the coefficients and 95% level confidence bands from a set of TWFE models estimated with least squares. On the x-axis is the set of dependent variables. A cohort is the set of cities in which the mayor died for a given year. All models contain year × month, city and industry fixed effects, as well as contract level control variables. Source: author.

Figure 3.3: City specific effects of mayor death



Note: This figure presents the coefficients and 95% level confidence bands from a set of TWFE models estimated with least squares. On the x-axis is the set of dependent variables. Colors indicate each of the three cities considered for city-level specific effects. All models contain year × month, city and industry fixed effects, as well as contract level control variables. Source: author.

3.5 Electoral turnover and procurement outcomes: evidence from a regression discontinuity design

In this section, we investigate whether changes in mayor identity associated with elections affect procurement outcomes. In order to obtain causal estimates, we rely on a regression discontinuity design, focusing on close electoral races. A first series of tests considers parametric estimation of the discontinuity using the full sample of contracts, and controlling for a polynomial function of vote margin in order to isolate the effect of the discontinuity at the 0 threshold of the win margin. A second set of tests considers nonparametric estimation, wherein a local linear regression is estimated around an algorithmically determined bandwidth. In both cases, results indicate that mayor changes significantly increase competition and reduce reliance on local or previously interacted with firms.

3.5.1 Empirical strategy

Parametric estimation

Regression discontinuity designs allow to capture the discontinuous effects of a continuous running variable. In our setting, the vote margin of for the contender candidate in the second round has a strongly discontinuous effect at the zero threshold: below it, the incumbent mayor remains in place and keeps the absolute majority at the council; above it, the challenger obtains an absolute majority at the mayor seat. Parametric RD designs aim at capturing the effect of this threshold, while controlling for the continuous effect of the running variable through a smooth function of the running variable (generally a polynomial). In order to estimate the effect of electoral turnovers on public procurement outcomes, we thus consider the following parametric RD equation:

$$Y_{i,c,t} = \beta_1 Turnover_{c,t} + g(MV_{c,t}) + \delta_1 X_{i,c,t} + \delta_2 X_{c,t} + \gamma_t + \varepsilon_{i,c,t} \tag{3.1}$$

where $g(\cdot)$ is a smooth function of the margin of victory MV, which we approximate with a symmetric second order polynomial function, following the recommendations from Gelman

and Imbens [2019] who advise against the use of higher-order polynomials [10]. β_1 captures the effect of the discontinuity at the 0 threshold. Other parameters of the equation include contract level controls $X_{i,c,t}$ (including industry dummies using the 4 first digits of the CPV code, the number of lots in the contract, a dummy indicating if there were multiple winners for the contract, the publicity threshold and the type of awarding procedure used), city and mayor level controls $X_{c,t}$ (population, population density, unemployment rate, median income, number of procurement contracts awarded over the term, and mayor characteristics such as gender, professional status, age and political affiliation) and year fixed effects. Standard errors are clustered at the mayor level.

We also consider that the effect of a municipal turnover can vary in intensity. Specifically, when a long standing mayor is overturned, the effect on procurement outcomes is expected to be stronger. Thus we interact the $Turnover$ dummy variable with a variable measuring the tenure (in years) of the previous mayor at the moment of the election:

$$\begin{aligned} Y_{i,c,t} =& \beta_1 Turnover + \beta_2(Turnover_{c,t} \times IncumbentTenure_{c,t}) + g(MV_{c,t}) \\ &+ \delta_1 X_{i,c,t} + \delta_2 X_{c,t} + \gamma_t + \varepsilon_{i,c,t} \end{aligned} \tag{3.2}$$

Coefficient β_2 captures the variations in intensity of the discontinuity at the 0 threshold, putting more weight on cities where the overturned mayor was in office for a large number of years.

Nonparametric estimation

Parametric estimation at the contract level presents some issues. First, the estimation puts more weights on cities which have a large number of contracts. Second, although we include many variables to control for contract characteristics, it cannot be guaranteed that all unobserved heterogeneity is accounted for. To strengthen the validity of our estimates, we thus turn to nonparametric estimation of discontinuity, with data aggregated at the city-term level. Most recent studies using election-based RD designs [Akhtari et al., 2022; Marx et al., 2024;

[10] We also provide results for estimation using third and fourth order polynomials in the Appendix.

Bazzi et al., 2025] rely on local regression around an algorithmically determined bandwidth. We use these state-of-the art methods, i.e. the Calonico et al. [2019] method for optimal bandwidth calculation, and the Calonico et al. [2014] robust estimators. For the nonparametric estimation, we aggregate at the term level, given that disaggregated data leads to mass points which reduce the reliability of local regression estimates. This also allows to put equal weights on each city. We also remove from the data cities with less than 20 contracts signed, as the mean may be unreliable. We rely on a local linear function of the vote margin, as advised in Imbens and Kalyanaraman [2012], and as a robustness check estimate a quadratic polynomial of the vote margin (as suggested in Calonico et al. [2014]). The primary model estimated is thus the following, with subscript e denoting election year, and γ_e an election fixed effect:

$$Y_{c,e} = \beta_1 Turnover_{c,e} + \beta_2 (Turnover_{c,e} \times MV_{c,e}) + \beta_3 MV_{c,e} + \gamma_e + \varepsilon_{c,e} \tag{3.3}$$

In our aggregation strategy, we take the city-term mean of each dependent variable. For variables which are not upper-bounded (such as price and number of bidders), we use trimmed means where the top 1% of the distribution are not used in computation, in order to avoid outliers biasing the aggregate variables. we remove cities with less than 20 contracts awarded from the estimation in order to avoid aggregate variables from being highly random. Aggregation of the data at the city-term level eliminates bias associated with unequal weights of cities, but comes with several costs. First, it drastically reduces the number of observations, and thus the potential statistical power of our estimates. Second, aggregation does not completely solve the problem of heterogeneity as cities may present discrepancies in the structure of their procurement contracts. Checks performed to assess the validity of our estimates are described in the following paragraph.

Validity of RD estimates

Testing for sorting. RD designs using election results as running variables are subject to critiques regarding the possibility to endogenously sort above the threshold [Caughey and Sekhon, 2017; Grimmer et al., 2011]. We provide evidence that sorting is unlikely by plotting

results for the Cattaneo et al. [2018] density tests for the running variable in order to check for bunching above the 0 threshold. Data is aggregated at the municipality $\times$ election level to ensure that each election represent only one observation. In both cases, the null hypothesis of no sorting above the threshold is widely accepted (p = 0.81) (see figure 3.9).

Balance checks. Due to the absence of covariates in the estimation (apart for the election year dummy variable), nonparametric estimates are sensitive to heterogeneity of units around the cutoff. To assess their validity, we implement balance checks by testing whether observable city characteristics display a significant discontinuity at the cutoff. To do so, we simply estimate the baseline nonparametric model using city characteristics $X_{c,e}$ at election time e as dependent variables:

$$X_{c,e} = \beta_1 Turnover_{c,e} + \beta_2 (Turnover_{c,e} \times MV_{c,e}) + \beta_3 MV_c + \varepsilon_{c,e} \tag{3.4}$$

Results for these balance checks are presented in the appendix. Table 3.33 presents the results for general city characteristics. They exhibit no significant discrepancies between treated and untreated cities, apart for the "median income" variable which indicates that cities undergoing an electoral turnover have poorer inhabitants on average. In table 3.34, we use shares of industries in city procurement as dependent variable, in order to assess whether results may potentially be driven by heterogeneity in the type of procurement between treated and untreated municipalities. We observe no significant difference between both groups around the cutoff, indicating that the structure of procurement does not significantly vary according to whether or not an electoral turnover took place. Overall, these results indicate that there is generally no significant imbalance in the type of city located around the cutoff value of the vote margin, supporting the validity of our nonparametric RD estimates.

Robustness checks. We run a set of robustness checks to assess the sensitivity of our estimates. To deal with potential selection bias, we present results for the sample restricted to cities which have been publishing award notices since 2015, similarly to the tests presented in the previous section. Concerning the parametric model, we provide results for estimation using third order and fourth order polynomials of the vote margin. We also test for heterogeneity by testing for industry-specific effects, and for size of contract specific effects. Finally, we remove contracts awarded during the COVID lockdown period to control for variations in procurement

outcomes associated with this specific time period. Concerning the nonparametric estimation, we perform several standard robustness checks such as varying the bandwidth (using 3/4 or 3/2 of the algorithmically determined bandwidth) and using a local quadratic (rather than linear) regression. In addition, we present tests gradually increasing the constraint on the minimum number of contracts per city necessary to be included in the sample (the default is 20, but we increase it to 30, then to 40).

3.5.2 Results

Results from parametric estimation

Baseline results. Results for the baseline parametric model are presented in tables 3.6 and 3.7. Concerning the intensity of competition, There is significant evidence of increased competition following a turnover, both using the continuous number of bidders variable, and when using splits of the number of bidders at the 3 and 5 thresholds. These results are consistent with the intuition that bidders anticipate higher chances of winning contracts when a new mayor is in place. On the other hand, however, we observe an increase in price following turnovers. It is complex to provide a definitive interpretation of this result. It may indicate a qualitative change in the type of goods and services procured, despite our controlling for the CPV code. It may also reflect increased quality in exchange for higher prices. Looking at "nature of competition" outcomes (table 3.7), electoral turnovers have a significant and negative effect on the likelihood that the winner is located in the same *département* as the municipality. This result supports the idea that newly elected mayors may be less partial to local firms, having not accumulated connections with them. Other coefficients are nonsignificant.

Interaction of turnover with previous mayor tenure. In a second series of tests, we interact the turnover dummy variable with the log of the tenure (in years) of the previous mayor. This variable is expected to reinforce the main effect of turnovers. Longstanding mayors are expected to develop a more intensive network of connections with firms, such that their removal should lead to stronger increases in competition for procurement contracts. Results are presented in tables 3.35 and 3.36. Concerning the intensity of competition, there is

Table 3.6: Electoral turnover and the intensity of competition (Parametric estimation, 2nd order polynomial)

Dependent Variables:	Log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	Log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Turnover	0.066**	0.011	0.048**	0.031*	0.255**
	(0.033)	(0.017)	(0.024)	(0.018)	(0.118)
Fixed-effects					
Industry	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	13,478	13,478	13,478	13,478	14,154
R^2	0.309	0.205	0.240	0.245	0.369
Within R^2	0.015	0.023	0.015	0.018	0.156

*Note: This table presents the results from parametric RD estimation, using a quadratic polynomial function of the vote margin. >1/3/5 Bidders indicates the share of lots in the contract for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 3.7: Electoral turnover and the nature of competition (Parametric estimation, 2nd order polynomial)

Dependent Variables:	CityWin	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Turnover	0.014	-0.069**	0.006	0.022
	(0.018)	(0.027)	(0.022)	(0.022)
Fixed-effects				
Industry	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	13,478	14,154	12,636	13,478
R^2	0.150	0.269	0.334	0.197
Within R^2	0.037	0.036	0.019	0.060

*Note: This table presents the results from parametric RD estimation, using a quadratic polynomial function of the vote margin. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

weak evidence that the effect of turnover on the number of bidders is driven by longstanding mayors: the coefficient for the interaction between turnover and previous mayor tenure is positive and significant at the 10% level, when considering the dummy variable splitting the number of bidders at the threshold of 3 bidders. Concerning the nature of competition outcomes, we find results in line with our predictions: the interaction of turnover with tenure of the previous mayor reduces has a significant and negative effect on the likelihood that the winner is located in the municipality, as well as with the likelihood that the winner has previously interacted with the municipality. [11] Again, these results suggest that municipal turnover disrupt pre existing connections with firms, and increase competition in procurement auctions.

Sample restricted to municipalities present since 2015. When restricting the sample to the municipalities which were already present in the data in 2015, the effect of municipal turnover on the number of bidders remain significant, and actually gains significance compared with the baseline sample. The effect on price remains positive, but only at the 10% level. Finally, the negative effect of municipal turnover is stronger and more significant than in the initial sample.

Heterogeneity analysis. To determine whether these effects are driven by specific industries, and whether electoral turnover has heterogeneous effects, we interact mayor turnover with industry dummies. Results (tables 3.39 and 3.40) do not indicate significant heterogeneity across industries. Specifically, no industry shows significantly anti-competitive effects of municipality turnover. A noteworthy observation is that the effects of turnover on the likelihood that the winner is located in the same *département* as the municipality is particularly robust across industries. In a second series of test, we interact the turnover treatment with price quartiles, in order to determine whether the effect is heterogeneous according to size of the good or service contracted out. Results are presented in tables 3.41 and 3.7. Interestingly, for smaller contracts, electoral turnovers significantly decrease average prices, while they have the opposite effects for large contracts. Additionally, the positive effect of municipal turnover on the number of bidders appears driven by large contracts. This result is consistent with the idea that mayor identity and corporate connections are relevant for the awarding of large contracts,

[11] Note: the coefficients the non-interacted "turnover" variable do not lend themselves to interpretation. Indeed, they correspond to the interaction of the turnover variable with the fact that tenure of the previous mayor is 0 years. By definition, previous mayor tenure cannot be 0 years.

whereas smaller contracts are not necessarily subjected to intervention of the mayor and to favoritism. Regarding the "nature of competition" outcomes, it appears that the effect of municipal turnover on the likelihood of a *département* winner is significant and of comparable magnitude across all contract value categories. The magnitude of this effect is of around 7 pp. for small, medium and large contracts.

Additional robustness checks. As robustness checks for the baseline parametric specifications, we also use third order (tables 3.43 and 3.44) and fourth order polynomial (tables 3.45 and 3.46) functions of the vote margin. Results concerning the number of bidders and the *département* winner variables are unchanged throughout these specification changes, comforting the robustness of our results. On the other hand, the positive effect of turnover on price, which was surprising in the baseline model, is no longer significant. In a final robustness check, we eliminate contracts awarded between March 2020 and June 2021, i.e. the period when COVID restrictions were in place (tables 3.47 and 3.48). The effect of turnover on number of bidders is no longer significant. However, the result on local winners remains significant at the 5% level.

Results from nonparametric estimation

Visual evidence. Before discussing regression results, we provide visual evidence of the discontinuities around the cutoff. We plot observations around an arbitrary bandwidth of 10pp. of total votes, and plot a local linear regression line (figures 3.10 and 3.11). Overall the visual evidence is rarely compelling. Notable exceptions include the plot for the "$>$ 5 bidders" variable where the discontinuity at the 0 threshold appears quite clearly positive, and the plot for the "DeptWinner" variable, where we also note a visible decrease in the share of local winners in cities undergoing electoral turnover.

Nonparametric estimation: baseline model. Results from the baseline nonparametric model are presented in tables 3.8 and 3.9. They confirm the findings from the parametric estimation: electoral turnover is associated with an increase in the number of bidders (through the ">3 Bidders" and ">5 Bidders" variables), and with a significant decrease in the probability that the winner is firm located in the same *département* as the municipality.

Table 3.8: Electoral turnover and the intensity of competition: Nonparametric RD estimates

	Log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	Log(Price)
Turnover	0.061	-0.064	0.1*	0.127***	0.198
	(0.061)	(0.046)	(0.056)	(0.055)	(0.294)
Obs.	165	165	165	165	165
Robust p-value	0.254	0.15	0.063	0.009	0.525
Bandwidth	12.86	9.46	9.51	10.51	9.52
N. effective obs.	105	78	78	85	78
Election year FE	Yes	Yes	Yes	Yes	Yes

Note: this table presents nonparametric RD estimates from local linear regression, using Calonico et al. [2014] to obtain p-values. Optimal bandwidth (in vote shares) is determined using Calonico et al. [2019]. The number of effective observations is the number observations within the bandwidth (which enter in the estimation of local linear regression). Data is aggregated at the municipal term level. Log(Bidders) is the average log number of bidders. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Signif. codes: ***: 0.01, **: 0.05, *: 0.1.

Robustness checks. A first series of robustness checks varies the length of the bandwidth for estimating the local linear regression. In tables 3.49 and 3.50, we use 3/4 of the initial bandwidth. The positive effect of municipal turnover on number of bidders remains significant, while it loses significance for the variables pertaining to geographical origin of the winner, arguably due to the very small number of effective observations remaining. In tables 3.51 and 3.52 we increase the bandwidth to 3/2 of its initial value. In this case, results concerning number of bidders and origin of the winner remain significant. A second set of robustness checks uses local quadratic regressions instead of local linear regression (tables 3.53 and 3.54). Concerning the number of bidders, results show a highly significant positive effect of turnover on the likelihood that over 5 bidders participated in auctions. Concerning the nature of competition, the "DeptWinner" coefficient loses significance, although it is close to the 10% significance threshold ($p = 0.105$). Finally, the last series of robustness checks restricts the threshold in number of contracts for inclusion in the sample. In the baseline models, we only admit cities with over 20 contracts. Here, we set the threshold to 30 contracts (tables 3.55 and 3.55, then to 40 (tables 3.57 and 3.58). Results concerning the number of bidders tend to lose significance in these subsamples. On the other hand results concerning the nature of competition remain highly significant. The coefficient for "DeptWinners" is negative and significant respectively at the 5% and 10% level. Interestingly, other coefficients for local origin of the winner also gain significance.

Table 3.9: Electoral turnover and the nature of competition: Nonparametric RD estimates

	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Turnover	-0.054	-0.169**	-0.042	0.013
	(0.041)	(0.078)	(0.083)	(0.057)
Obs.	165	165	164	165
Robust p-value	0.227	0.024	0.339	0.769
Bandwidth	11.87	8.19	6.16	10.97
N. effective obs.	101	67	49	91
Election year FE	Yes	Yes	Yes	Yes

Note: this table presents nonparametric RD estimates from local linear regression, using Calonico et al. [2014] to obtain p-values. Optimal bandwidth (in vote shares) is determined using Calonico et al. [2019]. The number of effective observations is the number observations within the bandwidth (which enter in the estimation of local linear regression). Data is aggregated at the municipal term level. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Signif. codes: ***: 0.01, **: 0.05, *: 0.1.

3.6 Conclusion

This study seeks to determine how exogenous mayor changes affect procurement outcomes. Essentially, the prediction we test is that incumbent mayors have had the opportunity to develop connections with firms, especially local ones. In turn, competing firms may be discouraged to participate in calls for tenders, knowing that a firm or set of firms is preferred. Our empirical strategy relies around two sources of change in mayor identity: death and election results. Death presents the advantage of being orthogonal to variables affecting procurement, but is not necessarily entirely unpredictable and remains a rare event, penalizing identification. Election results are highly endogenous, but can be treated as exogenous in the case of close electoral races, which our regression discontinuity design focuses on.

The results concerning mayor death appear generally inconclusive. Potential follow-ups to this quasi-experiment should seek to include exogenous resignations, and perhaps focus on unpredictable deaths. Overall, small sample size penalizes identification regardless of design choices. An avenue to increase sample size would be to use data from other types of procurement, as we have access to contracts awarded by other units of local and national government.

On the other hand, results from our regression discontinuity analysis provide robust evidence that electoral turnovers do lead to increased competition in public procurement calls for tenders. We use two types of estimation, parametric and nonparametric. Results from the

parametric and nonparametric models complement each other. Nonparametric estimation allows to focus on close electoral races by focusing on a subset of observations, and puts equal weights on cities thanks to the aggregation strategy. Parametric models allow to control for a rich set of contract, city and mayor characteristics, and drastically increase sample size. In both cases, a particularly strong result is that local firms are less likely to win procurement contracts in cities where an electoral turnover took place. This effect is constant across types of goods/services and across categories of contract value. Whether municipalities are less likely to favor local firms, or whether local firms are more likely to participate in auctions under newly elected mayors remains uncertain. Additionally, there is evidence in a large array of tests that the average number of bidders increases as a result of turnovers.

The theoretical drivers for our results remain to be discussed. While favoritism is an appealing explanation, other channels may drive our results. First, it is possible that our results stem only from expectations of favoritism on the supply side, which is supported by the fact that more bidders place offers under newly elected mayors. Second, it is also possible that newly elected mayors purchase different types of goods and services, and that this heterogeneity drives the increase in competition and decrease in local preference, despite the fact that we account for industry heterogeneity using CPV codes.

Although the results from this study point in the direction that municipal turnover is overall beneficial to public procurement, increased competition and reduction in local preference are not necessarily synonymous with improved performance. Further works should thus seek to relate the effects of mayor turnover on competition with *ex post* performance outcomes.

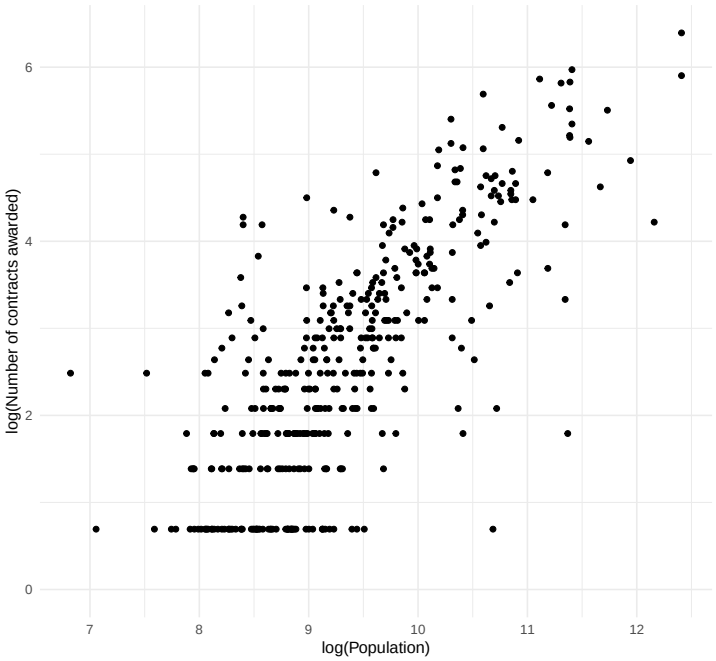
Appendix C

C1: Additional descriptive statistics

Table 3.10: Distribution of awarding procedures

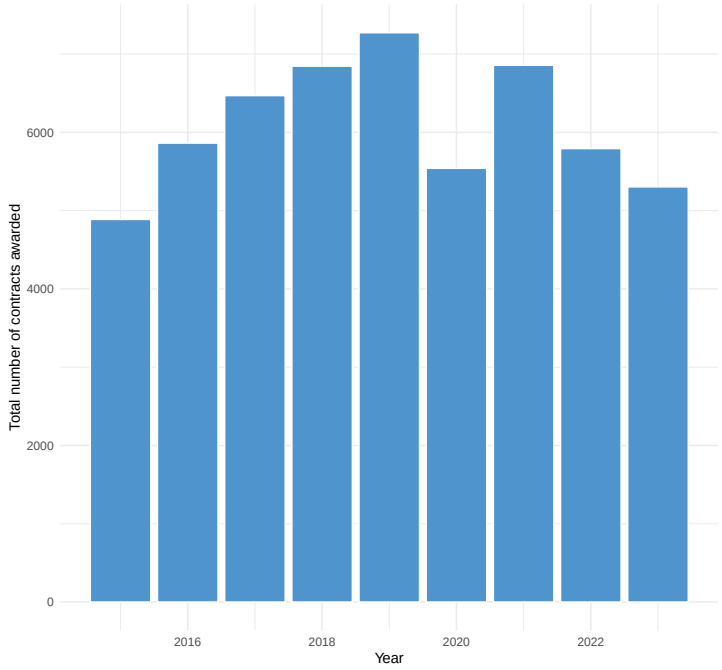
Procedure type	Number of contracts	Share (%)
Open	39903	72.4
Adapted	11057	20.1
Negotiated	1931	3.5
Restricted	1863	3.4
Competitive dialogue	206	0.4
No competition	133	0.2
Restricted design contest	10	0
Open design contest	1	0

Figure 3.4: Population and number of contracts awarded



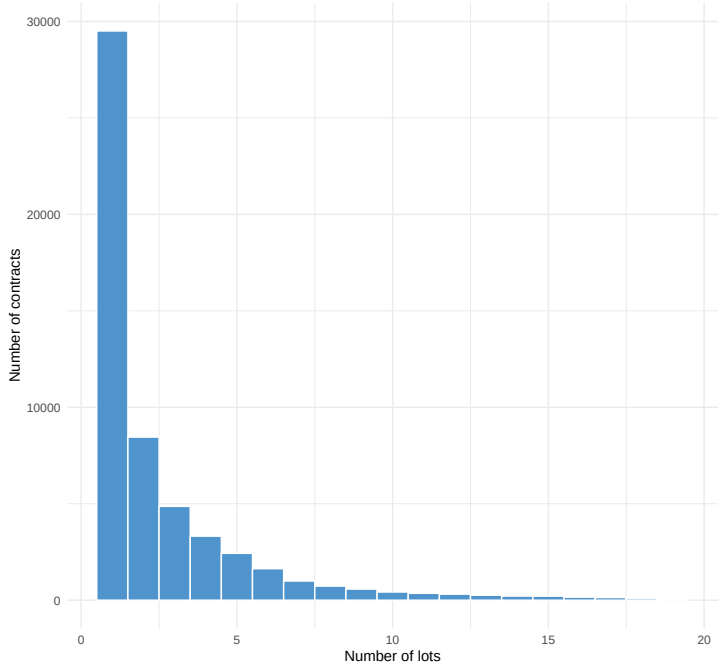
Note: this figure plots the number of contracts (log transformed) as according to city population (also log transformed). Source: Author.

Figure 3.5: Number of contracts awarded per year



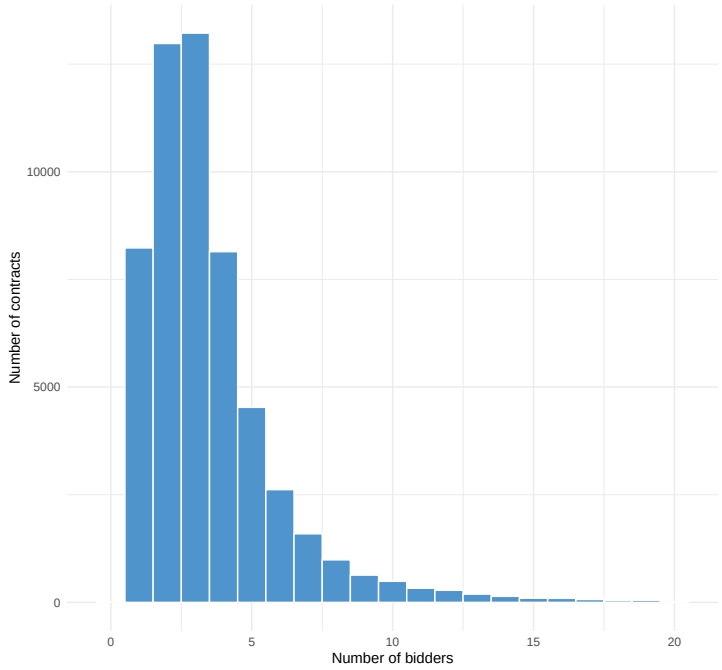
Note: this figure plots the total number of contract award notices published each year in our data. Source: Author.

Figure 3.6: Distribution of lots per contracts



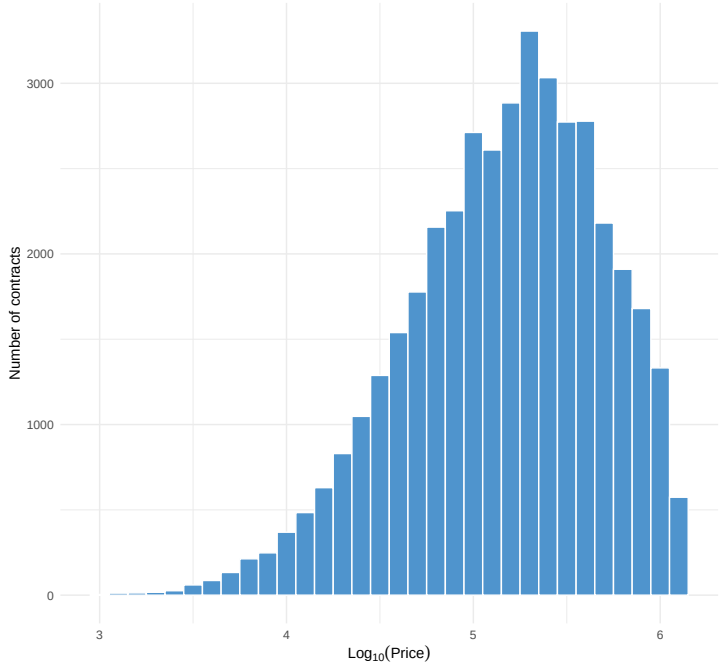
Note: this figure plots the distribution of contracts according to the number of lots in the award notice. Source: Author.

Figure 3.7: Distribution of bidders per contract



Note: this figure plots the distribution of contracts according to the average number of bidders per lot in the award notice. Source: Author.

Figure 3.8: Distribution of contract prices



Note: this figure plots the distribution of contracts according to the average price per lot in the award notice. Prices are expressed as a base 10 logarithm. Source: Author.

C2: Complementary data and tests for diff-in-diff

Table 3.11: Description of the treated sample

	City	Lots (Pre)	Lots (Post)	Notices (Pre)	Notices (Post)
1	LE MANS	458	236	100	54
2	CHAMPIGNY-SUR-MARNE	53	29	20	18
3	THIONVILLE	28	36	17	18
4	DIJON	24	101	4	31
5	CLUSES	16	7	6	3
6	ERNEE	16	0	1	0
7	MEYREUIL	14	1	8	1
8	SAINT-JEAN-DE-LUZ	11	9	8	2
9	LODEVE	8	19	2	5
10	AUCHEL	6	2	2	2
11	GRAND-COURONNE	6	94	4	37
12	LES AVANCHERS-VALMOREL	6	0	1	0
13	PAVILLY	5	0	1	0
14	BEGARD	2	0	1	0
15	CHAMPIGNEULLES	2	1	2	1
16	COUERON	2	46	1	19
17	IVRY-SUR-SEINE	2	209	2	77
18	LINSELLES	2	17	2	3
19	SAINTES-MARIES-DE-LA-MER	2	0	2	0
20	BEUVRAGES	1	0	1	0
21	GENAY	1	2	1	2
22	PONT-DE-VEYLE	1	6	1	4
23	SAINT-VINCENT-DE-TYROSSE	1	10	1	2
24	TREVIERES	1	0	1	0
25	ARENTHON	0	1	0	1
26	BELLEY	0	10	0	5
27	BERNEX	0	3	0	3
28	BRINDAS	0	5	0	2
29	DOURGES	0	2	0	2
30	ENTRAINS-SUR-NOHAIN	0	1	0	1
31	L'ISLE-D'ABEAU	0	25	0	10
32	LA ROCHE-SUR-FORON	0	2	0	1
33	LE BOULOU	0	11	0	5
34	LE CASTELLET	0	7	0	2
35	LE TREPORT	0	4	0	1
36	LE VAL D'HAZEY	0	1	0	1
37	MAURON	0	1	0	1
38	MONBALEN	0	16	0	2
39	MORNE A L'EAU	0	18	0	8
40	MOUROUX	0	1	0	1
41	SAINT-GERMAIN-EN-LAYE	0	3	0	3
42	VERDUN-SUR-GARONNE	0	23	0	6

Table 3.12: Sample of deceased mayors

	Municipality	Surname	Name	Gender	DateOfBirth	DateOfDeath
1	Brindas	BEFFY	CHRISTIAN	M	1948-03-19	2014-09-19
2	Le Boulou	OLIVE	CHRISTIAN	M	1948-03-20	2014-10-04
3	Verdun-sur-Garonne	BOTKOVITZ	PHILIPPE	M	1966-10-14	2014-10-24
4	Dourges	DEFRANCQ	PATRICK	M	1957-03-09	2014-11-07
5	Entrains-sur-Nohain	PAQUETTE	MICHEL	M	1961-01-24	2014-12-30
6	Belley	JIMENEZ	CHRISTIAN	M	1950-02-28	2015-01-20
7	Ivry-sur-Seine	GOSNAT	PIERRE	M	1948-08-20	2015-01-25
8	Couëron	FOUGERAT	JEAN PIERRE	M	1953-05-11	2015-02-20
9	L'Isle-d'Abeau	GRISOLLET	JOEL	M	1951-05-09	2015-06-12
10	Grand-Couronne	LAMAZOUADE	MICHEL	M	1954-02-21	2015-06-20
11	Le Blanc	PASQUER	ALAIN	M	1952-09-01	2015-06-27
12	Dijon	MILLOT	ALAIN	M	1952-04-15	2015-07-27
13	Esquelbecq	DEVYNCK	JEAN MICHEL	M	1955-05-11	2015-08-10
14	Beauvoir-sur-Mer	THIBAUD	CHRISTIAN	M	1953-06-17	2015-08-11
15	Le Val d'Hazey	RECHER	JEAN LUC	M	1953-03-09	2015-09-11
16	Trévières	RICHARD	JEAN PIERRE	M	1943-12-27	2015-12-06
17	Le Castellet	TAMBON	GABRIEL	M	1930-03-23	2015-12-26
18	Les Avanchers-Valmorel	VORGER	ROBERT	M	1955-05-15	2016-01-10
19	Le Tréport	LONGUENT	ALAIN	M	1948-05-17	2016-01-17
20	Morne-à-l'Eau	LOMBION	JEAN CLAUDE	M	1951-06-12	2016-04-05
21	Thionville	GROMMERCH BRANDENBOURGER	ANNE	F	1970-12-11	2016-04-15
22	Pont-de-Veyle	MOUTOT	JEAN PAUL	M	1942-05-29	2016-05-02
23	Leers	VANBELLE	JEAN CLAUDE	M	1946-06-25	2016-06-05
24	Bernex	TRINCAT	JOSEPH	M	1943-05-27	2016-06-11
25	Monbalen	CALLIGARIS	DENIS	M	1948-04-01	2016-07-07
26	Rebais	LANTENOIS	CHRISTIAN	M	1942-02-08	2016-08-07
27	Linselles	REMORY	JACQUES	M	1943-11-20	2016-09-15
28	La Roche-sur-Foron	FLAMMIER	GUY	M	1952-03-13	2016-10-16
29	Pavilly	LEMESLE	CLAUDE	M	1937-05-03	2016-11-09
30	Champigneulles	HARTMANN	CLAUDE	M	1957-04-12	2016-11-29
31	Beuvrages	LENQUETTE	ANDRE	M	1940-09-30	2016-12-20
32	Ernée	LEMONNIER	GERARD	M	1950-04-21	2017-02-12
33	Arenthon	VELLUZ	ALAIN	M	1947-04-23	2017-03-03
34	Vaivre-et-Montoille	LORTET	PIERRE	M	1939-12-15	2017-04-01
35	Saint-Germain-en-Laye	LAMY	EMMANUEL	M	1948-05-11	2017-05-24
36	Fontcouverte-la-Toussuire	ANSELME	BERNARD	M	1961-04-22	2017-06-21
37	Bordères-sur-l'Échez	PAUL	CHRISTIAN	M	1950-07-02	2017-08-21
38	Chevreuse	GENOT	CLAUDE	M	1942-09-26	2017-08-22
39	Monnerville	BILLARD	JACKY	M	1952-11-09	2017-10-06
40	Meyreuil	LAGIER	ROBERT	M	1949-06-28	2017-10-21
41	Lodève	BOUSQUET	MARIE CHRISTINE	F	1955-08-01	2017-11-15
42	Cluses	MIVEL	JEAN LOUIS	M	1965-10-17	2017-11-21
43	Saint-Jean-de-Luz	DUHART	PEYUCO	M	1947-03-21	2017-12-07
44	Bégard	LE CAER	GERARD	M	1956-07-30	2018-01-24
45	Rosny-sur-Seine	GUILLAMAUD	MICHEL	M	1944-04-20	2018-01-28
46	Genay	ROCHE	ARTHUR	M	1949-05-30	2018-02-05
47	Mouroux	ALLEBE	JOSEPH	M	1945-04-16	2018-02-05
48	Auchel	JARRETT	RICHARD	M	1952-08-20	2018-03-05
49	Saint-Vincent-de-Tyrosse	APHATIE	MARIE	F	1953-10-01	2018-03-11
50	Champigny-sur-Marne	ADENOT	DOMINIQUE	M	1954-05-27	2018-04-05
51	Mauron	GRASLAND	EUGENE	M	1943-12-04	2018-04-24
52	Le Mans	BOULARD	JEAN CLAUDE	M	1943-03-28	2018-05-31
53	Saintes-Maries-de-la-Mer	CHASSAIN	ROLAND	M	1947-02-05	2021-02-09

Table 3.13: Summary Statistics by treatment status (diff-in-diff)

	Pre-treatment			Post-treatment			Control		
Statistic	N	Mean	Median	N	Mean	Median	N	Mean	Median
Number of bidders	112	3.43	3.00	295	3.24	3.00	26,211	3.56	3.00
Price (euros)	112	280,343.90	71,626.50	295	384,876.00	144,996.40	26,211	424,607.50	166,000.00
City winner	112	0.09	0.00	295	0.07	0.00	26,211	0.07	0.00
Dept winner	112	0.35	0.00	295	0.36	0.00	26,211	0.41	0.00
Neighbor winner	103	0.56	0.80	274	0.63	1.00	24,174	0.65	1.00
Known Winner	112	0.28	0.00	295	0.28	0.00	26,211	0.27	0.00
Number of lots	112	3.94	1	295	2.91	1	26,211	2.98	1
Multiple winners	112	0.40	0	295	0.38	0	26,211	0.42	0
Open	112	0.62	1	295	0.72	1	26,211	0.75	1
EU threhold	112	0.64	1	295	0.78	1	26,211	0.79	1
Year	112	2,016.39	2,017	295	2,018.20	2,019	26,211	2,017.68	2,018
Industry: TransportEquipment	112	0.02	0	295	0.03	0	26,211	0.05	0
Industry: Furniture	112	0.07	0	295	0.09	0	26,211	0.05	0
Industry: Materials	112	0.02	0	295	0.02	0	26,211	0.04	0
Industry: Construction	112	0.20	0	295	0.14	0	26,211	0.20	0
Industry: Maintenance	112	0.07	0	295	0.06	0	26,211	0.06	0
Industry: Finance	112	0.06	0	295	0.07	0	26,211	0.06	0
Industry: CivilEngineering	112	0.02	0	295	0.05	0	26,211	0.08	0
Industry: Agricultural	112	0.01	0	295	0.04	0	26,211	0.04	0
Industry: Services	112	0.06	0	295	0.02	0	26,211	0.04	0
Industry: Waste	112	0.05	0	295	0.06	0	26,211	0.05	0
Industry: Other	112	0.42	0	295	0.42	0	26,211	0.34	0

Table 3.14: Mayor death and the intensity of competition (TWFE): sample restricted to municipalities present since 2015

Dependent Variables:	log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Post_Mayor_Death	-0.037	0.068**	0.015	-0.064*	0.704**
	(0.083)	(0.033)	(0.078)	(0.033)	(0.313)
Fixed-effects					
year-month	Yes	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,756	12,756	12,756	12,756	10,757
R^2	0.406	0.317	0.337	0.332	0.459
Within R^2	0.015	0.003	0.014	0.024	0.105

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. The sample is restricted to cities which have been publishing award notices since 2015. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. >1/3/5 Bidders indicates the share of lots in the contract for which over 1/3/5 bidders responded. Log(Price) is the mean of the log of the award price. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.15: Mayor death and the nature of competition (TWFE): sample restricted to municipalities present since 2015

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Post_Mayor_Death	0.041	0.125**	0.035	-0.033
	(0.064)	(0.062)	(0.145)	(0.052)
Fixed-effects				
year-month	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes
Fit statistics				
Observations	12,756	12,756	11,915	12,756
R^2	0.296	0.456	0.503	0.365
Within R^2	0.002	0.006	0.004	0.089

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. The sample is restricted to cities which have been publishing award notices since 2015. All specifications contain city and time (year $\times$ month fixed effects as well as industry fixed effects and contract level controls. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.16: Mayor death and the intensity of competition (TWFE): industry specific effects

Dependent Variables:	log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Post_Mayor_Death × TransportEquipment	-0.089	0.023	-0.153**	-0.126**	0.269
	(0.141)	(0.173)	(0.067)	(0.054)	(0.439)
Post_Mayor_Death × Furniture	-0.023	-0.003	-0.117	0.071	0.140
	(0.102)	(0.050)	(0.086)	(0.055)	(0.340)
Post_Mayor_Death × Materials	-0.064	-0.110	0.273	-0.034	0.960***
	(0.240)	(0.144)	(0.187)	(0.070)	(0.145)
Post_Mayor_Death × Construction	-0.160***	0.053	-0.310***	-0.044	0.249
	(0.059)	(0.050)	(0.061)	(0.041)	(0.466)
Post_Mayor_Death × Maintenance	0.190	0.035	0.097	0.044	0.759***
	(0.212)	(0.084)	(0.170)	(0.107)	(0.261)
Post_Mayor_Death × Finance	0.052	0.050	-0.052	0.047	-0.212
	(0.108)	(0.062)	(0.122)	(0.095)	(0.275)
Post_Mayor_Death × CivilEngineering	0.008	0.084	-0.051	-0.041	0.209
	(0.184)	(0.099)	(0.158)	(0.091)	(0.299)
Post_Mayor_Death × Agriculture	-0.032	0.181***	-0.257*	-0.165***	0.757*
	(0.080)	(0.058)	(0.156)	(0.051)	(0.422)
Post_Mayor_Death × Services	0.796***	0.167***	0.193***	0.450***	
	(0.100)	(0.048)	(0.074)	(0.065)	
Post_Mayor_Death × Waste	-0.038	0.102	-0.066	-0.080	0.322*
	(0.183)	(0.080)	(0.146)	(0.105)	(0.166)
Post_Mayor_Death × Other	0.095	0.127**	-0.023	0.020	0.003
	(0.078)	(0.054)	(0.048)	(0.052)	(0.318)
Fixed-effects					
year-month	Yes	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	25,376	25,377	25,377	25,377	19,000
R^2	0.390	0.295	0.322	0.313	0.457
Within R^2	0.014	0.003	0.015	0.019	0.107

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. The treatment is interacted with industry dummy variables. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. >1/3/5 Bidders indicates the share of lots in the contract for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.17: Mayor death and the nature of competition (TWFE): industry specific effects

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Post_Mayor_Death × TransportEquipment	0.091	-0.240	0.143	-0.062
	(0.062)	(0.151)	(0.296)	(0.051)
Post_Mayor_Death × Furniture	-0.036	-0.018	-0.031	-0.014
	(0.030)	(0.089)	(0.104)	(0.078)
Post_Mayor_Death × Materials	-0.061**	0.018	-0.202	0.015
	(0.026)	(0.108)	(0.127)	(0.166)
Post_Mayor_Death × Construction	-0.084*	0.123	0.190	0.060
	(0.044)	(0.108)	(0.145)	(0.091)
Post_Mayor_Death × Maintenance	0.259***	0.092	-0.012	0.145
	(0.084)	(0.118)	(0.181)	(0.123)
Post_Mayor_Death × Finance	0.027	0.026	0.152	-0.073
	(0.034)	(0.084)	(0.127)	(0.092)
Post_Mayor_Death × CivilEngineering	-0.098***	-0.016	-0.289*	-0.157***
	(0.034)	(0.173)	(0.156)	(0.057)
Post_Mayor_Death × Agriculture	-0.020	0.086	-0.081	-0.085
	(0.057)	(0.174)	(0.178)	(0.092)
Post_Mayor_Death × Services	0.414***	0.312***	0.287**	0.208***
	(0.046)	(0.093)	(0.117)	(0.053)
Post_Mayor_Death × Waste	0.021	0.045	-0.123	-0.093
	(0.109)	(0.125)	(0.128)	(0.104)
Post_Mayor_Death × Other	0.024	0.056	-0.007	-0.045
	(0.038)	(0.087)	(0.113)	(0.041)
Fixed-effects				
year-month	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes
Fit statistics				
Observations	25,377	25,377	23,490	25,377
R^2	0.276	0.441	0.489	0.358
Within R^2	0.002	0.003	0.004	0.119

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. The treatment is interacted with industry dummy variables. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.18: Mayor death and the intensity of competition (TWFE): contract size specific effects

Dependent Variables:	log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Post_Mayor_Death × Low value	0.016	0.133	-0.147	-0.032	-0.654**
	(0.134)	(0.087)	(0.097)	(0.065)	(0.312)
Post_Mayor_Death × Mid value	0.153	0.195***	-0.009	-0.011	0.174
	(0.117)	(0.067)	(0.078)	(0.068)	(0.318)
Post_Mayor_Death × High value	0.076	0.164***	-0.154	0.045	0.611**
	(0.128)	(0.062)	(0.108)	(0.059)	(0.251)
Post_Mayor_Death × VeryHighValue	-0.065	0.097	-0.195*	-0.096	1.245***
	(0.105)	(0.060)	(0.105)	(0.061)	(0.364)
Fixed-effects					
year-month	Yes	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	18,999	19,000	19,000	19,000	19,000
R^2	0.421	0.329	0.354	0.340	0.460
Within R^2	0.010	0.003	0.009	0.008	0.111

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. The treatment is interacted with contract size dummy variables. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. >1/3/5 Bidders indicates the share of lots in the contract for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.19: Mayor death and the nature of competition (TWFE): contract size specific effects

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Post_Mayor_Death × Low value	0.037	0.010	0.033	-0.027
	(0.036)	(0.093)	(0.122)	(0.040)
Post_Mayor_Death × Mid value	-0.015	0.082	0.041	-0.135***
	(0.051)	(0.061)	(0.084)	(0.048)
Post_Mayor_Death × High value	-0.011	-0.014	-0.102	-0.064
	(0.056)	(0.126)	(0.155)	(0.079)
Post_Mayor_Death × VeryHighValue	0.066	0.193*	-0.056	0.007
	(0.070)	(0.114)	(0.151)	(0.059)
Fixed-effects				
year-month	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes
Fit statistics				
Observations	19,000	19,000	17,525	19,000
R^2	0.305	0.473	0.519	0.380
Within R^2	0.002	0.004	0.004	0.110

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. The treatment is interacted with contract size dummy variables. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.20: Mayor death and the intensity of competition: 2016 cohort specific effects

Dependent Variables:	log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Post_Mayor_Death	0.144	0.152***	0.070	-0.008	0.361
	(0.097)	(0.039)	(0.061)	(0.046)	(0.297)
Fixed-effects					
year-month	Yes	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	20,512	20,512	20,512	20,512	15,375
R^2	0.731	0.696	0.700	0.670	0.806
Within R^2	0.015	0.005	0.025	0.021	0.106

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Only cities treated in 2016 are considered in the treatment group. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.21: Mayor death and the nature of competition: 2016 cohort specific effects

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Post_Mayor_Death	0.060	0.040	-0.131	0.017
	(0.077)	(0.080)	(0.179)	(0.080)
Fixed-effects				
year-month	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes
Fit statistics				
Observations	20,512	20,512	19,026	20,512
R^2	0.635	0.771	0.801	0.641
Within R^2	0.001	0.004	0.005	0.126

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Only cities treated in 2016 are considered in the treatment group. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.22: Mayor death and the intensity of competition: 2017 cohort specific effects

Dependent Variables:	log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Post_Mayor_Death	0.006	0.116	-0.182	0.100	-0.044
	(0.195)	(0.119)	(0.166)	(0.109)	(0.215)
Fixed-effects					
year-month	Yes	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	20,203	20,204	20,204	20,204	14,733
R^2	0.728	0.694	0.696	0.672	0.803
Within R^2	0.018	0.006	0.030	0.021	0.108

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. >1/3/5 Bidders indicates the share of lots in the contract for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Only cities treated in 2017 are considered in the treatment group. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.23: Mayor death and the nature of competition: 2017 cohort specific effects

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Post_Mayor_Death	0.021	0.030	-0.062	0.148
	(0.042)	(0.069)	(0.099)	(0.111)
Fixed-effects				
year-month	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes
Fit statistics				
Observations	20,204	20,204	18,729	20,204
R^2	0.645	0.763	0.792	0.639
Within R^2	0.002	0.003	0.007	0.151

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Only cities treated in 2017 are considered in the treatment group. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.24: Mayor death and the intensity of competition: 2018 cohort specific effects

Dependent Variables:	log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Post_Mayor_Death	0.274	0.301**	0.124	0.153	-0.226
	(0.253)	(0.147)	(0.201)	(0.166)	(0.326)
Fixed-effects					
year-month	Yes	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	19,758	19,759	19,759	19,759	14,273
R^2	0.737	0.689	0.709	0.676	0.806
Within R^2	0.019	0.009	0.027	0.025	0.123

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. >1/3/5 Bidders indicates the share of lots in the contract for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Only cities treated in 2018 are considered in the treatment group. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.25: Mayor death and the nature of competition: 2018 cohort specific effects

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Post_Mayor_Death	0.033	-0.012	-0.131	-0.012
	(0.047)	(0.079)	(0.197)	(0.041)
Fixed-effects				
year-month	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes
Fit statistics				
Observations	19,759	19,759	18,301	19,759
R^2	0.644	0.767	0.794	0.642
Within R^2	0.003	0.006	0.009	0.131

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Only cities treated in 2018 are considered in the treatment group. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.26: Mayor death and the intensity of competition: city specific effect (Champigny-sur-Marne)

Dependent Variables:	log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Post_Mayor_Death	-0.173***	0.113***	-0.235***	-0.196***	-0.338***
	(0.036)	(0.021)	(0.038)	(0.020)	(0.069)
Fixed-effects					
year-month	Yes	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	25,071	25,072	25,072	25,072	18,804
R^2	0.390	0.294	0.323	0.314	0.456
Within R^2	0.014	0.003	0.014	0.019	0.106

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Only the city of Champigny-sur-Marne is considered in the treatment group. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.27: Mayor death and the nature of competition: city specific effect (Champigny-sur-Marne)

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Post_Mayor_Death	0.087***	-0.033	-0.381***	-0.032
	(0.015)	(0.032)	(0.033)	(0.026)
Fixed-effects				
year-month	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes
Fit statistics				
Observations	25,072	25,072	23,208	25,072
R^2	0.277	0.442	0.492	0.359
Within R^2	0.001	0.003	0.003	0.118

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Log(Price) is the log of the award price. Only the city of Champigny-sur-Marne is considered in the treatment group. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.28: Mayor death and the intensity of competition: city specific effect (Thionville)

Dependent Variables:	log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Post_Mayor_Death	-0.023	-0.048**	-0.086**	0.077***	0.002
	(0.039)	(0.025)	(0.037)	(0.030)	(0.066)
Fixed-effects					
year-month	Yes	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	25,071	25,072	25,072	25,072	18,799
R^2	0.390	0.294	0.322	0.314	0.456
Within R^2	0.014	0.003	0.014	0.019	0.106

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Only the city of Thionville is considered in the treatment group. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.29: Mayor death and the nature of competition: city specific effect (Thionville)

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Post_Mayor_Death	0.029	-0.119***	-0.302***	0.000
	(0.018)	(0.031)	(0.031)	(0.028)
Fixed-effects				
year-month	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes
Fit statistics				
Observations	25,072	25,072	23,210	25,072
R^2	0.277	0.442	0.491	0.360
Within R^2	0.001	0.003	0.003	0.118

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Only the city of Thionville is considered in the treatment group. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.30: Mayor death and the intensity of competition: city specific effect (Le Mans)

Dependent Variables:	log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Post_Mayor_Death	-0.115**	0.000	-0.038	-0.028	0.218***
	(0.051)	(0.031)	(0.033)	(0.028)	(0.084)
Fixed-effects					
year-month	Yes	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	25,134	25,135	25,135	25,135	18,829
R^2	0.390	0.294	0.322	0.313	0.456
Within R^2	0.014	0.003	0.014	0.019	0.106

Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Only the city of Le Mans is considered in the treatment group. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.31: Mayor death and the nature of competition: city specific effect (Le Mans)

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Post_Mayor_Death	0.010	0.200***	0.115***	0.054**
	(0.026)	(0.032)	(0.032)	(0.023)
Fixed-effects				
year-month	Yes	Yes	Yes	Yes
city	Yes	Yes	Yes	Yes
Industry_5	Yes	Yes	Yes	Yes
Fit statistics				
Observations	25,135	25,135	23,274	25,135
R^2	0.276	0.441	0.490	0.359
Within R^2	0.001	0.003	0.003	0.118

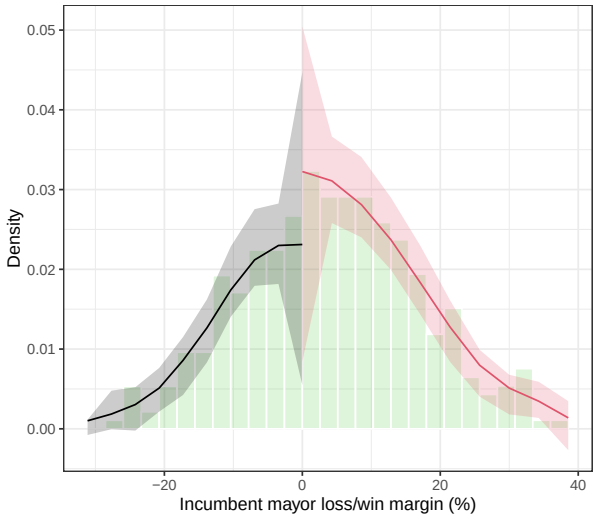
Note: this table presents the results of a series of two-way-fixed effects models estimated via OLS. All specifications contain city and time (year × month fixed effects as well as industry fixed effects and contract level controls. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Only the city of Le Mans is considered in the treatment group. Standard errors are clustered at the city level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

C3: Complementary data and tests for RD analysis

Table 3.32: Summary Statistics by according to treatment status: RD analysis

	Incumbent mayor overturned			Incumbent mayor reelected		
Statistic	N	Mean	Median	N	Mean	Median
Number of bidders	5,450	3.67	3.00	8,763	3.58	3.00
Award price (euros)	5,771	472,897.80	200,000.00	9,325	511,673.80	200,000.00
City winner	5,450	0.10	0.00	8,763	0.08	0.00
Dept winner	5,771	0.38	0.00	9,325	0.39	0.00
Neighbor winner	5,071	0.63	1.00	8,246	0.64	1.00
Known Winner	5,450	0.34	0.11	8,763	0.35	0.17
Number of lots	5,771	2.94	1	9,325	2.87	1
Multiple winners	5,450	0.42	0	8,763	0.42	0
Open	5,771	0.74	1	9,325	0.77	1
EU threhold	5,771	0.78	1	9,325	0.83	1
Year	5,771	2019.42	2020	9,325	2020.05	2021
Industry: TransportEquipment	5,771	0.05	0	9,325	0.05	0
Industry: Furniture	5,771	0.05	0	9,325	0.06	0
Industry: Materials	5,771	0.04	0	9,325	0.03	0
Industry: Construction	5,771	0.19	0	9,325	0.16	0
Industry: Maintenance	5,771	0.06	0	9,325	0.06	0
Industry: Finance	5,771	0.05	0	9,325	0.04	0
Industry: CivilEngineering	5,771	0.09	0	9,325	0.10	0
Industry: Agricultural	5,771	0.04	0	9,325	0.04	0
Industry: Services	5,771	0.05	0	9,325	0.06	0
Industry: Waste	5,771	0.06	0	9,325	0.06	0
Industry: Other	5,771	0.34	0	9,325	0.34	0

Figure 3.9: Density test from Cattaneo et al. [2018] ($p = 0.81$)



Note: this figure tests for endogenous sorting of winning candidates above the 0 cutoff. It plots the density of vote margins in our election data set, aggregated at the cit-term level. The p-value indicates the probability that the density of cities above the 0 threshold is not significantly different from that below the 0 threshold.

Table 3.33: Balance checks: city characteristics

	Population	Density	Unemployment	Median income	Latitude	Longitude	Number of contracts
Turnover	-0.348	-0.474	0.412	-0.205*	-0.315	0.057	-0.341
	(0.622)	(0.546)	(0.926)	(0.137)	(1.130)	(1.238)	(0.557)
Obs.	152	165	165	165	165	165	165
Robust p-value	0.722	0.527	0.579	0.087	0.699	0.875	0.632
Bandwidth	10.67	7.86	11.01	7.89	8.52	8.53	11.51
N. effective obs.	79	64	92	64	68	68	97

Note: This table presents tests for balance between the treated and control group. It presents nonparametric RD estimates from local linear regression, using city characteristics as covariates and using Calonico et al. [2014] to obtain p-values. Optimal bandwidth (in vote shares) is determined using Calonico et al. [2019]. The number of effective observations is the number observations within the bandwidth (which enter in the estimation of local linear regression). Variables "Population", "Density", "Median Income" and "Number of contracts" are log transformed. Signif. codes: ***: 0.01, **: 0.05, *: 0.1.

Table 3.34: Balance checks: Type of goods and services procured

	Construction	CivilEngineering	Maintenance	Finance	Waste	Furniture	Services	TransportEquipment	Agricultural	Materials
Turnover	0.08	-0.001	0.014	0.036	-0.04	0.026	-0.019	-0.007	-0.011	-0.016
	(0.071)	(0.042)	(0.030)	(0.033)	(0.029)	(0.045)	(0.023)	(0.037)	(0.024)	(0.023)
Obs.	165	165	165	165	165	165	165	165	165	165
Robust p-value	0.183	0.902	0.433	0.302	0.106	0.523	0.34	0.839	0.529	0.606
Bandwidth	8.27	8.43	7.43	12.21	12.49	9.13	9.75	9.86	10.21	10.42
N. effective obs.	68	68	60	103	104	74	80	82	84	84
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents tests for balance between the treated and control group. It presents nonparametric RD estimates from local linear regression, using shares of industries in city procurement as dependent variables, and using Calonico et al. [2014] to obtain p-values. Optimal bandwidth (in vote shares) is determined using Calonico et al. [2019]. The number of effective observations is the number observations within the bandwidth (which enter in the estimation of local linear regression). Signif. codes: ***: 0.01, **: 0.05, *: 0.1.

Table 3.35: Electoral turnover, previous mayor tenure and the intensity of competition (Parametric estimation, 2nd order polynomial)

Dependent Variables:	Log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	Log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Turnover	0.038	0.026	-0.024	0.041	0.018
	(0.073)	(0.038)	(0.055)	(0.034)	(0.225)
Turnover × Tenure (past mayor)	0.019	-0.006	0.040*	-0.002	0.105
	(0.034)	(0.018)	(0.023)	(0.014)	(0.093)
Fixed-effects					
Industry	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	12,828	12,828	12,828	12,828	13,486
R^2	0.313	0.207	0.242	0.248	0.375
Within R^2	0.016	0.023	0.015	0.017	0.157

Note: This table presents the results from parametric RD estimation, using a quadratic polynomial function of the vote margin, interacting the discontinuity at the cutoff with tenure of the previous mayor. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.36: Electoral turnover, previous mayor tenure and the nature of competition (Parametric estimation, 2nd order polynomial)

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Turnover	0.074*	-0.030	0.015	0.142***
	(0.040)	(0.057)	(0.057)	(0.052)
Turnover × Tenure (past mayor)	-0.030*	-0.021	-0.006	-0.056**
	(0.015)	(0.024)	(0.026)	(0.022)
Fixed-effects				
Industry	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	12,828	13,486	11,996	12,828
R^2	0.156	0.269	0.339	0.198
Within R^2	0.039	0.034	0.020	0.061

Note: This table presents the results from parametric RD estimation, using a quadratic polynomial function of the vote margin, interacting the discontinuity at the cutoff with tenure of the previous mayor. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.37: Electoral turnover and the intensity of competition (Parametric estimation, 2nd order polynomial): sample restricted to municipalities present since 2015

Dependent Variables:	Log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	Log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Turnover	0.076**	0.011	0.059**	0.045**	0.201*
	(0.037)	(0.020)	(0.027)	(0.023)	(0.113)
Fixed-effects					
Industry	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	9,942	9,942	9,942	9,942	10,464
R^2	0.340	0.235	0.270	0.271	0.324
Within R^2	0.014	0.027	0.013	0.020	0.122

Note: This table presents the results from parametric RD estimation, using a quadratic polynomial function of the vote margin. The sample is restricted to cities which have been publishing award notices since 2015. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.38: Electoral turnover and the nature of competition (Parametric estimation, 2nd order polynomial): sample restricted to municipalities present since 2015

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Turnover	0.014	-0.102***	-0.017	0.031
	(0.021)	(0.029)	(0.025)	(0.020)
Fixed-effects				
Industry	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	9,942	10,464	9,436	9,942
R^2	0.170	0.299	0.356	0.227
Within R^2	0.038	0.036	0.021	0.063

Note: This table presents the results from parametric RD estimation, using a quadratic polynomial function of the vote margin. The sample is restricted to cities which have been publishing award notices since 2015. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.39: Electoral turnover and the intensity of competition (Parametric estimation, 2nd order polynomial): industry specific effects

Dependent Variables:	Log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	Log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Turnover × TransportEquipment	-0.024	-0.031	-0.019	-0.002	0.275
	(0.068)	(0.048)	(0.047)	(0.028)	(0.207)
Turnover × Furniture	0.020	0.010	0.039	-0.023	0.197
	(0.060)	(0.031)	(0.061)	(0.038)	(0.153)
Turnover × Materials	0.021	-0.050	-0.034	0.042	0.297
	(0.078)	(0.047)	(0.070)	(0.041)	(0.192)
Turnover × Construction	0.015	0.007	0.010	0.031	0.127
	(0.043)	(0.020)	(0.042)	(0.030)	(0.148)
Turnover × Maintenance	0.124*	0.015	0.143***	0.023	0.147
	(0.067)	(0.039)	(0.050)	(0.040)	(0.202)
Turnover × Finance	0.068	0.009	0.058	-0.006	0.245
	(0.052)	(0.033)	(0.056)	(0.023)	(0.178)
Turnover × CivilEngineering	0.050	-0.019	0.024	0.063	0.492***
	(0.066)	(0.031)	(0.048)	(0.045)	(0.142)
Turnover × Agricultural	0.027	0.013	0.026	0.020	0.376*
	(0.082)	(0.040)	(0.068)	(0.063)	(0.203)
Turnover × Services	0.038	-0.014	0.043	0.026	0.024
	(0.081)	(0.040)	(0.066)	(0.050)	(0.183)
Turnover × Waste	0.085	0.051	0.066	0.038	0.370**
	(0.086)	(0.042)	(0.048)	(0.059)	(0.185)
Turnover × Other	0.072*	0.029	0.053*	0.023	0.301**
	(0.038)	(0.024)	(0.030)	(0.020)	(0.129)
Fixed-effects					
Industry	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	13,224	13,224	13,224	13,224	13,224
R^2	0.305	0.207	0.238	0.237	0.370
Within R^2	0.019	0.025	0.017	0.021	0.145

Note: This table presents the results from parametric RD estimation, using a quadratic polynomial function of the vote margin, interacting the discontinuity with industry dummy variables. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.40: Electoral turnover and the nature of competition (Parametric estimation, 2nd order polynomial): industry specific effects

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Turnover × TransportEquipment	0.037	-0.059	0.014	0.045
	(0.044)	(0.052)	(0.057)	(0.046)
Turnover × Furniture	0.009	-0.101**	-0.061	0.052
	(0.023)	(0.041)	(0.051)	(0.046)
Turnover × Materials	0.015	-0.062	-0.086	0.007
	(0.033)	(0.053)	(0.060)	(0.055)
Turnover × Construction	-0.006	-0.067*	0.033	0.058*
	(0.028)	(0.037)	(0.034)	(0.034)
Turnover × Maintenance	0.059*	-0.086	0.042	-0.025
	(0.032)	(0.054)	(0.050)	(0.050)
Turnover × Finance	-0.007	-0.080**	-0.003	-0.051
	(0.017)	(0.032)	(0.040)	(0.045)
Turnover × CivilEngineering	0.022	-0.113**	-0.047	-0.003
	(0.036)	(0.055)	(0.053)	(0.040)
Turnover × Agricultural	0.045	-0.089	0.001	-0.021
	(0.038)	(0.062)	(0.046)	(0.055)
Turnover × Services	0.000	-0.042	0.038	0.011
	(0.036)	(0.051)	(0.053)	(0.052)
Turnover × Waste	0.023	-0.088	0.066	0.008
	(0.053)	(0.053)	(0.047)	(0.048)
Turnover × Other	0.016	-0.064*	0.031	0.034
	(0.020)	(0.033)	(0.027)	(0.028)
Fixed-effects				
Industry	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	13,224	13,224	12,396	13,224
R^2	0.152	0.270	0.338	0.200
Within R^2	0.038	0.036	0.021	0.060

Note: This table presents the results from parametric RD estimation, using a quadratic polynomial function of the vote margin, interacting the discontinuity with industry dummy variables. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.41: Electoral turnover and the intensity of competition (Parametric estimation, 2nd order polynomial): size of contract specific effects

Dependent Variables:	Log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	Log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Turnover × LowValue	0.048	0.018	0.039	0.026	-1.232***
	(0.036)	(0.019)	(0.030)	(0.021)	(0.102)
Turnover × MediumValue	0.040	0.000	0.032	0.024	-0.106
	(0.040)	(0.020)	(0.031)	(0.024)	(0.099)
Turnover × HighValue	0.087**	0.001	0.065**	0.042*	0.511***
	(0.040)	(0.022)	(0.026)	(0.023)	(0.097)
Turnover × VeryHighValue	0.079**	0.022	0.051*	0.030	1.488***
	(0.036)	(0.020)	(0.027)	(0.021)	(0.105)
Fixed-effects					
Industry	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	13,478	13,478	13,478	13,478	14,154
R^2	0.309	0.205	0.240	0.245	0.525
Within R^2	0.016	0.023	0.015	0.018	0.364

Note: This table presents the results from parametric RD estimation, using a quadratic polynomial function of the vote margin, interacting the discontinuity with size of contract dummy variables. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.42: Electoral turnover and the nature of competition (Parametric estimation, 2nd order polynomial): size of contract specific effects

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Turnover × LowValue	0.019	-0.077***	-0.013	0.037
	(0.020)	(0.029)	(0.026)	(0.027)
Turnover × MediumValue	0.011	-0.084***	-0.021	0.023
	(0.019)	(0.031)	(0.024)	(0.025)
Turnover × HighValue	0.011	-0.073**	0.035	0.000
	(0.017)	(0.030)	(0.026)	(0.023)
Turnover × VeryHighValue	0.015	-0.048	0.014	0.033
	(0.024)	(0.033)	(0.029)	(0.028)
Fixed-effects				
Industry	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	13,478	14,154	12,636	13,478
R^2	0.150	0.269	0.335	0.198
Within R^2	0.037	0.037	0.021	0.060

Note: This table presents the results from parametric RD estimation, using a quadratic polynomial function of the vote margin, interacting the discontinuity with size of contract dummy variables. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.43: Electoral turnover and the intensity of competition (Parametric estimation, 3rd order polynomial)

Dependent Variables:	Log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	Log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Turnover	0.085**	0.014	0.059**	0.043**	0.209
	(0.041)	(0.023)	(0.029)	(0.022)	(0.157)
Fixed-effects					
Industry	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	13,478	13,478	13,478	13,478	14,154
R^2	0.308	0.205	0.240	0.245	0.366
Within R^2	0.015	0.023	0.014	0.018	0.152

Note: This table presents the results from parametric RD estimation, using a third order polynomial function of the vote margin. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.44: Electoral turnover and the nature of competition (Parametric estimation, 3rd order polynomial)

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Turnover	0.032	-0.074**	-0.004	0.005
	(0.023)	(0.033)	(0.031)	(0.025)
Fixed-effects				
Industry	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	13,478	14,154	12,636	13,478
R^2	0.145	0.267	0.332	0.197
Within R^2	0.031	0.034	0.017	0.060

Note: This table presents the results from parametric RD estimation, using a third order polynomial function of the vote margin. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.45: Electoral turnover and the intensity of competition (Parametric estimation, 4th order polynomial)

Dependent Variables:	Log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	Log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Turnover	0.085**	0.014	0.059**	0.043**	0.210
	(0.041)	(0.023)	(0.029)	(0.021)	(0.157)
Fixed-effects					
Industry	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	13,478	13,478	13,478	13,478	14,154
R^2	0.308	0.206	0.240	0.245	0.366
Within R^2	0.015	0.023	0.014	0.018	0.152

Note: This table presents the results from parametric RD estimation, using a fourth order polynomial function of the vote margin. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.46: Electoral turnover and the nature of competition (Parametric estimation, 4th order polynomial)

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Turnover	0.031	-0.074**	-0.004	0.005
	(0.023)	(0.033)	(0.030)	(0.025)
Fixed-effects				
Industry	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	13,478	14,154	12,636	13,478
R^2	0.146	0.267	0.333	0.197
Within R^2	0.032	0.034	0.018	0.060

Note: This table presents the results from parametric RD estimation, using a fourth order polynomial function of the vote margin. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.47: Electoral turnover and the intensity of competition (Parametric estimation, 2nd order polynomial): excluding the COVID lockdown period

Dependent Variables:	Log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	Log(Price)
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Turnover	0.050	0.008	0.034	0.028	0.281**
	(0.035)	(0.018)	(0.025)	(0.018)	(0.111)
Fixed-effects					
Industry	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	11,068	11,068	11,068	11,068	11,632
R^2	0.307	0.210	0.245	0.246	0.394
Within R^2	0.017	0.025	0.015	0.019	0.155

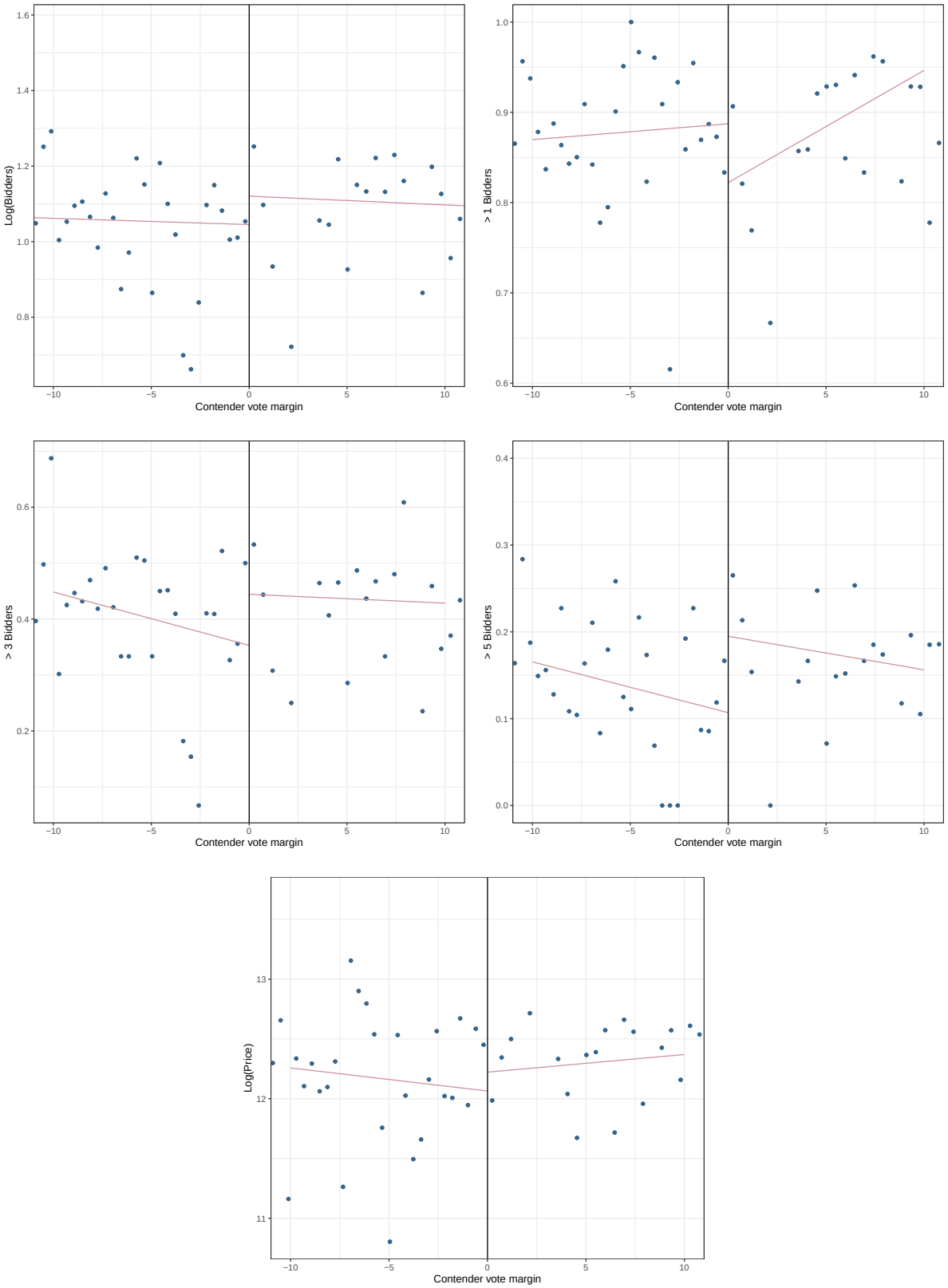
Note: This table presents the results from parametric RD estimation, using a quadratic function of the vote margin, and excluding contracts awarded during Covid restrictions. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3.48: Electoral turnover and the nature of competition (Parametric estimation, 2nd order polynomial): excluding the COVID lockdown period

Dependent Variables:	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Model:	(1)	(2)	(3)	(4)
Variables				
Turnover	0.018	-0.067**	0.006	0.012
	(0.018)	(0.028)	(0.023)	(0.023)
Fixed-effects				
Industry	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	11,068	11,632	10,428	11,068
R^2	0.166	0.281	0.348	0.217
Within R^2	0.036	0.037	0.021	0.069

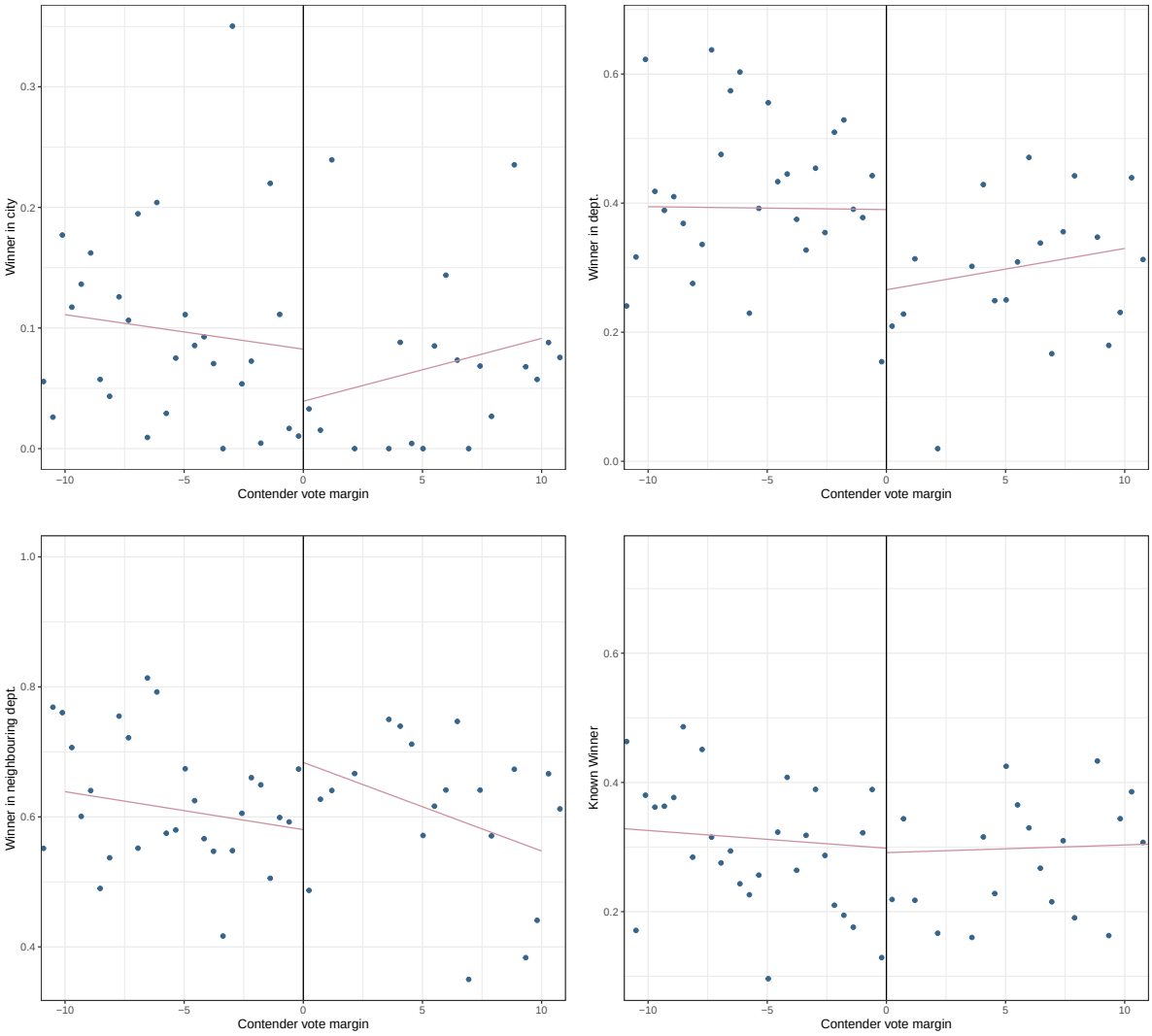
Note: This table presents the results from parametric RD estimation, using a quadratic function of the vote margin, and excluding contracts awarded during Covid restrictions. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Std errors are clustered at the mayor level. All specifications control for industry and year fixed effects, as well as for city, mayor and contract characteristics. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Figure 3.10: Electoral turnover and the intensity of competition: visual evidence



Note: This set of figure plots local linear regression lines using an arbitrary bandwidth of 10pp.above at below the cutoff. The x axis indicates the vote margin (with positive values indicating turnover). The y-axis indicates one of the "intensity of competition" dependent variables. Data is aggregated at the city-term level. Source: Author.

Figure 3.11: Electoral turnover and the nature of competition: visual evidence



Note: This set of figure plots local linear regression lines using an arbitrary bandwidth of 10pp.above at below the cutoff. The x axis indicates the vote margin (with positive values indicating turnover). The y-axis indicates one of the "nature of competition" dependent variables. Data is aggregated at the city-term level. Source: Author.

Table 3.49: Electoral turnover and the intensity of competition: Nonparametric RD estimates (3/4 bandwidth)

	Log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	Log(Price)
Turnover	0.085*	-0.05	0.101	0.127**	0.096
	(0.079)	(0.062)	(0.074)	(0.058)	(0.428)
Obs.	165	165	165	165	165
Robust p-value	0.058	0.921	0.449	0.049	0.429
Bandwidth	8.89	7.17	7.4	6.87	6.46
N. effective obs.	69	57	60	55	54
Election year FE	Yes	Yes	Yes	Yes	Yes

Note: this table presents nonparametric RD estimates from local linear regression, using Calonico et al. [2014] to obtain p-values. Bandwidth (in vote shares) is determined using 3/4 of the Calonico et al. [2019] optimal bandwidth. The number of effective observations is the number observations within the bandwidth (which enter in the estimation of local linear regression). Data is aggregated at the municipal term level. Log(Bidders) is the average log number of bidders. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Signif. codes: ***: 0.01, **: 0.05, *: 0.1.

Table 3.50: Electoral turnover and the nature of competition: Nonparametric RD estimates (3/4 bandwidth)

	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Turnover	-0.061	-0.158	-0.079	-0.011
	(0.047)	(0.146)	(0.117)	(0.069)
Obs.	165	165	164	165
Robust p-value	0.284	0.788	0.245	0.982
Bandwidth	8.9	6.15	4.62	8.23
N. effective obs.	69	49	34	68
Election year FE	Yes	Yes	Yes	Yes

Note: this table presents nonparametric RD estimates from local linear regression, using Calonico et al. [2014] to obtain p-values. Bandwidth (in vote shares) is determined using 3/4 of the Calonico et al. [2019] optimal bandwidth. The number of effective observations is the number observations within the bandwidth (which enter in the estimation of local linear regression). Data is aggregated at the municipal term level. Log(Bidders) is the average log number of bidders. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Signif. codes: ***: 0.01, **: 0.05, *: 0.1.

Table 3.51: Electoral turnover and the intensity of competition: Nonparametric RD estimates (3/2 bandwidth)

	Log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	Log(Price)
Turnover	0.085	-0.05	0.092**	0.094***	0.232
	(0.060)	(0.045)	(0.053)	(0.045)	(0.288)
Obs.	165	165	165	165	165
Robust p-value	0.103	0.149	0.047	0.005	0.611
Bandwidth	17.79	14.34	14.8	13.74	12.92
N. effective obs.	130	111	113	109	105
Election year FE	Yes	Yes	Yes	Yes	Yes

Note: this table presents nonparametric RD estimates from local linear regression, using Calonico et al. [2014] to obtain p-values. Bandwidth (in vote shares) is determined using 3/2 of the Calonico et al. [2019] optimal bandwidth. The number of effective observations is the number observations within the bandwidth (which enter in the estimation of local linear regression). Data is aggregated at the municipal term level. Log(Bidders) is the average log number of bidders. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Signif. codes: ***: 0.01, **: 0.05, *: 0.1.

Table 3.52: Electoral turnover and the nature of competition: Nonparametric RD estimates (3/2 bandwidth)

	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Turnover	-0.06	-0.162**	0.006	0.008
	(0.040)	(0.081)	(0.086)	(0.056)
Obs.	165	165	164	165
Robust p-value	0.201	0.037	0.319	0.722
Bandwidth	17.8	12.29	9.25	16.46
N. effective obs.	130	103	77	124
Election year FE	Yes	Yes	Yes	Yes

Note: this table presents nonparametric RD estimates from local linear regression, using Calonico et al. [2014] to obtain p-values. Bandwidth (in vote shares) is determined using 3/2 of the Calonico et al. [2019] optimal bandwidth. The number of effective observations is the number observations within the bandwidth (which enter in the estimation of local linear regression). Data is aggregated at the municipal term level. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Signif. codes: ***: 0.01, **: 0.05, *: 0.1.

Table 3.53: Electoral turnover and the intensity of competition: Nonparametric RD estimates (quadratic specification)

	Log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	Log(Price)
Turnover	0.092	-0.007	0.1	0.127***	-0.075
	(0.077)	(0.060)	(0.062)	(0.049)	(0.393)
Obs.	165	165	165	165	165
Robust p-value	0.218	0.784	0.121	0.007	0.652
Bandwidth	12.27	8.65	12.56	13.8	8.91
N. effective obs.	103	69	104	109	69
Election year FE	Yes	Yes	Yes	Yes	Yes

Note: this table presents nonparametric RD estimates from local quadratic regression, using Calonico et al. [2014] to obtain p-values. Optimal bandwidth (in vote shares) is determined using Calonico et al. [2019]. The number of effective observations is the number observations within the bandwidth (which enter in the estimation of local linear regression). Data is aggregated at the municipal term level. Log(Bidders) is the average log number of bidders. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Signif. codes: ***: 0.01, **: 0.05, *: 0.1.

Table 3.54: Electoral turnover and the nature of competition: Nonparametric RD estimates (quadratic specification)

	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Turnover	-0.055	-0.169	-0.091	-0.024
	(0.046)	(0.101)	(0.098)	(0.067)
Obs.	165	165	164	165
Robust p-value	0.224	0.105	0.202	0.496
Bandwidth	13.59	11.05	9.09	11.87
N. effective obs.	108	92	74	101
Election year FE	Yes	Yes	Yes	Yes

Note: this table presents nonparametric RD estimates from local quadratic regression, using Calonico et al. [2014] to obtain p-values. Optimal bandwidth (in vote shares) is determined using Calonico et al. [2019]. The number of effective observations is the number observations within the bandwidth (which enter in the estimation of local linear regression). Data is aggregated at the municipal term level. Log(Bidders) is the average log number of bidders. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Signif. codes: ***: 0.01, **: 0.05, *: 0.1.

Table 3.55: Electoral turnover and the intensity of competition: Nonparametric RD estimates (only cities with $>$ 30 contracts)

	Log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	Log(Price)
Turnover	0.04	-0.065	0.031	0.088*	0.311
	(0.088)	(0.052)	(0.071)	(0.051)	(0.283)
Obs.	120	120	120	120	120
Robust p-value	0.649	0.171	0.605	0.063	0.239
Bandwidth	11.49	11.76	11.95	12.49	11.6
N. effective obs.	69	71	72	74	70
Election year FE	Yes	Yes	Yes	Yes	Yes

Note: this table presents nonparametric RD estimates from local linear regression, using Calonico et al. [2014] to obtain p-values. Optimal bandwidth (in vote shares) is determined using Calonico et al. [2019]. The number of effective observations is the number observations within the bandwidth (which enter in the estimation of local linear regression). Data is aggregated at the municipal term level. Sample restricted to cities with over 30 contracts awarded. Log(Bidders) is the average log number of bidders. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Signif. codes: ***: 0.01, **: 0.05, *: 0.1.

Table 3.56: Electoral turnover and the nature of competition: Nonparametric RD estimates (only cities with >30 contracts)

	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Turnover	-0.068*	-0.2***	-0.038	-0.004
	(0.043)	(0.065)	(0.094)	(0.064)
Obs.	120	120	119	120
Robust p-value	0.097	0.001	0.421	0.769
Bandwidth	11.35	9.8	7.98	9.37
N. effective obs.	68	57	44	55
Election year FE	Yes	Yes	Yes	Yes

Note: this table presents nonparametric RD estimates from local linear regression, using Calonico et al. [2014] to obtain p-values. Optimal bandwidth (in vote shares) is determined using Calonico et al. [2019]. The number of effective observations is the number observations within the bandwidth (which enter in the estimation of local linear regression). Data is aggregated at the municipal term level. Sample restricted to cities with over 30 contracts awarded. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Signif. codes: ***: 0.01, **: 0.05, *: 0.1.

Table 3.57: Electoral turnover and the intensity of competition: Nonparametric RD estimates (only cities with $>$ 40 contracts)

	Log(Bidders)	>1 Bidders	>3 Bidders	>5 Bidders	Log(Price)
Turnover	0.108	0.014	0.076	0.064	0.227
	(0.108)	(0.049)	(0.081)	(0.052)	(0.254)
Obs.	93	93	93	93	93
Robust p-value	0.271	0.759	0.3	0.237	0.291
Bandwidth	11.94	12.64	12.16	12.53	10.82
N. effective obs.	54	57	55	56	46
Election year FE	Yes	Yes	Yes	Yes	Yes

Note: this table presents nonparametric RD estimates from local linear regression, using Calonico et al. [2014] to obtain p-values. Optimal bandwidth (in vote shares) is determined using Calonico et al. [2019]. The number of effective observations is the number observations within the bandwidth (which enter in the estimation of local linear regression). Data is aggregated at the municipal term level. Sample restricted to cities with over 40 contracts awarded. Log(Bidders) is the average log number of bidders. >1/3/5 Bidders indicates the share of contracts for which over 1/3/5 bidders responded. Log(Price) is the log of the award price. Signif. codes: ***: 0.01, **: 0.05, *: 0.1.

Table 3.58: Electoral turnover and the nature of competition: Nonparametric RD estimates (only cities with $>$ 40 contracts)

	CityWinner	DeptWinner	NeighborWinner	KnownWinner
Turnover	-0.007	-0.141***	-0.175***	-0.001
	(0.058)	(0.057)	(0.061)	(0.059)
Obs.	93	93	93	93
Robust p-value	0.908	0.009	0.001	0.921
Bandwidth	15	10.16	7	10.6
N. effective obs.	62	43	27	45
Election year FE	Yes	Yes	Yes	Yes

Note: this table presents nonparametric RD estimates from local linear regression, using Calonico et al. [2014] to obtain p-values. Optimal bandwidth (in vote shares) is determined using Calonico et al. [2019]. The number of effective observations is the number observations within the bandwidth (which enter in the estimation of local linear regression). Data is aggregated at the municipal term level. Sample restricted to cities with over 40 contracts awarded. CityWinner, DeptWinner and NeighborWinner indicate the share of winners located respectively in the same city, in the same département, in a neigboring département (including the département itself) as the city. KnownWinner indicates the share of winners which the municipality had previously interacted with. Signif. codes: ***: 0.01, **: 0.05, *: 0.1.

General conclusion

Does identity matter?

This dissertation aimed at empirically identifying the way identity shapes public-private relationships. Identity can be thought of either as organizational or individual. In the former case, the main question is to know how interacting with a previously known partner affects the potential outcomes of a contractual relationship. In the latter, the main debate is whether identity of individuals within organizations (such as elected officials for public authorities and managers for private companies) is relevant to the awarding, design and performance of public-private contracts. The question of individual identity is the focus of Chapter 1, where I provide some evidence that changes in management reduce the effects of relational contracting on the level of contractual detail. Organizational identity is at stake in Chapter 2, where the effect of repeated interactions between municipalities and a single firm on renegotiation is studied. Finally, Chapter 3 provides evidence both on individual and organizational identity, by providing two tests for the effect of mayor changes on procurement outcomes: in the first test, we consider the effect of mayor deaths, which is relevant only in terms of individual identity, as mayor death does not affect the organizational composition of the municipality. The second test looks at the effects of electoral turnover, which may be analyzed through the lens of organizational identity, as the entire municipal council is renewed. Further identification of organizational identity effects should seek to understand whether electoral turnovers generate bureaucratic turnovers, i.e. upheavals in the nonelected members of municipal administrations. The relationship between political turnovers and bureaucratic turnovers is for instance studied in Akhtari et al. [2022].

To the question "Does identity matter?" our results generally point towards a positive answer.

This does not come as a surprise: Goldberg [1976] already emphasized the fact the most contractual relationships with public authorities involve the long run or some element of repetition. In the case of concessions, such as studied in Chapter 2, this is evident. However, even in the case of standard, short term procurement contracts, which are the main focus of Chapter 3, identity plays a defining role: this is due to the fact that many of those short-term procurement contracts are repeated. Municipalities tend to face similar needs over time such that constructions, maintenance of infrastructures, materials and services are rarely purchased once and for all. This appears in our data: despite the fact that we only observe procurement contracts between 2015 and 2023, 35% of them are signed with firms that had already obtained previous contracts within our observation span. In this perspective, the study of nonmarket mechanisms such as relational contracts, reputation and favoritism appear crucial to design efficient procurement policies.

Main results and prospects for future research

Our results provide novel evidence on various outcomes of interest for public contracting, but also raise new questions.

Chapter 1 studies the way identity shapes how contracts are written, and which types of clauses are included. It provides some evidence that relational contracting may reduce reliance on formal enforcement mechanisms such as penalties, and may reduce monitoring costs, overall pointing in the direction of reduced transaction costs. Future works on the matter should seek to relate this evidence of non-contract mechanisms with data on court procedures. One expected effect could be that low rigidity contracts are not meant to rely extensively on court enforcement and are thus less subject to judicial disputes. This would provide complementary evidence on the relational quality of the effect we observe. Additional research avenues include taking into account the heterogeneity of manager changes (e.g. distinguishing national CEO and branch manager), as well as taking into account individual characteristics on managers to more precisely assess the channel through which manager changes affect contract writing. Moreover, a complementary analysis should examine whether bureaucratic changes on the municipality side, especially within the officials in charge of public procurement, have similar effects on relational dynamics to changes in the identity of private company managers.

In Chapter 2, I question how identity affects renegotiation. Renegotiation is one of the main topics of interest in public procurement: because it may undermine the validity of *ex ante* awarding procedures, it is often thought of as a means of achieving opportunism or favoritism. On the other hand, because public contracts are highly rigid, and rely only marginally on informal enforcement, renegotiations of formal contracts are crucial, especially in long term deals such as the concession contracts considered in Chapter 2. Our analysis contributes to the literature on renegotiation in several ways. First, it provides a novel level of precision in the coding of renegotiation outcomes. Second, it uses panel data to control for contract fixed effects and eliminate unobserved heterogeneity between contracts. Finally, it uses new proxies of demand for renegotiation to obtain time varying identification variables in a panel setting. Results seem to indicate that in long standing relationships, the private firm obtains additional leverage to renegotiate the contract. While an intuitive theoretical explanation for this is hold-up, this result is not inconsistent with a relational contradicting approach: these firm-favorable renegotiations may be the counterpart of unobservable performance outcomes realized by the firm. Many avenues in the empirical study of public contract renegotiation remain open. One path for future research could concern the identification of relational contracts *per se*, through proxies for the value of future business between parties, in order to adequately determine the effect of relational contracts on formal contract modifications. Moreover, combining a reflection on renegotiation with the topic of the first chapter, a promising path would be to measure how renegotiation is used to *ex post* adjust contract rigidity. In Chapter 1, I only consider rigidity as it is determined at the time when the contract is signed. However, one could posit that if relational contracts develop, parties may wish to renegotiate the terms of the initial contract to make it more flexible. The dynamic analysis of rigidity remains an uncharted field of empirical research which may provide new evidence concerning the development of relational contracts.

Finally, Chapter 3 sheds light on the role of identity on competition in public procurement calls for tenders. Our results point in the direction that mayor changes associated with electoral turnovers increase the competitiveness of awarding procedure, and reduce preference for local firms. While it is tempting to analyze these results through the lens of favoritism, further evidence must be gathered to properly identify such a phenomenon. The data studied in Chapter 3 provides many opportunities for a future research agenda on the matter. In order to more closely examine the nature of the effects we observe, the identification of

connections between elected officials and firms could prove promising. The French authority for the transparency of public life (HATVP) publishes individual level data on the revenues and wealth of mayors, allowing to obtain information on stocks detained by mayors. These data can thus be used to identify municipality-firm links through financial participations. Extensions to our work could also consider procurement contracts awarded by other entities than municipalities: while our study focused on municipal procurement, we have access to data to procurement contracts awarded by other public authorities (*départements*, regions, and national procurement).

Résumé en français (version longue)

Introduction

Les contrats conclus entre des administrations publiques et des entreprises privées constituent un sujet essentiel pour la recherche économique contemporaine. La commande publique représente 13% du PIB de l'OCDE [OECD, 2023], et 8% du PIB Français. Elle constitue donc un pan important de l'économie, et en particulier de la dépense publique. Par ailleurs, la commande publique est à l'origine de la production de services publics, soit de manière directe (lorsque l'exploitation d'un service public est déléguée contractuellement à une administration), soit de manière indirecte (en fournissant aux administrations publiques les biens et services nécessaires à la production de services publics). La production de services publics, souvent difficile à quantifier, est progressivement amenée à être prise en compte dans les mesures de la richesse et de la pauvreté des nations [Gethin, 2024b,a]. Au titre de son poids économique, et de sa contribution à la production de services publics, la commande publique et les conditions de son efficacité doivent donc être analysés par la recherche économique.

La recherche sur la commande publique est aujourd'hui un domaine riche de la littérature à la fois empirique et théorique. Ses racines se trouvent dans l'analyse de la régulation des industries en réseau (ferroviaire, télécommunications, électricité) et des relations entre l'administration publique et les entreprises chargées de la gestion de ces réseaux. Les travaux fondateurs ont mis en évidence la nécessité d'attribuer ces monopoles de manière temporaire via des mécanismes concurrentiels [Demsetz, 1968], ainsi que sur les questions d'incitations résultant de l'asymétrie d'information entre régulateur et régulé [McCall, 1970]. Ce courant de recherche s'est étendu à l'ensemble des relations d'agence entre une administration publique et une entreprise privée [Laffont and Tirole, 1993].

Un second pan important de la recherche sur la commande est issu de la théorie des coûts de transaction. L'économie des coûts de transaction [Williamson, 1973] s'est traditionnellement focalisée sur la question de « faire ou faire-faire », à savoir celle de l'arbitrage entre intégration verticale et externalisation par le contrat. Cette question a plus tard été étendue au cas des services publics, posant la question de l'arbitrage entre production du service public par l'administration, ou délégation du service public à un contractant. L'arbitrage est généralement présenté dans les termes suivants : l'externalisation permet de bénéficier de la productivité accrue du contractant, qui bénéficie d'une expertise et d'économies d'échelles, mais génère des coûts de transaction associés à l'incomplétude du contrat, qui se traduisent généralement par une baisse de la qualité du service public. De manière symétrique, la production par l'administration permet d'internaliser les incitations à produire un service public de haute qualité, mais est plus coûteuse en raison de la moindre efficacité de l'administration [Hart et al., 1997].

Les développements ultérieurs de la théorie des coûts de transaction nous rapprochent du sujet de cette thèse : Dès 1985, Williamson [1985] reconnaît que, plutôt que d'être une dichotomie, la question de « faire ou faire-faire » est celle du placement sur un continuum. En développant la notion de forme hybride, la théorie économique reconnaît alors l'existence d'une série d'arrangements contractuels n'obéissant pas en premier lieu aux lois du marché concurrentiel, mais avant tout à celles d'une relation bilatérale de long terme fondée en grande partie sur l'identité des parties. Parmi ces formes hybrides, les contrats de long terme mais aussi les relations d'approvisionnement répétées, soit autant de situations récurrentes dans la commande publique. En effet, les relations de commande publique sont souvent fondées sur des contrats d'une durée étendue (certaines concessions durent plus de trente ans), ou sur des relations répétées avec un producteur privilégié.

L'objectif de cette thèse est donc d'évaluer le rôle que joue l'identité des parties dans les relations public-privé. Nécessaire à l'établissement de contrats relationnels et à l'établissement de la réputation, éléments cruciaux en présence de contrats incomplets ou d'asymétries d'information, la prise en compte de l'identité renvoie aussi au risque de favoritisme, contre lequel ont été érigées les règles d'attribution des marchés publics. Les conséquences bénéfiques ainsi que les coûts associés au rôle de l'identité dans la commande publique sont l'objet de cette thèse.

L'intuitu personae ou la prise en compte de l'identité dans le droit français des contrats publics

La notion d'*intuitu personae* est utilisée par les juristes francophones pour qualifier une catégorie de contrats pour lesquels, en raison de leur nature ou de leur objet, l'identité des parties revêt une importance particulière. En droit public, il est reconnu de longue date que les contrats de concession (et, par extension l'ensemble des délégations de service public) sont conclus *intuitu personae*. Il résulte de cela un pouvoir historiquement quasi-discrétionnaire des autorités adjudicatrices dans le choix de leur contractants pour cette catégorie de contrats. Par contraste, les marchés publics (qui constituent l'autre grande catégorie de contrats publics) sont soumis à des règles strictes d'attribution.

A la fin XXe siècle, la loi et la jurisprudence ont poussé vers une convergence progressive des régimes juridiques applicables à ces deux catégories de contrats. D'une part, les procédures d'attribution des délégations de service public ont été renforcées, avec notamment la loi "Sapin" de 1993, et sous l'influence du droit de l'Union Européenne. D'autre part, un certain assouplissement du droit applicable aux marchés publics a pu être constaté, avec la création de procédures adaptées et négociées au début des années 2000. Cette convergence est illustrée par la création du Code de la commande publique, réunissant le droit des marchés publics et celui des délégations de service public, entré en vigueur en 2019.

Cette convergence pose la question de savoir ce qui justifie, au fond, la coexistence d'un régime juridique fondé sur l'identité avec celle d'un régime juridique fondé sur la concurrence. La théorie économique permet, en partie, de répondre à cette question, pointant les conditions dans lesquelless les mécanismes concurrentiels sont efficaces, et celles dans lesquelles la liberté de choix du cocontractant par l'administration apparaît préférable. Ces théories sont présentées dans le paragraphe suivant.

Quel rôle pour l'identité dans la commande publique ? Les réponses de la théorie économique

L'(in)efficacité des mécanismes concurrentiels. La théorie économique néoclassique considère traditionnellement des agents anonymes guidés uniquement par le signal prix. Cette théorie se reflete dans l'approche concurrentielle de la commande publique, qui considère que la production de services publics par des agents privés doit être confiée à des opérateurs sélectionnés par le jeu de la concurrence. Ainsi, Demsetz [1968]; Stigler [1968]; Posner [1972] mettent en avant les bienfaits d'une concurrence "pour le marché" via des appels d'offres. L'efficacité des mécanismes concurrentiels d'attribution des marchés publics a néanmoins été contestée, du fait de la présence d'asymétries d'information [Laffont and Tirole, 1987, 1990], mais surtout du fait de l'incomplétude des contrats qui entraîne la possibilité de renégociations *ex post* de contrats attribués de manière concurrentielle [Hart and Moore, 1988; Bajari et al., 2014; Herweg and Schmidt, 2017]. En réponse aux imperfections des mécanismes concurrentiels, certaines théories ont développé les conditions pour que le choix discrétionnaire du cocontractant soit efficace.

La réputation et les contrats relationnels comme substituts aux mécanismes concurrentiels. Sous certaines conditions, le choix discrétionnaire du contractant par l'administration peut être source d'efficacité. La théorie des contrats relationnels, dont les jalons sont posés par Macaulay [1963], a plus tard été traduite en termes microéconomiques [Telser, 1980; Bull, 1987]. Cette théorie reconnait l'efficacité d'interactions répétées entre deux parties en présence de contrats incomplets: les incitations à préserver la relation contractuelle permettent d'éviter les comportements opportunistes, à condition que les deux parties puissent, de manière crédible, s'engager à poursuivre la relation.

Par ailleurs, la théorie des coûts de transaction, initialement centrée sur l'arbitrage entre marché et intégration verticale, a progressivement reconnu l'existence de formes contractuelles hybrides dans lesquelles l'identité des contractants joue un rôle prépondérant [Williamson, 1985]. En effet, en présence d'actifs spécifiques, mais dans les situations où l'intégration verticale représenterait un coût excessif, les parties à un contrat peuvent choisir de s'engager dans une relation de long terme dans laquelle le signal prix n'est plus un critère déterminant.

Dans ces deux cadres théoriques, le choix du partenaire est crucial, et le maintien de la relation avec un unique contractant également. Appliqués à la commande publique, ces cadres théoriques soutiennent l'existence de relations de long terme entre autorité adjudicatrice et entreprise privée, ce qui est rendu impossible lorsque l'exploitation du service public est réattribuée de manière périodique par un appel d'offre. Ces cadres théoriques permettent donc d'éclairer la régime juridique des délégations de service publique, octroyant une grande liberté de choix aux autorités adjudicatrices.

Les favoritisme et les risques liés à l'attribution discrétionnaire des contrats publics. L'un des risques inhérents à la liberté de choix de ses contractants par l'administration est celui du favoritisme. En effet, en admettant que les responsables politiques obéissent avant tout à des intérêts privés [Buchanan and Tullock, 1962], rien ne garantit que la liberté de choix conduise les administrations à sélectionner leurs contractants sur des critères économiques légitimes. En présence de corruption, de conflits d'intérêts et autres connexions privées avec des entreprises, des dérives dans l'attribution des contrats publics sont possibles et pénalisent la production de biens publics ainsi que l'utilisation efficiente des deniers publics.

Le rôle dans l'identité des contrats publics : état de l'art empirique

Cette section vise à résumer l'état de la recherche empirique sur le rôle de l'identité dans les contrats publics.

Contrats relationnels et réputation. De nombreuses études on mis en évidence l'existence de contrats informels régissant les relations privées [Macchiavello and Morjaria, 2022]. Ceux-ci reposent sur la théorie des contrats relationnels, selon laquelle des clauses non vérifiables par une tierce partie peuvent être appliquées par les parties, qui tirent plus de bénéfice à la coopération et à la préservation d'une relation de long terme qu'à l'opportunisme de court terme. En matière de contrats publics, diverses études ont identifié empiriquement des effets relationnels dans la commande publique [Gil and Marion, 2013; Corts and Singh, 2004; Beuve and Saussier, 2021; Desrieux et al., 2013]. Néanmoins, ces études ne mettent pas en évidence l'existence de contrats informels à proprement parler.

Règles *versus* discrétion: le rôle de l'identité dans l'attribution des contrats publics.

Un pan important de la littérature empirique a cherché a identifier dans quelle mesure les procédures strictes d'attribution des contrats publics amélioraient ou déterioraien la qualité de la commande publique en comparaison avec des procédures laissant plus de liberté de choix à l'autorité adjudicatrice. Certaines études montrent que la liberté de choix a des effets positifs [Coviello et al., 2018a; Bafundi et al., 2023]. D'autres études mettent en avant la manipulation volontaire de la valeur des contrats publics afin de bénéficier de procédures simplifiées, parfois aux fins de favoritisme [Baltrunaite et al., 2020; Szucs, 2023; Celis Galvez et al., 2025].

La mise en évidence du favoritisme. Fortement liée à la précédente, une branche de la littérature empirique cherche à mettre en évidence l'ampleur et les effets du favoritisme dans l'attribution des marchés publics. Certaines études se sont appuyées sur la mise en place de procédures simplifiées pour mettre en évidence le fait que les procédures simplifiées étaient utilisées de manière démesurée par des politiciens possédant des liens personnels avec les entreprises participant aux marchés publics [Szucs, 2023]. De manière générale, les études sur le favoritisme tâchent d'identifier des liens personnels entre responsables politiques et entreprises, ou entre dirigeants d'entreprises et partis politiques, et mettent en évidence les effets délétères de ces liens sur la conduite des appels d'offre dans la commande publique [Titl and Geys, 2019; Titl et al., 2024].

Questions ouvertes pour la recherche empirique

Des questions demeurent ouverte pour la recherche empirique. Celles-ci sont traitées au cours de la thèse, parfois de manière transversale.

Le rôle de l'informalité dans les contrats publics. La commande publique est de manière générale hostile à l'informalité, expliquant la présence de contrats particulièrement rigides. Néanmoins, le rôle des contrats relationnels dans l'établissements de contrats publics plus ou moins rigides reste à établir : puisqu'il est admis qu'une marge de manoeuvre existe [Beuve et al., 2019, 2021], le choix d'un niveau de rigidité contractuelle peut être influencé par l'établissement d'une relation de confiance entre les parties. La question demeure donc ouverte de déterminer si l'existence de contrats relationnels permettrait de réduire le formalisme des contrats publics.

Le rôle des individus dans les relations inter-organisationnelles La théorie des contrats relationnels présente généralement un rapport de confiance "calculée" entre deux organisations. Néanmoins, les changements dans la direction des organisations peuvent représenter des perturbations dans ces relations. Cette question est traitée en premier lieu dans le premier chapitre, où nous estimons l'effet des changements de dirigeants au sein d'un opérateur privé sur les caractéristiques de contrats publics. La question de l'identité individuelle est également traitée de manière subsidiaire dans le troisième chapitre, où nous estimons l'effet causal de changements dans l'identité des maires sur la conduite des appels d'offre.

Le rôle de l'expérience passée dans les contrats publics. La répétition d'une relation contractuelle public-privé dans le temps demeure un phénomène ambigu au plan normatif. Tandis qu'elle peut être associée au développement de contrats relationnels, et donc à une efficacité accrue, elle est aussi associée à la théorie du *hold-up* et à des relations de dépendances bilatérales caractérisées par des comportements opportunistes. Le Chapitre 2 analys cette question sous le prisme de la renégociation, en mettant en évidence l'effet d'interactions répétées sur les tentatives d'appropriation de rentes via les modifications *ex post* du contrat.

La suite de ce résumé est composé d'une synthèse, chapitre par chapitre, des trois études réalisées au cours de cette thèse.

Chapitre 1. Identité des dirigeants, contracts relationnels et rigidité contractuelle

Cadre théorique et motivation

Ce chapitre s'interroge sur le rôle de l'informalité dans les contrats publics et sur les effets de changements au sein de l'équipe dirigeante. Bien que certaines études aient mis en évidence des effets relationnels dans la commande publique [Corts and Singh, 2004; Gil and Marion, 2009; Desrieux et al., 2013], l'existence de mécanismes informels d'adaptation reste incertaine. Elle est *a priori* fortement limitée par des facteurs institutionnels, en particulier liés à l'opportunisme de tierces parties [Spiller, 2009; Moszoro et al., 2016; Beuve et al., 2019,

2021].

Ce chapitre vise à étudier la question du contrat informel de manière négative, en se demandant si des facteurs perturbant un contrat relationnel se traduisent par une rigidité contractuelle accrue. En effet, il peut être supposé qu'en la présence d'un contrat relationnel, le recours à des clauses écrites, coûteux (notamment termes de coûts *ex post* de renégociation) soit limité au strict nécessaire. Des modèles tels que Kvaloy and Olsen [2009] considèrent le lien entre contrat relationnel et investissement dans le contrat formel. Si un facteur exogène affaiblit la capacité à reposer sur un contrat relationnel, nous faisons l'hypothèse que la rigidité du contrat formel sera accrue en retour.

La seconde étape du cadre théorique consiste donc à identifier un élément perturbateur dans les contrats relationnels. Nous faisons l'hypothèse que les changements au sein de l'équipe dirigeante du cocontractant privé peuvent représenter une perturbation dans un contrat relationnel, réduisant la confiance qu'a en lui l'acheteur public, et par conséquent incitant les acheteurs publics à exiger des garanties sous la forme de contrats fortement rigides. Notre proposition principale est donc que les contrats signés avec des dirigeants non connus de l'acheteur public sont en moyenne plus rigides.

Dans un second temps, nous testons si des facteurs supposés affecter la faisabilité de contrats relationnels jouent un rôle médiateur dans la relation entre identité des dirigeants et rigidité. En premier lieu, le type de contrat joue un rôle crucial : les délégations de service public, caractéristées par le long terme et l'incomplétude, sont beaucoup plus à même de voir se développer des contrats relationnels que des marchés publics. Nous anticipons donc que l'effet des changements de dirigeant soit plus fort sur ces contrats. En second lieu, il a été souligné par la théorie de l'opportunisme des tiers que les contrats relationnels dans la commande publique étaient fortement restreints en présence de tierces parties cherchant à déstabiliser le contrat. Nous utilisons des variables liées à la politique locale pour approximer le risque d'opportunisme des tiers, qui devrait réduire l'effet des changements de dirigieants sur la rigidité.

Données et stratégie empirique

Nous testons ce cadre théorique sur une base de données de 415 contrats publics issus du secteur du stationnement français, signés entre un opérateur et une série de municipalités. Nous reconstituons un historique des dirigeants nationaux et régionaux de l'entreprise à partir des signatures trouvées sur les contrats. Nous créons une variable catégorielle qui sera la variable explicative principale. Celle-ci prend 3 valeurs : "premier contrat" si le contrat est le premier de l'historique entre la municipalité et l'entreprise, "dirigeant connu" si l'un des dirigeants a déjà contracté avec la municipalité, "dirigeant inconnu" si aucun des dirigeants n'a contracté avec la municipalité. Afin de mesurer la rigidité des contrats, nous construisons un nouveau score de rigidité dérivé de la méthode TF-IDF, permettant de détecter les termes saillants caractérisant particulièrement certains contrats par rapport à d'autres. Plus spécifiquement, nous construisons 10 scores de rigidité associés à des catégories particulières de clauses (révisions du contrat, faute du contractant, entretien de l'infrastructure, litiges, pénalités, autorisations et permis, qualité, retards, état de l'infrastructure, renseignements et statistiques). Nous agrégeons ensuite ces scores individuels en un score de rigidité synthétique. Afin de tester notre dernière proposition, nous utilisons deux variables pour approcher le risque d'opportunisme des tierces parties : le nombre effectif de partis politiques, tel que défini par Beuve et al. [2019], et une variable binaire indiquant si le contrat a été signé dans les 12 mois précédant une élection municipale.

Notre stratégie empirique repose sur un modèle linéaire simple, où nous contrôlons pour une série de caractéristiques du contrats et de la municipalité, ainsi que des effets fixes par demi-décennie.

Résultats

Nos résultats indiquent que les contrats signés avec des dirigeants inconnus tendent à inclure un suivi plus strict du contractant, et contiennent plus fréquemment des clauses prévoyant pénalités. Ces résultats sont cohérents avec notre cadre théorique. Néanmoins, notre variable explicative n'affecte pas significativement le score de rigidité global. Comme anticipé, l'effet des changements de dirigeants sur la rigidité est principalement le fait des délégations de

services public, qui sont le type de contrat dans lequel des effets relationnels peuvent être attendus. En revanche, nous ne trouvons pas d'effet des variables politiques sur la relation entre identité des dirigeants et rigidité.

Chapitre 2. Pouvoir de négociation, renégociation et interactions répétées: une analyse empirique des contrats publics de long terme

Motivation et cadre théorique

Les renégociations sont l'une des préoccupations majeures dans l'étude des contrats publics. Souvent accusée de mettre à mal *ex post* les procédures concurrentielles, elles peuvent être l'outil de comportements opportunistes et de favoritisme. Néanmoins, les contrats publics étant caractérisés par une rigidité importante [Moszoro et al., 2016; Beuve et al., 2019], des modifications formelles sont nécessaires à l'ajustement de ces contrats [Beuve et al., 2021].

Cet article questionne comment le développement d'une relation de long-terme entre un acheteur public et une entreprise privée affecte le processus de renégociation. La répétition d'une relation contractuelle entre deux parties peut être appréhendée par deux cadres théoriques principaux. La théorie des coûts de transaction (TCT) avance que le développement d'une relation de long terme bilatérale peut-être le signe d'une situation de *hold-up* liée à la présence d'opportunisme, et dans laquelle les parties tentent d'extraire les quasi-rentes appropriables via les renégociations. Dans ce contexte théorique, les renégociations sont avant tout opportunistes. Par contraste, la théorie des contrats relationnels soutient que des interactions répétées sont nécessaires pour générer des incitations à coopérer sur des aspects non contractualisables de la relation. Les renégociations opportunistes font partie de ces aspects: il est complexe, voire impossible de s'engager *ex ante* à ne pas renégocier le contrat. Par conséquent, une approche des interactions répétées par la théorie des contrats relationnels voudrait que les parties évitent les renégociations opportunistes afin de préserver le maintien de cette relation. L'existence d'un stock important de contrats par le passé serait alors le signe

de l'existence d'un contrat relationnel. Nous formons donc deux catégories d'hypothèses reflétant ces deux cadres théoriques. La première série d'hypothèses est inspirée de la théorie des coûts de transaction, et suppose que l'une des deux parties accroisse son pouvoir de renégociation à mesure que la relation se prolonge, l'amenant à obtenir plus de renégociations en sa faveur. En second lieu, nous formulons une hypothèse basée sur la théorie des contrats relationnels : si la relation prolongée est le signe de l'existence d'un contrat relationnel, alors elle réduit les incitations qu'ont les deux parties à tenter des renégociations opportunistes.

Données

Notre base de données est composée de 281 contrats de délégation de service public (DSP) issus du secteur du stationnement, signés entre le premier opérateur français et une série de municipalités françaises. Nous transformons la base de données de manière à avoir un panel à l'échelle annuelle. Les données sur les renégociations ont été obtenues en lisant individuellement l'ensemble des avenants et en codant les résultats de ces renégociations en fonction de leurs conséquences sur la vie du contrat. Ces conséquences peuvent concerner les prix, les redevances, des investissements ou travaux supplémentaires, l'extension de parcs de stationnement, la prolongation du contrat. D'autres avenants, n'ayant pas de conséquences substantielles sur le contrat, sont classés dans une catégorie "mineur". A partir de ces données, nous estimons comment le stock d'expérience passée affecte les renégociations.

Obtenir un estimateur fiable de l'effet de l'expérience passée est complexe puisque nous contrôlons l'hétérogénéité entre contrats à l'aide d'effets fixes par contrat. Afin de rendre ces hypothèses opérationnelles, nous estimons donc l'effet de l'interaction entre l'expérience passée et des *proxys* pour la demande de renégociation du côté de l'opérateur et du côté de l'acheteur. Du côté de l'opérateur, nous tirons profit de la structure multi-contrat des données, à savoir que les parties signent parfois plusieurs contrats concomittants. Nous faisons l'hypothèse que le contractant est plus susceptible de tenter des renégociations opportunistes après s'être vu attribuer un nouveau contrat. Par conséquent, nous codons une variable indiquant si le contractant s'est vu attribuer un nouveau contrat au cours des deux dernières années, et utilisons cette variable comme proxy pour la demande de renégociation du côté de l'opérateur. Du côté des municipalités, nous faisons l'hypothèse que ces dernières sont plus enclines à deman-

der des renégociations en situation de tension financière. Par conséquent, nous utilisons les variations de la dette municipale comme *proxy* pour la demande de renégociation du côté de la municipalité.

Résultats de l'analyse économétrique

Une première série des tests économétriques vise à estimer l'effet des *proxys* pour la demande de renégociation sur la probabilité de renégociation. Nous utilisons un modèle *two way fixed effects*, contrôlant pour des effets fixes au niveau du contrat et du temps. Nous introduisons également des variables de renégociation différées d'un an en tant que variable de contrôle. En premier lieu, nos résultats montrent que l'opérateur est plus à même d'obtenir des renégociations favorables après s'être vu attribuer un nouveau contrat, conformément à nos prédictions. De même, nos résultats montrent que la probabilité de renégociation des conditions financières du contrat est plus importante lorsque la dette de la municipalité a fortement augmenté.

Dans une seconde série de tests, nous étudions l'effet de l'interaction de ces *proxies* avec l'expérience passée entre les parties, mesurée en années. Nos résultats montrent que la capacité de l'opérateur à obtenir des renégociations favorables augmente à mesure que la relation se prolonge. A l'inverse, les renégociations financières en réaction aux augmentations de la dette municipales sont de moins en moins probables. Ces résultats semblent globalement indiquer que le pouvoir de négociation de l'opérateur augmente à mesure que la relation contractuelle se prolonge.

En conclusion, ces résultats semblent plutôt aller dans le sens d'une interprétation fondée sur la théorie des coûts de transaction, dans laquelle l'opérateur privé accroît son pouvoir de négociation. Néanmoins, ces résultats peuvent aussi être interprétés à l'aune de la théorie des contrats relationnels : il est possible que les renégociations favorables obtenues par le contractant soient la contrepartie de réalisations inobservables. Ce type de mécanisme est décrit dans des modèles tels que Watson et al. [2020] et Kostadinov [2021].

Changements de maire et concurrence : une étude des appels d'offre dans la commande municipale française (2015-2023)

Cet article, coécrit avec Adrien Deschamps (Université d'Avignon) vise à étudier si les changements d'identité des maires affectent les caractéristiques des appels d'offre de commande publique. L'hypothèse principale est que les maires installés ont eu l'occasion de développer des relations privilégiées avec certains fournisseurs, réduisant la concurrence et augmentant le recours à des entreprises locales. En conséquence, les changements de maire devraient accroître la concurrence et réduire la prévalence d'entreprises locales parmi les vainqueurs de marchés publics. Nous utilisons une base de données répertoriant des données sur les appels d'offre publiés par les municipalités françaises entre 2015 et 2023. Notre stratégie repose sur deux quasi-expériences : dans un premier temps, nous étudions les effets des décès de maire sur la concurrence dans les appels d'offre subséquent. Dans un second temps, nous utilisons les résultats des élections municipales et utilisons les élections municipales fortement disputées pour obtenir une source quasi-aléatoire de variation dans l'identité des maires.

Données

La base de donnée que nous utilisons pour les informations relatives aux appels d'offres est la base $BeauAMP$ [Deschamps and Potin, 2025]. Cette base, exhaustive pour les appels d'offre publiés entre 2015 et 2023, contient des données sur la nature du bien/service concerné par l'appel d'offre, le nombre de répondants, le prix final, l'identité du vainqueur et son origine géographique. Pour la première partie de l'analyse empirique, nous utilisons également la base BREF [Labatut et al., 2020] qui contient des informations exhaustives sur les décès de maires survenus entre 2015 et 2020. Dans la seconde partie, nous utilisons des données de l'INSEE et issues du répertoire national des élus (RNE) afin de contrôler les caractéristiques des municipalités et des maires. Nous avons également collecté des données sur la durée totale des mandats municipaux en utilisant l'API de Wikipédia.

Décès de maires et concurrence : résultats d'un *diff-in-diff*

Afin d'estimer l'effet causal du décès des maires, nous estimons un modèle en différences de différences, avec pour traitement le décès du maire, et comme groupe de contrôle, les municpalités dans lesquelles les maires ne sont pas décédés. Nous utilisons un ensemble de variables dépendantes destinées à mesurer l'intensité de la concurrence (nombre de répondants à l'appel d'offre, prix final) et la nature de la concurrence (origine géographique du vainqueur, existence de contracts passés entre la municipalité et le vainqueur). En plus du modèle simple, nous tenons compte de l'effet hétérogène du traitement, qui est un problème prégnant dans le cas de traitements différés [Goodman-Bacon, 2018; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Borusyak et al., 2024]. Pour ce faire, nous estimons des effets de traitements spécifiques aux "cohortes" de traitements (une cohorte étant définie de manière artificielle comme l'ensemble des municipalités où le maire est décédé pour une année t), puis nous estimons des effets de traitements spécifiques aux communes qui ont un nombre substantiel d'observations pré et post traitement. De manière générale, nos résultats ne permettent pas de conclure de manière univoque à un effet pro-concurrentiel du décès des maires. Cela est probablement dû au petit nombre de maires décédés sur la période d'observation, et à la petite taille des villes concernées par ces décès.

Elections municipales disputées et concurrence : résultats d'une régression sur discontinuité

Dans cette section, nous utilisons les résultats des élections municipales de 2014 et 2020 comme source de variation dans l'identité des maires. Notre stratégie empirique repose sur le fait que lorsque l'élection est fortement disputée, le fait que le maire sortant reste élu ou soit évincé est quasi-aléatoire. Nous comparons donc les villes pour lesquelles les marges de victoire au second tour ont été particulièrement réduites.

Sur le plan économétrique, nous utilisons deux méthodes pour mesurer l'effet de cette discontinuité. En premier lieu, nous utilisons une méthode paramétrique. Celle-ci consiste à utiliser l'ensemble de l'échantillon, mais a contrôler pour l'effet "continu" de la marge de victoire en contrôlant pour un polynôme de la marge de victoire. En second lieu, nous util-

isont l'estimateur non paramétrique développée par Calonico et al. [2014, 2019]. La méthode non paramétrique repose sur des régressions locales dans un sous échantillon de villes situées proches du point de discontinuité (le seuil de 50% en faveur du maire sortant dans notre cas). Nous conduisons également un grand nombre de tests de robustesse. Nos résultats soutiennent de manière stable que, dans les municipalités où le maire est nouvellement élus, le nombre de répondants aux appels d'offre est plus important, et que la proportion d'entreprises locales parmi les vainqueurs est moindre.

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List of Figures

List of Tables