

*Lucas Braga de Melo and Valdemar
Pinho Neto*
**The Economic Returns of Firms'
Political Connections**

Aboa Centre for Economics

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ABSTRACT

This paper analyzes the economic returns to political connections in Brazilian local elections, focusing not only on traditional campaign donations but also on two novel channels: firms that provide goods or services to candidates during campaigns and firms' owners affiliated with parties within a coalition running for mayor. Employing regression discontinuity and event study methods around close mayoral races, we find that politically connected firms substantially increase both their likelihood of securing procurement contracts and the value of those contracts, though without corresponding gains in employment or wages. This paper contributes to the literature on political connections by documenting the emergence of indirect political connections and public procurement allocation in a context of weak institutional constraints.

JEL Classification: D72, H72, D73

Keywords: political connections, procurement, firms

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I Introduction

Understanding the returns to private firms’ political connections is essential for assessing the integrity of democratic institutions and the efficiency of public resource allocation. A growing body of research shows that firms with political ties often benefit from preferential treatment—such as regulatory leniency, favorable policy outcomes, or access to public contracts. This paper focuses on political connections and public procurement, which comprise an important share of total government revenues: 14.2% in the US and 20% in Brazil (U.S. Government Accountability Office, 2023; Dahis et al., 2023). Political connections are frequently associated with some sort of gains for firms, raising concerns about the impact of firms’ political connections on public procurement and administrative impartiality (Fazekas et al., 2025; Akcigit et al., 2023; Katovich and Moffette, 2024; Arvate et al., 2016; Brollo et al., 2017; Baltrunaite, 2020; Boas et al., 2014; Titl and Geys, 2019).

Political connections might emerge well beyond traditional lobbying (Kang, 2016; Bertrand et al., 2014; Cohen and Malloy, 2014; Vidal et al., 2012), PAC (Teso, 2023), campaign donations (Ruiz, 2017; Battaglini and Patacchini, 2018; Cagé et al., 2021), and employees becoming politicians’ (and vice versa) (Acemoglu et al., 2016; Akcigit et al., 2023). The literature on how alternative and indirect firms’ political connections is scarce and still emerging, with a few exceptions. For example, Bertrand et al. (2020) measures indirect political connections through corporate charity donations. Alternative and indirect political connections of firms are difficult to measure due to data availability. Measuring them helps us further understand how they can distort procurement allocation and how to make these firm-politician connections more transparent to voters. We fill this gap by studying traditional campaign donations and adding two new types of connections that, to our knowledge, have never been studied before. Our second and novel connection type consists of firms that sell goods or services to candidates’ campaigns, such as printing flyers, organizing events, or merely selling fuel. The candidates pay for these services or goods with money from their own pockets, derived from their campaign revenues. These can include candidates’ own resources, individual donations, firm donations, public funds, and others. Our third connection type consists of firms whose owners are affiliated with a party (the firm owner is a member of the party) in one of the

top two mayoral candidate coalitions. So, to identify this connection, at least one of the firm owners must be affiliated with one of the parties composing the coalition of one of the two most voted candidates.

We investigate how these direct and indirect political connections shape procurement allocations in a setting with weak institutions. Do firms that are politically connected to a winning candidate have a higher probability of obtaining a contract? Are their contracts larger? Is there a significant reallocation of the municipal budget in this procurement allocation process? What are the impacts of larger and more contracts on firms' employment levels and employees' wages? This paper examines the extent to which political connections—via campaign donations, services/goods provided to candidates during electoral campaigns, and firm owners affiliations with a party in the winning coalition—translate into tangible advantages in local public procurement in Brazil.

Our analysis consists of three different political connections between firms and candidates: donor firms, service/good providers, and coalition affiliated firms. In the first type, donations are the link between candidates and firms. Firms donating to candidates campaigns can be part of a quid-pro-quo transaction or a "tax" on the firm so it can obtain procurements. In the second type, firms supplying services or goods to candidate campaigns form a connection; it may be the case that firms charge less for their supply to the candidates, expecting procurement gains in the future. Additionally, it may be the case that candidates acquire information about these firms while hiring them for their campaigns, and they may decide to hire them after being elected. Last but not least, coalition affiliated firms whose owners are affiliated with a party are very likely to be in the same social circle as the candidate and more likely to be known by the candidate. In this paper, we focus on the procurement reallocation process through these connections and briefly tackle the mechanisms behind it.

Leveraging a regression discontinuity design on close mayoral elections, we provide causal evidence that firms connected to winning candidates experience substantial procurement gains compared to the counterfactual case in which their connections had lost the election. Donating to a marginally winning candidate leads to a 37% increase in contract opportunities relative to the counterfactual case where their connected candidate lost. Providing services

or goods during the campaign to the winning candidate increases a firm’s probability of securing procurement by 38%. Finally, firms whose owners were affiliated with a party in the coalition of the winning candidate’s party experience a 20% increase in the probability of having procurements.

Similar patterns emerge when measuring procurement revenues as a share of the municipal budget: the intensive margin. Donor firms experience 72% larger contracts when their connection wins the election compared to the case in which they had lost the election. Service provider firms have 39% larger contracts in the same situation. Firms whose owners are affiliated with the winning coalition have 40% larger contracts than when they are affiliated with a marginally losing coalition. Importantly, the outcome for the intensive margin is defined in terms of the total municipal budget; thus, the budget is being reallocated through these connections.

To further explore the dynamics of procurement gains over time, we implement an event study analysis using firm-level data surrounding one municipal election cycle. This approach enables us to trace the evolution of procurement outcomes before and after closely contested races. The results reveal no significant pre-treatment trends, thereby strengthening our identification strategy. Following an electoral win, the probability of winning a procurement contract increases sharply for connected firms, peaking two to three years post-election and gradually declining as the next election cycle approaches. These dynamic effects underscore the temporal nature of connections’ gains and highlight how political connections translate into tangible economic returns over the electoral cycle.

We present evidence that the probability of winning a contract is higher if service/goods providers and coalition affiliated firms operate in relatively more corrupt sectors; however, this probability is the same for donor firms. Further, for service provider firms, the gains in procurement opportunities seem to be greater in more corrupt sectors; however, the magnitude of these gains’ seems to be the same for donor- and coalition affiliated firms when comparing them to more or less corrupt sectors.

While we document substantial procurement gains for politically connected firms, we find no evidence that these benefits translate into broader firm-level growth in employment or

wage bills. We examine whether firms linked to marginally winning candidates expand their workforce following the election. Our figures present clear null treatment effects on firms' employment and wage bills. This suggests that the gains in these procurement allocation processes through political connections may not be reinvested in productive capacity or job creation, but rather reflect private transfers to firm owners and/or mayors or other investments by the firms that are not on labor force.

A key question concerns the mechanisms underlying the returns to political connections. Are firms donating to obtain larger and more contracts in a quid-pro-quo manner? Do they obtain larger profits by doing so? Or are they making zero profit because donations are an "entry tax" to the procurement process? Do service/goods providers charge candidates less in expectation of larger procurement contracts?

We find no evidence that donating is positively correlated with larger contracts when exploiting variation from the same firm donating to multiple municipalities and having procurements from multiple municipalities. This result provides suggestive evidence that donations are not functioning as a tax on firms to obtain procurements. It might be the case that firms donate more where their bargaining power is low. On the other hand, we find a positive correlation between the value of services or goods provided and the size of contracts. Together with the result that service and goods firms gain greater benefits in more corrupt sectors, these results serve as suggestive evidence of a quid-pro-quo mechanism. However, we cannot rule out the possibility that candidates acquire information about these firms during the campaign and efficiently hire them. Finally, having the status of coalition affiliated firms is highly correlated with larger contracts; these firms have contracts six times larger than those of firms whose owners are not affiliated with a coalition.

A key strength of this study lies in the construction of a novel and comprehensive dataset that links local firm-level procurement records with campaign finance data at the transaction level from firms and candidates. We combine three administrative sources from Brazil: (i) public procurement data from the open source MiDeS (Dahis et al., 2023), (ii) detailed electoral campaign finance and party affiliation disclosures from the Electoral Court (TSE), and (iii) employee-employer-level characteristics from the Annual Social Information Report

(RAIS). We also complement this merged data with Colonnelli and Prem (2022); Corbi et al. (2019) data on corruption and municipal characteristics. Our merged dataset allows us to track firms over time, identifying their donations, their supply of services and goods to candidates' campaigns, the firm's owners party affiliations, and their subsequent involvement in public contracting. One setback of our data is that we cannot identify different types of tenders, nor can we identify the firms that participated in the tenders and did not win. We observe only whether firms obtained procurements, the municipality that acquired the services or goods through procurement, and the contract value. This is still a rich dataset that allows us to estimate the treatment effects of a firm's political connections in winning an election on this same firm procurement opportunities, the size of the contract and the size of connected firms.

This study contributes to two main strands of the political economy literature, offering both complementary insights and novel contributions. First, we add to the literature on politically connected firms by showing that not only direct campaign donations, but also indirect ties—such as the provision of goods or services to candidates' campaigns and firm owners' party affiliation—translate into preferential access to public procurement. While previous work has documented the economic advantages of connections through donations (Boas et al., 2014; Arvate et al., 2016; Battaglini and Patacchini, 2018), employees becoming politicians (Acemoglu et al., 2016; Akcigit et al., 2023), corporate charity donations (Bertrand et al., 2020), and lobbying (Kang, 2016; Bertrand et al., 2014), our study introduces two novel and quantifiable forms of connections, illustrating new forms of connections that translate into economic advantages.

Second, we contribute to the literature on procurement reallocation by providing causal evidence of how political connections to winning candidates affect both the extensive and intensive margins of contract allocation in local governments. The focus on mayoral elections contrasts with most studies that concentrate on centralized or national procurement systems (Fazekas et al., 2025; Coviello and Mariniello, 2014; Brugués et al., 2024; Boas et al., 2014; Ruiz, 2017). Furthermore, our data allow us to identify connected firm-candidate pairs and firms subsequent procurements in the same municipality where the candidate has run.

Finally, we corroborate Fazekas et al. (2025) results in which firms do not grow by having preferential procurement in uncompetitive tenders. We also show that firm-level gains do not translate into job creation or greater wage bills. This result is important because we use two different types of connections in a quite different setting.

The remainder of the paper is organized as follows: Section II outlines the institutional setting of Brazilian local elections and municipal budgets, including key reforms introduced in 2015. Section III describes the data sources and presents descriptive statistics. Section IV presents the identification strategy; Section V presents the main results; Section VI presents the mechanisms. Finally, Section VII makes final remarks.

II Institutional Background

Local Politics and Local Procurement

In Brazil, local elections occur every four years, and candidates are required to be affiliated with a political party to stand for office. These elections determine both the municipal mayor and the members of the local council. In nearly all municipalities (98%), where there are fewer than 200,000 registered voters, mayors are elected by simple plurality. However, in municipalities with 200,000 or more registered voters, a candidate must secure at least 50% of the vote to win; otherwise, a runoff election is held. Mayors are subject to a two-term limit.

One of our analyzed political connections consists of firms whose owner(s) are affiliated with a party that comprises a candidate's coalition. In Brazil, any voter can voluntarily affiliate with a party, but it is mandatory for anyone intending to run for elected office. Each party sets its own membership rules and statutes: some require registration fees and monthly dues, while others allow for straightforward online registration. Party members typically have the right to vote in internal elections to select candidates and may also engage in campaign activities.

Coalition formation is a strategic and often complex process driven by considerations such

as access to campaign resources and electoral viability. Parties frequently form coalitions to enhance their competitiveness by pooling time on free electoral broadcasts (*horário eleitoral gratuito*), sharing campaign infrastructure, and signaling broader support to voters. Mayoral coalitions are often shaped by local interests, personal alliances, and negotiations over future access to municipal resources and appointments. These coalitions are formalized during the candidate registration period and can include parties with divergent ideological orientations, reflecting Brazil’s fragmented multiparty system and the decentralized nature of political competition at the municipal level. It is expected that firms whose owner is affiliated with a party within a candidate coalition are more likely to know and to be known by the candidate.

In Brazil, mayors play a central role in the procurement process due to the country’s high degree of fiscal and administrative decentralization. Municipal governments are responsible for a wide range of public services—including education, healthcare, sanitation, and infrastructure—that require extensive procurement of goods and services. The mayor, as the chief executive of the municipality, holds significant discretion over budget allocation and contract execution. Mayors are responsible for structuring the budget and appointing key bureaucrats who oversee the tendering process. These bureaucrats are tasked with defining the terms of reference and technical specifications included in bid documents, which can shape the outcome of procurement decisions. Mayors can also call for emergency tenders as a way to circumvent the formal bidding process.

Public procurement refers to the process by which governments acquire goods and services. Generally, governments are required to conduct competitive tenders when procuring goods and services. The specific procurement method, such as reverse auctions, calls for tenders, or framework agreements, depends on the type and estimated value of the item or service being acquired. A large fraction of expenditures (30-40%) consists of tenders with one bidder, as there is a threshold of 1,600 USD (2023) to call a competitive tender (Dahis et al., 2023). The median municipality total expenditure on public procurement in 2008 and 2012 is approximately 5 million USD. The median number of firms hired is 420 (authors’ data, details in Section III).

One can look into the sectors in which connected firms operate. Most of the firms donating

or providing services/goods to the candidates’ campaigns, which also had local procurements, are in the retail and manufacturing sectors. Firms whose owners are affiliated with the mayoral candidate coalition (coalition affiliated firms) and that also had local procurement are mostly in retail, construction, financial/real estate, and IT services.

On the other hand, we can look at what connected firms are selling to the local government. Among the key procurement items for donor firms there are civil construction, car rental/purchase, and food retail; for service/good provider firms, they include advertising agencies, fuel retail, and event organization services. The main services/goods purchased by the candidates from service providers are flier distribution, advertising, fuel, and brokers’ fees. Meanwhile, for coalition-affiliated firms, most items are food retail and construction inputs.

Mayor Campaign Finance

Before the 2015 ban, corporate donations represented 14% of total mayoral campaign resources. Campaign finance in Brazil is tightly regulated. Candidates must report all contributions and expenditures through dedicated bank accounts monitored by the Electoral Court. Table I summarizes mayoral campaign finance for the 2008 and 2012 elections (adjusted to 2023 USD). Average revenues ranged from \$48,000 to \$57,000, with individual donations around 33%, firm donations near 14%, and the rest from self-funding or party transfers/other sources.

Table I: Mayoral Campaign Finance Summary (in 2023 USD)

Year	Total Revenue	Individuals Donations (%)	Firms Donations (%)	Own Funds (%)	Other (%)	Expenditure
2008	48,399	33.1	13.9	28.8	24.3	19,328
2012	57,131	33.1	13.7	30.8	22.5	22,226

Note: Values are averages per candidate. Total revenue is broken down by contribution source.

Firm donations constituted less than 14% of total revenue in both election cycles.

III Data

We use data that draws from three key sources, providing a comprehensive view of electoral candidates, electoral campaign activities, party affiliation, and employment. First, election results, records of the candidates, voters' party affiliations, candidates' campaign service/goods providers, and donors are obtained from the Tribunal Superior Eleitoral (TSE), Brazil's Superior Electoral Court. This data provides detailed information on individuals who have registered to run for public office. There is information on their electoral results, demographics, and detailed campaign accountability from both the revenue and expenditure sides. The Superior Electoral Court data allow us to identify firms and individuals that donated to candidates. It also allows us to identify firms that provided services to the candidates' campaigns, as well as the types of services or goods provided and the total value that the candidates spent on these firms. Additionally, by combining the Superior Electoral Court data with Receita Federal data, we can identify individuals who are firm owners and are affiliated with a party in a given coalition of a candidate running for mayoral races. From this combination and MiDeS data, we identify those firms in which the firm owner is affiliated with a party and has or has not had procurement contracts with the local council.

In the Brazilian setting, there were 28,693 mayoral candidates for the 2008 and 2012 local elections. The average number of mayoral candidates in a municipality is 3.02 (authors' calculations). In our analysis, we keep the two most voted candidates in the sample and normalize their vote share in order to perform a close race RD, reducing this average to two. We show that our results are robust if we use the non-normalized vote share; Table IB in Online Appendix B reports that our main results do not change significantly by using non-normalized vote shares. In less than 2% of the municipalities, there are runoffs; we consider only the two candidates in the runoff and disregard the first round of elections between two or more candidates in these municipalities. We merge the firms that are donors, good/service providers, and owned by a businessperson affiliated with a party in a coalition running for the top two most voted candidates available in this data set with the procurement data available in MiDeS through their unique tax identification (CNPJ).

Procurement data are derived from an open-source database available through MiDeS, which is described by Dahis et al. (2023). This procurement data includes information on local government contracts and transactions, supporting an examination of procurements at the firm-municipality level. Our dataset on the procurement of Brazilian municipalities is originally sourced from the State Audit Courts (Tribunais de Contas dos Estados, TCEs). These courts are independent institutions affiliated with the state legislative branch that supervise the public finances of the municipalities' executive branch. The caveat of using these data is that they are only available for eight key states in Brazil. Only the states in which the State Accountability Court (Tribunal de Contas do Estado - TCE) publishes their municipalities' accounts are available. In the following, we refer to them as MiDeS data. Most of our analysis restricts the sample to three years after the elections of 2008 (2009, 2010, and 2011) and 2012 (2013, 2014, and 2015).

Figure 1A in Online Appendix A shows procurement data availability by state. In 2008, we had data for six out of 27 states: Ceará, Paraíba, Rio de Janeiro, São Paulo, Rio Grande do Sul, and the Distrito Federal. Our sampled states' GDP corresponds to 58.9% of the total Brazilian GDP and 43.3% of the total population in 2010; it also includes 1,529 municipalities out of 5,570. In 2012, the sample added Paraná and Minas Gerais, combining the GDP of these eight key states, which account for 71.8% of the total Brazilian GDP in 2012 and 59.1% of the Brazilian population. The 2012 data cover 2,791 municipalities out of 5,570. Combined, these eight states have a GDP equivalent to that of Canada and a GDP per capita equivalent to that of Chile in 2012.

Table IA compares the characteristics of municipalities across two samples: municipalities not included in our procurement data sample and municipalities included in the MiDeS data. On average, municipalities in the procurement sample are slightly wealthier and more populous, indicating systematic differences between the groups. This disparity likely arises from the greater availability of municipal procurement data in more developed states due to stronger accountability institutions, such as the state accountability courts (TCE).

In order to uncover mechanisms and perform balance tests at the firm level, we use *Relações Anuais de Informações Sociais* (RAIS). RAIS allows us to infer firms' total employ-

ment, wage bill, and other characteristics, such as the shares of employees that are white, female, and college educated. Another complementary source is the Corbi et al. (2019) dataset containing municipalities' public budget data to apply balance tests in municipalities' financial characteristics. Finally, we use Colonnelli and Prem (2022) to infer corruption at the firms' sector level to tackle a possible mechanism.

To analyze firm-level outcomes, we merge the TSE and MiDeS datasets, constructing three different samples. The first sample focuses on firms that donated during the 2008 or 2012 mayoral elections (Electoral Court data) merged with their procurement revenues in the MiDeS data; we denote this as the donor firms sample. If a firm in the Electoral Court data is not matched with the MiDeS data, we assume that their procurement revenue is zero. The second sample also merges the Electoral Court data and MiDeS; however, in the Electoral Court data, we select firms that provided services or goods for candidates during their campaigns and merge them with their corresponding CNPJ in the MiDeS data to infer their procurement contract value; we denote this as the service/good provider firms sample. Finally, the third sample merges firms whose owners were affiliated¹ with one of the two coalitions of the top two candidates running for mayor and who also received procurement; we denote this as the coalition affiliated firms sample. Matching is precise, as it relies on firms' unique tax identifier in Brazil, the CNPJ, which is available in both datasets.

Note that we focus on races where at least one firm was connected to one of the top two mayoral candidates and at least one other firm was connected to the other of the top two candidates. In the donor firms sample, we exclude 1.7% of firms that simultaneously donated to both of the two most-voted mayoral candidates in the same municipality and election. In the service/goods provider sample, we exclude 3.2% of firms that simultaneously provided services to the two most-voted candidates. For the coalition affiliated firms sample, 1.3% of the firms had at least one owner affiliated with one party and another owner affiliated with the adversary party; we keep these firms in the analysis since doing so does not change our results. Our samples are conditional on municipalities that had at least two different firms connected to one distinct candidate each. These two candidates should be the top two voted

¹Firm owners signed up to be affiliated in the election years of 2008, 2012, or earlier.

candidates. Therefore, our three samples differ in size from one another depending on the type of connection to which they refer.

The samples are almost mutually exclusive. About 20% of the donor firms are also in the service providers' sample, but less than 2% of the firms in the service providers sample are also in the donor sample. About 6% of the donor firms' sample is in the coalition-affiliated sample, but less than 0.5% of the owners affiliated with a party are in the donor sample. Finally, less than two percent of the firms in the service provider sample are also in the coalition affiliated sample, and less than 0.5% of the coalition affiliated firms are in the service provider sample.

Our main definition of a firm is based on establishments. For example, Tesco supermarkets have many stores; our definition considers a single Tesco store as a firm. We use this definition for the donor and service provider firm samples. In this way, we rule out in our samples establishments operating in multiple municipalities. For the coalition-affiliated firms, we use a more general concept of a firm, defining a firm as a composition of stores. In this example, the brand Tesco as a whole would be a firm. Less than 2% of the firms in the coalition-affiliated firm sample operate in multiple municipalities.

To capture the extensive margin treatment effects, we input zeros for the procurement values of firms that are connected but did not receive procurements. In this way, we are comparing firms that received a procurement with firms that did not receive a procurement, but all were connected. It is not possible to compare firms that participated in tenders and received a procurement with firms that participated and did not receive a procurement because there aren't enough observations for tender' participants. In the intensive margin analysis, we do not input zeros, and we exclude procurements in the top 2 percentile due to their high variance (results are robust to not excluding these outliers).

Table II highlights systematic differences in procurement outcomes across the three samples of interest: donor firms, service providers, and firms whose owners belong to political coalitions. Donor firms stand out with a relatively high probability of winning contracts (35% in all elections), followed by service providers (17%), while coalition affiliated firms are much less likely to obtain contracts (5.4%). Conditional on securing a contract, however,

coalition affiliated firms receive substantially larger amounts, with average contract sizes (the sum of the three years following election contracts) exceeding those of donors and service providers.

Table II: Procurement Outcomes by Firm Type and Election Category

	All Elections			Close Elections (Margin ≤ 5)		
	<i>Donor Firms</i>	<i>Service Providers</i>	<i>Coalition-Affiliated Firms</i>	<i>Donor Firms</i>	<i>Service Providers</i>	<i>Coalition-Affiliated Firms</i>
Number of Firms	6,322	126,473	645,916	1,195	25,856	101,367
Probability of winning procurement	0.35	0.17	0.054	0.33	0.19	0.07
Avg contracts conditional on having procurement (US\$)	274.2k	203.6k	873.6k	138.0k	66.5k	96.3k

Note: The table summarizes procurement statistics from MiDeS-TSE merged data. Each of our three samples is represented in the Table. It is possible to infer that Donor and Service provider firms have high probability of having a contract, whereas coalition affiliated firms have larger sum of contracts (average total procurement value a firm receives over the three post-election years, conditional on having at least one contract). Figures for "Close Elections" reflect races decided by a narrow margin of 5 percentage points.

IV Identification Strategy

A key empirical challenge in identifying the returns to political connections is that firms do not randomly select which candidates to connect with. Additionally, candidates' victories might depend on their connections with firms, raising concerns about reverse causality. There could also be issues with self-selection. For example, politically connected firms might be systematically different from those without connections; in non-close races, firms might also anticipate the winning candidate and support only those more likely to win, regardless of whether their connections' returns are small. These issues make simple OLS estimates

confounded by selection effects.

Our RD framework uses the quasi-random assignment of electoral success in close races to estimate the firms' gains from links to candidates. We use two main outcomes: an indicator variable that takes the value of one when winning one procurement contract or more, and the firms' procurement revenues as a percentage of the total municipal budget. To make causal inference, we compare the average firm connected to a marginally winning candidate versus a marginally losing candidate.

We assume that whether a politically connected firm has its connection with a winning or losing mayoral candidate close to the threshold is random. This randomization creates a natural treatment group—comprising donor, service-provider and coalition affiliated firms linked to winning candidates—and a control group consisting of their counterparts tied to losing candidates. By comparing these groups, we can identify treatment effects between comparable units. Crucially, we assume that the sole systematic difference between the treatment and control groups is the election outcome, which is as good as randomly assigned in tightly contested races. Under this assumption, concerns about unobserved variables, simultaneity bias, and reverse causality are attenuated.

We next provide evidence corroborating that this assumption seems to hold for a range of observed characteristics of firms, candidates, and municipalities. To carry out our balancing test, we run RD estimates using the estimator developed by Calonico et al. (2014). We use characteristics of firms, candidates, and municipalities from the years 2008 and 2012, which are before the periods for which we analyze our outcomes in the main analysis (2009, 2010, 2011 for the election of 2008 and 2013, 2014, 2015 for the election of 2012). We cluster standard errors at the municipality-election year and add election fixed effects. We average firm-level characteristics at the candidate level. These are the same methods and samples we use later in estimating our treatment effects.

Figure 1B in Online Appendix B examines candidates' characteristics, including dummies for working-class status, business owner, age, college education, sex, campaign experience, and membership in major parties (PT, PSDB, MDB). The three samples are at the candidate level.

Figure 2B also in Online Online Appendix B examines the characteristics of firms in which variables are averaged at the candidate-election level. Characteristics include donations, expenditures in the campaign, wage bill, share of whites, college graduates, females, and number of employees.

Finally, Figure 3B in Online Online Appendix B as well uses the characteristics of municipalities, also at the candidate level, and includes the ln of municipal GDP, the ln of population, public expenditures and public revenues, total firms' donations, candidates' expenditures, and the number of firms.

For our three samples, none of the variables differ between the treatment and control groups at a robust confidence interval of 90%, except for i) candidates expenditure level in Figures 1(b) and 3(b) and ii) share of female employees for firms characteristics (Figure 3(c)). Full regression results of the balancing tests are reported in tables 2B, 3B, and 4B in Online Online Appendix B. Tests for incumbent candidates are positive or negative depending on the sample. They are marginally significant (at the 10% level), but there is low variability in estimating the optimal bandwidth. These results suggest that our assumption that the only systematic difference between the treatment and control groups is the election outcome is plausibly true.

Next, we proceed with a histogram showing that the frequency of candidates is continuous around the cut-off. In Online Online Appendix B, the histograms in Figure 4B show that candidate frequencies seem to be continuous around the cutoff of our running variable. Each histogram plots the number of candidates according to their vote margins. The Panel 4(a) corresponds to the donor firms' sample, where no evidence of clustering is observed around the cut-off, as the frequency of candidates seems to be similar on both sides of the threshold. Similarly, Panel 4(b) refers to the firms providing services to the candidates' campaign sample, also providing no indication of manipulation or unusual bunching near the threshold. Finally, Figure 4(c) shows the same reassuring results for the coalition affiliated firms sample.

Figure 5B in Online Online Appendix B tests whether the vote margin densities have discontinuities around the cut-off using the local polynomial density estimators proposed by Cattaneo et al. (2020). These figures also feature graphical procedures with valid confidence

bands (levels of 95%) based on Cattaneo et al. (2015). The evidence visualized in Figures 5(a), 5(b) and 5(c) indicates that there are no statistically significant differences between densities on different sides of the cut-off.

The findings above jointly support the validity of the regression discontinuity design by corroborating the absence of strategic sorting or bunching at the threshold. Together with the balance tests, these results indirectly support the core RD identifying assumption that the conditional expectations of the potential outcomes are continuous in the running variable at the threshold.

V Main Results

To assess the impact of political connections on firm procurement outcomes, we employ two primary measures. Both are constructed at the candidate level and capture firm activity in the three years following municipal elections. The first outcome is the probability that a firm connected to a candidate secures any procurement contract during the period after the close mayoral election. Specifically, for each firm f and candidate c , we compute a binary indicator y_{tfc} , where y_{tfc} equals 1 if the firm connected to candidate c obtained a procurement contract in any $t \in \{t + 1, t + 2, t + 3\}$ and 0 otherwise, where t refers to the election period 2008 and 2012.

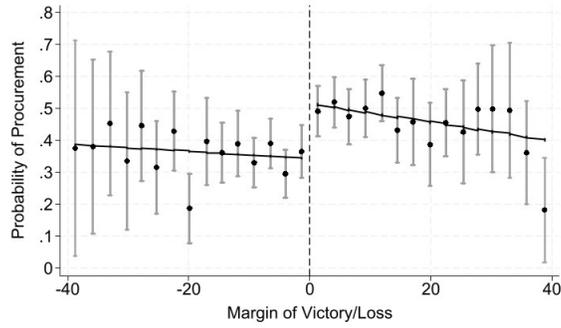
The final outcome is averaged at the candidate level:

$$\bar{Y}_{tc} = \frac{\sum_{f=1}^F y_{tfc}}{F} \quad (1)$$

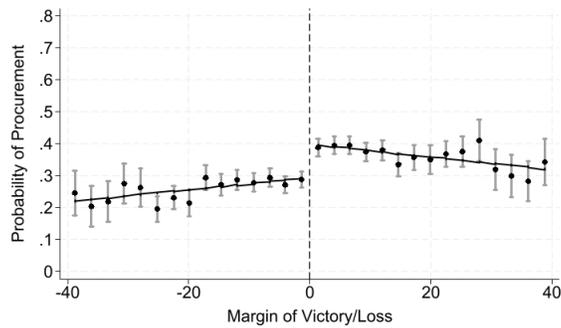
Where F denotes the number of firms linked to candidate c (either through donations, service provision, or coalition membership), and t denotes the election periods of 2008 or 2012. While this aggregation reduces granularity compared to a firm-candidate level analysis, our results remain robust even under a specification at the firm-candidate level, as shown by the event study discussed later in this section. The second outcome measures the procurement revenues of connected firms as a percentage of the total municipal budget. Here,

the outcome represents the total procurement revenue of all connected firms in the three years after the elections, divided by the municipal budget. This calculation is constructed after excluding firms with zero procurement and trimming the top 2 percentile, as there is high variance in procurement values at the top of the distribution (results are robust to these exclusions).

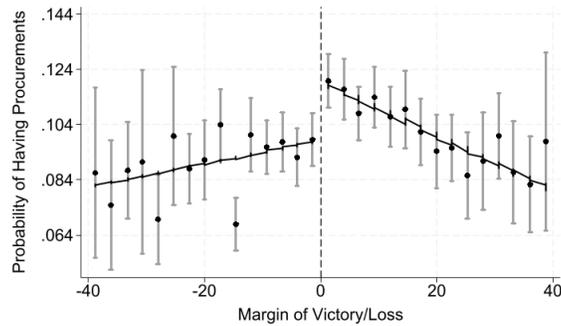
Figure 1 presents our main results in three panels arranged in a 3x1 layout, one for each sample; all panels report discontinuities in the probability of procurement for connected firms that have their connection marginally winning the election. All panels use a polynomial of degree one, employing the method developed in Calonico et al. (2015a). We use 20 bins on each side of the cutoff. Each panel displays the averages of the bins with 95% confidence intervals. The horizontal axis in all panels reflects candidates' margins of victory or loss.



(a) Donor Firms



(b) Service-Provider Firms



(c) Coalition Affiliated Firms

Figure 1: Discontinuities in Firms' Probability of Receiving Procurement Contracts (Donor, Service-Provider, and Coalition Affiliated firms Samples)

Note: Figure 1 presents three panels in a 3x1 layout, each corresponding to one of the samples: donor firms, service-provider firms, and coalition affiliated firms. The vertical axis shows the average candidate-level probability of obtaining procurement contracts, while the horizontal axis represents the candidates' margin of victory or loss. All panels use local polynomial regressions with 20 fixed bins on each side of the cutoff, as proposed by Calonico et al. (2015a), and include 95% confidence intervals. The plots reveal clear and visually comparable discontinuities across all samples.

Figure 1(a) illustrates a discontinuity of more than 10 percentage points in the probability of obtaining a procurement for firms that donate to candidates near the threshold. Figure 1(b) also shows a discontinuity of more than 10 percent for firms providing goods/services to candidates. Finally, Figure 1(c) shows a 2 percentage point increase for firms whose owners are affiliated with a coalition that marginally won races.

For comparison, Online Appendix B Figure 6B presents similar results using the default settings developed by Calonico et al. (2015a), which automatically selects the polynomial degree, number of bins, and width of the bins. The discontinuity results are robust to the standard setting, as shown in all three panels (one for each sample) of Figure 6B.

Table III estimates treatment effects, which confirm the graphical analysis. As in the balance tests, we use the approach developed by Calonico et al. (2015b). This nonparametric estimator selects the optimal bandwidth and implements the bias-corrected inference procedure proposed by Calonico et al. (2014), which is robust to large bandwidth choices. We add election fixed effects, and standard errors are clustered at the municipality-election level. All estimates are based on local polynomial RD estimators.

Table III presents the treatment effect point estimates, along with robust standard errors in parentheses. The first block of columns reports a polynomial of degree one specification, and the second block refers to a polynomial of degree 2. We report, for each polynomial, the default bandwidth selected by the estimator, half of the default bandwidth, and double the default bandwidth. Our preferred specification is the first column, the default bandwidth, with a polynomial of degree one.

In Table III, Panel A reports results for donor firms. In our preferred specification (default bandwidth and linear polynomial), the estimated coefficient is 0.15, corresponding to a 15 percentage point increase in the probability of securing a procurement contract. Relative to the baseline probability among firms donating to marginally losing candidates, this effect represents a 37% increase. The estimates are statistically significant at the 1% level for the default and double bandwidths but lose significance when using half of the default bandwidth. Across specifications (including the quadratic ones), the magnitude of the effect is stable, ranging between 0.12 and 0.16.

Panel B of Table III focuses on service-provider firms. The preferred specification yields a coefficient of 0.12, indicating a 12 percentage point increase, or roughly a 38% higher probability of obtaining procurement contracts for firms that supplied goods or services to marginally winning candidates compared to those linked to marginally losing candidates. Estimates are statistically significant at the 1% level across all bandwidth and polynomial choices, with limited variation in magnitude (0.11 to 0.13).

Finally, Panel C of Table III examines coalition affiliated firms. The preferred estimate is 0.02, corresponding to a 2 percentage point increase, or about a 20% higher probability of contracting for firms whose owners are affiliated with parties in the winning coalition relative to those linked to marginally losing coalitions. These estimates are statistically significant at the 1% level for the default and double bandwidths, and at the 5% level for the half-bandwidth specifications. As with donor and service-provider firms, the magnitude of the effect for coalition affiliated firms is stable across the choice of bandwidth and polynomial. The coefficients vary between 0.02 and 0.03. These results suggest that firms connected to the winning candidate are substantially more likely to have procurement opportunities than in the counterfactual case in which their connection lost the elections.

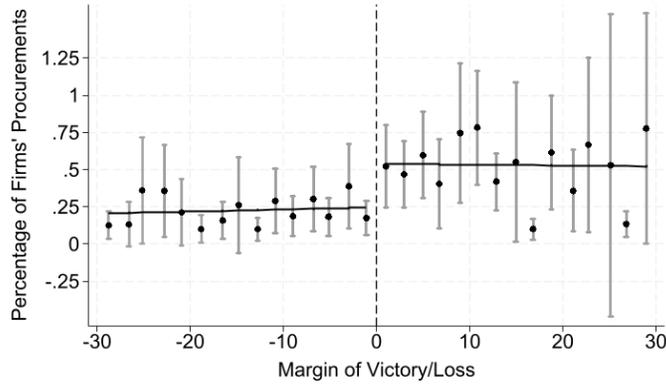
Figure 2 examines the intensive margin of treatment effects, where the outcome is defined as the share of procurement revenues relative to the municipal budget (excluding firms with zero procurement). The Figure follows the same structure as Figure 1 and reveals visible discontinuities across all three samples. In Panel 2(a), the average procurement share for donor firms increases from 0.25% among marginally losing donors to 0.50% among marginally winning donors. In Panel 2(b), the corresponding jump for service-provider firms is from 0.15% to 0.22%. Finally, Panel 2(c) shows that the procurement share rises from 0.045% for coalition affiliated firms linked to losing coalitions to 0.07% for those connected to winning coalitions. These results suggest that connected firms not only obtain contracts more frequently but also secure larger contract values when their political allies win office. Estimates reported in Table IV corroborate the graphical evidence.

Table IV reports the regression discontinuity estimates for the intensive margin of procurement. Our preferred specification (Column 1, default bandwidth with a linear polyno-

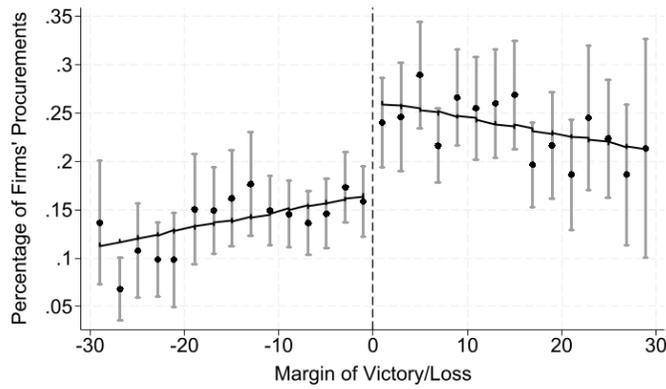
Table III: Treatment Effects Estimates on Probability of Having a Procurement

	Polynomial Degree 1			Polynomial Degree 2		
	Default BW	Half BW	Double BW	Default BW	Half BW	Double BW
Panel A: Donors						
Coefficient	0.15***	0.14	0.16***	0.13*	0.12	0.15**
Robust Std. Error	(0.06)	(0.14)	(0.06)	(0.08)	(0.15)	(0.07)
Bandwidth (h)	19.94	9.97	39.89	23.38	11.69	46.76
Eff. Observations	960	606	1256	1056	682	1308
Average Outcome		0.41			0.41	
Panel B: Service Providers						
Coefficient	0.12***	0.13***	0.11***	0.12***	0.13***	0.12***
Robust Std. Error	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)
Bandwidth (h)	14.01	7.00	28.01	21.94	10.97	43.88
Eff. Observations	3642	2096	5236	4716	3024	5886
Average Outcome		0.31			0.31	
Panel C: Coalition Affiliated						
Coefficient	0.02***	0.03**	0.02***	0.02***	0.03**	0.02***
Robust Std. Error	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Bandwidth (h)	17.41	8.71	34.82	21.89	10.95	43.78
Eff. Observations	3930	2324	5238	4408	2806	5542
Average Outcome		0.10			0.10	

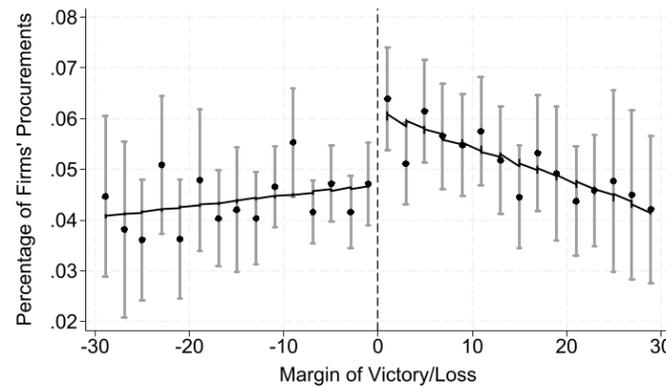
Note: Table III reports local polynomial RD estimates of the effect of having a connection marginally elected on the probability of a firm receiving procurement contracts. Estimates are computed using the bias-corrected method of Calonico et al. (2014, 2015b), with clustered standard errors at the municipality-election level and election fixed effects. Each panel shows results for a different firm sample. Columns vary around the optimal bandwidth (half, default, double) and use polynomials of degree 1 and 2. The outcome mean in the polynomial of degree one half of the default bandwidth is 0.41 in Panel A (Donors), 0.31 in Panel B (Service Providers), and 0.10 in Panel C (coalition affiliated Firms).



(a) Donor Firms



(b) Service-Provider Firms



(c) Coalition Affiliated Firms

Figure 2: Discontinuities in Firms' Procurement Revenue Percentage

Note: Figure 2 presents a 3x1 panel layout to show discontinuities of procurement revenues percentage across three samples: donor firms (2(a)), service-provider firms (2(b)), and coalition affiliated firms (2(c)). Results suggest that contracts are ²³larger for firms connected to winning candidates compared to the counterfactual case where they lost.

mial) shows that donor firms (Panel A) experience an increase of 0.23 percentage points in procurement revenues relative to the municipal budget, significant at the 10% level. This corresponds to contracts that are approximately 72% larger for firms donating to marginally winning candidates compared to those donating to marginally losing candidates. Across specifications with a polynomial of degree one, the estimates range from 0.16 to 0.23 and remain significant at the 10% level for the default and double bandwidths. With a polynomial of degree two, estimates vary between 0.03 and 0.24, but only the double-bandwidth specification reaches statistical significance (10% level).

Panel B of Table IV focuses on service-provider firms. The preferred specification yields an effect of 0.07 percentage points, statistically significant at the 10% level, implying contracts are roughly 39% larger for firms providing goods or services to marginally winning candidates relative to those linked to marginally losing candidates. With a polynomial of degree one, estimates range between 0.07 and 0.08 and are statistically significant at the 10% level for the default bandwidth and at the 1% level for the double bandwidth. With a polynomial of degree two, estimates range between 0.07 and 0.08 and only the double of the bandwidth is statistically significant at the 5% level.

Panel C examines coalition affiliated firms. The preferred specification indicates a 0.02 percentage point increase, significant at the 5% level, corresponding to contracts that are about 40% larger for firms whose owners are affiliated with parties in the winning coalition compared to those linked to losing coalitions. With a polynomial of degree one, the estimate is consistently 0.02 across bandwidth choices and statistically significant at the 5% level (default bandwidth), 10% level (half bandwidth), and 1% level (double bandwidth). For a polynomial of degree two, the estimate is consistently 0.02 and significant at the 10% level.

To put results into perspective, we can use a back of the envelope calculation and extrapolate our samples to the whole country. If municipalities' budgets compose 10% of the GDP. Then a 0.1 percentage point change in it refers to a 0.01% change in GDP. In this way, donor firms are reallocating about 0.02% of the GDP in larger procurements. Service providers are reallocating 0.007% and coalition affiliated firms are reallocating 0.002% of GDP in larger procurements. These results also suggest that firms connected to winning candidates are

receiving larger contracts, which would have been smaller had their connected politician lost the election.

We now proceed with an event study at the firm level, not averaging outcomes at the candidate level. Our event study design examines the evolution of firm outcomes before and after closely contested elections, focusing on the 2012 elections, where we identify close races between candidates. By doing that, we can check whether there were treatment effects before the elections as a placebo (pre-trend) test. We can also verify how firms' gains evolve along the electoral cycle. Finally, we can check whether the results are robust to not averaging firms' outcomes (at the candidate level). The empirical specification takes the following form:

$$Y_{ft} = c + \sum_{t=2009}^{2018} \tau_t T_f \times I_t + I_t + p(MV_f) + T_f \times p(MV_f) + \epsilon_{ft} \quad t \neq 2012 \quad (2)$$

In this specification, Y_{ft} represents the outcome variable for firm f in year t . The treatment indicator T_f equals 1 if firm f is linked to a marginally winning candidate and 0 if linked to a marginally losing candidate. MV_f denotes the margin of victory for the candidate linked to firm f , while I_t represents year fixed effects. The polynomial function $p(MV_f)$ flexibly controls for the margin of victory, and the coefficients τ_t capture the dynamic treatment effects over time. The pre-treatment coefficients (2009–2011) serve as placebo tests, where we expect statistically insignificant results. The 2012 election year is normalized to zero, while post-treatment coefficients (2013–2018) reveal how effects evolve over time.

The identification strategy exploits the quasi-random assignment of treatment status in close elections. We focus on elections where the winner's margin of victory fell below 5 and 15 percentage point bandwidths. The coefficients τ_t trace the dynamic effects of a candidate's electoral victory on connected firms' probability of procurement.

Figures 3(a)–3(c) depict the evolution of firms' probabilities of obtaining procurement contracts over the electoral cycle. Panel 3(a) shows that donor firms experience substantial increases in procurement likelihood post-election. The peak effect occurs in year 2, reaching an increase of between 10 and 12 percentage points (coefficients between 0.10 and 0.12),

Table IV: Treatment Effects Estimates on % of Procurement in terms of municipality budget

	Polynomial Degree 1			Polynomial Degree 2		
	Default BW	Half BW	Double BW	Default BW	Half BW	Double BW
Panel A: Donors						
Coefficient	0.23*	0.16	0.19*	0.24	0.03	0.17*
Robust Std. Error	(0.13)	(0.79)	(0.11)	(0.16)	(0.24)	(0.09)
Bandwidth (h)	7.12	3.56	14.23	9.59	4.80	19.18
Eff. Observations	323	158	567	411	207	664
Average Outcome		0.32			0.36	
Panel B: Service Providers						
Coefficient	0.07*	0.07	0.08***	0.07	0.08	0.07**
Robust Std. Error	(0.04)	(0.09)	(0.02)	(0.05)	(0.17)	(0.03)
Bandwidth (h)	7.52	3.76	15.04	10.73	5.37	21.46
Eff. Observations	1,634	787	2,794	2,183	1,157	3,389
Average Outcome		0.18			0.19	
Panel C: Coalition Affiliated						
Coefficient	0.02**	0.02*	0.02***	0.02*	0.02*	0.02***
Robust Std. Error	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Bandwidth (h)	7.93	3.97	15.86	13.04	6.52	26.08
Eff. Observations	1,580	813	2,673	2,361	1,348	3,522
Average Outcome		0.05			0.05	

Note: Table IV reports local polynomial RD estimates of the effect of being marginally elected on the percentage of procurement revenues of total municipal budget. Estimates are computed using the bias-corrected method of Calonico et al. (2014, 2015b), with clustered standard errors at the municipality-election level and election fixed effects. Each panel shows results for a different firm sample. Columns vary around the optimal bandwidth (half, default, double) and use polynomials of degree 1 and 2. The outcome mean for the default bandwidth in a polynomial of degree one is 0.32% in Panel A (Donors), 0.18% in Panel B (Service Providers), and 0.05% in Panel C (coalition affiliated Firms). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

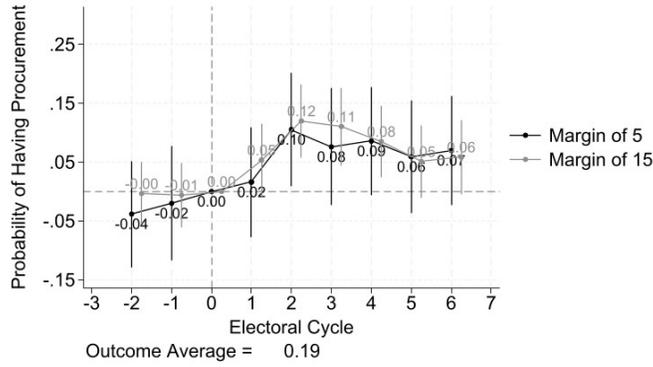
against an average outcome of 0.19 — a 63% increase. For the 15 percentage point bandwidth (gray line), treatment effects are statistically significant from the first year of the mandate ($t=1$) up to the last year of the mandate ($t=4$) at the 5% level. Treatment effects lose significance after this ($t > 4$). This pattern suggests that after a new round of elections, the opportunities to gain procurement through donations are less salient. Note that prior to the elections ($t \leq 0$), there are no pre-trends, indicating null anticipatory procurement gains to donor firms' connections.

Panel 3(b) for service-provider firms exhibits a smaller but still notable peak of 0.09 in year 2, the second year of the connections' mandate ($t=2$), representing a 9 percentage point increase. The outcome average of 0.21 indicates a 43% increase in the probability of having a procurement. For both bandwidths (5 and 15 percentage points), treatment effects are statistically significant at the 1% level for all periods between the first year of the mandate and the fourth year. Treatment effects decrease by year 5, the year in which the candidate's mandate ends; this occurs just as it does in the case of the donor firms. It is noteworthy that, in the case of the service provider, there are no pre-trends as well. This pattern is reassuring regarding how the probability of having a procurement increases just after the election in the first year of the mandate ($t=1$) and diminishes just after the last year of the connection mandate ($t \geq 5$).

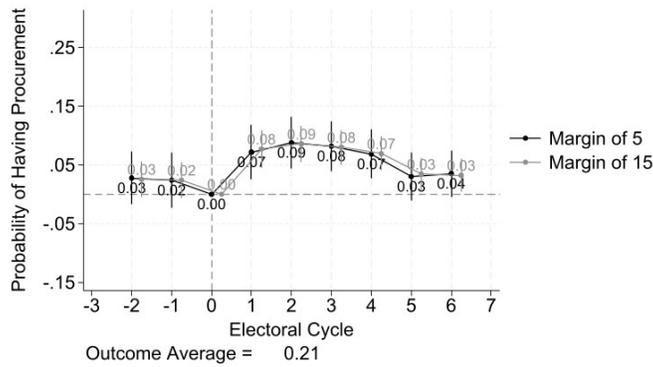
Panel 3(c) illustrates the increases in the probability of getting a procurement for firms whose owner was affiliated with a party in a coalition that marginally won compared to firms whose owner was affiliated with a party that marginally lost. Here, the peak effect is approximately 0.016 to 0.025 (in year 3), against a baseline of 0.04 — an increase of 40%. The pattern mirrors that of other groups: pre-election flatness, post-election rise, and decay by the time the next electoral cycle starts.

Overall, the temporal pattern — no anticipatory effects, a post-election surge, and decay after mandates end — supports the interpretation that electoral victories enable temporary gains in public procurement opportunities. Across all panels, there is no evidence of significant pre-treatment trends, supporting the validity of our identification strategy. These results suggest a clear and substantial post-election advantage for firms connected to

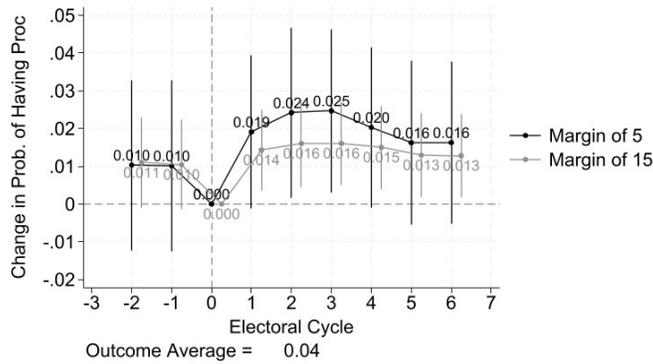
marginally winning candidates, particularly in the two to four years following the election. This effect diminishes as the next electoral cycle approaches. In summary, when the candidate connected to a firm wins an election, there seem to be more procurement opportunities than the counterfactual case where she lost. These gains rise in the first year of election and diminish after the last year of mandate.



(a) Donor Firms



(b) Service Provider Firms



(c) Coalition Affiliated Firms

Figure 3: Event Study: Treatment Effects Around the 2012 Election – Probability of Having a Procurement

Note: Figures 3(a)–3(c) depict the evolution of firms’ probability of obtaining procurement contracts over the electoral cycle. Each panel uses a local polynomial regression within 5% and 15% winning margins. Coefficients are normalized to zero in the election year. Panel 3(a) shows donor firms’ procurement probability rises by 63% post-election, peaking in year 2. Panel 3(b) shows a 43% increase for service providers, also peaking in year 2 and fading by year 5. Panel 3(c) shows a 40% rise for coalition affiliated firms, following a similar post-election surge and decline.

Estimates in this section are subject to some caveats, primarily due to potential measurement error. Specifically, we cannot account for unobserved transfers to candidates that occur outside formal channels, which are reportedly common according to Carazza (2018). This issue is particularly relevant for firms that donate through informal channels (not declared to authorities), which are not captured in our data. In this case, our estimates of returns for donors would be an upper bound, as we do not account for informal connections. On the other hand, our analysis does not capture other potential benefits that firms or individuals may receive beyond procurement revenues, such as non-pecuniary advantages or informal arrangements, as highlighted by Katovich and Moffette (2024). In this case, we would be estimating a lower bound for the returns to connections, as the benefits are not fully accounted for. These limitations are intrinsic to the topic we study but should be considered when interpreting our results. Next, we proceed with a sensitivity analysis.

Sensitivity Analysis

We begin the sensitivity analysis by using non-normalized vote shares instead of normalizing them. Table IB in Online Appendix B replicates Table III using non-normalized vote share and the probability of having procurement as outcomes. Results are robust to this change. Also, Table VB in Online Appendix B shows that point estimates do not change significantly when we fix the pool of states to create a balanced panel. In this sense, the results are robust to excluding the two extra states added in the 2012 election, Minas Gerais and Paraná.

Next, we add covariates to our regressions. We add firms' sector (4-digit) fixed effects in a local polynomial regression analysis to check whether the coefficients are affected by the introduction of covariates. In this way, we check whether our observed treatment effects are driven by omitted variables, such as those that are correlated with the sector of the firm. The analysis in Table VIB in Online Appendix B refers to our main outcome at the firm level. We can observe that, in general, the estimates are of similar magnitude and are more statistically significant when we add sector fixed effects, except for donor firms. For donor firms, coefficients increase in magnitude and statistical significance after including the sector level.

Our results suggest that the inclusion of sector fixed effects does not significantly alter the estimated treatment effects, except in the case of donor firms, where the coefficients are larger whenever sector fixed effects are added. These findings indicate that the results are not driven by omitted variables correlated with firms' sectors, at least for service providers and coalition affiliated firms. This strengthens the interpretation that firms' sectors do not play a role in shaping access to public procurement (at least for service providers and coalition affiliated firms).

Our RDD excludes municipalities where the margin of victory was not close. This raises questions about the external validity of our natural experiment. After all, municipalities in which close races occur can differ from municipalities in which elections are not close. Table VIIB in Online Appendix B shows how non-close race municipalities differ from close race municipalities (margin of 5 percentage points) for each of our procurement revenue samples.

In general, close race municipalities are significantly smaller, have fewer candidates, fewer connected firms, fewer connected firms having procurement revenues, and a smaller public budget. This means that close race municipalities are more likely to be less developed, not have a run-off for mayor, and have smaller councils. A caveat of our analysis is that we cannot generalize the above results for non close race municipalities. Close race municipalities comprise approximately 22% of the sample.

VI Mechanisms and Implications

This section aims to explore the mechanisms that explain why we find our treatment effects. Why do firms connected to winning candidates have more procurement than in the case where they had lost the election? First, we examine whether firms in corrupt sectors have a higher probability of securing procurement and whether our treatment effects in more corrupt sectors are greater than those in less corrupt sectors. Secondly, we tackle the implications of these procurement allocation processes; we use firms' outcomes, such as the number of employees and the wage bill, to check whether firms tied to winning candidates employ more people or have greater wage bills. This allows us to check whether procurement gains are

being redistributed to employees. Finally, we exploit firms that operate in more than one municipality to check whether firms that donate more or provide services/goods of greater value in one municipality than another have larger or smaller contracts in the former. We also check whether firms whose owners are party-affiliated are more likely to receive procurements. These correlations provide insights into the kind of connection that firms and candidates have.

We start by investigating whether the treatment effects of electoral success on firms' procurement opportunities differ across candidates linked to more or less corrupt economic sectors. As a first step, we identify corruption levels at the firm-sector level by relying on the corruption index constructed by Colonnelli and Prem (2022), which provides measures at the firm 4-digit sector classification (CNAE). Key sectors considered highly corrupt in the dataset include the wholesale of pharmaceutical goods, passenger land transport, and fuel retail.

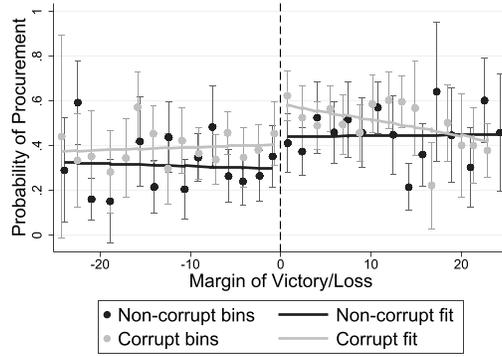
We then aggregate this information at the candidate level by computing the average corruption index of all firms linked to a given candidate. The candidate with an average corruption index above the index's median (averaged at the candidate level) is labeled as a "corrupt" candidate. Analogously, the candidate with an average index below the median is denoted as "non-corrupt". Using this classification, we explore whether the electoral success treatment effects on firms' procurement probabilities differ across these types of candidates.

Donor firms in more corrupt sectors do not experience a significantly greater probability of obtaining procurement contracts (Figure 4(a)). The discontinuities for both types of sectors (corrupt and non-corrupt) also do not seem to differ. For donor firms, those that donate to a winning candidate have the same magnitude of increase in their procurement opportunities compared to the case they had donated to a losing candidate, regardless of the corruption level in their sector.

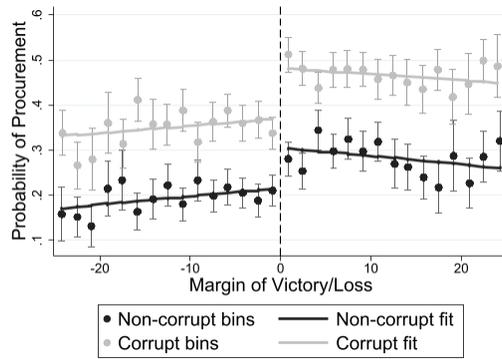
On the other hand, Figure 4(b) shows that firms that are services/goods in corrupt sectors are significantly more likely to get procurement, regardless of whether their connection is a winning or a losing candidate. Furthermore, in corrupt sectors, services/goods providers to the winning candidate seem to experience larger gains in procurement opportu-

nities compared to those in the non-corrupt sector. In other words, the gains in procurement opportunities are larger in corrupt sectors than in non-corrupt sectors for services/goods providers.

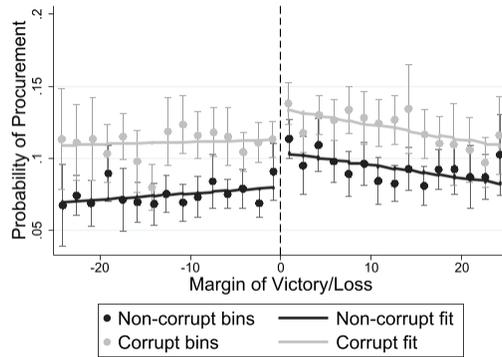
Next, we find that firms whose owners are affiliated with a party within a coalition supporting a candidate and who are also in a corrupt sector have a greater probability of obtaining procurement compared to firms in less corrupt sectors, regardless of whether their connections are winning or losing the election (Figure 4(c)). However, as in the case of donor firms, the discontinuity does not seem to differ between coalition affiliated firms in corrupt sectors and those in non-corrupt sectors. This result suggests that firms connected to the winning coalition in a corrupt sector have the same procurement gains as firms in non-corrupt sectors, but their baseline probability of obtaining procurement is higher in corrupt sectors.



(a) Donor Firms



(b) Service Provider Firms



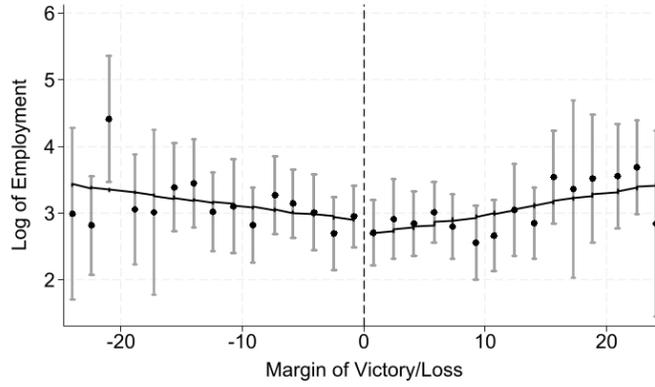
(c) Coalition Affiliated Firms

Figure 4: Treatment Effects Heterogeneity: Corrupt vs Non-Corrupt Sectors

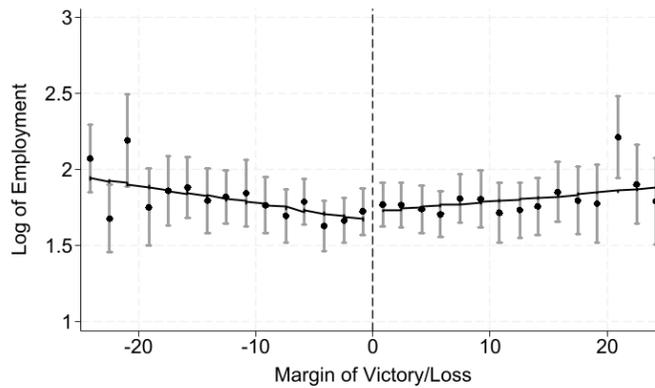
Note: Figures 4(a), 4(b), and 4(c) divide the sample into candidates tied to firms in more/less corrupt sectors. Figure 4(a) refers to donor firms, 4(b) to service provider firms, and 4(c) to coalition affiliated firms. The Y-axis shows the probability of having procurement averaged at the candidate level, and the X-axis shows the candidates' margin of victory/loss. Gains between firms linked to more or less corrupt candidates are similar, except for service provider firms. The probability that a connected firm has a procurement contract in corrupt sectors is greater for service providers and for coalition affiliated firms, but not for donor firms.

Now we focus on our implication: whether the observed connection gains are being redistributed to firms' employees. It is an important aspect to understand whether this procurement reallocation process is being distributed to workers, as opposed to being entirely captured by politicians and/or firm' owners. One interesting phenomenon suggesting that this procurement allocation process is not being reinvested in firms' employees is the fact that these firms are not growing.

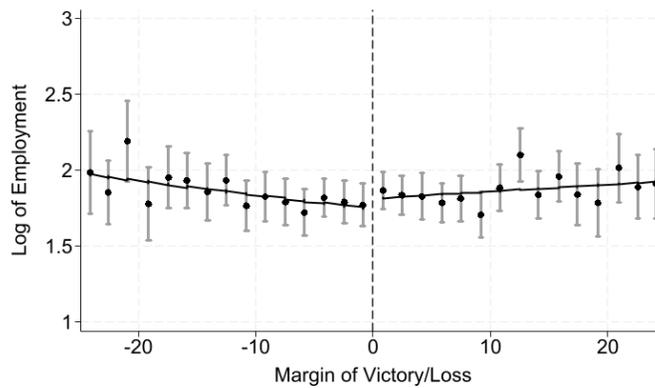
In the same spirit as Figure 1, Figure 5 shows null effects for another outcome: the log of firms' employment. We construct this variable in three steps. First, for each firm, we compute the average number of employees over the three years following the election. Second, we average this measure across all firms connected to a given candidate, so the outcome is defined at the candidate level. Finally, we take the logarithm of this candidate-level average. We repeat this procedure separately for donor firms (Panel 5(a)), service providers (Panel 5(b)), and coalition-affiliated firms (Panel 5(c)). Table IC in Online Appendix C confirms the visual evidence by showing precisely estimated null effects on the log of employees. Additionally, Figure 1C in Online Appendix C shows a similar pattern when using the logarithm of the average wage bill as the outcome. These results suggest that firms have more procurement opportunities and larger contracts but do not employ more or pay more to their employees when their connections win the elections, which is consistent with this surplus being absorbed by either capital investments, profits, or rents paid to politicians.



(a) Donor Firms



(b) Service Provider Firms



(c) Coalition Affiliated Firms

Figure 5: Treatment Effects on Log of Employment

Note: Figures 5(a), 5(b), and 5(c) use the log of employment as the outcome and employ the method of Calonico et al. (2015a) to generate the figures. Panel (a) refers to donor firms, Panel (b) to service provider firms, and Panel (c) to coalition affiliated firms. The Y-axis shows the log of employment of the average firm tied to candidates, and the X-axis shows the candidates' margin of victory/loss. Across all panels, there is evidence of null treatment effects on employment for the average firm.

A key question concerns the mechanisms underlying the returns to political connections. For donor firms, one possibility is a direct quid-pro-quo: larger donations may translate into more and larger procurement contracts. Alternatively, firms with stronger bargaining power may donate less yet still secure larger contracts, suggesting an inverse relationship between donations and subsequent procurement. For service-provider firms, campaign-period transactions could serve as either implicit discounts in anticipation of future contracts or as a reputational signal whereby candidates learn about firms' capacities and reliability. Finally, coalition membership may correlate with contract size if partisan ties shape procurement allocations. While providing definitive answers lies beyond the scope of this paper, we present suggestive evidence that is consistent with some of these mechanisms.

To explore potential mechanisms, we start by examining whether firms that donate or provide services in multiple municipalities receive larger contracts in those where their connective activity is greater. Specifically, we test whether within-firm variation in the value of donations or services provided correlates with subsequent procurement receipts across municipalities. For donor firms, a positive correlation would support a quid-pro-quo channel, while a negative correlation would suggest that firms donate more precisely where they expect lower procurement returns. For service providers, a negative correlation would be consistent with implicit discounts in campaign services and greater expected procurement gains, while a positive correlation would suggest informational or reputational channels. We also test whether the coalition affiliation of the firm owner is associated with larger contracts. Table V reports results estimated using OLS with firm and year fixed effects (except in column 3, where we use municipality fixed effects). The analysis is also conditioned on firms having a positive procurement value after elections. Note that the sample used in this analysis differs from our previous analysis, as we don't condition on firms being connected.

We find a negative, though imprecisely estimated, coefficient for the correlation between donation and procurement value in Table V (Column 1), suggesting that firms may donate more in municipalities where they have weak ties and donate less in municipalities where they have strong ties. It is possible that the firm would have greater bargaining power to negotiate the terms of the procurement allocation process in cities where it has more

leverage. Importantly, the probability of donor firms securing procurements is not greater in more corrupt sectors, nor are their gains. They also do not redistribute their gains to employees.

Secondly, we find that a greater value of services and goods provision during the campaign-period is associated with larger procurement contracts afterward (Column 2). This finding is consistent with informational or reputational mechanisms: candidates may reward reliable campaign-period suppliers with post-election contracts. Alternatively, firms may charge more when they already hold stronger political connections. However, firms that sell services or goods to candidates' campaigns experience greater gains in more corrupt sectors, and the probability of securing procurement contracts is also higher for these firms in more corrupt sectors. These two factors are suggestive evidence that firms and candidates are engaging in quid-pro-quo. Not to mention the null effect on firms employment found earlier in this section, meaning that there is no redistribution of procurement gains to employees.

Finally, we find that status of being a coalition affiliated firms captured by a dummy reported in Column 3 have a strong and positive correlation with procurement value. Column 3 reveals that within municipalities, firms whose owners are affiliated with a party in one of the top two mayoral coalitions are associated with contracts that are six times larger.

VII Conclusion

This paper provides novel causal evidence on the economic returns to political connections in Brazil's municipal elections. Using a regression discontinuity design on close races and rich administrative data, we show that firms tied to marginally winning candidates—either through campaign donations, the provision of services or goods during the campaign, or the firm owner being affiliated with the winning coalition—experience substantial gains in local procurement allocation when compared to the counterfactual case in which their political connections lost. These gains are observed both on the extensive margin, with an increased likelihood of winning contracts, and on the intensive margin, with larger contract sizes relative to the municipal budget. This means that the municipal budget is being reallocated

Table V: Connections on Procurement (Proc.): OLS Estimates

	(1) Ln Proc.	(2) Ln Proc.	(3) Ln Proc.
Ln of Donation	-0.560 (0.424)		
Ln of Spending		0.126*** (0.048)	
Coalition Affiliated [I=1]			1.827*** (0.027)
Observations	60	1,920	2,944,456
Clusters	36	1,084	3,372
Adj. R-squared	0.514	0.629	0.421
Fixed Effects	Firm, Year	Firm, Year	Mun, Year

Note: All regressions estimated using OLS with high-dimensional fixed effects reghdfe. Robust standard errors clustered at the candidate (columns 1–2) or firm (column 3) level. Coefficients are elasticities except for the binary coalition affiliated variable. Identification relies on within-firm or within-municipality variation across years and connections[*] $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

towards procurements with firms that have formed ties to the winning candidate.

Taken together, these findings deepen our understanding of how political connections reallocate public procurement in weakly institutionalized settings. They highlight the importance of looking beyond traditional campaign donations to identify new, less-studied forms of political connections that shape the allocation of public resources. Future research should further investigate whether these procurement advantages represent quid pro quo exchanges or instead reflect a selection process favoring firms perceived as more efficient or trustworthy. Collaboration between academia and policymakers in this avenue seems to be a fruitful path to follow.

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Online Appendix

Online Appendix A

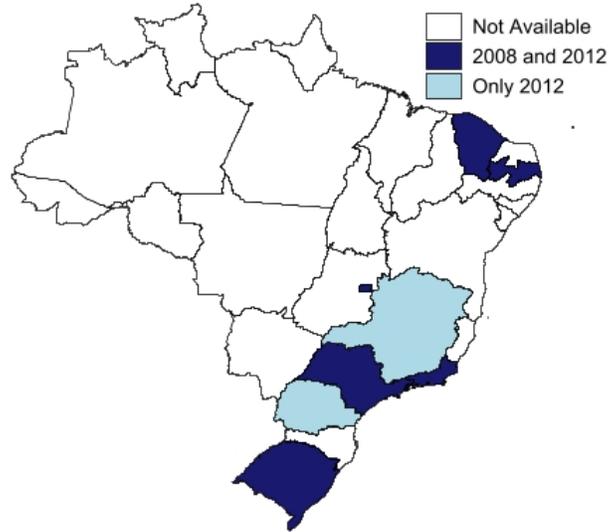


Figure 1A: Data Availability by States

The Figure shows the available data for the states of Ceará, Paraíba, Rio de Janeiro, São Paulo, Rio Grande do Sul, Distrito Federal in 2008 and adding Paraná and Minas Gerais in 2012. Their GDP consists of 71.8% of the total Brazilian GDP in 2012 and 59.1% of the Brazilian population. The 2012 data covers 2,791 municipalities out of 5,570. Combined, these eight states have a GDP equivalent to Canada and a GDP per capita equivalent to Chile.

Table IA: Comparison of Means between All Brazilian Municipalities Characteristics Except Procurement Sample and Only Procurement Sample for Pooled Election Years 2008 and 2012.

Variable	All except proc. sample (N=7,803)	Proc. sample (N=2,854)	Diff (Std. Dev.)
Ln tot. exp.	16.84 (0.99)	16.97 (1.16)	-0.13*** (0.02)
Ln wage bill	16.05 (1.02)	16.16 (1.19)	-0.11*** (0.02)
Ln capital exp.	14.19 (1.26)	14.55 (1.28)	-0.36*** (0.03)
Ln tot. revenues	16.86 (0.99)	16.99 (1.16)	-0.14*** (0.02)
Ln local taxes	13.71 (1.55)	14.11 (1.83)	-0.40*** (0.04)
Population	30,966 (110,726)	44,974 (350,015)	-14,009*** (4,471)
Ln GDP	18.54 (1.36)	18.79 (1.58)	-0.25*** (0.03)

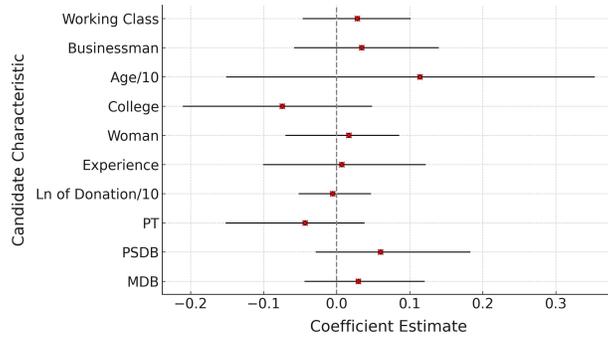
Note: Standard deviations in parentheses. *** $p < 0.01$. Municipalities included in the procurement sample have higher average total expenditures, wage bill, capital expenditures, public revenue, and local taxes, as well as larger population and GDP.

Online Appendix B

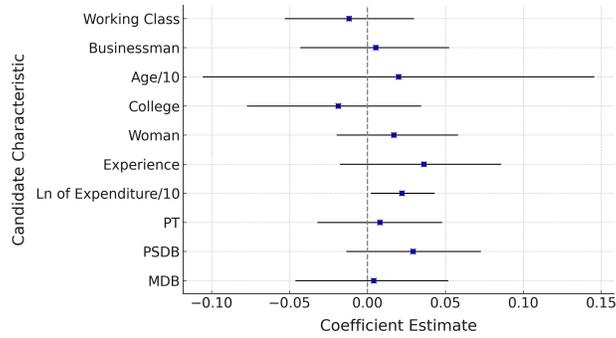
Table IB: Treatment Effects Estimates on Probability of Having a Procurement: Non-normalized Vote Share

	Polynomial Degree 1			Polynomial Degree 2		
	Default BW	Half BW	Double BW	Default BW	Half BW	Double BW
Panel A: Donors						
Coefficient	0.14***	0.12	0.15***	0.10	0.10	0.14*
Robust Std. Error	(0.05)	(0.11)	(0.05)	(0.08)	(0.13)	(0.08)
Bandwidth (h)	21.41	10.70	42.81	20.89	10.44	41.78
Eff. Observations	1,050	684	1,306	1,030	668	1,304
Average Outcome		0.41			0.40	
Panel B: Service Providers						
Coefficient	0.12***	0.12***	0.11***	0.12***	0.13***	0.11***
Robust Std. Error	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)
Bandwidth (h)	13.86	6.93	27.72	22.33	11.17	44.67
Eff. Observations	3,794	2,148	5,336	4,907	3,234	5,982
Average Outcome		0.31			0.31	
Panel C: Coalition Affiliated						
Coefficient	0.02***	0.02***	0.02***	0.02***	0.02***	0.02***
Robust Std. Error	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Bandwidth (h)	10.44	5.22	20.89	18.94	9.47	37.88
Eff. Observations	3,974	3,146	5,050	4,924	3,866	5,672
Average Outcome		0.10			0.10	

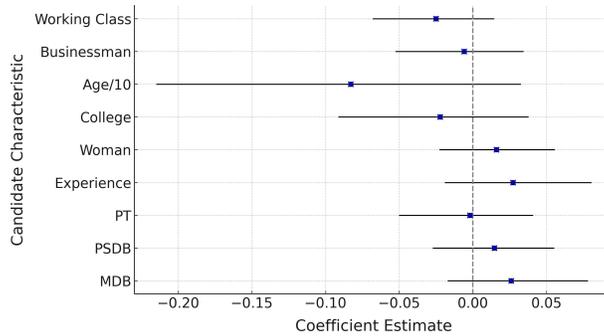
Note: Table IB reports local polynomial RD estimates of the treatment effect on the probability of a firm receiving procurement contracts using the non-normalized vote share instead of the normalized vote share. Estimates are computed using the bias-corrected method of Calonico et al. (2014, 2015b), with clustered standard errors at the municipality-election level and election fixed effects. Each panel shows results for a different firm sample. Columns vary around the optimal bandwidth (half, default, double) and use polynomials of degree 1 and 2. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ based on robust standard errors.



(a) Donor Firms



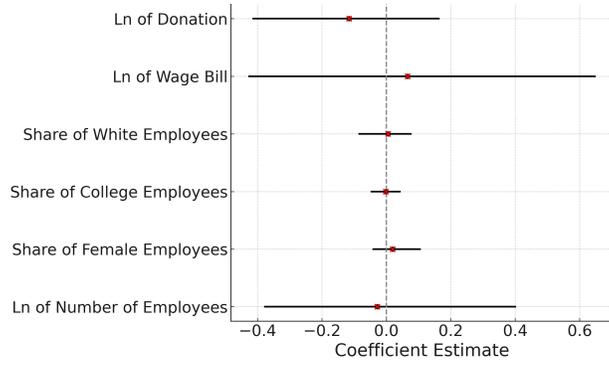
(b) Service Provider Firms



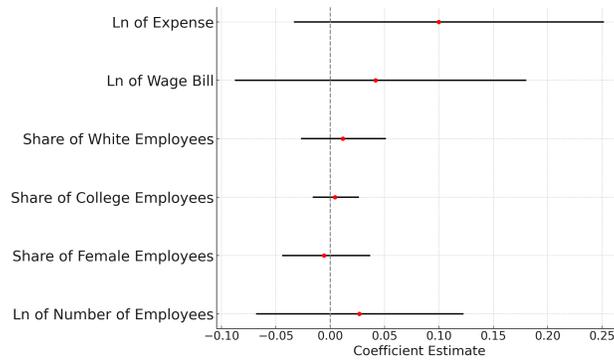
(c) Coalition Affiliated Firms

Figure 1B: Balance Test for Candidate Characteristics

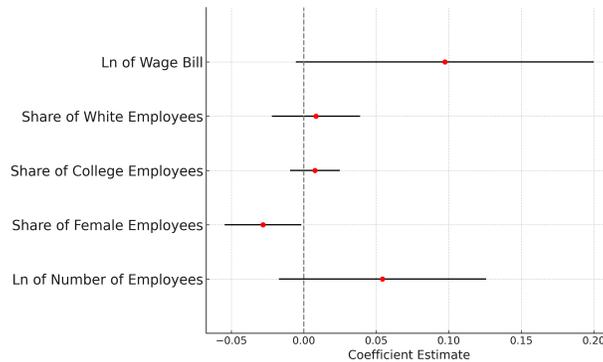
Note: The figures show RD regressions using candidate characteristics. Point estimates and confidence interval of 90% levels are plotted for the three samples. The unit of observation is at candidate-election level. Tested characteristics include dummies for working-class status, business owner, age, college education, sex, campaign experience, donations/expenditures and membership in major parties (PT, PSDB, MDB). Tests for incumbents do not have enough variability, being sometimes positive other negative depending on the sample and significant at the 10% level.



(a) Donor Firms



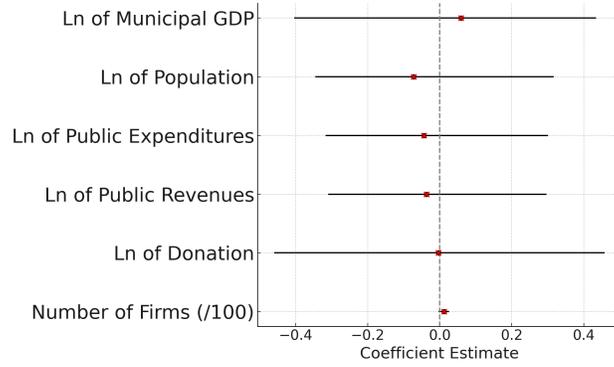
(b) Service Provider Firms



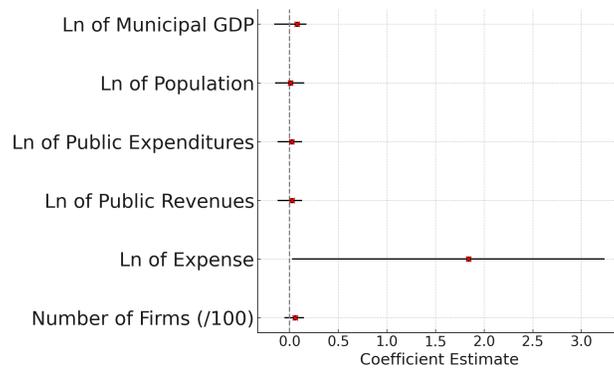
(c) Coalition Affiliated Firms

Figure 2B: Balance Test for Firms Characteristics

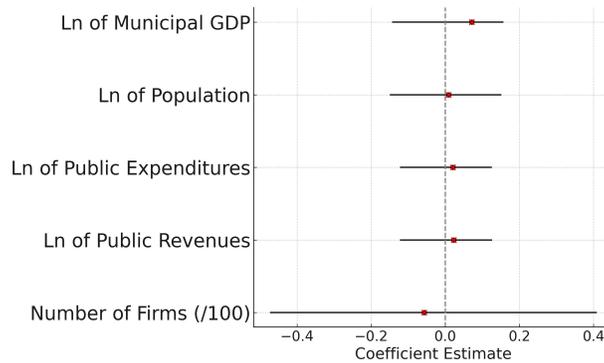
Note: The figures show RD regressions using firm characteristics. Firms characteristics are averaged at the candidate level. Point estimates with confidence interval at 90% level are plotted. Figure 2(a) refers to donor firms, Figure 2(b) refers to service provider firms, and Figure 2(c) refers to coalition affiliated firms. Most of the firms characteristics are insignificant, including donation, expenses of candidates, wage bill, share of white, college, and number of employees. Except for share of female employees, which is statistically significant at 10% level.



(a) Donor Firms



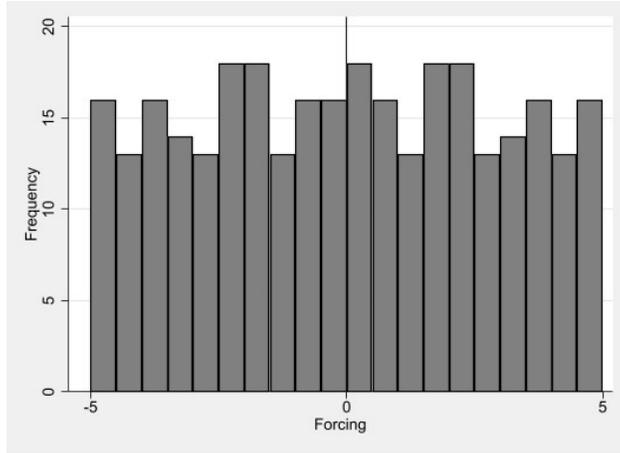
(b) Service Provider Firms



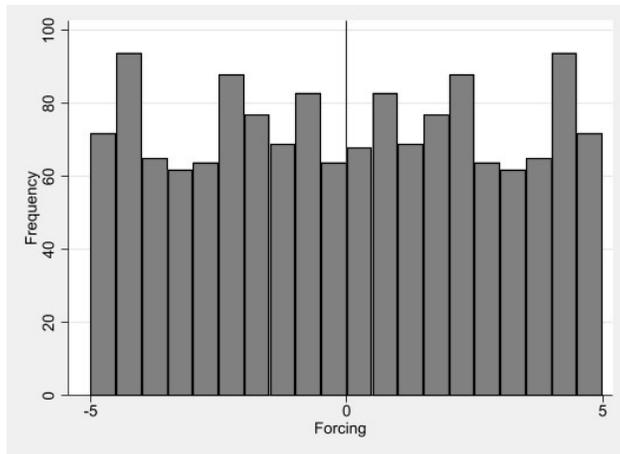
(c) Coalition Affiliated Firms

Figure 3B: Balance Test for Municipalities Characteristics

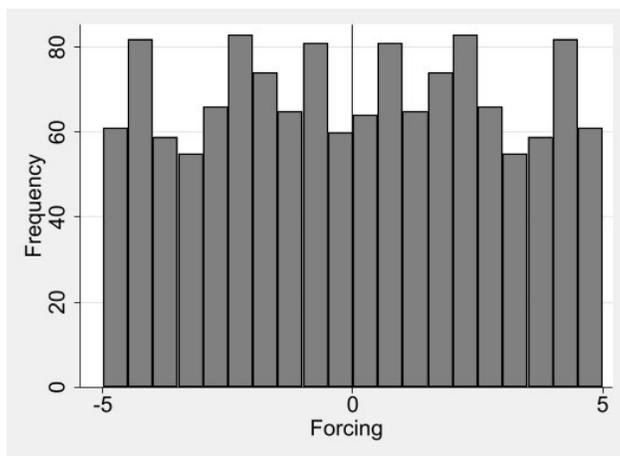
Note: The figures show RD regression using municipalities characteristics. The unit of observation is at the candidate-election level. Figure 3(a) refers to donor firms, Figure 3(b) refers to service providers and Figure 3(c) refers to coalition affiliated firms. Tested characteristics include ln of municipal GDP, ln of population, public expenditures and revenues, candidates donation revenues and expenditures in firms, and number of firms⁴⁹. All placebo treatment effects are statistically insignificant.



(a) Donor Firms



(b) Service-Provider Firms



(c) Coalition Affiliated-Firms

Figure 4B: Histograms of Candidates' Margin of Victory/Loss for Donor, Service-Provider, and coalition affiliated Firms Samples

Note: Figure 4B presents histograms of number of candidates by their margin of victory/loss for the three samples around the threshold. All sub-figures show no evidence of bunching.

Table IIB: Regression Discontinuity Estimates of Balance Tests for Candidates Characteristics in Each Sample

Panel	Candidate Characteristics	Coefficient	Robust Std. Error	N. of observations
A: Donor Firms	Working class	0.028	0.047	842
	Businessperson	0.034	0.051	940
	Age	0.114	0.173	966
	College	-0.074	0.072	848
	Woman	0.016	0.103	910
	Experience	0.007	0.045	924
	Campaign Revenue	-0.055	0.676	890
	PT	-0.043	0.044	858
	PSDB	0.060	0.050	830
	MDB	0.030	0.039	980
B: Service Provider Firms	Working Class	-0.012	0.026	4,168
	Businessperson	0.005	0.033	4,156
	Age	0.020	0.077	3,893
	College	-0.019	0.030	4,546
	Woman	0.017	0.021	4,042
	Campaign Experience	0.036	0.033	4,162
	Campaign Expenditure	0.221*	0.121	4,100
	PT	0.008	0.025	4,074
	PSDB	0.029	0.026	4,228
	MDB	0.004	0.042	4,208
C: Coalition Affiliated Firms	Working Class	-0.025	0.024	3,962
	Businessperson	-0.006	0.017	4,280
	Age	-0.083	0.068	3,683
	College	-0.022	0.033	3,636
	Woman	0.016	0.023	3,684
	Campaign Experience	0.027	0.027	4,130
	PT	-0.002	0.011	3,510
	PSDB	0.015	0.026	4,200
	MDB	0.026	0.025	3,966

Note: Candidates characteristics are all balanced between treatment and control groups. This Table is summarized in Figure 1B. We use the Calonico et al. (2014) estimator. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IIIB: Regression Discontinuity Estimates of Balance Tests for Firms Characteristics for Each Sample

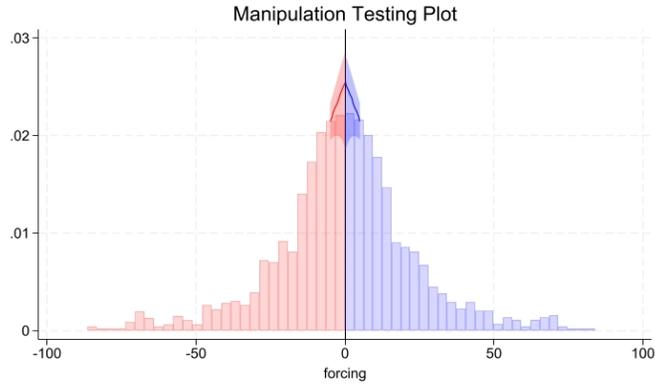
Panel	Firm Characteristic Variable	Coefficient	Robust Std. Error	Effective N
A: Donor Firms	Ln of Donations	-0.115	0.163	902
	Ln of Wage Bill	0.066	0.195	599
	Share of White	0.006	0.069	671
	Share of College	-0.002	0.024	604
	Share of Woman	0.020	0.028	616
	Ln of Number of Employees	-0.028	0.573	623
B: Service Provider Firms	Ln of Expenditure	0.100	0.079	3,956
	Ln of Wage Bill	0.042	0.073	2,485
	Share of White	0.012	0.023	2,924
	Share of College	0.004	0.011	2,686
	Share of Woman	-0.006	0.038	2,477
	Ln of Number of Employees	0.027	0.057	2,695
C: Coalition Affiliated Firms	Ln of Wage Bill	0.097	0.066	2,690
	Share of White	0.008	0.016	2,660
	Share of College	0.008	0.009	2,895
	Woman	-0.028*	0.017	3,399
	Ln of Number of Employees	0.054	0.049	2,711

Note: Firms characteristics are all balanced between treatment and control groups. This Table is summarized in Figure 2B. We use the Calonico et al. (2014) estimator and variables averaged at the candidate level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Observations in this Table are lower than in Table IIIB because firms characteristics are not available for all firms in our main data.

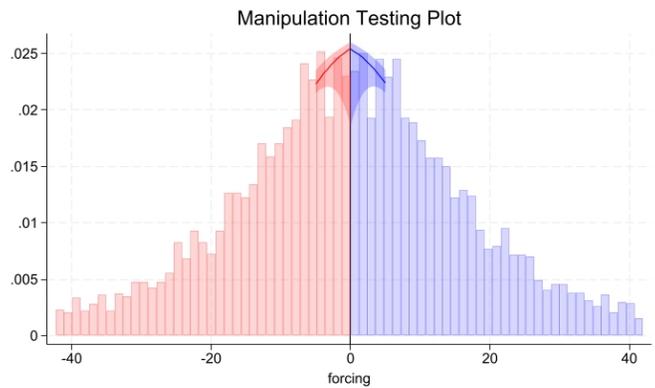
Table IVB: Regression Discontinuity Estimates of Balance Tests for Municipalities Characteristics for Each Sample

Panel	Municipality Characteristic	Coefficient	Robust Std. Error	Effective N
A: Donor Firms	N. of Firms	0.949	0.776	754
	Ln of Campaigns Total Revenues	-0.022	6.747	886
	Ln of Public Revenues	-0.006	0.155	966
	Ln of Public Expenditure	-0.007	0.154	938
	Ln of Population	-0.012	0.162	940
	Ln of GDP	0.012	0.211	910
B: Service Provider Firms	N. of Firms	5.680	7.069	3,344
	Ln of Campaigns Total Expenditure	0.225*	0.122	4,026
	Ln of Public Revenues	0.002	0.063	3,656
	Ln of Public Expenditure	0.001	0.063	3,636
	Ln of Population	0.001	0.072	3,694
	Ln of GDP	0.007	0.085	3,994
C: Coalition Affiliated Firms	N. of Firms	-5.689	47.412	2,072
	Ln of Public Revenues	0.001	0.064	3,584
	Ln of Public Expenditure	0.001	0.064	3,566
	Ln of Population	0.001	0.076	3,576
	Ln of GDP	0.006	0.077	4,272

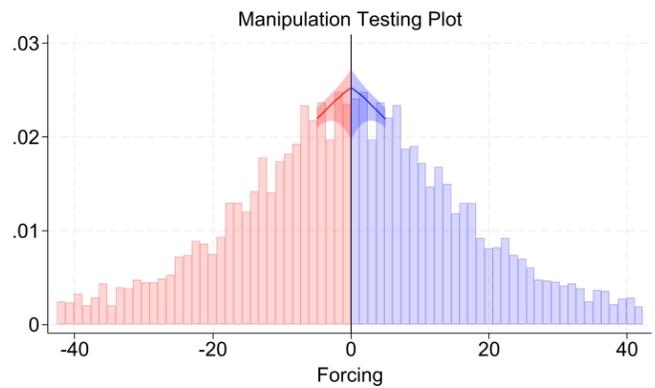
Note: Municipal characteristics are all balanced between treatment and control groups. This Table is summarized in Figure 2B. We use the Calonico et al. (2014) estimator and variables averaged at the candidate level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.



(a) Donor Firms



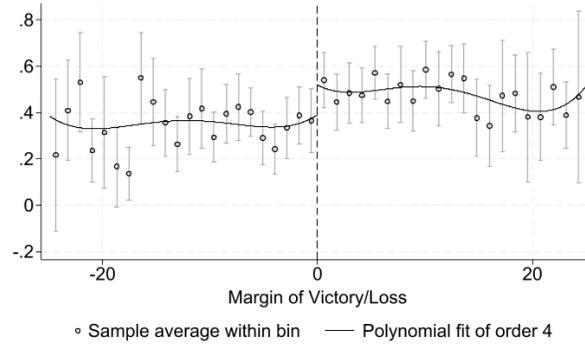
(b) Service-Provider Firms



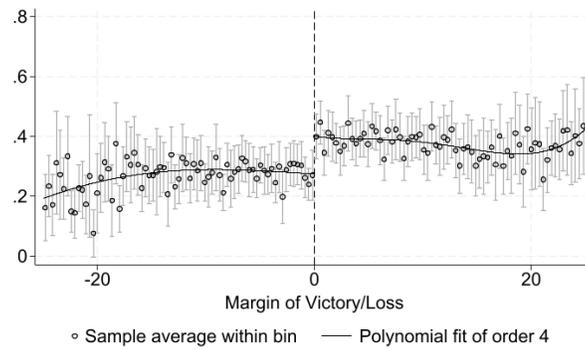
(c) Coalition Affiliated Firms Sample

Figure 5B: Candidates Densities' Margin of Victory/Loss for Donor, Service-Provider and Coalition Affiliated Firms Samples.

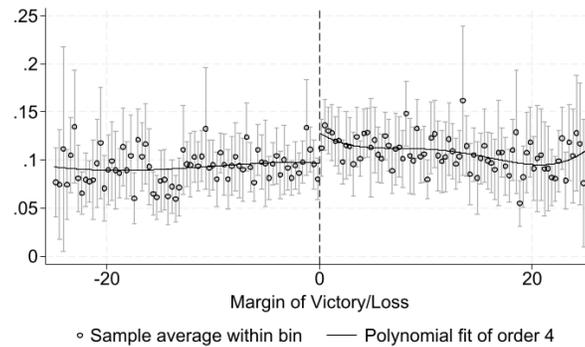
Note: Figures 5(a) to 5(c) display density estimates of the forcing variable around the threshold for three samples—donor, service-provider and coalition affiliated firms. No significant differences in densities are observed across the cutoff in any of them, providing evidence against manipulation of the forcing variable.



(a) Donors



(b) Service Providers



(c) Coalition Affiliated Firms

Figure 6B: Discontinuities in Firms' Probability of Having a Procurement Contract Using Default Binning and Polynomial Choices

Note: Figure 6B presents three panels using the default settings of the Calonico et al. (2015a) method, which selects the number of bins, bin width, and polynomial order in a data-driven way. Panel 6(a) corresponds to donor firms, 6(b) to service providers, and 6(c) to coalition affiliated firms. All panels exhibit visible discontinuities around the zero threshold, reinforcing earlier results based on fixed-bin regressions.

Online Appendix C

Table VB: Treatment Effects Estimates on Probability of Having Procurement: Constant Pool of States

	Polynomial Degree 1			Polynomial Degree 2		
	Default BW	Half BW	Double BW	Default BW	Half BW	Double BW
Panel A: Donors						
Coefficient	0.12*	0.12	0.12**	0.12	0.14	0.12*
Robust Std. Error	(0.06)	(0.09)	(0.06)	(0.08)	(0.15)	(0.07)
Bandwidth (h)	26.04	13.02	52.08	29.18	14.59	58.36
Eff. Observations	686	343	882	823	412	891
Average Outcome		0.42			0.42	
Panel B: Service Providers						
Coefficient	0.15***	0.14***	0.14***	0.16***	0.14***	0.14***
Robust Std. Error	(0.02)	(0.04)	(0.02)	(0.03)	(0.04)	(0.02)
Bandwidth (h)	14.07	7.03	28.13	25.18	12.59	50.36
Eff. Observations	3,557	1,778	4,104	4,032	2,016	4,382
Average Outcome		0.31			0.32	
Panel C: Coalition Affiliated						
Coefficient	0.02***	0.02	0.02***	0.02**	0.02	0.03**
Robust Std. Error	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
Bandwidth (h)	19.61	9.80	39.21	22.52	11.26	45.03
Eff. Observations	3,604	1,802	4,025	3,716	1,858	4,090
Average Outcome		0.10			0.10	

Note: The table presents treatment effects estimates for donors (Panel A), service providers (Panel B) and coalition affiliated firms (Panel C) using different bandwidths and polynomial orders. We use a balanced panel of states excluding those states present only on the 2012 election cycle. Point estimates do not change significantly. Coefficients are shown with robust standard errors in parentheses. */**/** indicate significance at the 10%/5%/1% levels respectively based on robust standard errors developed by Calonico et al. (2014). Effective observations are the sum of observations on both sides of the cutoff within each bandwidth.

Table VIB: Treatment Effects on Probability of Having Procurement: With and Without Firm Sector Fixed Effects

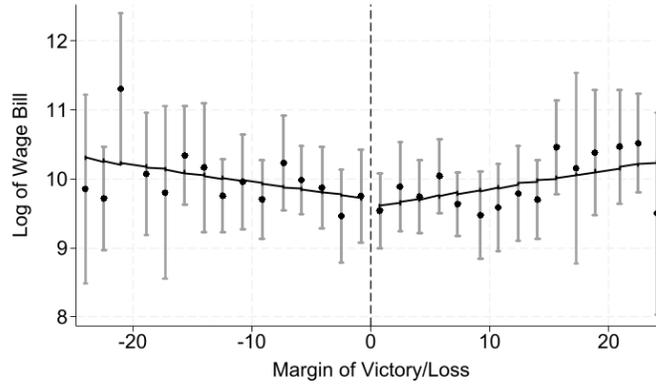
	Dependent Var: Prob. of Procurement					
	Margin < 10	Margin < 15	Margin < 20	Margin < 10	Margin < 15	Margin < 20
Panel A: Donors						
Coefficient	0.078	0.139**	0.035	0.076*	0.057	0.089**
Robust SE	(0.060)	(0.055)	(0.049)	(0.040)	(0.046)	(0.037)
Election FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	No	Yes	No	Yes	No	Yes
Avg. Outcome	0.366	0.366	0.369	0.369	0.361	0.361
Number of Obs	2,221	2,103	3,298	3,166	3,822	3,694
Panel B: Service Providers						
Coefficient	0.028	0.026	0.051	0.050	0.065*	0.064**
Robust SE	(0.044)	(0.042)	(0.037)	(0.035)	(0.034)	(0.032)
Election FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	No	Yes	No	Yes	No	Yes
Avg. Outcome	0.230	0.230	0.225	0.225	0.213	0.213
Number of Obs	38,199	38,134	53,620	53,565	66,955	66,901
Panel C: Coalition affiliated firms						
Coefficient	0.028*	0.025*	0.027*	0.025**	0.021*	0.020**
Robust SE	(0.015)	(0.013)	(0.014)	(0.012)	(0.011)	(0.010)
Election FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	No	Yes	No	Yes	No	Yes
Avg. Outcome	0.074	0.074	0.063	0.063	0.063	0.063
Number of Obs	189,080	189,053	309,688	309,668	371,902	371,883

Note: This table presents treatment effects on the probability of firms obtaining public procurement contracts with local linear polynomial regressions, across different bandwidths. The analysis is split into three panels: Panel A focuses on donor firms, Panel B on service providers, and Panel C on coalition affiliated firms. Estimates are generally more statistically significant when including firm sector fixed effects. Estimates are of similar magnitude for service providers and coalition affiliated firms, but greater when adding sector fixed effects for donor firms.

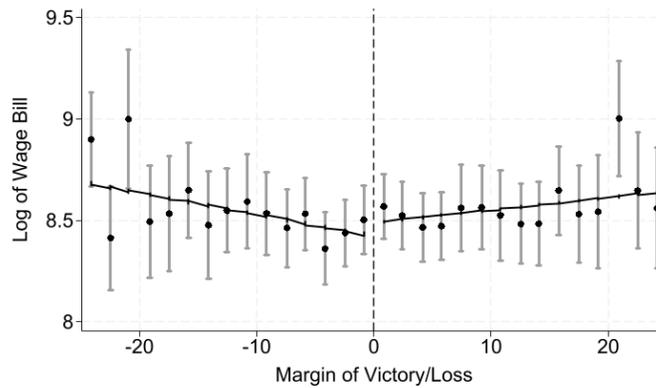
Table VIIB: Comparison of Means between Not Close and Close Races (margin within 5 percentage points)

Variable	Not Close Race	Close Race	Difference
Panel A: Donor Firms Sample			
N of donors	9.25 (11.24)	7.83 (8.46)	1.42
N of donors with procur.	3.32 (4.07)	2.54 (1.94)	0.78**
N of candidates	2.98 (1.16)	2.89 (1.12)	0.10
Ln of Pub. Revenues	17.90 (1.35)	17.55 (1.27)	0.35***
Ln of Pub. Expenditures	17.88 (1.36)	17.53 (1.25)	0.35***
Population	127,597 (699,183)	92,311 (505,533)	35,286
Ln of GDP	20.00 (1.73)	19.52 (1.66)	0.48***
Number of observations	548	151	-
Panel B: Service Providing Firms Sample			
N of service prov.	40.71 (173.92)	34.99 (119.62)	5.72
N of service prov. w/ procur.	6.75 (7.28)	6.70 (7.95)	0.05
N of candidates	2.68 (0.96)	2.57 (0.89)	0.11***
Ln of Pub. Revenues	17.07 (1.16)	16.89 (0.98)	0.18***
Ln of Pub. Expenditures	17.05 (1.16)	16.87 (0.98)	0.18***
Population	49,414 (363,666)	32,162 (234,041)	17,252
Ln of GDP	18.90 (1.58)	18.70 (1.35)	0.21***
Number of observations	2,419	722	-
Panel C: Coalition Affiliated Firms Sample			
N of connected firms	237.34 (1,170.69)	148.97 (460.64)	88.37*
N of connected firms w/ procur.	12.04 (20.10)	10.78 (13.34)	1.25
N of candidates	2.70 (0.96)	2.56 (0.89)	0.14***
Ln of Pub. Revenues	17.18 (1.17)	16.93 (0.98)	0.24***
Ln of Pub. Expenditures	17.16 (1.17)	16.91 (0.98)	0.24***
Population	52,797 (375,060)	32,968 (242,024)	19,829
Ln of GDP	19.09 (1.56)	18.80 (1.32)	0.29***
Number of observations	2,271	672	-

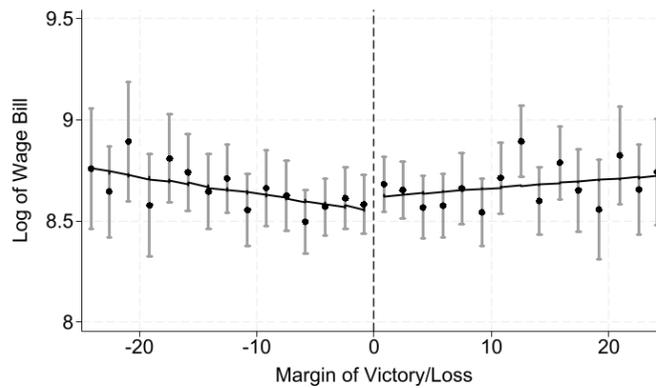
Note: This table presents means with standard deviations in parentheses. Close-race municipalities are defined by a winning margin of 5 percentage points or less. Differences are calculated from two-sample t-tests assuming equal variances. Significance levels: * p<0.1, ** p<0.05, *** p<0.01. Sample sizes: Panel A (Not Close: 548; Close: 151), Panel B (Not Close: 2,419; Close: 722), Panel C (Not Close: 2,271; Close: 672).



(a) Donor Firms



(b) Service Provider Firms



(c) Coalition Affiliated Firms

Figure 1C: Treatment Effects on Log of Wage Bill

Note: Figures 1(a), 1(b), and 1(c) use the log of the wage bill as the outcome and employ the method of Calonico et al. (2015a) to generate the figures. Panel (a) refers to donor firms, Panel (b) to service provider firms, and Panel (c) to coalition affiliated firms. The Y-axis shows the log of the wage bill of the average firm tied to candidates, and the X-axis shows the candidates' margin of victory/loss. In all panels, there is evidence of null treatment effects on the wage bill of the average firm.

Table IC: Treatment Effects Estimates on log of Employees

	Polynomial Degree 1			Polynomial Degree 2		
	Default BW	Half BW	Double BW	Default BW	Half BW	Double BW
Panel A: Donors						
Coefficient	-0.10	-0.08	-0.20	-0.01	-0.19	-0.08
Robust Std. Error	(0.40)	(0.14)	(0.68)	(0.23)	(0.25)	(0.49)
Bandwidth (h)	20.95	10.47	41.89	24.16	12.08	48.33
Eff. Observations	669	412	889	739	456	926
Average Outcome		3.00			3.02	
Panel B: Service Providers						
Coefficient	0.09	0.09	0.05	0.11	0.07	0.09
Robust Std. Error	(0.07)	(0.17)	(0.05)	(0.10)	(0.34)	(0.07)
Bandwidth (h)	21.65	10.83	43.30	23.53	11.76	47.05
Eff. Observations	2911	1852	3695	3054	1972	3757
Average Outcome		1.80			1.81	
Panel C: Coalition						
Coefficient	0.07	0.10	0.06	0.08	0.13	0.08
Robust Std. Error	(0.08)	(0.09)	(0.06)	(0.09)	(0.13)	(0.08)
Bandwidth (h)	16.31	8.15	32.61	29.20	14.60	58.39
Eff. Observations	2878	1675	4014	3868	2675	4536
Average Outcome		1.87			1.88	

Note: Table III reports local polynomial RD estimates of the effect of having a connection marginally elected on the log of number of employees a Firm has. Estimates are computed using the bias-corrected method of Calonico et al. (2014, 2015b), with clustered standard errors at the municipality-election level and election fixed effects. Each panel shows results for a different firm sample. Columns vary around the optimal bandwidth (half, default, double) and use polynomials of degree 1 and 2. The outcome mean is 3.00 in Panel A (Donors), 1.80 in Panel B (Service Providers), and 1.87 in Panel C (Coalition Affiliated Firms).

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