Expiring Budgets and the Return of Political Connections: Evidence from Brazil.[†]

Akin Mert[*]

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Advisor:
Alexsandros Cavgias
Universitat de Barcelona

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Abstract

Do fiscal policy regulations affect the degree of political favouritism in the allocation of public resources? To answer this overlooked question, we investigate whether and how the interaction of two customary practices of democracies, namely campaign financing by corporate contributions and the presence of expiring budgets, affect the allocation of procurement contracts across firms in the context of Brazil. Using OLS regressions with fixed effects, we document three complementary findings. First, we document that procurement activity peaks in the last quarter of the fiscal year in Brazil. Second, we find that contributing firms win between 11.6 and 13.4 per cent more procurement tenders than non-contributing ones. Third, our preferred specification shows that the return of campaign contributions increase 2.65 p.p. in the last quarter, which corresponds to 20 per cent of its most pronounced estimate. Our main findings are robust to very stringent fixed-effect specifications that eliminate biases caused by omitted firm-specific trends across years and sector- and town-specific trends across quarters. Our findings reveal a previously unknown link between fiscal policy regulations and political favouritism.

Keywords: Expiring Budgets, Political Contributions, Return of Campaign Contribution, Political Favouritism
JEL Classifications: H61, H57, D72

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1   Introduction

Financing elections and procuring goods are universal features of most governments. Public procurement is an economically relevant outcome, accounting for up to 25 per cent of the worldwide GDP (World Bank, 2017). In addition, most countries allow corporate contributions to finance political campaigns, as only 46 of 180 countries ban corporate contributions to electoral campaigns or political candidates (IDEA, 2020). Therefore, understanding whether campaign financing regulations and fiscal policy rules affect procurement outcomes is a very relevant question from a public policy perspective.

The procurement process is not always evenly distributed across time within the fiscal year calendar. Popular terms like *March Madness*, *December Fever*, and *Spring Sale* have become popular terms to describe periods when the government demand is peaking. More recently, academic studies have documented pronounced spikes in public spending at the end of a fiscal year in Canada (Hurley et al., 2014), United States (Liebman and Mahoney, 2017), Germany (Fitzenberger et al., 2016) and the United Kingdom and Northern Ireland (Baumann, 2019). Moreover, using a sample of OECD countries, Eichenauer (2020) finds that 30% of the public spending happens in the last quarter of a fiscal year. A priori, such spikes in the volume of public procurement may affect the marginal costs and benefits of public organizations and firms participating in the procurement process and, in turn, change procurement outcomes and welfare.

While extensive literature has documented that corporate contributions increase the number of procurement contracts won by contributing firms (Baltrunaite, 2019; Boas et al., 2014; Titl and Geys, 2019), whether and how such influence depends on institutional factors such as fiscal policy regulation is so far an open question. Moreover, the extent and consequences of the end of the year spending in developing countries remain open questions to our knowledge. Using Brazil as a laboratory, we answer three questions to help to fill such literature gaps:

- How extensive is the incidence of end-of-the-year spending in Brazil?

- Do firms that contribute to electoral campaigns win more procurement contracts?

- How do the spikes of end-of-the-year spending affect the returns of campaign contribution of firms?

Brazil is an ideal laboratory to answer these three research questions. First, corporate contributions were the primary source of campaign revenue in Brazil between

2007 and 2014, covering around 75% of campaign spending in national elections (Cavgias and Granella, 2020). Second, previous studies have found evidence for positive returns of campaign contribution in Brazil (Boas et al., 2014; Arvate et al., 2018; Claessens et al., 2008). Third, there are expiring budgets in Brazil (*Lei nº 4.320 64*), which we show to cause spikes at the end of the year in the results section. Finally, Brazil offers granular data about the universe of purchase acts from the federal government and identified data with Brazil's universe of political contributions.

Expiring budgets may cause spikes at the end-of-the-year spending for different reasons. Recent contributions argue that precautionary savings (Liebman and Mahoney, 2017) and procrastination (Baumann, 2019) are the main reasons behind the observed spikes in public spending at the end of the year. In the Brazilian context, we speculate that precautionary saving by the federal government caused by tax revenue uncertainty and primary surplus targets is a plausible explanation behind the peaks in procurement activity at the last quarter of the fiscal year.

Expiring budgets may increase the return of political connections by making the opportunity cost of exhausting the budget close to zero for public officials. Hence, they create a window of opportunity for public officials to allocate contracts to firms in exchange for campaign donations at a lower marginal cost for the public administration. Contributing firms may also perceive such a window of opportunity for *quid pro quo* exchanges and concentrate their procurement activities during demand peaks. Besides, decreases in auditing capacity and media attention at the end of the year may facilitate political favouring.

We provide three complementary pieces of evidence. First, we collapse data about the universe of procurement tenders by the Brazilian federal government to describe the distribution of the number of purchase acts across quarters. Second, we merge such data with information about the universe of national campaign contributions to estimate returns of political connections in Brazil using OLS regressions with fixed effects. Third, we use such fixed-effect models to estimate the distribution of the return of campaign contributions across peak and non-peak quarters. We document three main findings. First, we find that the number of purchase acts monotonically increases across the year and peaks at the last quarter of the fiscal year. +Second, we show that contributing firms win between 11.6 to 13.4% more public tenders than non-contributing ones. Third, we find that the return of contributing firms is 2.65 p.p. more extensive in the last quarter of the fiscal year. Hence, the return of campaign contributions increases 24.6 per cent in the fourth quarter.

We take two main steps to probe the robustness of our results. First, we show

that the increase in the return of contributions at the last quarter remains stable when estimated using very stringent specifications (with firm, firm-year, sector-quarter, and location-quarter fixed effects), which reduces the likelihood of omitted variable biases. Second, we show that results remain qualitatively similar when using a broader measure of political connections and outcomes measuring the allocation of contracts at the extensive margin.

Our paper is related to three strands of literature. First, we communicate with recent literature studying the consequences of end-of-the-year spending (Liebman and Mahoney, 2017; Fitzenberger et al., 2016; Klymak and Baumann, 2021). While such literature investigates the incidence of end-of-year spending in OECD countries, it is silent about the extent of such phenom in developing countries. To our knowledge, we provide the first time to find evidence for heightened year-end spending in a developing nation, which we show to have similar magnitudes to the ones estimated in OEDC countries [1]. While previous studies have focused on how end-of-the-year spending affects prices (Klymak and Baumann, 2021), quality of the purchased goods (Liebman and Mahoney, 2017), or effectiveness of public services (Fitzenberger et al., 2016), we find evidence for a nonobvious and yet unexplored relationship between end-of-the-year spending and political favouritism.

Second, we communicate with the extensive literature about the returns of political connections (Tahoun, 2014; Goldman et al., 2013; McMenamin, 2013; Szakonyi, 2020; Faccio, 2006; Fisman, 2001; Knight, 2006). Recent research within this literature has shown that political campaign contribution can be beneficial for firms (Boas et al., 2014; Baltrunaite, 2019; Titl and Geys, 2019; Claessens et al., 2008; Carvalho, 2014; Arvate et al., 2018; Ansolabehere et al., 2003; Fowler et al., 2020). Despite extensive evidence that firms benefit from political connections, this literature is still relatively silent about which institutional factors influence the return from connections. We contribute to this literature by showing that budgeting rules inflate the benefits received by corporate campaign contributors in Brazil.

Finally, we contribute to the literature investigating the empirical determinants of public procurement outcomes. Most of this literature has focused on the role of the supply side of procurement - i.e., the influence of firms' and markets' characteristics on procurement outcomes, paying less attention to the demand side of public procurement - i.e., how public organization characteristics' influence procurement outcomes. Recent papers started to investigate the role of demand factors such as competition

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[1]The magnitude of the end-of-the-year spikes in Brazil is very similar to the OECD spikes (Fitzenberger et al., 2016).

([McAfee and McMillan](), [1989](); [Vagstad](), [1995](); [Bajari and Tadelis](), [2001]()), price policy
([Bandiera et al.](), [2009]()), electronic procurement ([Lewis-Faupel et al.](), [2016]()), type of con-
tracts ([Bajari and Tadelis](), [2001]()) or preferences for firms with certain characteristics like
reputation and discretion ([Banerjee and Duflo](), [2000](); [Coviello and Mariniello](), [2014]()).
Another contribution of this paper is that we add a new aspect on the demand side of
public procurement, namely the importance of the budget calendar.

2   Institutional Background

2.1   Electoral campaign financing in Brazil

Brazil is a presidential democracy with a bicameral congress. The election cycle lasts
for four years in national and local elections, but local elections happen two years af-
ter the national ones. Members of the executive power (i.e., president, governors, and
mayors) may reelect only once. In contrast, members of the legislative power (i.e.,
senators, deputies, and city councillors) face no reelection constraints, creating room
for repeated interaction with campaign sponsors. Two-round majoritarian elections
define the executive positions, except in cities with fewer than 200 thousand inhabi-
tants, where runoffs are not allowed. An open-list proportional representation system
defines the legislative positions.

The Brazilian Supreme Court banned corporate contributions to political campaigns
in September of 2015 amid a massive corruption scandal, known as *Lava Fato*, involv-
ing allocating overpriced contracts from Brazil's state-own oil company to corporate
contributors. In an attempt to prevent the corporate ban, congress passed a law en-
forcing campaign spending limits in 2015, which has been shown to increase political
competition in Brazil ([Avis et al.](), [2017]()). Due to both reforms, we focus our analysis on
the years between 2006 and 2014, the pre-reform years.

Even after such reforms, campaign contributions are practically unlimited in Brazil.
Before the corporate ban of 2015, corporations could donate 2% of their gross annual
revenues. Besides, individuals can still contribute up to 10% of their annual gross
personal income. Hence, contribution limits are very generous, especially for wealthy
donors.

Any form of campaign contribution can only occur during the campaign period,
which starts 90 days before the elections and must be publicized a few days after be-
ing completed. The contribution data is published online on the electoral authority's

website, creating a suitable environment for high-quality empirical investigations.

The Brazilian political system is prone to expensive campaigns. First, the open list system with proportional elections promotes competition between candidates within the same coalition, increasing the need for individual campaign spending. Second, the party system is weak and disorganized (Bourdoukan, 2010; Samuels, 2001). The missing support, logistically and financially, of well-organized party structures implicates that candidates are urged to bear the campaign costs by their own in order to complete a successful candidacy. Due to the weak party structures political influence is rarely policy-induced and ideological (i.e. the "pro-guns" lobby in the U.S.). In Brazil, corporations expect governmental services and benefits in exchange for their campaign contributions (Samuels, 2001), since political influence is mainly service-induced. Third, political contributions are serious investment decisions by corporations to find their way to influence politics since business associations are weakly organized (Schneider et al., 2004; Schneider, 2010). These organization are incapable to represent business interests in politics. Thus, a different channel of political influence, namely campaign contributions, evolved.

Not surprisingly, electoral campaigns are expensive in Brazil. In 2014, campaign expenses of the national elections corresponded to approximately 0.15% of the GDP (Cavgias and Granella, 2020). Costs are exceptionally high in legislative elections, as the average campaign cost for electing a congress member was more than R$ 1.4 million in 2014. In comparative terms, the cost per vote in Brazil ($23) is higher than in the United States ($20) (Pearson and Trevisani, 2017).

Contribution by private influence groups financed such expensive political campaigns in Brazil. Between the 2006 and 2014 elections, corporate contributions funded approximately 75% of the total campaign expenses in national elections while they corresponded for nearly 50% of the total campaign expenses local elections. In contrast, the share of personal contributions over total campaign revenues is modest in national (14%) and local (38%) elections (Cavgias and Granella, 2020).

The influence of the most prominent influence groups is disproportional in Brazil. For instance, the highest 5% of contributions in national elections correspond to three-quarters of the total contributions. The disproportional influence of most extensive contributions is at odds with countries like the US, Canada and Germany, where most of the campaign revenue comes from small contributions (Bouton et al., 2018).

Brazil is a context where corporate contributions are likely buying influence in allocating government resources. Boas et al. (2014) find evidence that contributing firms

obtain more government than non-contributing ones, in magnitude correspond to 14 to 39 times the average contribution. Likewise, Cavgias and Granella (2020) show that contributing firms get between 20 to 25 per cent more government contracts. Besides, contributing firms also obtain more subsided credit from the Brazilian development bank (BNDES) (Claessens et al., 2008; Lazzarini et al., 2015).

2.2   Public procurement in Brazil

Public procurement consumes a relatively large fraction of public resources worldwide. For instance, it consumed 4.2 trillion euros among the OECD in 2013, around 12% of the GDP and one-third of governmental expenditure (OECD, 2016). The picture is not different in Brazil, where public procurement accounted for about 9% of the GDP in 2008 (OECD, 2012).

The Public Procurement Act (Law 8666 of 1993) regulates the allocation of public contracts to firms in Brazil. The law establishes two main guidelines for public procurement. First, except for tenders of small values and specific cases, goods and services should be procured by competitive price-auctions open to any supplier without legal impediments. Second, most goods and services should be procured in non-presential electronic auction auctions. Except for large infrastructure projects, standardized goods and services have auctions using the online platform *ComprasNet*, which constitutes the data set used in this paper. *ComprasNet* is the most relevant tool to procure standardized goods, comprising almost half [2] of all federal government tenders by the year 2010 (Szerman, 2012).

The fiscal year coincides with the calendar year in Brazil. Hence, the fiscal year starts in January and ends in December. According to the Brazilian law, textitLei n° 4.320 64, budgets of public organizations expire at the end of the fiscal year. As a consequence, unspent budgets get lead back to the budget distributor at the end of the fiscal year.

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[2]Szerman (2012) finds that around 46% of the total number of tenders were procured in *ComprasNet*.

3 Theory

3.1 Theories of end-of-the-year spending

Despite being observed in most developed countries (Eichenauer, 2020), there is still no consensus about which mechanisms explain the spikes in public spending at the end of the fiscal year. By now, the literature proposes two main explanations for such phenom: precautionary saving (Liebman and Mahoney, 2017) and procrastination (Baumann, 2019).

Liebman and Mahoney (2017) develop a two-period principal-agent model where the principal is the Congress, who determines the budget, and the agent is a government agency, who implements the budget. The fiscal year has two periods: 1 (start of the fiscal year) and 2 (end of the fiscal year). To mirror the US budgeting process, the model assumes there are expiring budgets, meaning that the part of the budget not consumed by the last day of the fiscal year returns to Congress. As the agent receives no utility if the budget returns to Congress, she will exhaust all remaining budget at the end of the fiscal year.

Public spending has decreasing marginal returns in the production of public goods in both periods. Besides, there is uncertainty about the marginal productivity of the public spending in both periods, which can be caused either by a demand-side (e.g., pandemic, natural disaster) or supply-side shock (e.g., unexpected increase in the price of public goods). The main result of the model states that if the utility of the agent allows for precautionary saving, she will spend more in the second period than in the first one, which captures the spikes of public spending at the end of the fiscal year observed in the data.

In the Brazilian context, precautionary saving is a plausible mechanism behind the pronounced end of the year spending. In contrast to Liebman and Mahoney (2017), the source of precautionary saving is likely caused by the national government's fiscal constraints instead of the public organizations individually. As Brazil has high internal debt, the federal administration subject itself to primary surplus targets to enforce a sustainable debt trajectory and push interest rates down. Despite the primary surplus targets, the budget allocation happens when tax revenue is still uncertain. Given the need to enforce such targets, the national government has mechanisms to suspend public spending within a fiscal year while tax revenue remains uncertain and cutting spending when primary surplus targets become unfeasible.

By arguing that precautionary saving is an argument for expending the entire bud-

get within a fiscal year but not at its final months, Baumann (2019) develops a different approach to understand the existence of year-end spikes, namely procrastination. Similarly to Liebman and Mahoney (2017), there are two periods, and decreasing marginal returns on the production of public goods. Public spending in each period depends on effort from public officials, which causes disutility to them. The public official's payoff depends on a performance evaluation happening at the end of the year, which depends on the production of the public good in both periods. The main results of the model state that if public officials discount effort over time, they will spend more and exert more effort in the second period than in the first one. In other words, the model can capture both spikes of public spending at the end of the fiscal year and procrastination, which is consistent with recent evidence from patent offices in the United States (Frakes and Wasserman, 2020).

There is convincing evidence that end-of-year spending causes welfare losses. Using procurement data from Ukraine, (Klymak and Baumann, 2021) show that prices are 2.5% higher at the end of the year, which should translate into considerable welfare losses because public procurement consumes a high share of the GDP. Besides, Liebman and Mahoney (2017) find that end-of-the-year spending causes low-quality spending in the context of IT projects of US federal agencies and low-quality services in the context of training programs for unemployed people Fitzenberger et al. (2016).

3.2   Mechanisms

There are static and dynamic incentives that make public officials consume all the budget before it expires. Since the population usually prefer a higher consumption of public goods, exhausting the budget increases the utility of mission- and politically-oriented bureaucrats. Besides, public officials also face dynamic incentives. First, not consuming all the budget may signal bureaucratic incompetence, which damage the career progression of public officials. Second the ratchet effect - i.e., the inertia in the budgeting process - may force bureaucrats to exhaust the budget within a fiscal year to prevent losing resources in the next year. Altogether, such static and dynamic forces make the opportunity cost of the public spending near-zero or, in some cases, negative at the end of the fiscal year.

If bureaucrats are politically oriented, several mechanisms may explain why expiring budgets may increase the return of political connections at the end of the fiscal year. First, the near-zero opportunity cost of public spending opens the possibility for pleasing political groups at a low marginal cost for the public administration. Sec-

ond, the spike in public contracts may prevent auditing mechanisms from identifying suspect contracts, facilitating political allocation from politically oriented bureaucrats. Third, if the fiscal year expires during holidays, a decrease in public attention to the budgeting process near the holidays may facilitate favouritism. Forth, public officials may purposely procrastinate the date of some tenders to the end of the fiscal year when public attention and audit mechanisms are weaker. Fifth, bureaucrats may have more discretion on the type of public spending procured at the end of the fiscal year (e.g., low priority items).

4   Data

4.1   Data Source

We use three primary data sources in our analysis. First, we use data from the *ComprasNet* system shared by the *Conselho Administrativo de Defasa Econômica* (CADE), the Brazilian antitrust authority.[3] *ComprasNet* data includes detailed information about the universe of federal government purchase acts. Second, we use data from the Tribunal Superior Eleitoral (TSE), the Brazilian electoral authority, with detailed information about the universe of campaign contributions to federal (2006, 2010, 2014) and local elections (2004, 2008, 2012). Third, we use data from the *CNPJ Aberto*, the Brazilian national registry of firms, with information about the universe of formal sector firms.

4.2   Variables

We compute three main sets of variables. First, using *ComprasNet* data, we compute our outcome variables: the number of procurement tenders won and an indicator for winning a tender for each firm-year-quarter. Second, we compute our treatment variables using TSE data: indicators equal to one when a firm made a corporate contribution in any (national) election held during our analysis period. We purposely construct time-invariant treatment to capture exchanges of (i) present campaign contributions for promised future contractual benefits and (ii) promises of future campaign contributions for present contractual benefits. Third, using *CNPJ Aberto* data, we compute location (region and municipality) and industry (2 and 5 digits) fixed effects based on

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[3]We thank Bruno Duarte Garcia and Felipe Leitão Valadares Roquete for gently sharing a complete version of the *SIASG Comprasnet* Data Warehouse.

information about the firm headquarter.

4.3   Data Sample

Between 2007 and 2014, the *CmprasNet* system procured 18.848.737 purchase acts involving 91 public organizations (layer 1). We collapse the universe of purchase acts between 2007 and 2014 at the year-quarter level to describe the intensity of end of year spending in Brazil. Despite not being uniformly distributed within the fiscal year, the number of purchase acts is relatively stable across fiscal years in our sample.

We make two main steps to select the firms used to estimate the return of political connections. First, we restrict the analysis to a set of potential government suppliers - i.e., firms who participated in any procurement auction between 2004 and 2018. Second, to have a perfectly balanced panel, we exclude firms that shut down between 2004 and 2014. By taking these two steps, we can focus on a subsample more relevant for fiscal policy studies without working with a vast dataset with a tiny treatment group (less than 1% of the firms).

There are 97.522 firms in the sample of potential government suppliers. In addition, 7.894 firms contributed to a national election and 13.405 to national or local elections. Hence, our treatment group corresponds to around 8 to 14 per cent of the sample. We compute a firm-year-quarter level dataset for the 97.522 firms for four quarters per year for eight years (2007 to 2014), accounting for 3.120.704 observations.

4.4   Descriptive statistics

Table 1 presents a summary of the outcome, treatment and control variables in our data set. Panel A of Table 1 shows the outcome variables. On average, 14.4 per cent of the firms won at least one tender. Panel B demonstrates that 8.1 per cent of the firms contributed to a national electoral campaign while 13.7 per cent them to a national or regional electoral campaign. In Panel C, we show the distribution of the firms according to their economic sector. In order, the three main economic sectors participating in *CmprasNet* auctions are *Trade* (52%), *Services* (21.4%), and *Low-tech manufacturing* (11.9%), the leading suppliers of off-the-shelf goods to the national government. Finally, panel D reports statistics about the geographical distribution of the corporations. Most *CmprasNet* suppliers have headquarters in the *South-east* (42.6%) and *South* (25.3%) regions, Brazil's more relevant economic regions.

Table 1: Summary statistics of the dataset

Variable	Mean	Std. Dev.	Min.	Max.
Panel A: Outcome Variables				
Log(1+ number of tenders won)	0.254	0.753	0	9.307
Dummy = 1, if tender won	0.144	0.351	0	1
Panel B: Treatmen Variables				
Contribution to national election	0.081	0.273	0	1
Contribution to national and local election	0.137	0.344	0	1
Panel C: Economic Sector				
Agriculture	0.006	0.079	0	1
Low-Tech Manufacturing	0.119	0.323	0	1
High-Tech Manufacturing	0.047	0.211	0	1
Construction	0.051	0.219	0	1
Trade	0.523	0.499	0	1
Transport/ Utilities/ Communication	0.04	0.197	0	1
Services	0.214	0.41	0	1
Panel D: Geographical Region				
North	0.067	0.25	0	1
North-East	0.175	0.38	0	1
South-East	0.426	0.495	0	1
South	0.253	0.435	0	1
Central-West	0.079	0.27	0	1

Note: This table shows the summary statistics of our dataset. Panel A shows the outcome variables which were obtained from the ComprasNet system by the Brazilian antitrust authority (CADE). Panel B entails the treatment variables of contribution which we obtained from the the Brazilian electoral authority (TSE). Panel C and Panel D display information about the economic sector of the firms on a macro-sector level and the geographical location of the firms on a regional level. This information was obtained by the Brazilian national registry of firms (CNPJ Alberto).

5 Methodology

5.1 End-of-the-year spending

We start by testing the economic hypothesis that the frequency of procurement tenders peaks in the last quarter of the year. Then, we proceed in two steps. First, we describe the aggregate number of purchase acts distribution across the four quarters of the year. Second, we estimate the following regression model to provide an statistical test for such economic hypothesis:

$$\log(1 + num\_tenders_{y,q,o,g}) = \alpha_y + \alpha_o + \alpha_g + \sum_{k \neq 1} \delta_k \cdot \mathbb{1}(q = k) + \epsilon_{y,q,o,g} \qquad (1)$$

where $\log(1 + num\_tenders_{y,q,o,g})$ measures the log number of tenders in quarter q in a year y made by public organization o buying good g plus one. $\mathbb{1}(q = k)$ is an indicator variable equal to one when $q = k \in \{2, 3, 4\}$. α_y, α_q, α_o, and , α_g capture, respectively, year, quarter, public organization and good fixed effects. We use the first quarter as the reference period and robust standard errors.

By documenting increasing coefficients across quarters (i.e., $\widehat{\delta_4} > \widehat{\delta_3} > \widehat{\delta_2} > \widehat{\delta_1} = 0$), we show that there are spikes at the end of the fiscal year. Besides, we show that such a pattern holds when comparing purchase acts within the same year, done by the same organization, and buying the same type of good.

5.2 Returns of campaign contribution

After describing the aggregate frequency of procurement tenders across quarters, we test the economic hypothesis that the contributing firms win more than non-contributing ones. In other words, we measure the return of campaign contributions in terms of the numbers of procurement tenders won by firms. To estimate the return of campaign contributions, we estimate the following regression model

$$\log(1 + num\_tenders_{f,q,y}) = \alpha_{sector(f),q,y} + \alpha_{city(f),q,y} + \gamma contributing_f + \epsilon_{f,q,y} \quad (2)$$

where $\log(1 + num\_tenders_{f,q,y})$ measures the log number of tenders won by a firm f in a quarter q in a year y plus one. $\alpha_{sector(f)}$ and $\alpha_{city(f)}$ capture, respectively, firm economic sector and firm location (headquarter) fixed effects. We cluster standard errors at the firm level. Our coefficient of interest γ measures, in percentage points, how much more procurement tenders contributing firms win concerning the non-contributing ones. As we estimate a simple difference of outcome averages, we do not claim a causal interpretation for γ. Instead, we argue that a γ with a high magnitude is suggestive of a context where corporate contributions buy influence, as suggested by their importance as a source of campaign revenue.

5.3 Effect of the peak quarter on the return of campaign contributions

We investigate if the return of campaign contributions increased when the demand peaks at the last quarter of the fiscal year by estimating an OLS model with fixed effects. We estimate by comparing the log-number of procurement tenders won by contributing and non-contributing firms across peak and non-peak quarters restricting

the comparison to firms in the same economic sectors and geographical location. More precisely, we estimate the following regression model

$$\log(1 + num\_tenders_{f,q,y}) = \alpha_f + \alpha_{q,y} + \beta_1 contributing_f + \beta_2 \cdot peak_q$$
$$+ \beta_3 contributing_f \cdot peak_q + \epsilon_{f,q,y} \tag{3}$$

where $\log(1 + num\_tenders_{f,q,y})$ measures the log number of tenders won by a firm f in a quarter q in a year y plus one. α_f and $\alpha_{q,y}$ capture, respectively, firm fixed effects and year-quarter fixed effects. $contributing_f$ is a dummy variable and equals one if a firm has contributed to a political campaign. Likewise, $peak_q$ equals one if the tender has a reference date on the peak quarter (the fourth) and zero otherwise.

Our coefficient of interest is β_3 which measures how the difference in the number of procurement tenders won by contributing firms concerning the non-contributing changes in the peak quarter. In other words, β_3 measures the effect of peak demand on the return of campaign contributions. β_3 has causal interpretation under the orthogonality condition

$$Cov[contributing_f \cdot peak_q, \epsilon_{f,q,y} | \alpha_f, \alpha_{q,y}] = 0.$$

Such a condition is restrictive in our setting because it imposes that all time-variant firm characteristics related to procurement choices are orthogonal to how procurement outcomes vary across fiscal years and across quarters within the same fiscal year.

We take two main steps to probe the plausibility of our identifying hypothesis. First, we implement specifications with firm and firm-year fixed effects to exclude biases from omitted time-variant firm characteristics and firm-specific trends across years. Second, we estimate specifications with sector-quarter and municipality-quarter fixed effects to decrease the probability of biases caused by omitted firms' characteristics that vary across quarters explain our results. Hence, by checking the stability of our coefficient across specifications with more restrictive identifying variation, we can understand whether the remaining biases are from omitted variable bias are plausible or not.
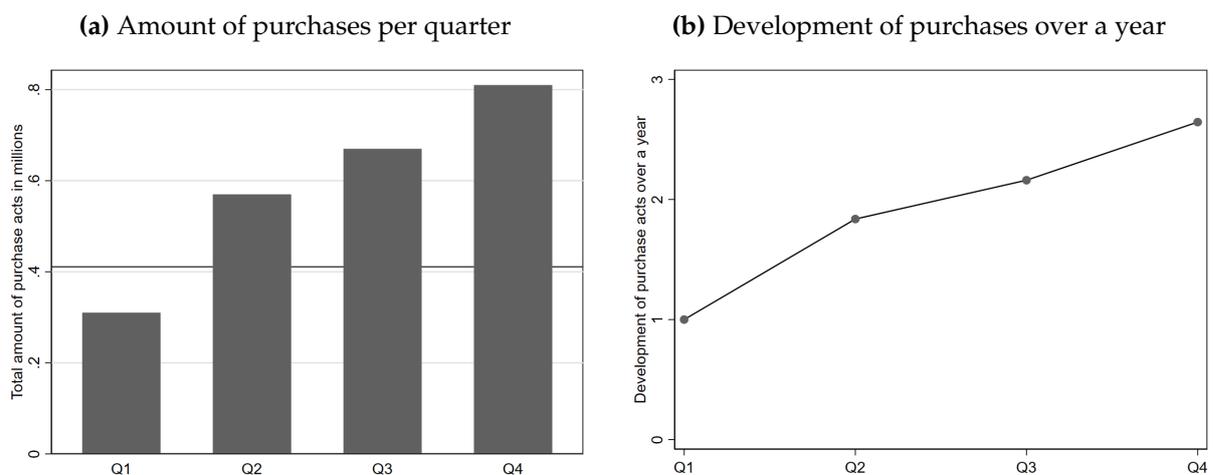
6   Results

In this section, we proceed in three steps. First, we describe the distribution of the number of purchase acts in the *CompraSNet* system across quarters of the fiscal year. Second, we use the OLS regression model of equation 2 to estimate the return of cam-

paign contribution as the number of procurement terms won by contributing firms in excess to non-contributing ones (in percentage points). Finally, we use the OLS regression model of an equation 3 to measure how the return of campaign contributions changes at the last quarter of the fiscal year, which is when the number of purchase acts peaks.

6.1 End-of-the-year spending in Brazil

Figure 1: Number of auctions peak in the last quarter in Brazil

(a) Amount of purchases per quarter   **(b)** Development of purchases over a year

Note: In figure 1 we display descriptive evidence about the distribution of the number of purchase acts across the quarters using data from the *ComprasNet* system of the years 2007-2014. In panel (a) each column represents the yearly average of purchase acts done in that quarter. In panel (b) we display the average development of purchase acts relative to the first quarter over the other three quarters. Both panels show evidence for a spike in spending in the final quarter of the fiscal year.

Figure 1 displays descriptive evidence about the number of purchase acts in the CompresNet system across quarters of the fiscal year. Panel A shows the yearly average (2007-2014) of the number of purchase acts across the four quarters of the fiscal year. To see how a counterfactual uniform distribution would look, we plot the yearly average (2007-2014) of the number of purchase acts per quarter with a red line. To have a sense of the magnitude of the peak, panel (b) plots the number of purchase acts across the four quarters of the fiscal year relative to the same variable in the first quarter - i.e., $\frac{num\_tenders_q}{num\_tenders_1}$ for $q \in \{1, 2, 3, 4\}$.

The resulting graphic patterns show an intense peak at the end of the year. Panel (a) shows that the fourth quarter hosts, on average, 318.747 thousand purchase acts per year, which corresponds to 32.5% of the year total and 7.5% more than what we would expect with purchase acts evenly distributed across quarters. Panel (b) shows that the

total number of purchase acts in the ComprasNet system is almost three times that observed in the first quarter. In both panels, we can see that the number of purchase acts increases monotonically across quarters, suggesting the changes in the costs and benefits of public purchases caused by expiring budgets are continuous functions of the distance to the end of the fiscal year.

6.2 Returns of campaign contribution

Table 2 shows the estimates of γ described in equation 2. In column (1), we report the estimate of γ in the model without fixed effects, which measures the unconditional difference in outcome means between contributing and non-contributing firms. To have a sense of whether omitted variables explain such positive difference in means, in columns (2) to (6), we sequentially add year quarter, 2-digit sector, 5-digit sector, region, and (headquarter) municipality fixed effects to the specification in column (1).

Table 2: Contributing firms win more procurement tenders than the non-contributing ones - i.e., there are positive returns of contributions

	(1) Log # of tenders	(2) Log # of tenders	(3) Log # of tenders	(4) Log # of tenders	(5) Log # of tenders	(6) Log # of tenders
Contributing	0.116***	0.116***	0.134***	0.132***	0.123***	0.127***
	(0.00775)	(0.00775)	(0.00766)	(0.00733)	(0.00728)	(0.00731)
Observations	3082656	3082656	3082656	3082656	3082656	3082656
R-squared	0.00175	0.00792	0.0393	0.0743	0.0760	0.0959
Year Quarter FE	No	Yes	Yes	Yes	Yes	Yes
Sector (2-digit) FE	No	No	Yes	Yes	Yes	Yes
Sector (5-digit) FE	No	No	No	Yes	Yes	Yes
Region FE	No	No	No	No	Yes	Yes
Town FE	No	No	No	No	No	Yes

Note: This table shows the estimates of the impact of contributing to a political campaign on the log number of tenders won by a firm in a given year-quarter. We estimate such coefficients by OLS using year-quarter-firm level data from 2007 to 2014 and the regression model described in equation 2. The baseline regression in column (1) does not include any fixed effects. In column (2), we add year-quarter fixed effects to the specification in column (1). Next, in columns (3) and (4), we sequentially include stricter definitions of sector fixed effects (2-digit and 5-digit classifications). Finally, in columns (5) and (6), we add sequentially narrower location fixed effects (region and municipality). (*), (**) and (*) indicate a statistical significance of 10%, 5%, and 1%. We use robust standard errors clustered at the firm level. We report standard errors between parenthesis.

We find evidence consistent with a large and positive return of campaign contributions. Overall, our results show that contributing firms win between 11.6% and 13.2% more public tenders than the non-contributing ones. In column (1), we document that

that contributing firms win on average 11.6% more procurement tenders than non-contributing ones. In columns (2) to (5), the magnitudes increase when estimated restricted to comparisons within the same sector and location. Since R^2 increases from nearly zero to 0.1, such a pattern suggest that the remaining unobserved differences between contributing and non-contributing firms are unlikely to explain all the return of campaign contributions.

Table 3: Returns of contributions increase at the last quarter of the fiscal year

	(1) Log # of tenders	(2) Log # of tenders	(3) Log # of tenders	(4) Log # of tenders	(5) Log # of tenders	(6) Log # of tenders
Contributing X Peak	0.0265***	0.0265***	0.0265***	0.0265***	0.0265***	0.0265***
	(0.00348)	(0.00348)	(0.00348)	(0.00349)	(0.00349)	(0.00349)
Observations	3082656	3082656	3082656	3082656	3082656	3082656
R-squared	0.00355	0.00794	0.0393	0.0744	0.0760	0.0959
Year Quarter FE	No	Yes	Yes	Yes	Yes	Yes
Sector (2-digit) FE	No	No	Yes	Yes	Yes	Yes
Sector (5-digit) FE	No	No	No	Yes	Yes	Yes
Region FE	No	No	No	No	Yes	Yes
Town FE	No	No	No	No	No	Yes

Note: This table shows the estimates of the impact of contributing to a political campaign on the log number of tenders won by a firm in a given year-quarter across peak and non-peak quarters. We estimate such coefficients by OLS using year-quarter-firm level data from 2007 to 2014 and the regression model described in equation 2. The baseline regression in column (1) does not include any fixed effects. In column (2), we add year quarter fixed effects to the specification in column (1). Next, in columns (3) and (4), we sequentially include stricter definitions of sector fixed effects (2- digit and 5-digit sectors). Finally, in columns (5) and (6), we add sequentially narrower location fixed effects (region and municipality). (*), (**) and (*) indicate a statistical significance of 10%, 5%, and 1%. We use robust standard errors clustered at the firm level. We report standard errors between parenthesis.

6.3   End of the year spending and returns of contributions

In Table 3, we present the estimates of β_3 described in equation 3. β_3 measures by how much the returns of campaign contribution increases in the peak quarter concerning the other quarters of the year (in percentage points). Column (1) shows the estimation of β_3 using a regression model without fixed effects. Next, in columns (2) to (6), we sequentially add year-quarter, 2-digit sector, 5-digit sector, region, and municipality fixed effects to the specification in column (1).

Our estimates show that the return of campaign contributions rises in the last quarter of the year by a large magnitude. In columns (1) to (6), we report an increase of 2.65 p.p. in the peak quarter. Compared to $\widehat{\beta_1} = .127$, the most pronounced estimate

16

of the return of contributions in Table 2, $\widehat{\beta}_3 = .0265$ represents an increase of more than 20% in the return of contributions during the peak quarter. Besides, the fact that $\widehat{\beta}_3$ remains stable while the R^2 increases when we add fixed effects to the regression model suggests that the remaining omitted variables are unlikely to cause a bias with the sign and the strength to generate a positive and significant $\widehat{\beta}_3$.

To check whether omitted variable biases explain our estimates, we estimate even more restrictive specifications in Table 4. Column (1) adds firm fixed effects to the specification in column (6) of Table 3, which is the specification described in equation 3. Next, in columns (2) to (4), we sequentially add firm-year, (5-digit) sector-quarter and town-quarter fixed effects to the specification displayed in column (1).

Results in Table 4 corroborate those in 3. In columns (1) and (2), the effect remains stable despite R^2 increasing to around 0.7. Such a pattern confirms that neither time-invariant firm characteristics nor firm-specific trends across years explain our results. In columns (3) and (4), the effect increases in comparison to the main estimates of $\widehat{\beta}_3 = .0265$, suggesting that unobserved firm characteristics varying across quarters mitigate the effects instead of inflating it. Overall, results in Table 4 suggest that our partial correlations may be a reasonable approximation for the true causal effect of expiring budgets on the return of contributions, likely bounding its magnitude from below.

7   Robustness checks

7.1   Robust peaks at the end of the fiscal year.

Table A.1 in the Appendix A displays the estimates of δ_k described in the specification in equation 1. We normalize all coefficients concerning the first quarter, meaning that we replicate the pattern presented in figure 1 by finding positive and increasing coefficients. To gauge the robustness of the end of the year spikes, in columns (2) to (5), we sequentially add year, public organization, municipality, and good fixed effects to the specification in column (1). Estimates in Table A.1 in the Appendix A show positive, increasing, and statistically significant coefficients in all specifications. Such estimates corroborate the economic hypothesis that the volume of purchase acts in the *Compras-Net* increase across quarters until peaking at the last quarter. As results remain similar when adding fixed effects, it is unlikely that differential trends in demand for public goods across years, public organizations, goods, and towns explain our results.

Table 4: Increase in the return of contributions in the peak quarter is robust to stringent specifications

	(1) Log # of tenders	(2) Log # of tenders	(3) Log # of tenders	(4) Log # of tenders
Contributing X Peak	0.0265***	0.0265***	0.0351***	0.0313***
	(0.00349)	(0.00349)	(0.00359)	(0.00361)
Observations	3082656	3082656	3082656	3082656
R-squared	0.505	0.697	0.700	0.702
Year Quarter FE	Yes	Yes	Yes	Yes
Sector (5-digit) FE	Yes	Yes	Yes	Yes
Town FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Firm Year FE	No	Yes	Yes	Yes
Sector(5-digit) Quarter FE	No	No	Yes	Yes
Town Quarter FE	No	No	No	Yes

Note: This table shows the estimates of the impact of contributing to a political campaign on the log number of tenders won by a firm in a given year-quarter across peak and non-peak quarters. We estimate such coefficients by OLS using year-quarter-firm level data from 2007 to 2014 and the regression model described in equation 2. In column (1) we add firm fixed effects to the specifications in 3 in column (6). In column (2), we add firm-year fixed effects. Next, in columns (3) and (4), we sequentially include (5-digit) sector-quarter and town-quarter fixed effects. (*) , (**) and (***) indicate a statistical significance of 10%, 5%, and 1%. We use robust standard errors clustered at the firm level. We report standard errors between parenthesis.

7.2 Robust returns of contributions.

In Table B.1 in the Appendix B, we report estimates from equation 2 but using a broader definition of contributing firms. More precisely, we define a firm as a contributing one if it has contributed to a national or local electoral campaign. Since the *ComprasNet* is a national procurement system, contributions to local elections are less likely to influence the allocation of national public spending. Hence, the hypothesis that contributions buy influence in the allocation of procurement contracts gains traction if we document positive and significant returns of contributions using a measure of influence that is *a priori* less likely to affect our outcome.

The results show that γ is significant has a positive sign but with a smaller magnitude than the ones in Table 2. Overall, our results in Table B.1 show that contributing firms win between 6.2% and 8.7% more public tenders than the non-contributing ones. In line with the hypothesis that local campaign contributions influence local procurement markets, we document the highest magnitudes with the broader treatment in the specification with town fixed effects in column (9).

Next, we whether our results in the *intensive margin* - i.e., in the number tender won - also manifest in the *extensive margin* - i.e., the likelihood of winning a procurement tender. To do so, in Table B.2 in the Appendix B, we report estimates of the return of contributions using an indicator variable equaling one when the firm has won at least one tender as the outcome. Hence, in this case, γ measures the increase in the probability of winning at least a procurement tender in a given year received by contributing firms concerning non-contributing ones. Overall, Table B.2 shows that contributing firms are between 3.6% to 4.0% more likely of winning a procurement tender than the non-contributing ones, confirming that positive returns of campaign contributions manifest both at the *intensive* and *extensive* margins.

7.3 Robust effect of the peak quarter on returns of campaign contribution

To probe the generality of our mechanism, in Table C.1 in Appendix C, we report estimates from equation 3 using a broader definition of contributing firms, which entails contributions to national and local elections. The effect of the peak quarter on the return of political connection is 1.42 p.p., which correspond to an increase of 16% of its most pronounced estimates using such treatment definition, which corroborate our main findings in Table 3.

Next, to verify whether our mechanism also impacts the extensive margin of procurement contracts, in Table C.2 in the Appendix C, we report estimates from equation 3 but using an indicator as dependent variable. The effect of the peak quarter on the return of political connection is 0.6 p.p. in all specifications, which corresponds to 15% of its most pronounced estimates using such outcome. Once more, the magnitudes remain in the ballpark of those in Table 3.

Conclusion

Do fiscal policy regulations affect the degree of political favouritism in the allocation of public resources? To answer this general question, we investigated whether the interaction of two customary practices of democracies, namely campaign financing by corporate contributions and the presence of expiring budgets, affect the allocation of procurement contracts across firms.

We use Brazil as a laboratory to document three main results that help answer-

ing such overlooked question. First, we document that procurement activity peaks in the fourth quarter, which is the first evidence of end-of-the-year spending from a non-OECD country to our knowledge. Second, we document a positive and significant return of campaign contributions, meaning that contributing firms win between 11.6 and 13.4 per cent more procurement tenders than non-contributing ones. Third, we show that the return of campaign contribution increase in the last quarter in magnitudes between 15.1 to 24.6 per cent of its most pronounced estimate, highlighting how influential are expiring budgets. This last finding is the first evidence showing that the budget calendar induces political favouritism, which bridges the consolidated literature documenting the return of political connections with the nascent research exploring the consequences of expiring budgets.

This study has three main limitations caused by data and time limitations. First, although robust to very demanding specifications, our estimates are partial correlations. For instance, by leveraging variation in the intensity of last quarter peaks across years, we could find a triple difference specification where parallel trends are plausible in the data.[4] Second, we are relatively silent about the mechanism causing the peaks in the returns to contributions in the last quarter of the fiscal year. For instance, is it the contractual design, privileged information, bidding behaviour, or ex-post bidding discretion? Third, we did not show that expiring budgets affect outcomes more directly connected to welfare, such as corruption, time- and cost-overrun of public contracts. We leave these issues for the future of this research.

Our findings linking expiring budgets to political favouritism invite more research investigating the consequences of the impact of fiscal policy institutions on the allocation of public resources. For instance, it is still unknown whether expiring budgets increase corruption and patronage, two crucial features of the political-economy literature. We leave such important questions for future investigations.

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[4]In this case, the three differences would be: peak vs non-peak at the quarter level, contributing vs non-contributing at the firm level, and high peak at the year level.

Appendix A   Robustness checks of the end-of-the-year spending in Brazil

Table A.1: There is evidence that public spending peaks in the final quarter

	(1) Log # of tenders	(2) Log # of tenders	(3) Log # of tenders	(4) Log # of tenders	(5) Log # of tenders	(6) Log # of tenders
Q1	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)
Q2	0.00771***	0.00807***	0.00740***	0.00739***	0.00520***	0.0144***
	(0.00123)	(0.00124)	(0.00130)	(0.00129)	(0.00125)	(0.00115)
Q3	0.0165***	0.0165***	0.0159***	0.0149***	0.0117***	0.0203***
	(0.00166)	(0.00166)	(0.00171)	(0.00167)	(0.00163)	(0.00139)
Q4	0.0307***	0.0311***	0.0304***	0.0302***	0.0256***	0.0330***
	(0.00208)	(0.00210)	(0.00209)	(0.00208)	(0.00203)	(0.00157)
Observations	7813400	7813400	7813400	7813400	7813400	7813400
R-squared	0.000575	0.000800	0.00367	0.0165	0.0402	0.321
Year FE	No	Yes	Yes	Yes	Yes	Yes
Public Organization (Layer 1) FE	No	No	Yes	Yes	Yes	Yes
Public Organization (Layer 2) FE	No	No	No	Yes	Yes	Yes
Public Organization (Layer 3) FE	No	No	No	No	Yes	Yes
Goods FE	No	No	No	No	No	Yes

Note: This table displays estimates of the quarter(s) effect on the log number of tenders in a quarter. We estimate such coefficients by OLS using quarterly data from 2007 to 2014 and the regression model described in equation 1. The baseline regression in column (1) does not include any fixed effects. In column (2), we add year fixed effects to the specification in column (1). Next, in columns (3) to (5), we sequentially include stricter definitions of organization fixed effects (layers 1, 2, and 3). Finally, in column (6), we include good fixed effects. (*), (**) and (*) indicate a statistical significance of 10%, 5%, and 1%. We use robust standard errors clustered at good-level. The standard errors of the coefficients are in the parenthesis.

Appendix B   Robustness checks of returns of contributions

Table B.1: Returns of contributions are robust to broader definitions of campaign contribution

	(1) Log # of tenders	(2) Log # of tenders	(3) Log # of tenders	(4) Log # of tenders	(5) Log # of tenders	(6) Log # of tenders
Contributing	0.0622***	0.0622***	0.0719***	0.0750***	0.0687***	0.0874***
	(0.00544)	(0.00544)	(0.00545)	(0.00537)	(0.00535)	(0.00540)
Observations	3082656	3082656	3082656	3082656	3082656	3082656
R-squared	0.000807	0.00698	0.0380	0.0733	0.0751	0.0954
Year Quarter FE	No	Yes	Yes	Yes	Yes	Yes
Sector (2-digit) FE	No	No	Yes	Yes	Yes	Yes
Sector (5-digit) FE	No	No	No	Yes	Yes	Yes
Region FE	No	No	No	No	Yes	Yes
Town FE	No	No	No	No	No	Yes

Note: This table shows the estimates of the impact of contributing to a political campaign on the log number of tenders won by a firm in a given year-quarter. We estimate such coefficients by OLS using year-quarter-firm level data from 2007 to 2014 and the regression model described in equation 2. We use the broader definition of contribution to a political campaign which includes campaign contributions of firms to national and local elections. The baseline regression in column (1) does not include any fixed effects. In column (2), we add year quarter fixed effects to the specification in column (1). Next, in columns (3) and (4), we sequentially include stricter definitions of sector fixed effects (2- digit and 5-digit sectors). Finally, in column (5) and (6), we add sequentially narrower location fixed effects (region, and municipality). (*), (**) and (*) indicate a statistical significance of 10%, 5%, and 1%. We use robust standard errors clustered at firm-level. We report standard errors between parenthesis.

Table B.2: The probability to win at least one tender is higher for contributing firms

	(1) Prob. to win	(2) Prob. to win	(3) Prob. to win	(4) Prob. to win	(5) Prob. to win	(6) Prob. to win
Contributing	0.0381***	0.0381***	0.0402***	0.0390***	0.0361***	0.0374***
	(0.00192)	(0.00192)	(0.00189)	(0.00183)	(0.00182)	(0.00181)
Observations	3082656	3082656	3082656	3082656	3082656	3082656
R-squared	0.00182	0.00834	0.0248	0.0454	0.0469	0.0648
Year Quarter FE	No	Yes	Yes	Yes	Yes	Yes
Sector (2-digit) FE	No	No	Yes	Yes	Yes	Yes
Sector (5-digit) FE	No	No	No	Yes	Yes	Yes
Region FE	No	No	No	No	Yes	Yes
Town FE	No	No	No	No	No	Yes

Note: This table shows the estimates of the impact of contributing to a political campaign on the indicator variable equaling one when the firm has won at least one tender in a given year-quarter. We estimate such coefficients by OLS using year-quarter-firm level data from 2007 to 2014 and the regression model described in equation 2. The baseline regression in column (1) does not include any fixed effects. In column (2), we add year quarter fixed effects to the specification in column (1). Next, in columns (3) and (4), we sequentially include stricter definitions of sector fixed effects (2- digit and 5-digit sectors). Finally, in column (5) and (6), we add sequentially narrower location fixed effects (region, and municipality). (*), (**) and (*) indicate a statistical significance of 10%, 5%, and 1%. We use robust standard errors clustered at firm-level. We report standard errors between parenthesis.

Appendix C   Robustness checks of the increase of returns of political connections in the peak quarter

Table C.1: The increase in the return of contributions in the peak quarter is robust to broader definitions of campaign contribution

	(1) Log # of tenders	(2) Log # of tenders	(3) Log # of tenders	(4) Log # of tenders	(5) Log # of tenders	(6) Log # of tenders
Contributing X Peak	0.014***	0.014***	0.014***	0.014***	0.014***	0.014***
	(0.0026)	(0.0026)	(0.0026)	(0.0026)	(0.0026)	(0.0026)
Observations	3082656	3082656	3082656	3082656	3082656	3082656
R-squared	0.00260	0.00699	0.0380	0.0733	0.0751	0.0954
Year Quarter FE	No	Yes	Yes	Yes	Yes	Yes
Sector (2-digit) FE	No	No	Yes	Yes	Yes	Yes
Sector (5-digit) FE	No	No	No	Yes	Yes	Yes
Region FE	No	No	No	No	Yes	Yes
Town FE	No	No	No	No	No	Yes

Note: This table shows the estimates of the impact of contributing to a political campaign on the log number of tenders won by a firm in a given year-quarter. We estimate such coefficients by OLS using year-quarter-firm level data from 2007 to 2014 and the regression model described in equation 3. We use the broader definition of contribution to a political campaign which includes campaign contributions of firms to national and local elections. The baseline regression in column (1) does not include any fixed effects. In column (2), we add year quarter fixed effects to the specification in column (1). Next, in columns (3) and (4), we sequentially include stricter definitions of sector fixed effects (2- digit and 5-digit sectors). Finally, in column (5) and (6), we add sequentially narrower location fixed effects (region, and municipality). (*), (**) and (*) indicate a statistical significance of 10%, 5%, and 1%. We use robust standard errors clustered at firm-level. We report standard errors between parenthesis.

Table C.2: The increase in the probability to win at least one tender in the peak quarter is robust to stringent specifications

	(1) Prob. to win	(2) Prob. to win	(3) Prob. to win	(4) Prob. to win	(5) Prob. to win	(6) Prob. to win
Contributing X	0.006***	0.006***	0.006***	0.006***	0.006***	0.006***
Peak	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0012)
Observations	3082656	3082656	3082656	3082656	3082656	3082656
R-squared	0.00395	0.00834	0.0248	0.0454	0.0469	0.0649
Year Quarter FE	No	Yes	Yes	Yes	Yes	Yes
Sector (2-digit) FE	No	No	Yes	Yes	Yes	Yes
Sector (5-digit) FE	No	No	No	Yes	Yes	Yes
Region FE	No	No	No	No	Yes	Yes
Town FE	No	No	No	No	No	Yes

Note: This table shows the estimates of the impact of contributing to a political campaign on the indicator variable equaling one when the firm has won at least one tender in a given year-quarter. We estimate such coefficients by OLS using year-quarter-firm level data from 2007 to 2014 and the regression model described in equation 3. The baseline regression in column (1) does not include any fixed effects. In column (2), we add year quarter fixed effects to the specification in column (1).). Next, in columns (3) and (4), we sequentially include stricter definitions of sector fixed effects (2- digit and 5-digit sectors). Finally, in column (5) and (6), we sequentially narrower location fixed effects (region and municipality). (*), (**) and (*) indicate a statistical significance of 10%, 5%, and 1%. We use robust standard errors clustered at firm-level. We report standard errors between parenthesis.

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