

Local Governments, Local Markets:

Campaign Finance Reform and Procurement Favoritism

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Abstract

Under which conditions do campaign finance reforms curb favoritism in public procurement? I argue that the effect of a donation ban depends on how concentrated the procurement market was beforehand. Drawing on semi-structured interviews and administrative records of 1.5 million procurement bids, I test this with a difference-in-differences design around Brazil's 2015 ban, comparing donor and non-donor firms within the same municipality across 381 municipalities. I find that before the ban, donor firms won 9.5 percentage points more often than the firms bidding against them for the same contract, and 43% of the tenders they won drew no other bid at all, against 32% for other winners, a gap of 11 percentage points holding the municipality, the year and the procedure fixed. These results survive restricting to close elections and a placebo on city council procurement. After the ban, they lost 27% of their market share overall, and 57% of it in the most concentrated third of municipalities. The ban reduced how often donor firms won without changing what they charged, a gap worth R\$51.6 million across the state of *Minas Gerais* over a decade.

Keywords: local governments, campaign finance reform, public procurement, political connections, Brazil.

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Introduction

Governments around the world have adopted a ban on corporate contributions to electoral campaigns as a tool to reduce political favoritism in public procurement. Public procurement accounts for large amounts of public money changing hands, from a tenth to a quarter of GDP in most countries (World Bank, 2020), and is one of the most important mechanisms of how politicians' influence what citizens get: roads, school lunches, ambulances. Firms that contribute to successful candidates are more likely to, and do, get more contracts, bigger contracts, than firms that don't (Boas, Hidalgo and Richardson, 2014; Titl and Geys, 2019; Dahlström, Fazekas and Lewis, 2021; Witko, 2011), and the favoritism distorts all the way to the quality of what citizens rely on (Olken, 2007; Ferraz and Finan, 2008). Surprisingly, the same reform can have a large impact on procurement markets in some places, and barely seem to pass through in others; this variation exists both between countries and within them, from municipality to municipality (Baltrunaite, 2020; Guerra, Mattos and Nakaguma, 2025). Under which conditions do campaign finance reforms curb favoritism in public procurement?

I argue that the success of donation bans in curbing favoritism in public procurement is moderated by how concentrated the procurement market was before the ban. It is known that the firms that funded the winning campaign are favored when contracts are out, and do get contracts that otherwise would not. Over enough time and won often enough, those contracts translate into a market position. In the cities where a few firms had most of the contract money, most of what the donor firm had was built into the position, and the ban is going to shake up most of the favoritism. In the cities where the money was dispersed among many firms, the same donation got the same favor, but the favor was a small addition to a position mostly won by the firm.

To test this argument, I exploit Brazil's 2015 ban on corporate donations as an exogenous policy shock. I link campaign contributions, public contracts, firm registries, elections, and municipal characteristics and build two datasets to (i) assess the impact of the ban on different procurement markets and (ii) to assess the implications of such favoritism in the local level

spending. The first panel is comprised of 868,528 firm-municipality-year observations, covering 381 municipalities across four states from 2014 to 2023. The second panel covers 4.5 million items bought by the state of *Minas Gerais*. Because I observe every winner and losers, I can compare firms donor and non-donor firms were paid for the same product, in the same municipality, in the same year.

I show that, before the ban, donor firms won 9.5 percentage points more frequently than the firms bidding against them for the same contract, and were paid about 7% more than other suppliers for the same goods. One might argue that donor firms were simply better, more skilled, or more capitalized. They were not: holding fixed registered capital, firm age, firm size, industry and legal form, the effect does not change. After the ban, donor firms lost 27% of their procurement market share, and the loss fell where the argument predicts. In the most concentrated third of municipalities they lost close to 2 percentage points, 57% of what they had held; in the other two thirds they lost 0.16 and 0.19 points, neither distinguishable from zero. What the ban did not reach was the favoritism itself: among donor firms that continued to compete, the price premium did not narrow in any post-ban year and, over ten years, favoritism cost *Minas Gerais* municipalities R\$51.6 million.

How did firms get favored? Through the standard red flags of a captured process: a single bidder, a thin field, an award at the published estimate, and a fast decision. Of the tenders donor firms won, 43% drew no other bid at all, against 32% for other winners. Fewer than three bidders appeared on 60%, against 42%. And on 52% the municipality paid the price it had itself published rather than anything below it, against 38%. Tenders were also awarded faster, within a week of the bids being opened. What donor firms did not do is shorten the notice period, write narrower specifications, or move their purchases into the procedures where a contest can legally be waived. On all three they are indistinguishable from other winners.

I rule out three alternative explanations. First, perhaps donor firms simply exited the market. They did not: they were no more likely than comparable firms to stop bidding, and

they were less likely to stop in the municipalities where they had been connected. Second, perhaps the connection moved from the firm to its owners, since individual donations remained legal after 2015. Only 25 of the 710 donor firms had a shareholder who gave to the winning 2016 mayoral candidate. Those firms lost nothing at all, while the loss fell entirely on the firms whose shareholders did not give, so substitution was available and effective and simply too rare to account for the decline. Third, perhaps a mayor returned to office would still have had reason to reward the firms that financed the first campaign, in which case the decline would reflect mayors leaving office rather than the ban. Donor firms should then have lost far less where the 2012 mayor stayed on and far more where a new one arrived. They lost a similar amount either way. Most work on political connections measures them as ties to individuals, valued by what happens to that person (Fisman, 2001; Faccio, 2006) and eroding once the person leaves office (Blanes i Vidal, Draca and Fons-Rosen, 2012). My results are closer to Gulzar, Ladino and Riaño (2025), who find that donors in Colombia continue to receive contracts through changes of mayor long after the election they financed. What the donation bought, across time, was a position inside the local procurement apparatus that outlasted the mayor who granted it.

I make three contributions. First, while existing work explains the disappointment of anti-corruption reforms by pointing to failures of implementation, I show that a reform can be implemented fully and still produce sharply uneven effects (Baltrunaite, 2020). Second, while Boas, Hidalgo and Richardson (2014); Titl and Geys (2019); Dahlström, Fazekas and Lewis (2021) show that donor firms get more contracts and bigger contracts, and (Fisman, 2001; Mironov and Zhuravskaya, 2016) that political connections have economic value, they study the award of contracts in aggregate and cannot disentangle favoritism from the quality of firms. I look at favoritism on the tender level. Finally, while previous studies on this reform looked at the contracts that firms won at the firm level (Dahis, Ricca and Scot, 2025), or per sectors and municipalities (Guerra, Mattos and Nakaguma, 2025), I look at all the firms that bid on all tenders, including the ones they lost, which enables me to distinguish a

firm that stopped winning from a firm that stopped entering, to see whether firms fail or just exit, and to verify whether the owners have rebuilt the channel under their own name.

This paper proceeds as follows. Section 2 develops the argument and derives two testable predictions. Section 3 situates the Brazilian case and the 2015 ban. Section 4 presents the data and identification strategy. Section 5 reports the results, the heterogeneity by market concentration, the mechanism behind them, and what favoritism cost. Section 6 concludes.

Argument

Firms that finance winning campaigns are treated better when public contracts are awarded. They win more of them, they win larger ones, and the advantage survives the firm characteristics that would otherwise explain it (Boas, Hidalgo and Richardson, 2014; Titl and Geys, 2019; Dahlström, Fazekas and Lewis, 2021; Fisman, 2001; Mironov and Zhuravskaya, 2016). As a consequence, governments around the world have responded by prohibiting corporate contributions outright. If they are effective, that is up for debate (Persson, Rothstein and Teorell, 2013; Mungiu-Pippidi, 2015; Fisman and Golden, 2017). Where a ban has been followed through to its consequences, the findings are few, inconsistent, and reported as national or sectoral averages (Baltrunaite, 2020; Guerra, Mattos and Nakaguma, 2025; Dahis, Ricca and Scot, 2025; Avis et al., 2022). What explains this sub-national variation?

I argue that the success of donation bans in curbing favoritism in public procurement is moderated by how concentrated the procurement market was before the ban. Consider a city where dozens of firms compete for public contracts but a few take most of the spending, what I call a *concentrated public procurement market*. A firm that donated to the winning politician, a donor firm from here on, receives preferential treatment that over time makes winning progressively easier for it and harder for its competitors: it accumulates experience, invests in equipment, hires workers, and builds relationships with officials. Political connection gradually becomes an economic advantage (Akcigit, Baslandze and Lotti, 2023). Now consider

a city where contracts are spread across many firms, a *dispersed public procurement market*. A ban takes a great deal from the first city and very little from the second. The reform is identical in both, and what differs is how much of the donor firm’s position was politically conditioned to begin with.

Local governments are fruitful investments for donor firms. Mayoral races are expensive relative and candidates depend heavily on private money to run them (Samuels, 2001; Avis et al., 2022; Speck and Mancuso, 2014; Mancuso, 2015). In return, mayors tend to reward donors through the allocation of public contracts. From Colombia, where looser contribution limits deliver more contracts to the winner’s donors (Gulzar, Rueda and Ruiz, 2022) and where those donors keep receiving them as long as eight years later (Gulzar, Ladino and Riaño, 2025), to Italy, where supplier concentration rises with a mayor’s time in office (Coviello and Gagliarducci, 2017), local governments have a history of favoring campaign donors. Even though mayors rarely sign contracts themselves, they tend to control the bureaucracy that does (Bersch, Praça and Taylor, 2017; Potter, 2019). Political control over bureaucrats raises corruption rather than reducing it precisely where politicians need campaign money and institutional constraints are weak (Brierley, 2020; Hellman, Jones and Kaufmann, 2000). In the case of Brazil, municipal bureaucracies are unusually open to that kind of staffing, and politicization concentrates where administrative capacity and internal oversight are weakest (Power and Taylor, 2011).

Public officials who audit public procurement describe exactly this control. A senior auditor in the Federal Court of Accounts of Brazil traced this problem to the “capture of procurement by the political apparatus”, which “installs people who then adopt the conduct that serves whoever appointed them.”¹ More specifically: a good part of the tender is “already steered so that only one firm can win.” This interviewee is describing what Brierley (2020)

¹Two rounds of field research I conducted in Brazil in 2022 and 2023, comprising 42 semi-structured interviews across the institutions that audit, prosecute and administer public procurement. Interviewees were interviewed in Portuguese under University of Virginia IRB-SBS Protocol No. 5130, are cited anonymously by institutional role, and I translated them. The Supporting Information lists the institutions covered and the selection logic.

sees in the local government of Ghana: an official whose career the mayor controls has reason to run a tender how the mayor wants.

I expect the local market to moderate the ban's effects in two ways. Donor firms will lose market share in the procurement market after the ban, while non-donor firms are expected to pick up business made available because of the ban. Second, these effects should be more pronounced in more concentrated procurement markets. In concentrated markets, donor firms have a larger portion of the market to lose and more of it was likely procured through relationships with local officials rather than earned by the firm itself. If those relationships can't be used any more to obtain contracts, donor firms should lose more market share and provide more opportunity to the firms that were competing with them. In a dispersed market, donor firms have less of the market to begin with and more of their market share likely came from their ability to compete, rather than through political connections. Thus there is less politically-influenced business to unwind and less for non-donor firms to pick up. ²

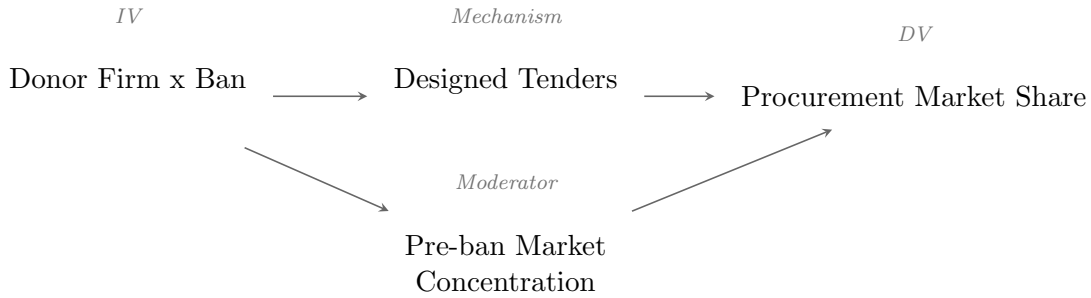


Figure 1: Theoretical model.

²Two recent studies are close to asking this question. Using a discontinuity in contribution limits in Colombia, Gulzar, Rueda and Ruiz (2022) show that relaxing them gets more contracts to the winner's donors, which are also more likely to be over cost. They test for heterogeneity on the donor side, whether contributions are concentrated among a few large givers, and do not find differences around the threshold. Gulzar, Ladino and Riaño (2025) shows how durable these positions are, as donors were receiving contracts eight years after the mayor they contributed to left office and before two mayoral turnovers. However, Colombia does not have a ban on corporate contributions, so what the ban would eliminate has to be inferred rather than observed, and neither study asks how the answer depends on the market the reform enters.

Empirical Case of Brazil’s 2015 Campaign Donation Ban

“Nobody donates to a political campaign out of kindness, or because they find the candidate good looking. Campaign donations are loans, collected later at high interest, in the form of contracts and procurement awards.”

Paulo Roberto Costa, former Director of Supply at Petrobras,
in testimony to the congressional inquiry into Petrobras, 2014.³

Paulo Roberto Costa ran the supply division of Petrobras, the state controlled oil company and the largest firm in Brazil. In 2014, he was arrested in the corruption investigation known as Operation Car Wash (*Operação Lava Jato*), being accused of taking bribes to award construction contracts at inflated prices and shared the benefits with politicians.⁴ The investigation that followed reshaped Brazilian politics, and its economic consequences have since been measured directly (Ferraz et al., 2023). It was in this context that the Brazilian Supreme Federal Court banned corporate campaign donations. This represented a substantial shock to the campaign finances of the 2016 candidates, since corporate donations had supplied 22.7% of all candidate receipts in the 2012 municipal races across these four states (Brazilian Electoral Court).

Before 2015, donor firms did not spread contributions evenly across the candidates: they focused on the candidates most likely to win. A senior official at the Office of the Comptroller General (the Brazilian Federal agency that audits public spending) described the logic directly:

³*O Globo*, “Paulo Roberto diz que doações de campanha são empréstimos cobrados a juros altos”, <https://oglobo.globo.com/politica/paulo-roberto-diz-que-doacoes-de-campanha-sao-emprestimos-cobrados-juros-altos-15546114>. Original: “Ninguém doa para campanha política por ser bonzinho ou achar a pessoa bonita. As doações de campanha são empréstimos cobrados posteriormente a juros altos, na forma de contratos e licitações.”

⁴Brazil has a history of money in politics. In 1993, a congressional inquiry found that members of the joint budget committee had been steering budget amendments to firms that paid them, in what became known as the scandal of the budget dwarves (*anões do orçamento*). In 1998, the construction of the labour court headquarters in São Paulo was found to have absorbed roughly R\$169 million in diverted funds. And between 1990 and 2004 the scheme known as Operation Vampire (*Operação Vampiro*) revealed that the federal government had paid 40 percent above market price for imported blood supplies, amounting to roughly USD 900 million (*Folha de S.Paulo*, “Entenda a Operação Vampiro”, 25 August 2006, <https://www1.folha.uol.com.br/folha/brasil/ult96u81977.shtml>).

electoral competition “involves spending a great deal of money on campaigns, and it is the winners who are granted these [procurement] favors.”⁵ These campaign donations were widely understood to purchase more than goodwill. Boas, Hidalgo and Richardson (2014) show that Brazilian firms donating to winning federal candidates subsequently receive substantially more government contracts, and audits of municipal governments document procurement irregularities across a large share of the municipalities examined (Ferraz and Finan, 2008, 2011). This “way of doing business” is not particular to Brazil: donor firms win more procurement in settings as varied as Lithuania (Baltrunaite, 2020), Russia (Mironov and Zhuravskaya, 2016), and the United States (Dahlström, Fazekas and Lewis, 2021), and where contract execution can be observed, donor contracts are delivered worse and renegotiated upward more often (Schoenherr, 2019).

Most of this literature focus on kickbacks on federal contracts. Far less is know about what happens in the local governments around the world. Take Brazil as an example: local governments lack proper and consistent audits, local leaders exert more power than we can imagine, patronage is alive and well. And yet, scholars have not grasped with one of the major anti-corruption reforms of the past 30 years in the country: did the ban on campaign donations achieve its goal? Is favoritism less present in public contracting then before? The place to test the effects of a national ban is any old little town. What is politics like in an average Brazilian municipality?

Cidadezinha Qualquer: Any Small Town

What Paulo Roberto Costa was describing was far being the reality only of the Federal Government. Brazil, the largest economy in Latin America, spends between 6 and 31 % of its GDP on public procurement (Dahis, Ricca and Scot, 2023), which has made contracting one of the main arenas of political corruption in the country. This is extremely common

⁵Author interview, senior official, Office of the Comptroller General, Brasília, March 2023. Conducted in Portuguese. The Controladoria-Geral da União is Brazil’s federal internal control body. It audits federal spending and federal transfers to states and municipalities, conducts disciplinary proceedings against federal servants, and negotiates leniency agreements with firms under the Clean Company Act.

in the day to day life of local governments. From the ambulances that one supplier sold to municipalities across the country by rigging their tenders, to the school meals that three cooperatives divided among 152 towns in São Paulo after offering mayors campaign financing in exchange for the contracts, local procurement is where the exchange Costa described hurts the people who pay for it.⁶

Drummond’s *cidadezinha qualquer* is where most Brazilian public contracting happens.⁷ The median municipality in this study has about 14,000 residents, and a quarter of them have fewer than 6,900. These are places where the town hall is among the largest employers and where most people know someone who works there. They are not wealthy: the median municipality has a human development index of 0.67 and a GDP per capita of around R\$23,500.⁸

What the Brazilian Constitution asks of these towns is nonetheless considerable. Since 1988, local governments have been the primary providers of early childhood and fundamental education and joint providers of public health care (Bardhan, 2002; Faguet, 2012). In a country in which 70% of Brazilians have no private health plan and depend entirely on the public system, in which 80% of the 47 million children in basic education attend a public school, and in which the national school meals program feeds 40 million of them every day, asking whether an anti-corruption reform actually reached the local level is crucial for

⁶The 2006 scheme, known as Operation Leech (*Operação Sanguessuga*), was run by a single supplier that paid legislators for budget amendments earmarking money to particular municipalities and then rigged the tenders that followed. Investigators estimated that the group moved R\$110 million, and an audit by the federal comptroller and the health ministry found overpricing in 70 % of the agreements examined (*Congresso em Foco*, “Entenda o caso: o que foi a Operação Sanguessuga”, <https://www.congressoemfoco.com.br/noticia/71007/entenda-o-caso-o-que-foi-a-operacao-sanguessuga>). The school meal cartel was investigated from 2016 as Operation Alba Branca, and in 2021 the competition authority fined seven firms and seven individuals R\$340.8 million (*Agência Brasil*, “Cade condena empresas por cartel na merenda escolar de São Paulo”, April 2021, <https://agenciabrasil.ebc.com.br/educacao/noticia/2021-04/cade-condena-empresas-por-cartel-na-merenda-escolar-de-sao-paulo>).

⁷Carlos Drummond de Andrade (1902–1987) is among the most widely read Brazilian poets of the twentieth century. He was himself from Itabira, a small mining municipality in *Minas Gerais*. *Cidadezinha Qualquer*, published in *Alguma Poesia* (1930), is a portrait of an ordinary town in the interior; the title translates roughly as “Any Small Town.”

⁸The median municipal population is stable across the study period, at 13,899 in 2012 and 14,505 in 2016. GDP per capita is for 2021. The Human Development Index (HDI) is the 2010 census value, the most recent year for which it is published at the municipal level (Programa das Nações Unidas para o Desenvolvimento, 2013).

understanding whether this reform improved the lives of the people it was meant to protect.⁹

In order to fulfill their constitutional responsibilities, municipalities rely heavily in contracting. And this endeavour takes a lot of money. The median of spending in contracting in any small awards R\$19.1 million every year. Mayors buy school meals, medicines, transportation, security services, and technology, just to name a few. For any firm in any small town of Brazil, this is not an occasional opportunity: this is a market. This is the market that can decide which firms remain in business and which ones do not.

How powerful are Brazilian mayors? Elected for a term of four years and able to run for only one consecutive term, mayors can influence local politics more than we can expect. The 1988 Brazilian Constitution made municipalities federative entities rather than administrative subdivisions: within the municipality the executive is theirs alone. Mayors are responsible to propose the city budget, freely appoint loyalists for strategic positions. Mayors face very little oversight in their towns. Usually, the city council is responsible to audit the mayor's spending. Outside of that, a Brazilian mayor has a very peaceful life when it comes to external control.

So what does a mayor actually control? They control the public administration in their local governments. The median town hall here employs about 580 people in its direct administration, and 7.1% of them are commissioned appointees who serve at the mayor's pleasure rather than entering by competitive examination, close to the 7.7% found across all municipalities in these four states and below the national figure of 8.7% (Figure 2). These are not unusually politicized administrations. But those posts include the people who write what a tender asks for, decide which procedure to use, and choose when to publish it.

⁹Constitution of 1988, arts. 30 (VI and VII), 198, 211 (§2) and 212, with the health minimum set by Constitutional Amendment 29 of 2000 and regulated by Complementary Law 141 of 2012. The 2023 school census (INEP) counts 47.3 million students in basic education, 49.3% of them in municipal schools and 69.5% of those in the early years of fundamental education. The National Fund for Educational Development (FNDE) transfers the money for school meals and leaves the purchasing to states and municipalities. The 2019 National Health Survey (IBGE) found that 74% of Brazilians had no private medical plan, rising to close to 90% in the North and Northeast. The 2017–2018 Household Budget Survey (IBGE) found some degree of food insecurity in 36.7% of Brazilian households and 49.3% of households with children. On school meals as a primary food source in areas of social vulnerability, see de Amorim, Dalio dos Santos and Ribeiro Junior (2022). A survey of public school students in the Rio de Janeiro metropolitan area found that 56% describe the school meal as their main or only meal of the day (Observatório da Alimentação Escolar, *Conta Pra Gente Estudante*, 2023).

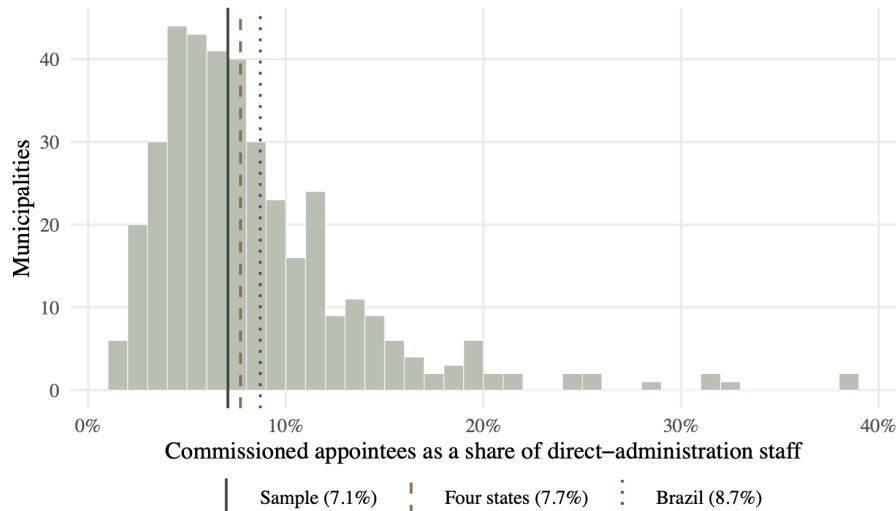


Figure 2: Commissioned Appointees in a City Hall

Notes: Commissioned posts are filled at the mayor’s discretion rather than by competitive examination, and are the standard measure of how much of a local bureaucracy is political rather than career. One observation per municipality, averaging across 2014–2023; 2016 and 2022 are missing nationally in the source. Reference lines give the median for the estimation sample, for all municipalities in the four sample states, and for all 5,570 Brazilian municipalities. Data from the *Laboratório de Monitoramento e Avaliação de Políticas e Eleições* (MAPE), IESP-UERJ.

While procurement is subject to rules designed to foster competition, these rules provide significant leeway for officials as to how procurement is organized. Officials choose how contracts are bundled, when tenders are issued, how technical specifications are drawn up, and which procedure to follow. Some procedures call for wide advertising and open competition (Coviello and Mariniello, 2014); others are much more permissive, for instance by limiting the pool of potential bidders or by allowing direct contracting under certain conditions¹⁰ Whether this leeway turns into political influence depends on what constrains it. In Brazil, those constraints are unevenly distributed. Federal oversight institutions audit only a small share of municipal procurement, while the State Courts of Accounts responsible for supervising local governments vary considerably in capacity and independence (Ferraz and Finan, 2008; Hidalgo, Canello and Lima-de Oliveira, 2016; Power and Taylor, 2011; Bersch, Praça and Taylor, 2017).

¹⁰These more discretionary procedures are common, not exceptional. Between 2009 and 2020 they accounted for nearly 40% of procurement procedures in Brazil’s largest states and, by 2020, represented 62% of federal procurement spending (Dahis, Ricca and Scot, 2025).

In Brazil, procurement in municipalities is generally not very competitive. The prevalence of non-competitive tenders is displayed in Figure 3. In a small municipality, 27% of tenders are let without a contest at all, either as a waived purchase or as a sole-source award, close to what has been reported for Brazil as a whole. Invitation-only tendering, in which the administration chooses which registered suppliers may bid, is rare almost everywhere, accounting for about 1% of tenders in the median municipality.¹¹ Most public contracts in these places are not competed for.

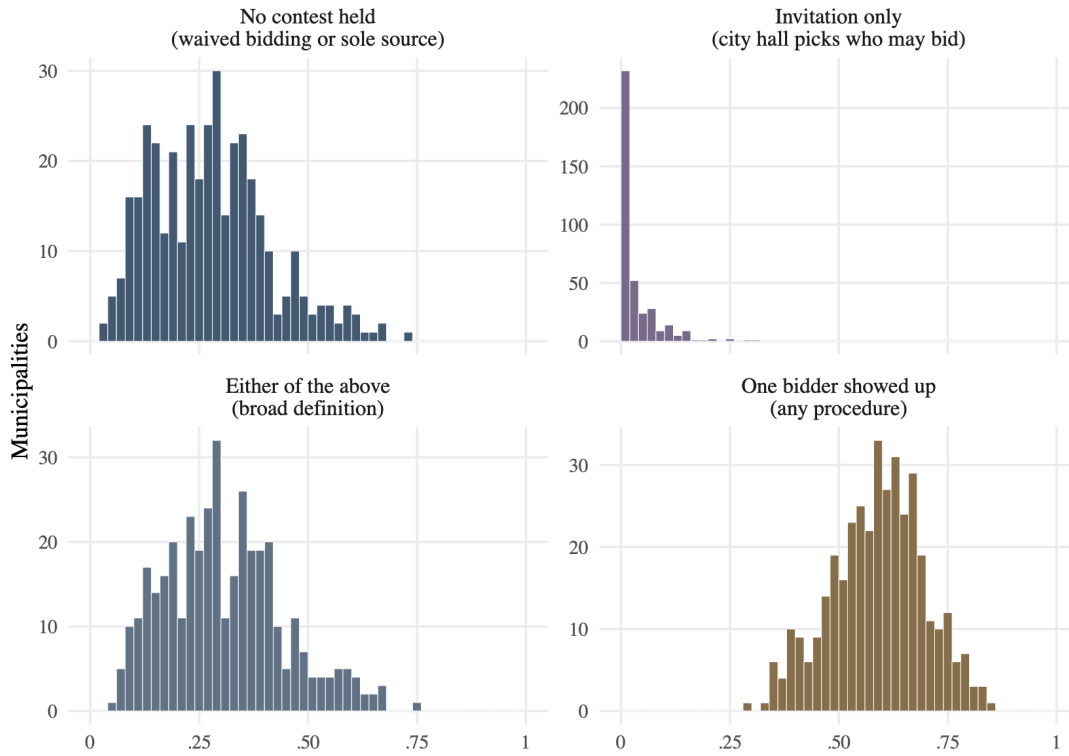


Figure 3: How competitive is procurement in these municipalities?

Notes: One observation per municipality, pooling 2014–2023. Shares are computed over distinct tenders rather than over bidder-tender records. The first three panels classify by procedure. Waived bidding (*dispensa*) and sole source (*inexigibilidade*) dispense with a contest altogether; invitation-only tendering (*convite*) is formally competitive but leaves the administration to choose which registered suppliers are asked to bid. The fourth panel classifies by outcome instead, counting tenders in which a single firm submitted a bid, whatever procedure was used.

This raises a more specific question: is the use of these non-competitive procedures related

¹¹It was abolished by Lei 14.133/2021. It is reported separately here because it is formally a competitive procedure while leaving the composition of the bidding field to the administration.

to how much of the municipal administration is under the mayor’s political control? The answer is not uniform across the executive branch. The relationship is concentrated in City Hall. Figure 5 shows the share of positions filled by commission against three indicators of non-competitive procurement, separately for tenders issued by City Hall and by other executive units. Two show little relationship: bidder counts are flat, and invitation-only tendering is rare everywhere. The third is different. The share of City Hall tenders not subject to competition at all rises with the share of City Hall positions filled at the mayor’s sole discretion, from about a quarter of tenders in the least politically staffed municipalities to nearly two fifths in the most. This effect is stronger for City Hall’s own tenders, suggesting that it is in the mayor’s principal administrative hub that political appointment is most closely linked to non-competitive procurement.

Against that background, the arrival of the ban is visible in the raw data. Figure 4 plots the average share of municipal procurement won by donor and by non-donor firms in each year of the panel. Donor firms held between two and three percent of their municipality’s contracting before 2017 and non-donor firms about half a percent, and the donor series falls sharply in the first year of the term elected under the ban and does not recover over the seven years that follow. No estimate is needed to see it, and nothing in the analysis depends on the figure, but it is the pattern the rest of the paper takes apart.

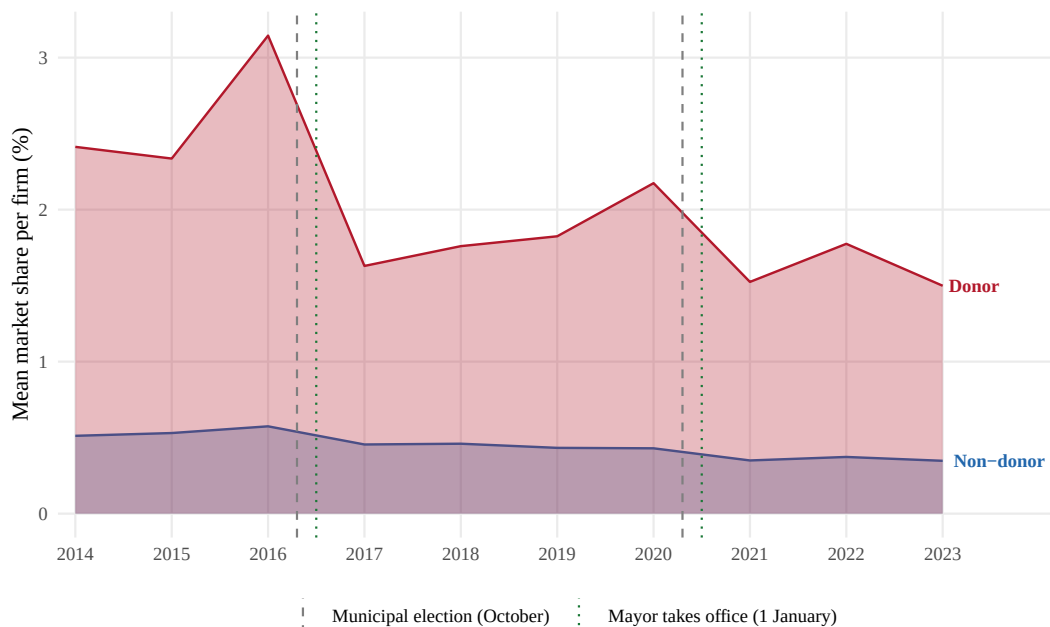


Figure 4: Mean municipal procurement market share, donor and non-donor firms

Notes: Annual mean market share, in percent, 2014–2023, executive-branch procurement in the 381 municipalities of the analysis panel. Dashed lines mark the October 2016 and October 2020 mayoral elections; dotted lines mark 1 January 2017 and 1 January 2021, when the winners took office. The first dotted line is the date from which the corporate donation ban binds in this panel. The figure is descriptive and carries no fixed effects or controls.

The ban did not affect all municipalities in the same way. The change in donor firms' share of tenders between the pre-ban and post-ban periods varies considerably across municipalities (Figure 6). If a donor firm's pre-ban position was sustained in part by its relationship with the mayor, removing the channel through which that relationship operated should matter most where that relationship had the most to do with the firm's position in the first place. I therefore test whether the ban had larger effects in municipalities where donor firms were more politically entrenched before the reform.

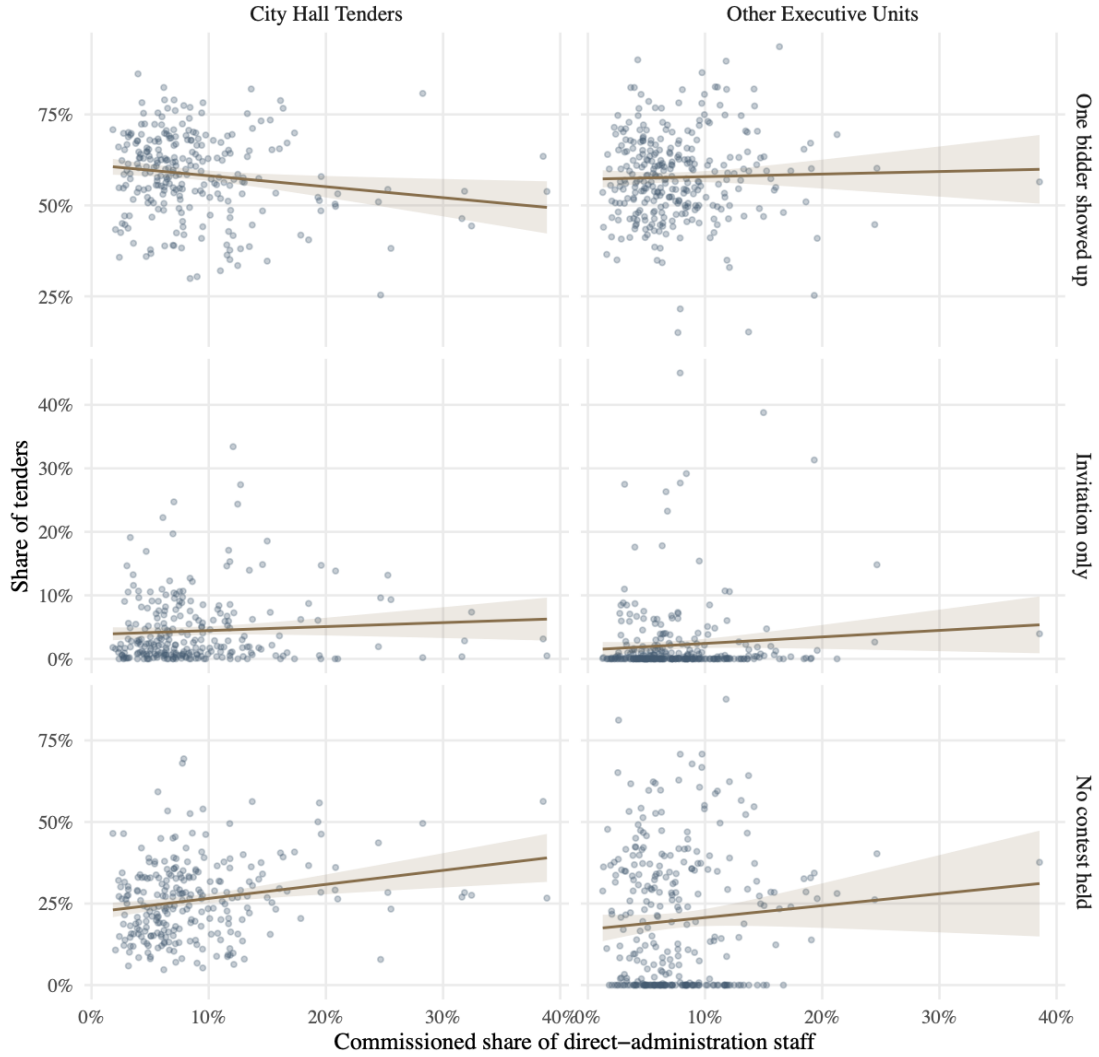


Figure 5: Patronage predicts which procedure is used, not how many firms bid

Notes: One point per municipality, averaging across 2014–2023. Columns separate tenders run by the prefeitura from those run by other executive units; rows are three measures of non-competitive procurement. Vertical scales differ across rows because invitation-only tendering is rare while single-bidder tenders are common. Fitted lines are unweighted ordinary least squares with 95% confidence bands. The relationships are descriptive and are not adjusted for municipal size.

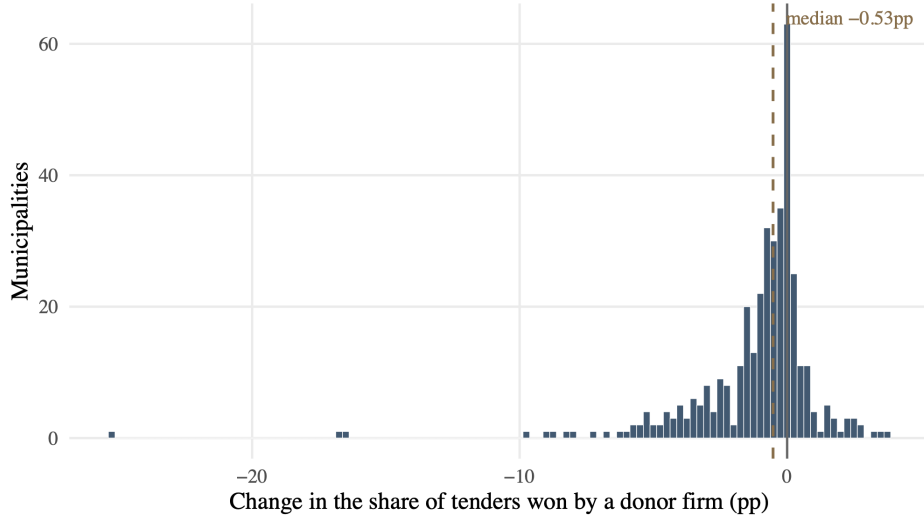


Figure 6: The same rule, very different outcomes

Notes: One observation per municipality. The horizontal axis is the change, in percentage points, in the share of tenders won by a firm that donated to the winning 2012 mayoral candidate, comparing the post-ban period to the pre-ban period. Restricted to municipalities with at least one donor firm and at least fifty tenders in each period, since elsewhere the share is either mechanically zero or too thin to compare. The dashed line marks the median.

Design

I leverage Brazil’s ban on corporate campaign donations as an exogenous shock to a legal channel of political exchange to test whether banning donations curbs favoritism in public procurement. I measure political connection using a corporate donation to the winning mayoral candidate in the last election before the ban, held in 2012. My treated group is made of firms that donated to mayoral winning candidates and that bid for contracts in the same municipality (donor firms). My control group is made of firms that bid for contracts in the same municipalities and never donated (non-donor firms).¹²

I build a panel of 868,528 firm-municipality-year observations across 381 municipalities in four Brazilian states from 2014 to 2023, restricted to municipalities that report losing bidders as well as winners in at least 5% of competitive tenders.¹³ I estimate the following equation:

¹²A firm that donated in municipality A and bids in B without donating is a donor only in A . The municipality fixed effects therefore compare donor with non-donor firms inside the same market rather than across markets.

¹³Section 2 of the Supporting Information documents the five administrative sources, Tables [A3](#), [A4](#),

$$Y_{imt} = \beta D_{im} + \delta(D_{im} \times P_t) + \alpha_m + \gamma_t + X_i' \varphi + \varepsilon_{imt}, \quad (1)$$

where Y_{imt} is firm i 's share of total municipal procurement value in municipality m and year t , in percentage points; D_{im} indicates a donation to the winning 2012 mayor in m ; and P_t indicates the post-ban period, $\mathbf{1}[t \geq 2017]$.¹⁴ α_m and γ_t are municipality and year fixed effects, and X_i holds log capitalization, firm size, firm age, industry and legal form. The main effect $\hat{\beta}$ is the pre-ban gap in market share between donor and non-donor firms in the same municipality, which I call the *donor premium*. $\hat{\delta}$ is my main coefficient of interest: the average effect of the ban on the firms that had donated, measured as their change relative to non-donor firms in the same municipality. The year effects absorb P_t , and because X_i is time-invariant it absorbs neither the treatment nor the post period. I report this specification alongside two simpler ones, with no fixed effects and with fixed effects but no controls, so the movement of $\hat{\delta}$ across them is visible in Table 2. Standard errors are clustered by municipality. Because treatment is rare, with 710 donor firms against roughly 152,000 non-donors, I do not rely on asymptotic cluster-robust errors alone: Table A13 of the Supporting Information reports a pairs cluster bootstrap over municipalities and Fisher randomization inference that permutes the donor flag within municipality.

A simple comparison of donor and non-donor firms would mislead. Donor firms are on average larger, older, better capitalized and more experienced, which would produce a positive correlation between donor status and procurement outcomes absent any political advantage. Municipalities where donors are active may differ systematically from those where they are not. To address this, I use municipality fixed effects (to absorb time-invariant differences

A5 and A6 reporting coverage, panel balance, descriptive statistics by donor status and the municipality profile, and Tables A1 and A2 the procurement activity and competition of the sample municipalities. It also documents the collection and harmonization of the 2020–2024 records, which I gathered from four state audit courts to extend the panel beyond the end of the published MiDES data (Dahis, Ricca and Scot, 2023).

¹⁴A firm that wins no contract has a market share of zero and remains in the sample. A share is the running total of awards won, which is what Section 2 argues repeated favoritism accumulates into, and it is the quantity a ban can move, because a prohibition reaches the relationship that produced the awards rather than the awards already made.

across markets), year fixed effects (to absorb aggregate shocks), and firm controls (to hold observable differences constant within municipality).

I expect the effect of the ban to be different within municipalities given how entrenched donor firms were in the local procurement market. I use market concentration as a proxy for that entrenchment. Supplier concentration rises with the length of a mayor’s tenure (Coviello and Gagliarducci, 2017) and with the discretion an administration holds over how contracts are let (Decarolis et al., 2025). In Colombian municipalities, Gulzar, Ladino and Riaño (2025) find that donors pay less to secure privileged access where procurement is more concentrated, since fewer rivals compete for it. A connection is therefore worth more in a concentrated market. Where a few suppliers already held most of a municipality’s contract money, more of what the donor firm held had accumulated through repeated favorable awards, and a ban therefore has more to undo. I measure concentration with the pre-ban Herfindahl-Hirschman index of awarded value over 2014–2016, computed over non-donor firms only, and split municipalities at the median.¹⁵ I compute the index over the pre-ban years alone because an index that included the post-ban years would itself respond to the reform. The moderator and the outcome are easy to confuse, since both are built from procurement shares. The outcome is how much of a municipality’s contract money a single firm holds in a given year.

¹⁶

To estimate the cost of favoritism to Brazilian municipalities, I construct a panel of 1,504,671 bidder–tender observations and use it to estimate:

$$W_{ijmt} = \beta_w D_{im} + \delta_w (D_{im} \times P_t) + \theta_j + \varepsilon_{ijmt}, \quad (2)$$

where W_{ijmt} is one if firm i won tender j and θ_j is a tender fixed effect. Because θ_j absorbs

¹⁵Restricting the index to non-donor firms means a municipality cannot count as concentrated merely because its donor supplier is large there. The restriction changes little: the donor-free and unrestricted indices correlate 0.991, and donor firms are 0.64% of the bidder pool, too few to move a Herfindahl index. I report both, and alongside them a count-based index built from contracts won rather than value, in Table A24.

¹⁶The moderator is how unevenly that money was spread across the municipality’s other suppliers before the ban. The donor firm’s own share enters the first and is excluded from the second.

the contest, β_w compares the donor with the specific firms bidding against it for the identical contract, holding constant its value, its product, its modality, its timing, and the number of firms in the field. Single-bidder tenders drop out, since a contest with one participant admits no comparison; they account for 37% of donor bidder-tender rows, which is why this estimate and the pooled win rate answer different questions.

And what did a municipality pay for what it bought? Brazilian buyers publish a reference price for each item before bidding opens, so every purchase carries its own benchmark. In my sample, only one state provides such granular data (*Minas Gerais*) item by item alongside that quote. I then build a second panel of 4,500,927 items bought by its 177 municipalities in the analysis sample between 2014 and 2024, of which 62,861 were won by donor firms. On it I estimate:

$$\log\left(\frac{p_{kimt}}{q_{kimt}}\right) = \beta_p D_{im} + \alpha_m + \gamma_t + \pi_{c(k)} + X'_i \varphi + \varepsilon_{kimt}, \quad (3)$$

where p_{kimt} is the price paid for item k , q_{kimt} is the reference quote published for it, and $\pi_{c(k)}$ is a fixed effect for the normalized product cell to which the item belongs.¹⁷ Unlike Equations 1 and 2, this specification is cross-sectional in donor status rather than a difference-in-differences: β_p is the gap in what donor and non-donor suppliers were paid, and I estimate it year by year to ask whether the ban moved it.

Equation 1 recovers the effect of the ban under three assumptions, each of which deserves scrutiny in this setting. The first is parallel trends: that absent the ban, donor and non-donor firms in the same municipality would have moved together. The pre-ban period is not perfectly flat. The 2014 coefficient is -0.639 and insignificant, 2015 is -0.764 and is not, and the joint pre-ban test rejects ($p = 0.042$), though not on the sample matched to donors on the pre-ban path ($p = 0.252$). Two features limit the threat. The pre-trend runs *against* the estimated effect: the premium was rising before the ban and collapses after it, so extrapolating it

¹⁷Product cells are normalized item descriptions recurring at least twenty times. The ratio is winsorized. Adding $\pi_{c(k)}$ to the municipality and year effects means the comparison is between the same product bought in the same municipality in the same year.

would make the true effect larger rather than smaller. And a widening premium across the term following the 2012 election is what the accumulation argument predicts. Applying the relative-magnitudes bound of Rambachan and Roth (2023) in closed form gives a breakdown value of $\bar{M} = 0.86$; the full analysis is in Tables A10 and A11 of the Supporting Information.

A second assumption concerns the outcome itself. Market share is a share, so within a municipality-year the shares of all firms sum to one. When a donor firm loses share, that share accrues to other firms in the same market, some of them non-donors in the control group, and the two groups are therefore not independent as a textbook difference-in-differences assumes. This interference is not a defect of the design. It is what the paper measures: the claim is that the ban redistributed position within a fixed-size market, and a bounded, adding-up outcome is the natural way to measure that redistribution (Baltrunaite, 2020). The estimate is the change in donors' *relative* standing. Two features guard against reading it as an accounting artifact. The effect is heterogeneous exactly as the argument predicts, whereas a mechanical adding-up constraint would bind everywhere. And win rate, which is not subject to the constraint, does not move in the pooled sample.

The design finally requires that firms did not adjust in anticipation of the ban. Treatment exposure is fixed by a donation made in 2012, three years before the reform was proposed, so firms could not have sorted into or out of it in anticipation of a reform that did not yet exist. The reform took effect with the 2016 election, leaving a narrow window for anticipatory behavior on the outcome, but the pre-ban interaction coefficients show no decline in donor market share running up to the ban; if anything the premium peaks in 2016. Because exposure is fixed in 2012 and never re-assigned, the concern that firms re-optimized their donation behavior does not arise.

Three falsification tests bound what else could produce the result, and each should return nothing if the design measures what it claims. Firms that donated to the *losing* 2012 candidate were politically active, willing to pay, and comparable in the traits that make a firm the kind that donates, but they had no relationship with the incoming administration;

if the estimate reflected the character of donor firms rather than their access, these firms should lose share too. Municipal legislative procurement is bought by the chamber rather than by the mayor’s administration, so a municipality-wide shock at the ban would move it while a mayor-specific mechanism would not. And pseudo-treatment dates placed inside the pre-ban window should produce no effect if the 2017 break is the reform rather than an artifact of the panel. All three are reported in Tables A16, A17 and A19 of the Supporting Information, alongside the within-loser comparison (Table A20), the close-race restriction (Table A18) and the sector-by-year test of the 2015–16 recession (Table A21).

Table 1 lists the two predictions and the two contract-level measurements, with the variables and the panel each uses.

Table 1: Predictions, measurements, and the panels they use

	Hypothesis or question	Independent variable	Dependent variable	Panel
<i>Predictions</i>				
H1	The ban reduces donor firms’ procurement market share	Donor $\times$ Post	Procurement market share	Firm–municipality–year (868,528)
H2	The reduction is larger where the pre-ban market was concentrated	Donor $\times$ Post $\times$ Concentration	Procurement market share	Firm–municipality–year (868,528)
<i>Favoritism measured on the contract</i>				
(2)	Does a donor beat the firms bidding against it for the same contract?	Donor, within tender	Won the tender	Bidder–tender (1,504,671)
(3)	Was a donor paid more for the same product?	Donor	Price over the published reference quote	Item, <i>Minas Gerais</i> (4,500,927)

Notes: Donor equals one if the firm contributed to the winning mayoral candidate in that municipality in the 2012 elections; Post equals one from 2017, the first procurement year under the ban; Concentration is the pre-ban Herfindahl index of awarded value, averaged over 2014–2016, computed over non-donor firms and split at the median. Market share is in percentage points, against a pre-ban donor mean of 2.582. Rows (2) and (3) correspond to Equations 2 and 3; they measure favoritism rather than testing a prediction, and (3) is cross-sectional in donor status rather than a difference-in-differences.

Results

The Effect of the Ban on donor Firms' Market Share

Table 2 presents the difference-in-differences estimates across three progressive specifications. Consistent with my prediction, I find that donor firms lost market share after the ban. Firms that donated to the winning 2012 candidate held 2.58% of their municipality's procurement before the ban and gave up 0.70 percentage points of it afterward, a decline of about 27% (Table 2, $p < .001$). The estimate is stable as municipality, year, industry, and legal-form effects and firm controls are added, which suggests that the finding is not driven by compositional differences across municipalities or by observable firm characteristics.¹⁸

The Donor coefficient is also informative. Within the same municipality, and among firms of the same size, age, capitalization, industry, and legal form, firms that had donated to the mayor held substantially more of the procurement market than firms that had not (Table 2, $p < .001$). This is consistent with a politically sustained position, though a pre-ban level alone cannot rule out that donor firms were better in ways the firm registry does not record.

¹⁸Win rate conditional on bidding, an intensive-margin falsification, is null across all specifications. Secondary extensive-margin outcomes are reported in Tables A22 and A23 of the Supporting Information.

Table 2: Progressive Specifications: Market Share

	Market share (pp)					
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.538*** (0.046)					
Donor	2.04*** (0.264)	1.80*** (0.235)	1.74*** (0.245)	0.914*** (0.213)	1.74*** (0.230)	1.23*** (0.268)
Post	-0.136*** (0.025)					
Donor $\times$ Post	-0.707*** (0.195)	-0.700*** (0.186)	-0.728*** (0.198)	-0.807*** (0.181)	-0.655*** (0.183)	-0.798** (0.261)
Log registered capital			0.009*** (0.002)			
Firm age (years)			0.002*** (0.0003)			
Firm size: small			0.037*** (0.006)			
Firm size: other			0.137*** (0.013)			
Observations	866,326	866,326	822,489	798,843	866,326	4,818
R ²	0.006	0.087	0.105	0.406	0.099	0.340
Municipality FE	No	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes
Firm's industry FE	No	No	Yes	No	No	No
Legal form FE	No	No	Yes	No	No	No
Firm FE	No	No	No	Yes	No	No
Year $\times$ Municipality	No	No	No	No	Yes	No
% of pre-ban mean	-27.4	-27.1	-28.2	-31.3	-25.4	-30.9

Notes: The dependent variable is the firm's share of its municipality's procurement, in percent; coefficients are therefore in percentage points. The pre-ban donor mean is 2.582. Column (6) matches each donor firm-municipality to up to three non-donors with the closest pre-ban path. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure 7 presents the effect year by year, with 2016 omitted as the reference. Every post-ban coefficient is negative and significant, and the largest is the last, -1.41 in 2023, seven years and two mayoral cycles after the ban. The effect does not fade. The one soft year is 2020, at -0.79 , when emergency purchasing under the pandemic statute raised the share of tenders bought without a contest from 41% to 47%.

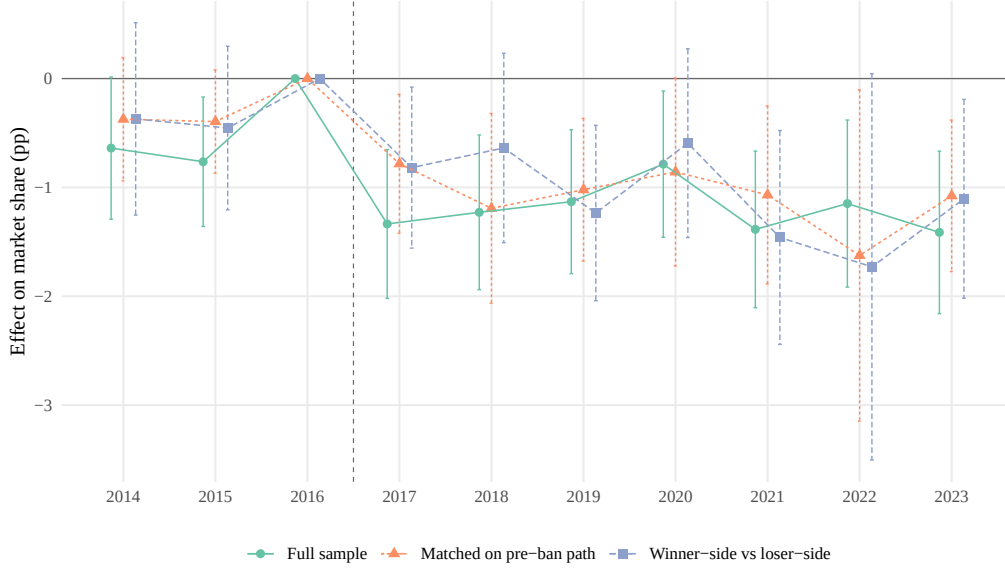


Figure 7: Event study, 2016 omitted

Notes: Coefficients on $\text{Donor} \times 1[\text{year}]$ with 2016 as the reference; bars are 95% confidence intervals from standard errors clustered by municipality. The dashed line marks the first post-ban year. Three counterfactuals are shown: all non-donor firms, firms matched on the pre-ban market-share path, and firms that donated to a losing 2012 candidate.

One concern with this analysis is that donor and non-donor firms may not have been on parallel paths before the ban, in which case the post-ban decline could reflect a trend already underway. The donor premium was 0.64 points smaller in 2014 than in 2016 and 0.76 points smaller in 2015, and the joint test of the pre-ban coefficients rejects ($p = .042$), though it does not reject on the sample matched to donors on their pre-ban path ($p = .252$). The direction of the divergence is worth noting. Donors were gaining on non-donors going into 2016 rather than slipping behind them, so a trend that continued would place donors above their 2016 position in every later year and make the post-ban decline larger than the estimate reports.

To address this concern, I apply the HonestDiD procedure of Rambachan and Roth (2023), which asks how large a post-ban departure from parallel trends would have to be, measured against the largest departure observed before the ban, before the estimate could no longer be distinguished from zero.¹⁹ The estimate survives a departure up to 86% as large as the worst pre-ban one, just short of the conventional benchmark of an equally large departure. I also

¹⁹I use the relative-magnitudes bound in closed form, which is exact for the single-parameter case rather than the full partial-identification procedure.

estimate a synthetic difference-in-differences model, which reweights control firms and pre-ban years rather than assuming parallel trends outright (Arkhangelsky et al., 2021). It returns -0.696 (SE 0.137). The two estimators rely on different identifying assumptions and return the same estimate, which indicates that the finding does not depend on the parallel-trends assumption that the pre-ban test calls into question. The sensitivity analysis is reported in Tables A10 and A11 of the Supporting Information, for the full and matched samples respectively, and the synthetic difference-in-differences estimates in Table A12.

A further concern involves what the donation was buying. If donor firms benefited from general political standing rather than from a relationship with the winning candidate, firms that donated to losing candidates should have lost ground after the ban as well. They did not. Loser-side donors held a pre-ban premium of 0.66 points, a third of the winner-side premium, and retained it afterward ($+0.15$, SE 0.23). A related question is whether the effect runs through the mayor. Procurement administered by the municipal chamber, which the mayor does not control, shows no effect (-0.056), while executive-branch procurement in the same municipalities shows the full effect (-0.700). Table 3 summarizes these tests together with four checks on the estimate itself; each row’s full results are named in the table notes.

Table 3: Robustness Checks

Check	What it asks	What it returns
<i>Tests of the argument</i>		
Loser-side placebo	Was it donating, or donating to the winner?	Loser-side donors held a premium of 0.66 and kept it (+0.15, SE 0.23)
Legislative-branch placebo	Does the effect run through the mayor?	−0.056 where the mayor has no authority, −0.700 where he does
<i>Tests of the estimate</i>		
Pairs cluster bootstrap	Are the standard errors an artifact of rare treatment?	SE 0.181 against the analytic 0.186
Randomization inference	Could reshuffling the donor flag produce this?	None of 1,000 permutations reaches −0.700
Close 2012 races	Is it about which candidate won?	−0.68 to −0.91 across three margin windows
Terciles vs. median	Does the conditionality depend on where the line falls?	Both reject equality ($p = .008$ and $p = .022$)

Notes: The headline estimate is −0.700 (SE 0.186). Full results for every row are in the Supporting Information; the specification and sample ladders are Tables A14 and A15, and the full event study is Table A9.

The Effect by Pre-Ban Market Concentration

Having shown that donor firms lost market share after the ban, I now turn to my second prediction, which expects that the loss should be larger where the pre-ban procurement market was concentrated. Table 4 splits municipalities by the Herfindahl index of awarded value in the pre-ban years, computed over non-donor firms so that a municipality cannot count as concentrated merely because its own donor firm is large there.²⁰

Consistent with my argument, I find that the effect of the ban in curbing favoritism in procurement is concentrated in the municipalities where procurement was concentrated. Donor firms in the most concentrated third of municipalities lost 1.96 percentage points of market share (Table 4, $p < .001$). In the middle and bottom thirds they lost 0.19 and 0.16, neither distinguishable from zero, and the test that the three are equal rejects ($p = .008$).

²⁰The donor-free and raw indices correlate 0.991, and donor firms are 0.64% of the bidder pool, too few to move a Herfindahl index in either direction. Table A24 reports the tercile split and Table A25 the market-level competition measures.

The median split returns the same pattern (-1.16 against -0.19 , $p = .022$), as does the continuous triple interaction on both versions of the index. The relationship is a threshold rather than a gradient: the bottom two terciles are indistinguishable from each other, and the entire separation sits in the top third.

Table 4: Effect by Pre-Ban Market Structure

	Estimate	SE	Pre-ban p	Municipalities
<i>Terciles of pre-ban concentration</i>				
Lowest third	-0.156	(0.091)	0.443	127
Middle third	-0.189	(0.197)	0.093	127
Highest third	-1.962***	(0.560)	0.063	127
Equal across terciles			$p = 0.0077$	
<i>Median split</i>				
Dispersed	-0.191*	(0.081)	—	191
Concentrated	-1.157**	(0.403)	—	190
Equal across halves			$p = 0.0222$	
Continuous, donor-free	-1.652*	(0.694)	triple interaction	
Continuous, raw	-1.668*	(0.669)	triple interaction	

Notes: Concentration is the Herfindahl index of awarded value across firms in the municipality, averaged over pre-ban years and computed over non-donor firms. Each row is estimated separately, then jointly to test equality across groups. “Pre-ban p ” is the joint test of the pre-ban event-study interactions within that group. Standard errors clustered by municipality. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

A lingering question involves whether this pattern reflects market concentration or simply firm size. Donor firms in the top tercile held a pre-ban premium of 3.42 percentage points against 0.43 in the bottom tercile, so a firm losing a constant *fraction* of its position would lose more percentage points in concentrated municipalities with no conditionality at all. To address this concern, I stratify. Cutting donor firms into quartiles of their own pre-ban share and re-running the concentration split inside each quartile conditions on nothing, since non-donor firms remain the within-municipality control group throughout and only the treated side is restricted. Concentration separates the estimates inside every quartile.²¹ This suggests that it is concentration, rather than firm size alone, that accounts for the observed

²¹The band levels are not themselves causal, because cutting on a noisy pre-period outcome induces regression to the mean. What identifies is the within-band difference, which mean reversion affects alike on both sides. Two further approaches were attempted and failed; both are described in the Supporting Information.

pattern.

Two descriptive findings help interpret this result. First, the municipalities where the ban had the largest effect are those in which a handful of firms took most of what the municipality bought, and in which competitors to the donor firm were present but losing. Second, those competitors did not enter after the ban - they began to win. Neither the average number of bidders per tender nor the share of tenders drawing a single firm moves after 2017, which suggests that the contracts donor firms gave up were absorbed by firms already bidding. Where procurement was instead spread across many suppliers, the donor firm was one of many, and little of its position depended on the donation.

I rule out three alternative explanations, each taken up in turn in Tables [A40](#) to [A42](#) of the Supporting Information. One could argue that firms simply had their owners give instead, since individual donations were still legal after the ban, and they rose by nearly 50% in the first election that followed (Reis and Eduardo, [2019](#)). If shareholders and board members started writing the checks their firms used to write, we would expect no change in the favoritism those firms enjoyed when competing for contracts. However, I find that most firms did not substitute individual donations for firm donations. A second alternative explanation is that these firms were simply leaving the market. I find no evidence for this. In fact, donor firms were no more likely than similar firms to stop bidding, and they were less likely to stop bidding in the towns where they had been donor. Finally, one could argue that what mattered was the relationship with a particular mayor rather than the donation itself. This is what most of the literature on political connections would predict, since connections are usually understood as ties to a person that lose their value once that person leaves office (Fisman, [2001](#); Faccio, [2006](#)). If that were the case here, donor firms should have been hurt far less where the 2012 mayor was re-elected and the relationship survived, and far more where the mayor was gone. I find no evidence for this either. This suggests that firms had become entrenched in the local procurement system itself rather than attached to any particular mayor.

Mechanisms

How did firms get favored? By exploiting the variation within tenders, I find that contests were arranged so that a donor firm met little competition. This data let me examine four relevant channels through which favoritism can happen.

First, I can assess the institution that is contracting. If the advantage came from the mayor's control over the administration, it should sit in the unit the mayor controls directly. I find that after the ban, donor firms lost 1.217 percentage points of city hall's procurement market share, against 0.524 points in other agencies within the local executive branch.

Second, I explore variation in the level of competition within tenders. If the connection worked by moving purchases into procedures that require no contest, the effect should show up there, and municipalities should have shifted toward those procedures once the ban took the connection away. I find that...

Then which firms bid. A tender written around one supplier should draw few others, even where the rules require a contest. Among tenders run under procedures in which bidding can be waived, 92.1% drew a single bidder. Among those in which it cannot, 42.4% still did. Against that baseline, donor firms are 10.8 points more likely to be the only bidder and 15.6 points more likely to face fewer than three (Panel C). They are not choosing the procedures where a contest can be skipped: those coefficients are -0.033 and 0.036 , both indistinguishable from zero.

Finally, which donor firms lost. If a position was built on tenders arranged in advance, the firms whose positions were most completely arranged should have lost the most when the arranging stopped. Panel D splits donor firms by whether they had ever lost a tender before the ban. Firms that had won every bid lost 1.383 points. Firms that had ever lost one lost 0.329, which is noise.

One last arrangement can be ruled out. A municipality that wanted to overpay a favored supplier could write the reference quote high and then award at a price that looks reasonable next to it. It did not. Regressing the logged quote on donor status returns 0.046 (SE 0.047),

so the benchmark was not inflated and the premium is overpayment against an honest quote.

Table 5: How did firms get favored?

	Estimate	SE	N
<i>Panel A. Which unit purchased</i>			
City hall	-1.217***	(0.341)	318,004
Other executive units	-0.524	(0.457)	560,491
Difference		$p = 0.234$	
<i>Panel B. Which procedure was used</i>			
Competitive	-0.661**	(0.200)	730,857
In-person auction	-1.346**	(0.462)	191,637
Discretionary	-1.072	(1.426)	159,405
Discretionary vs competitive		$p = 0.819$	
Share discretionary, Post	0.013	(0.017)	—
Donor present $\times$ Post	-0.000	(0.034)	—
<i>Panel C. Which firms bid (pre-ban donor level)</i>			
Only bidder	0.108**	(0.042)	1,027,980
Fewer than three bidders	0.156***	(0.039)	1,027,980
Bidding under a discretionary procedure	-0.033	(0.049)	1,027,980
Bidding under a competitive procedure	0.036	(0.048)	1,027,980
<i>Panel D. Which donor firms lost</i>			
Won every pre-ban bid	-1.383***	(0.325)	864,757
Ever lost a pre-ban bid	-0.329	(0.170)	864,096
Reference quote on donor status	0.046	(0.047)	507,504

Notes: Panels A, B and D report Donor $\times$ Post; Panel C reports the pre-ban donor level, since the question there is what the tenders donor firms entered looked like rather than how they changed. In Panels A and B market share is recomputed within each subset, so a firm's city hall share is its share of what city hall bought rather than of all municipal procurement. The last two rows of Panel B test whether municipalities shifted purchases toward procedures in which competitive bidding may be waived. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Full results are reported in Tables ??, ?? and ?? of the Supporting Information.

Implications: The Cost of Favoritism

That corruption is costly to public budgets is well established (Olken, 2007; Bandiera, Prat and Valletti, 2009; Fisman and Golden, 2017), but measures of how large those costs are remain scarce, because the counterfactual is rarely available: to know whether a donor firm was paid too much, one needs the price the same purchase would have commanded without the connection. Brazilian municipal procurement supplies that benchmark directly. Before bidding opens, a municipality publishes a reference price for each item it intends to buy, and the award record reports what it paid, so every purchase carries its own comparison on a

scale comparable across goods as different as school meals and road repair.

Measuring this requires observing each item purchased rather than the value of each contract, and among the four states studied here only *Minas Gerais* publishes procurement at the item level. This section therefore examines *Minas Gerais* while the preceding analysis draws on all four states. Since it is the largest state in the sample, the case is not a marginal one standing in for the rest. Restricting to the 177 *Minas Gerais* municipalities in the analysis panel, and to items carrying both a usable award price and a reference quote, leaves 4,500,927 items across 2014 to 2024, of which 62,861 were won by donor firms.²²

Table 6 estimates the premium and shows where the capability explanation runs out. The outcome is the logged ratio of the award price to the reference quote, so a larger value means the municipality kept less of the price it had set for itself. Donor firms are paid 16% more than other suppliers with no controls. Comparing firms selling in the same municipality in the same year reduces this to 12%, adding industry, legal form, capital, size, and age reduces it to 10%, and comparing the same normalized product in the same municipality in the same year reduces it to 6.7% ($p < .01$). Of the total reduction, 83% comes from comparing like products rather than like firms. This suggests that a municipality paying 6.7% more for an identical good is not paying for quality it can observe.

²²Coverage year by year is reported in Table A31 of the Supporting Information, along with the quote-integrity and manipulation checks (Tables A32 and A33), the lot-size and item-withdrawal tests (Tables A34 and A35), the price robustness ladder (Table A37) and the exposure and sector splits (Table A38), along with two features of the raw data: discounts against the published quote widened over the period, from a median of 10–12% before the ban to 14–21% afterward, and the share of items bought *above* the municipality’s own quote fell from 14.9% in 2014 to 2.9% in 2024. Municipal purchasing became more disciplined against its own benchmark over the decade, which is the background against which the donor premium has to be read.

Table 6: Award Price Relative to the Reference Price

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Constant	-0.337*** (0.009)						
Donor	0.161*** (0.019)	0.122*** (0.017)	0.104*** (0.018)	0.095*** (0.026)	0.055** (0.019)	0.057*** (0.015)	0.065** (0.021)
Log registered capital			-0.009*** (0.002)	-0.011*** (0.003)			
Firm size class			-0.004 (0.002)	-0.007 (0.004)			
Firm age (years)			0.002*** (0.0003)	0.002*** (0.0004)			
Observations	4,500,927	4,500,927	4,032,204	597,944	670,552	654,706	507,504
R ²	0.001	0.056	0.092	0.136	0.296	0.556	0.752
Municipality FE	No	Yes	Yes	Yes	Yes	Yes	No
Year FE	No	Yes	Yes	Yes	Yes	No	No
Firm's industry FE	No	No	Yes	Yes	No	No	No
Legal form FE	No	No	Yes	Yes	No	No	No
Product FE	No	No	No	No	Yes	No	No
Product-Year FE	No	No	No	No	No	Yes	No
Product-Municipality-Year FE	No	No	No	No	No	No	Yes

Notes: Winsorized log of price over the reference quote published before bidding. Product cells are normalized item descriptions recurring at least twenty times. The sample is items carrying both a usable award price and a reference quote in the 177 *Minas Gerais* municipalities of the analysis panel, 2014–2024. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The premium is a standing difference between firms rather than something the ban created or removed. Estimated year by year against 2016, no post-ban coefficient differs significantly from zero (Table A36), so donor firms were paid more before the ban and continued to be paid more afterward by about the same margin. Municipalities where donor firms had been most active before the ban show no different price path than those where they had been marginal. The tenders donor firms entered did not fill either: returning to Panel C of Table 5, the post-ban change in how often a donor firm was the only bidder is -0.020 (SE 0.033), and in how often it faced fewer than three, -0.004 (SE 0.033). Neither is distinguishable from zero. The ban reduced how often donor firms won without changing what their winning cost or how thin a field they met.

What this cost municipalities can be stated directly. A 6.7% gap means that for every item a donor firm won, the municipality paid roughly seven percent more than it would have paid at the prices other firms charged for the same good in the same year. Across *Minas Gerais* the excess comes to R\$51.6 million over the decade, and across all four states R\$121.4 million (Table A39). As a share of procurement this is one tenth of one percent, or about

R\$68,610 per municipality per year in which a donor firm won anything. This is a transfer from municipal budgets to politically donor suppliers, expensive on the contracts it touched and close to invisible in the aggregate.

Conclusion

That firms financing winning candidates go on to receive more public business has been documented so often that governments around the world have banned corporate donations outright, hoping to curb favoritism in public procurement. Whether those bans achieve what they intend is far less settled. On one hand, they appear to work: former donors become markedly less likely to win contracts (Baltrunaite, 2020), new firms enter local markets, supplier concentration falls, and former donors receive fewer and smaller contracts than they did (Guerra, Mattos and Nakaguma, 2025). On the other hand, the money finds another route. The firms that gave most before a ban shift to giving through individuals, and those personal donations come to buy what corporate ones used to (Aparicio and Avenancio-León, 2022). What we know very little is what explain variation within-countries for the success of this reform.

Under which conditions do campaign finance reforms curb favoritism in public procurement? In this paper, I argue that the success of a donation ban is moderated by how concentrated the procurement market was before it, and specifically by how much of a donor firm's position was politically conditioned to begin with. I find that before the ban, donor firms were favored: they met thinner fields, they won more often than the firms bidding against them for the same contract, and they were paid more for the identical product. After it they won less, giving up about 27% of the share they had held. But the contracts they continued to win carried the same premium as before, and the tenders they entered drew no more competitors than they had. A position built over years of favorable awards does not go away quickly. The specifications written around a firm's products, the officials familiar

with its representatives, and the reference quotes calibrated to what it has always charged all outlast the transaction that produced them. The loss was also uneven, and predictably so. Almost all of it fell in municipalities where a few suppliers had already taken most of the contract money; where purchasing was spread across many firms, donor firms lost nothing distinguishable from zero.

These findings speak to a long-standing question about why firms fund campaigns. On the consumption view, donations are expressive and should carry no measurable economic return (Ansolabehere, de Figueiredo and Snyder, 2003). Removing the channel would then leave procurement unchanged, but it did not. The investment view fits better (Ferguson, 1995; Gilens and Page, 2014), and the results specify what the investment bought: favorable treatment in the allocation of public resources.

What was the cost of such favoritism? Across *Minas Gerais*, local governments paid 6.7% more for every item a donor firm won than they paid for the same good bought from another supplier in the same municipality that year. Over the decade that premium comes to R\$51.6 million, and across all four states R\$121.4 million. Overpricing is the smaller of the two costs. donor firms won R\$820.8 million of the R\$52.02 billion those municipalities awarded, and the ban moved roughly a quarter of their share to other suppliers.

Chile makes the contrast concrete. When it banned corporate contributions in 2016, it did so over a procurement system run through centralized electronic platform under strong national oversight, in which local executives hold little of the discretion that lets political relationships shape who competes and supplier concentration is correspondingly low. The framework developed here predicts that the same reform should have restructured Chilean procurement far less than it did Brazil's, because the condition that drove the Brazilian effect is largely absent. Whether a ban curbs favoritism or merely relocates it is settled, in the end, by the market it meets.

Supporting Information

Additional supporting information may be found in the online Supporting Information for this article.

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Supporting Information

Local Governments, Local Markets: Campaign Finance Reform and
Procurement Favoritism

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1 Fieldwork Interviews

This paper began with a question I could not answer from the data. Brazil had banned corporate campaign donations outright, in a single ruling, everywhere at once, and nobody I asked could tell me whether it had changed how municipalities buy things. So in 2022 and again in 2023 I went to ask the people who would know: the auditors who look for fraud in tenders, the prosecutors who take the cases, the analysts who build the algorithms that flag suspicious contracts, and the officials who write the tenders in the first place. Forty-two semi-structured interviews, conducted in Portuguese and translated here by the author.

Interviewees span the institutions that shape and oversee procurement rather than any single population. They include senior and line auditors in the public-procurement unit of the Federal Court of Accounts; directors and career auditors at the Office of the Comptroller General, including its strategic-information and open-government directorates; officials in the cartel-detection and bid-rigging division of Brazil’s federal antitrust authority; a public prosecutor attached to the audit courts; officials of a state audit court, a state finance secretariat, the National Treasury and the Ministry of the Economy; procurement administrators ranging from a state legislature’s purchasing department to a municipal buyer who signs off on direct purchases herself; scholars of public administration and campaign finance; and civil-society and international organizations, including the United Nations Office on Drugs and Crime.

What they described, repeatedly and without prompting, is the mechanism this paper measures.

The tender is written before it is published

The clearest account came from a procurement specialist who works with federal agencies on how they buy. Asked what would most help prevent corruption, the answer was transparent planning, because “it is very common for managers to ask the bidders themselves for the specifications, and then publish the tender with that slant.” A contest whose terms were drafted by one of the firms competing in it is formally competitive and substantively decided. This is what the bid record shows from the outside: connected firms are roughly eleven percentage points more likely to be the only bidder, in procedures where a contest is legally required.

A senior auditor at the Federal Court of Accounts put the same point in institutional terms, tracing the problem to the “capture of procurement by the political apparatus,” which “installs people who then adopt the conduct that serves whoever appointed them.” Another was blunter: a good part of the tender is “already steered so that only one firm can win.”

The most direct testimony came from the person doing the buying. A municipal purchasing officer described her own post as designated by the president of the chamber rather than filled by examination, and her authority as covering direct purchases up to R\$59,000, a threshold at which the process is “almost automatic.” Asked which procedures her office actually uses, she said they rarely run electronic auctions and mostly issue invitations, and then explained what that means in practice: *only the ones they want get in*. Asked how someone would report a problem, she said there is no way to, because the ombudsmen who would receive the complaint are “hand-picked by the president of the chamber.”

Why local governments

Several interviewees had worked at both levels and volunteered the contrast. An auditor who had served in the purchasing department of her own home town said arrangements among bidders are “more common than in the federal government, given the size of the tenders” and the reach of federal oversight. She named the practice directly: bidders arriving with *cartas marcadas*, marked cards, sometimes in arrangement with the official running the auction, “which can happen in smaller cities.” What sustains it, she added, is tenure: these things “tend to perpetuate with people in the same post for a long time.”

The oversight that might interrupt this is thin and unevenly distributed. A federal auditor observed that his agency does not have “even half the auditors” needed for the number of municipalities receiving decentralized funds. Another noted that many municipalities lack the staff to separate purchasing duties at all, so the person who specifies, the person who selects and the person who pays can be the same person. A senior figure was harsher about the bodies formally responsible: the audit courts, “especially the state and municipal ones, are parcelled out” among political interests, which makes the work harder. A prosecutor put the opportunity plainly: if those courts genuinely audited town halls, “we could have a revolution in this country’s fight against corruption.”

There is also a reason small municipalities receive attention out of proportion to their budgets, and it is not their budgets. As one official remarked of the federal audit lottery, arriving in a town of five thousand people makes headlines easily. The same official’s colleague described the technical limit from the other side: the agency is equipped, as he put it, to eat small fish.

Donations

On campaign finance, interviewees converged on the same structural description. A scholar who has studied Brazilian contributions characterized the pre-2015 system as a market with

concentrated supply and pulverized demand: money came from a handful of very large donors while the electoral system scattered it across thousands of candidates, which gave donors disproportionate leverage. The largest of them, he noted, were construction firms, banks seeking favorable regulation, and health-sector companies. What a large financier wants, in his phrase, is “friendly regulation wholesale: open doors.” And a campaign finance system can be designed, he added, so that it “favors those interested in government contracts.”

A senior official at the Office of the Comptroller General, who had audited state procurement and investigated bid-rigging cartels, described the exchange in one sentence: electoral competition “involves spending a great deal of money on campaigns, and it is the winners who are granted these [procurement] favors.” A political scientist offered the longer genealogy, calling the Car Wash investigation the daughter of the earlier *Mensalão* scandal, both of them symptoms of a campaign finance model that was, in his word, rotten.

Not all of them expected the ban to work. One anti-corruption official said that after 2015 “one tendency was to push things into illegality,” with “unregistered payments that keep circulating” through individuals and intermediaries rather than disappearing. The paper’s shareholder-substitution analysis in Section 8 is an attempt to test exactly that expectation, and finds it true of a few dozen firms and false of the rest.

Interviews

All interviews were conducted under University of Virginia IRB-SBS Protocol No. 5130. Names were collected at the start of each interview for scheduling and consent, as the first question of the instrument below records, and are not reported here or held with the analysis files. Interviewees are cited anonymously by institutional role, and quotations are the author’s translations from Portuguese. Interviewees were selected to span the institutions that shape and oversee procurement. The composition of the sample and the interview instrument follow.

Composition of the sample

Table A1: Interviews by institution

Institution	2022	2023	Total
<i>Federal oversight and control</i>			
Office of the Comptroller General (CGU)	11	3	14
Federal Court of Accounts (TCU)	3	1	4
Council for Economic Defense (CADE)	0	1	1
<i>Federal administration and legislature</i>			
National Congress	2	0	2
Ministry of the Economy	1	0	1
National Treasury	1	0	1
<i>Subnational</i>			
State comptroller general (CGE)	2	0	2
State legislature, purchasing department	0	1	1
<i>Outside government</i>			
Academics	5	3	8
Civil society and international organizations	2	2	4
Institution not recorded	0	4	4
Total	27	15	42

Notes: Counts are interviews rather than individuals. Three 2022 interviewees held a second appointment, most often an academic post alongside a position in federal control, and are counted once under the institution through which they were recruited. Fieldwork ran from 6 June to 27 July 2022 and from 8 June to 15 September 2023, in Brasília, São Paulo, Belo Horizonte and remotely.

Table A2: Interview roster

<i>2022 wave</i>		<i>2023 wave</i>	
ID	Institution	ID	Institution
22-01	Federal Court of Accounts; public prosecution	23-01	Federal Court of Accounts, procurement audit
22-02	Comptroller General; academic	23-02	Civil society organization
22-03	Comptroller General	23-03	Comptroller General; competition authority
22-04	Comptroller General; academic	23-04	Academic; international consultant
22-05	Comptroller General; academic	23-05	Competition authority
22-06	Comptroller General	23-06	Not recorded
22-07	Federal Court of Accounts	23-07	Not recorded
22-08	Comptroller General	23-08	State legislature, purchasing department
22-09	Comptroller General	23-09	Academic
22-10	National Congress	23-10	Academic
22-11	Ministry of the Economy	23-11	Comptroller General, open government
22-12	National Congress	23-12	Not recorded
22-13	State comptroller general	23-13	International organization
22-14	Federal Court of Accounts	23-14	Comptroller General
22-15	Comptroller General	23-15	Not recorded
22-16	Academic		
22-17	Academic		
22-18	Academic		
22-19	Comptroller General		
22-20	Academic		
22-21	National Treasury		
22-22	State comptroller general		
22-23	Civil society organization		
22-24	Comptroller General		
22-25	Academic		
22-26	Comptroller General		
22-27	Civil society organization		

Notes: Identifiers are internal to the project and carry no information about the interviewee. A semicolon separates a second appointment held at the time of interview. The 2022 wave was fielded between 6 June and 27 July 2022, in Brasília and remotely; the 2023 wave between 8 June and 15 September 2023, in Brasília, São Paulo, Belo Horizonte and remotely. Dates are not reported at the level of the individual interview. The 2022 fieldwork record is a scheduling sheet rather than a complete log and covers 22 of the 27 interviews conducted, so per-interview dates could not be assigned with confidence for the whole wave, and reporting an institution alongside an exact date would in several cases identify the interviewee.

Interview Questionnaire

The questionnaire was drafted in English and administered orally in Portuguese. Interviews were semi-structured, so the schedule below was followed in order but not read verbatim, and

follow-up questions varied with what each interviewee raised. Two versions were used. They share Sections I, IV and VI, and differ in the substantive sections between them; the officials' version is given in full, followed by the two sections that replace II, III and V for academics.

Section I. Introduction.

1. Could you kindly provide me with your name?
2. May I ask about your professional experience? How long have you been in your current occupation?
3. Please briefly describe your main occupation and the responsibilities associated with it.
4. What is the primary goal or mission of your institution?
5. Additionally, do you hold any secondary occupation or position? If so, could you briefly describe your responsibilities in that role?
6. What is the primary goal or mission of your secondary institution?
7. I am interested in learning about your career trajectory. Could you share your main professional experiences leading up to your current position?
8. What are your professional goals and expectations for your career? Do you believe that your current occupation will contribute to the achievement of those goals?

Section II. Public procurement.

1. As a bureaucrat with experience in public procurement, what are the key administrative aspects within the public administration system that influence the occurrence of corruption in procurement processes?
2. In your role dealing with public procurement, are there specific administrative structures, processes, or practices that you have observed to be more susceptible to corruption? How do you believe these aspects can be improved to mitigate corruption risks?
3. How do you perceive the influence of organizational culture within the public administration on the occurrence of corruption in public procurement? From a bureaucratic standpoint, what measures can be taken to foster a culture of integrity and ethics in the public procurement process?
4. Based on your experience, have you encountered any successful instances or case studies where specific administrative reforms or changes within the public administration system

have resulted in a significant reduction in corruption in public procurement? What lessons can be drawn from these examples that can inform future efforts?

5. As a public procurement bureaucrat, how can the principles of transparency, accountability, and merit-based decision-making be better integrated into the administrative procedures and practices to enhance the integrity of public procurement processes?
6. From your perspective as a bureaucrat, what are the main challenges or obstacles that hinder the implementation of effective anti-corruption measures within the public procurement system? How do you suggest addressing these challenges from an administrative standpoint?
7. In your opinion, what are the key competencies and skills that public procurement bureaucrats should possess in order to effectively combat corruption in the procurement process? How can these competencies be developed or enhanced within the bureaucratic framework?
8. How can the application of technology, such as e-procurement systems or digital platforms, be leveraged within the bureaucratic processes to improve the efficiency, transparency, and accountability of public procurement?
9. In your experience, what potential benefits can be derived from promoting interagency cooperation and coordination within the public administration system to combat corruption in public procurement? Are there any notable examples of successful collaboration among bureaucratic entities?
10. Drawing on your bureaucratic expertise, what do you consider to be the most crucial administrative reforms or changes that should be prioritized within the public administration of Brazil to establish a more resilient and corruption-resistant public procurement system?

Section III. Bid rigging and political corruption.

1. Do you know what “bid rigging” is? If yes, please state your understanding of bid rigging.
2. How do you think bid rigging can most likely happen in a bidding process?
3. Could you provide an overview of the measures and mechanisms that are currently in place to prevent bid rigging and ensure fair competition in public procurements in Brazil?

4. What challenges do you encounter in detecting and preventing bid rigging practices? How does the government address these challenges effectively?
5. How transparent is the public procurement process in Brazil? What steps are being taken to enhance transparency and accountability in this process?
6. What actions have been taken to address political corruption in public procurements? Can you describe any specific measures or initiatives that have been successful in combating corruption?
7. In your experience, what are the major factors contributing to bid rigging and corruption in public procurements in Brazil? How can these factors be mitigated or eliminated?
8. Are there any recent reforms or policies implemented to improve the integrity of the public procurement system? Can you explain their impact and effectiveness so far?
9. What role does technology play in preventing bid rigging and corruption in public procurements? Are there any innovative solutions being used to enhance transparency and fairness in the process?
10. How does the government collaborate with civil society organizations, academia, and the private sector to combat bid rigging and corruption? Can you provide examples of successful collaborations or partnerships?
11. Are there any ongoing initiatives to strengthen the legal framework governing public procurements in Brazil? What changes or improvements are being considered to address the loopholes that facilitate corruption?
12. How do you ensure that those involved in bid rigging and corrupt practices are held accountable? Can you elaborate on the measures in place to investigate, prosecute, and punish individuals or entities engaged in these activities?

Section IV. Corruption.

1. What is your definition of corruption?
2. What is your institution's definition of corruption?
3. Does your institution combat or prevent (*previne*) corruption? And more broadly, do other institutions within the federal government?
4. Where do the resources for these initiatives come from?
5. During your career, when was the period in which these initiatives had the most financial or material investment?

6. If needed, do you know how to report any corrupt activity at your institution? What is the protocol?
7. Have you ever tried to report any corrupt activity at the federal level? If not, why not? If yes, how did that happen and what were the consequences?
8. Have you ever felt constrained by your occupation from reporting corrupt activity? If yes, why? Or have you ever felt resistance from your peers when reporting corruption in your career?
9. What do you know about the accountability and anti-corruption networks in Brazil? Which institutions are part of them? How do you perceive the interaction between these institutions?
10. What areas within the institution you work for could be improved to combat or prevent corruption?

Section V. Oversight institutions. Asked of interviewees in audit and control bodies.

1. As someone working in oversight institutions, what are the main responsibilities and functions of your role in relation to public procurement? How do you contribute to ensuring transparency and combating corruption in this area?
2. From your experience, what are the most common types of corruption or irregularities that you have come across in public procurement processes? Are there any specific patterns or red flags that indicate potential corruption?
3. How does your institution conduct audits or investigations to detect and address corruption in public procurement? Can you describe the methodologies, tools, and processes employed to uncover irregularities?
4. Are there any specific challenges or obstacles that oversight institutions face in their efforts to combat corruption in public procurement? How do you overcome these challenges to ensure effective oversight?
5. In your view, what role does information sharing and cooperation between oversight institutions and other stakeholders play in combating corruption in public procurement? How do you facilitate collaboration to achieve better results?
6. Are there any notable successes or achievements of your oversight institution in uncovering corruption or promoting integrity in public procurement? Can you share specific examples and the impact they had on improving accountability?

7. How do you assess the effectiveness of anti-corruption measures and reforms implemented within the public procurement system? What indicators or metrics are used to evaluate progress in reducing corruption risks?
8. What measures can oversight institutions recommend or advocate for to enhance transparency and integrity in public procurement? How do you engage with policymakers and other relevant stakeholders to promote these measures?
9. How can technology and data analytics be leveraged within oversight institutions to strengthen their ability to detect and prevent corruption in public procurement? Are there any innovative approaches or tools being utilized in this regard?
10. Based on your expertise and experience, what are the key areas or aspects within the public procurement process that require further attention and improvement to effectively combat corruption? What policy or procedural changes would you prioritize?

Section VI. Close.

1. Is there anything else you would like to add?

The academic version. Sections I, IV and VI are unchanged. The two sections below replace Sections II, III and V.

Public administration and corruption.

1. Based on your research, what are the key characteristics of the public administration system in Brazil that can either contribute to or mitigate corruption in public procurement?
2. Are there specific administrative structures, processes, or practices that are more susceptible to corruption within the public procurement system? How can these be improved to minimize corruption risks?
3. In your view, what role does organizational culture play in shaping the propensity for corruption in public procurement? How can a culture of integrity and ethics be fostered within public administration?
4. Are there any successful examples or case studies where specific reforms or changes in the public administration system have led to a significant reduction in corruption in public procurement? What lessons can be learned from these examples?

5. How can the principles of transparency, accountability, and merit-based decision-making be better incorporated into the public administration of Brazil to enhance the integrity of public procurement processes?
6. Are there any specific challenges or obstacles within the public administration system that hinder effective anti-corruption measures in public procurement? How can these challenges be overcome?
7. In your opinion, what are the key competencies and skills that public administration officials should possess to effectively combat corruption in public procurement? How can these be developed or enhanced?
8. How can the use of technology, such as e-procurement systems or digital platforms, be leveraged to improve the efficiency, transparency, and accountability of public procurement processes within the public administration of Brazil?
9. What are the potential benefits of promoting interagency cooperation and coordination within the public administration system to combat corruption in public procurement? Are there any successful examples of such collaboration?
10. Based on your expertise, what are the most crucial reforms or changes that need to be prioritized within the public administration of Brazil to create a more resilient and corruption-resistant public procurement system?

Public procurement.

1. From your research and analysis, what are the key challenges and vulnerabilities in public procurement processes that can lead to corruption?
2. In your opinion, what are the main factors that contribute to bid rigging and corruption in public procurements? Are there any specific institutional or systemic issues that facilitate such practices?
3. Based on your expertise, what are some effective strategies or reforms that can be implemented to enhance transparency and integrity in public procurements?
4. Can you provide examples of successful anti-corruption measures implemented in other countries or regions that have significantly reduced corruption risks in public procurement? How applicable are these measures to the Brazilian context?
5. What role does academic research play in understanding and addressing corruption in public procurements? Can you share any notable studies or findings that shed light on this issue?

6. How can technology and data analytics be utilized to improve transparency, efficiency, and accountability in public procurement processes? Are there any emerging trends or innovations in this area?
7. In your view, what are the potential consequences of corruption in public procurements for the economy, society, and governance? How does it affect public trust and confidence?
8. Are there any specific legal or regulatory reforms that you believe should be prioritized to combat corruption in public procurements? What are the key elements that should be included in these reforms?
9. How can civil society organizations, academia, and policymakers collaborate to address corruption in public procurements? Are there any successful examples of such collaborations that have yielded positive outcomes?
10. Based on your research and understanding, what are the long-term implications of tackling corruption in public procurements for Brazil's development and governance? How can these efforts contribute to a more transparent and accountable public sector?

2 Sample and Data

The final panels used in this paper were assembled from different sources. It was constructed from five administrative sources that share no common identifier, cover different periods, employ different vocabularies for equivalent objects, and, in the case of the individual taxpayer number, suppress precisely the field required to link firms to donors. This section documents the construction step-by-step: the sources retrieved, the records assembled directly from state audit courts, the methods used to match records across sources, and the validation performed at each stage. Where a construction decision admitted a defensible alternative, I state both the decision taken and the reasoning that supports it.

Data Sources

Table A3 lists them. Two are national registries published in bulk, one is an election archive with a different schema in every release, one is a research dataset that ends before my panel does, and one I collected myself from four state audit courts.

Table A3: Administrative sources

Source	Content	Coverage	Role in the analysis
MiDES (Dahis, Ricca and Scot, 2023)	Harmonized municipal procurement: tenders, bidders, items	2009–2020	Backbone of the procurement panel
State audit courts (SICOM-MG, SAGRES-PB, TCE-PE, TCE-PR)	The same records, collected directly from each portal	2021–2025	Extends the panel past the end of MiDES
TSE campaign finance (<i>prestação de contas</i>)	Every declared donation to every municipal candidate date	2012, 2016, 2020	Defines political connection
TSE candidates and returns (via electionsBR)	Candidacies and zone-level vote counts	2012, 2016, 2020	Identifies who won, by how much, and whether they stayed
Receita Federal CNPJ registry (<i>Empresas, Estabelecimentos, Sócios, Simples</i>)	The universe of registered Brazilian firms, their establishments, their registered partners, and their tax regime	Current snapshot	Firm covariates, and the ownership link that survives the ban

Notes: Two further inputs are hand-assembled rather than downloaded: a codebook of purchasing-organ and spending-unit codes for each municipality, used to classify branch of government, and the Base dos Dados directory that crosswalks TSE municipality codes to IBGE codes.

Extending the procurement record past 2020

MiDES (Dahis, Ricca and Scot, 2023) is the point of departure and it stops in 2020, which is three years after the ban takes effect. A panel that ends there observes the reform’s first term and nothing after it, and the questions this paper asks about persistence—whether the same firms came back through their owners in 2016 and again in 2020—cannot be asked of it at all. So I collected the 2021–2025 records myself, from the four state audit courts that publish them: SICOM in *Minas Gerais*, SAGRES in *Paraíba*, and the transparency portals of the *Pernambuco* and *Paraná* courts of accounts. These arrive as separate state-year extracts and are stored partitioned the same way, one directory per year per state, so that a single state-year can be rebuilt without touching the rest.

Combining the two collections required substantial reconciliation, since they were assembled by different parties for different purposes and share little beyond a common definition of the unit of observation. Three difficulties had to be resolved before the sources could be stacked.

Types. Collectors make different choices about what is a number and what is a string, and a tender identifier that is character in one extract and numeric in another silently fails to join. Every column in every piece is coerced to character before binding, and typed once at the end, on the stacked object, so that the type is a property of the panel rather than of whichever file happened to be read first.

Overlap. My collection and MiDES both cover parts of 2020–2021. Duplicates are removed on the identifier that defines a row in each table—the tender, the item, or the bidder within a tender—with my locally collected rows ordered first, so that on any overlap the more recent extraction survives. The number of dropped overlaps is logged for each table rather than silently absorbed.

Vocabulary. The procedure field is the one the design depends on, since it separates competitive tendering from waived and sole-source purchasing, and each portal writes it differently. *Minas Gerais* appends a spurious .00 to some tender identifiers, which is stripped. More consequentially, SAGRES appends the enabling statute to the procedure name, so that what MiDES calls *pregão presencial* arrives from *Paraíba* as *pregão presencial (Lei Nº 10.520/2002)*. An exact match against the MiDES vocabulary fails on every *Paraíba* row, and fails silently, dropping the state into an unclassified residual. I strip the legal-citation parenthetical with a pattern that recognises the four citation forms in use (*Lei*, *Decreto*, *Medida Provisória*, *MP*) and then match against a controlled vocabulary of fourteen procedure names validated against their frequencies in the published data. Ten labels from Lei 8.666/93 account for 549,346 of the 549,676 tenders in the panel, leaving 0.06% unclassified.

Which municipalities enter the panel

The design compares firms that bid and won against firms that bid and lost, so it needs to observe losing. Brazilian procurement portals differ in whether they publish the full field of bidders or only the winner, and this varies by municipality rather than by state or by year. A municipality that records only winners looks, in the data, like a municipality where every bidder wins.

Rather than assume the problem away or filter on intuition, I measure it. For every municipality I compute the share of its competitive tenders in which at least one losing bidder is recorded, and sort municipalities into three groups: those that never record a loser, those

that record one in fewer than 5% of competitive tenders, and those that record them routinely. The panel keeps the third group. This yields the 381 municipalities used throughout the paper—177 in *Minas Gerais*, 121 in *Paraná*, 48 in *Pernambuco* and 35 in *Paraíba*. The rule is a property of the reporting, not of the outcome: it is computed before any donor variable exists and before any tender value is examined, and it conditions on how a municipality publishes rather than on what it bought or from whom. Table A9 compares the municipalities that clear the rule with those that do not.

Which purchases count as the mayor’s

The treatment is a mayoral donation, so the exposed purchases are the ones a mayor controls. Municipal buying in Brazil is not all executive: the city council, the municipal courts, and the local audit and prosecutorial offices run their own tenders, and a donation to the mayor buys nothing in a council procurement. No source carries a branch field, so it has to be built.

What the records do carry is the free-text name of the purchasing organ and of the spending unit, and those names are informative—*Câmara Municipal de Uberaba* and *Secretaria Municipal de Saúde* are not ambiguous to a reader. They are ambiguous to a string comparison, which is the problem to solve. The branch variable is therefore constructed from text: I convert several hundred thousand unstructured entity names into a categorical measure using a dictionary classifier, the approach to text as data best suited to a task in which the relevant categories are known in advance and the vocabulary that signals them is closed and institutionally fixed (Grimmer and Stewart, 2013). Construction proceeds in four steps.

First, normalization. Names are transliterated to ASCII so that *Educação* and *Educacao* are the same token, lowercased, stripped of punctuation, and whitespace-squished. Second, a hand-assembled codebook maps the numeric organ and spending-unit codes each state uses to the entity names they stand for, joined on the municipality and the zero-padded code. Third, classification by dictionary: a legislative dictionary (*câmara*, *vereador*, *tribunal de contas*), a judicial one (*vara*, *foro*, *ministério público*, *defensoria*, *procuradoria geral de justiça*), and an executive dictionary of roughly ninety administrative terms covering the forms municipal executive units actually take—*prefeitura*, *secretaria*, *fundo municipal*, *autarquia*, *fundação*, *serviço autônomo de água e esgoto*, and the rest. The rules are applied in order, most specific first, so that a unit whose name contains both *procuradoria* and *justiça* resolves to the judiciary rather than to the executive.

Fourth, the state-specific fallbacks. Two states depart from the general procedure, and each departure is handled explicitly rather than permitted to fail silently. *Paraná*’s collector stores the entity name in the spending-unit field but publishes no organ or unit codes, so every

codebook join returns missing for every *Paraná* row; the classifier falls back to the inline text, which is the same information in a different column. *Minas Gerais*’s codebook has gaps, and there SICOM’s own convention resolves them: organ 01 is the *Câmara Municipal*, 02 is the *Prefeitura*, and 03 upward are secretariats, autarchies and funds. A purchase that reaches the end of the ladder unclassified is labelled unknown and reported as such rather than assigned to the executive by default. Dictionary classifiers are only as reliable as the correspondence between their lexicon and the corpus to which they are applied, so the lexicons used here were constructed against the observed distribution of entity names in these four states rather than imported from another setting, and the unclassified residual is reported rather than suppressed. Table A5 reports the resulting distribution. The same classification produces the city-hall indicator used in the mechanism tests, which flags purchases made by the *Prefeitura* itself as against those made by subordinate executive units.

Identifying political connection

This is the step on which the paper’s identification depends, and it is substantially more demanding after the ban than before it.

Firms are identified by the eight-digit root of their tax number, which designates the legal entity, rather than by the full fourteen-digit number, which designates one of its establishments. A company that bids through its headquarters in one tender and a branch in the next is one firm, and treating it as two would attribute a firm’s own withdrawal to entry and exit.

Before the ban, the link is direct. Corporate donations were legal and reported with the donor’s tax number, so a 2012 firm donor is identified by joining the donation record to the bidding record on municipality and tax root. A donation counts only in the term it could purchase: 2012 donations identify connection over 2013–2016, the term of the mayor elected with them.

After the ban, there is no such number to join on. Corporate donations are illegal from 2016 onward, so if the exchange continued it continued through people—the firm’s owners donating in their own names. Testing that requires linking an individual donor in the campaign-finance archive to a registered partner in the corporate registry, and neither source publishes an identifier that permits it. Both mask the individual taxpayer number: the TSE publishes eleven digits with five suppressed, and the Receita Federal partner registry stores only the six unsuppressed digits. There is no key.

What there is, is a masked identifier and a name, on both sides. This is a record-linkage problem of the kind that arises whenever administrative datasets must be merged in the absence of a shared key (Enamorado, Fifield and Imai, 2019). Where the probabilistic

approach estimates match probabilities from observed agreement patterns across fields, the structure here permits something stricter: one field is a partial identifier, informative but not unique, and the other is a name, discriminating but not standardized. I therefore employ blocking followed by deterministic verification, with the verification step made deliberately conservative. The block is the six visible tax digits together with the donor’s first given name; candidate pairs within a block are then *verified* by requiring exact equality of the complete normalized name. Blocking on the digits and the first name makes the comparison tractable across tens of millions of partner records; the exact full-name requirement is what turns a candidate pair into a match. A shared surname does not link; a shared first name does not link; initials do not link.

Four filters guard the match against its predictable failure modes. Records with fewer than two name tokens are dropped, since a single token is consistent with too many people to verify. The sentinel tax number 00000000000, used in the archive for undeclared donors, is dropped rather than matched. Corporate partners are dropped from the partner registry before matching—a firm whose registered partner is another firm carries a company name in the person field, and strings containing *LTDA*, *S.A.*, *EIRELI*, *EPP*, *MEI*, *FUNDO*, *CIA* or *INCORPORA* are removed. And names are normalized on both sides before comparison by the same transliterate-uppercase-trim routine, so that the exactness of the final comparison is exactness about the name rather than about its accents.

The residual risk is a false positive: two distinct individuals who share a complete name and the same six visible tax digits, and who are both connected to the same municipality. Such cases are possible, though rare, and their consequence is attenuation rather than inflation, since a false match assigns connected status to a firm that does not possess it. The procedure runs separately for 2016 donations, which identify connection over 2017–2020, and for 2020 donations, which identify connection over 2021–2024.

Three further constructions follow from the same matching machinery.

The losing-donor control. The same matching procedure is applied to donations made to candidates who *lost*. These firms undertook the same actions as the treated firms—they selected a candidate, made a contribution, and bore the associated risk—but emerged from the election without an officeholder. They therefore provide the appropriate comparison for any account resting on donation behaviour rather than on access, and they are employed as such in the falsification tests.

Persistence. A firm that donated corporately in 2012 and whose registered partners then donated personally in 2016, and again in 2020, is flagged as persistent. This is the direct test of whether the ban closed the channel or renamed it.

New entry. The symmetric flag identifies firms that did *not* donate corporately in 2012

but whose partners entered the personal channel afterwards. Without it, persistence could not be distinguished from a general post-ban increase in individual giving.

Firm attributes

Covariates come from three tables of the CNPJ registry, joined on the tax root. *Empresas* supplies legal form, the official size band, and registered capital, which is published in Brazilian decimal notation and parsed accordingly before logging. *Simples* supplies enrolment in the simplified tax regime and micro-entrepreneur status, both of which condition eligibility for the bidding preferences Lei Complementar 123/2006 grants small firms. *Estabelecimentos* supplies sector, registration status and founding date, from which firm age is computed; it is restricted to headquarters records so that a firm with forty branches contributes one sector rather than forty. Each lookup is reduced to one row per firm before joining, which is what keeps a covariate merge from multiplying the panel.

Campaign finance across three release formats

The TSE has redesigned its campaign-finance schema twice in the period covered. The 2012 files carry human-readable Portuguese headers with accents and inconsistent internal spacing (CPF/CNPJ do doador, Sigla Partido); 2016 and after carry machine codes (NR_CPF_CNPJ_DOADOR, SG_PARTIDO); and some fields changed name again in 2020. Rather than write one reader per release, I maintain an explicit schema crosswalk that maps twenty-seven canonical fields to every historical name each has borne, with a case-insensitive fallback for spacing and capitalization drift. Adding a future release means extending the crosswalk, not rewriting the pipeline.

One substantive change could not be handled by renaming. Through 2016 the archive labelled donations by source—*Recursos de pessoas físicas*, *Recursos de pessoas jurídicas*—which is exactly the individual-versus-corporate distinction the paper needs. In 2020 the TSE collapsed private donations into a single *Outros recursos* category and the label stops being informative. I recover the distinction from the donor’s own identifier, which is unambiguous: eleven digits is a natural person, fourteen is a firm. The pipeline asserts that every election year contributes a non-empty set of donations after filtering, and identifies the schema crosswalk as the probable source of failure if one does not, since a year that silently reads as empty is the most consequential error and the least visible one.

Elections

Candidacies and zone-level returns come from the TSE through `electionsBR`. Votes are summed from the zone to the candidate, candidates are joined to their returns, and the final round is retained. Margins are computed with explicit runoff logic: where a municipality went to a second round, the margin is between the second-round winner and the second-round runner-up; otherwise it is the top-two margin in the first round. Taking the first-round margin everywhere would misstate exactly the competitive municipalities where margin matters most. Mayor identity, party, and margin are carried for all three cycles, from which incumbency and party-continuity flags are constructed for the 2012→2016 and 2016→2020 transitions; these support the mayoral-turnover tests reported below. Municipality codes differ between the electoral and statistical systems, so TSE codes are crosswalked to IBGE codes through the Base dos Dados directory, with leading zeros stripped on both sides before the join.

The item panel

The price evidence runs on individual line items rather than tenders, and those exist only for *Minas Gerais*, which publishes both the unit price paid and the reference quote the administration published as its expectation before bidding opened. *Paraná* began publishing item prices in 2022, entirely after the ban, and *Pernambuco* and *Paraíba* publish none. The item analysis is therefore restricted to the 177 *Minas Gerais* municipalities of the panel and says so.

Holding the product fixed requires knowing what a product is, and the records carry only a free-text description. The same good is written a dozen ways across municipalities and years, and the variation is systematic rather than random: case and punctuation vary freely, and embedded digits usually encode a lot or a package size rather than the good itself. I construct a product key by lowercasing, stripping punctuation, stripping digits, squishing whitespace, and appending the unit of measure, so that a description and its unit together define the product. Products are retained when the key recurs at least twenty times, which is what makes a within-product comparison possible at all.

That retention rule changes the sample as well as the specification, and the two effects have to be separated. The item tables therefore report a bridge row: the paper’s own specification, unchanged, estimated on the product subsample. If the estimate there sits near the full-sample figure, the movement between the two ladders is the product controls doing work; if it sits near the product rows, the movement is the change of sample and the product rows are answering a different question about a different set of goods. Reporting the ladder without that row would leave the two indistinguishable.

Retrieval at scale

The corporate registry is published as approximately forty gigabytes of compressed monthly extracts, divided into ten slices per table, and its retrieval required non-standard procedures. The documented public paths return 404; the files are reachable only through a WebDAV share endpoint whose share token has to be passed as a credential. Downloads run in parallel in the background with resume enabled, so an interrupted transfer continues rather than restarts, with retry limits, reconnection on refusal, and a minimum-throughput abort that kills a stream that has degraded to nothing instead of leaving it to hang.

A supervisor polls the transfers every thirty seconds and classifies each as growing, stalled, complete or abandoned, since a failed download and a slow one are indistinguishable from file size alone at any single moment. Two integrity checks catch what the transport layer misses: archives are verified by reading their central directory before extraction, and any archive that fails is deleted and re-fetched automatically rather than partially extracted; and any file under one megabyte is treated as a captured error page rather than data and removed, since a 404 served with a 200 status is otherwise indistinguishable from a very small file. Extraction is selective—only the candidate-revenue tables for the four analysis states are unpacked from each national archive, which holds the working footprint to roughly 150 megabytes per election cycle rather than 600.

What is checked, and what is reported

Every join that should not change the number of rows is asserted to not change it, and the build halts if one does. Every merge key is asserted unique on the side it is merged from before the merge runs. Deduplication reports how many rows it dropped rather than absorbing them. The classification steps print their full cross-tabulation—branch by state, reporting category by state, donations by year and state—so that a state silently dropping out is visible in the log rather than discovered in a coefficient. These are the checks that turned up the two data problems documented in the rest of this section: the reporting heterogeneity that determines which municipalities can be studied, and the 2022 source break described below.

Table A4: Procurement activity per municipality-year

	Median	Mean	P25	P75
<i>Procurement activity, per municipality-year</i>				
Tenders run	85	144	51	146
Distinct suppliers	145	228	81	263
Bidders per tender	2.20	2.55	1.80	2.82
Share of tenders with one bidder	0.593	0.582	0.489	0.692
Awarded value (R\$ millions)	19.05	186.54	9.81	43.98
Procurement / municipal revenue	—	—	—	—

Notes: One observation per municipality-year across the 381 municipalities of the analysis panel, 2014–2023. Counts are of distinct tenders and distinct winning suppliers. Values are nominal reais and are not deflated.

Table A5: Non-competitive procurement and political staffing

	Median	Mean	P25	P75
<i>Non-competitive procurement, share of tenders</i>				
No contest held (<i>dispensa</i> , <i>inexigibilidad</i>)	0.267	0.276	0.165	0.353
Convite	0.011	0.031	0.001	0.042
Either, broad definition	0.291	0.307	0.204	0.391
Single bidder	0.596	0.589	0.522	0.663
<i>Political control of the bureaucracy</i>				
Commissioned share, direct administration	0.071	0.085	0.049	0.107

Notes: One observation per municipality, pooling 2014–2023. Shares are computed over distinct tenders rather than over bidder–tender records. Waived bidding (*dispensa*) and sole source (*inexigibilidad*) dispense with a contest altogether; invitation-only tendering (*convite*) is formally competitive but leaves the administration to choose which registered suppliers are asked to bid.

Table A6: Coverage and missingness

	Share missing
Contract value	0.0277
Firm's industry	0.0000
Legal form	0.0000
Registered capital	0.0490
Firm age	0.0007
Bidders per tender	0.0000
Bidder-tender observations	1,504,671
Municipalities	381
Years	2014–2023

Notes: Share of bidder–tender records for which each field is missing, across the 381 municipalities of the analysis panel, 2014–2023. Registered capital and contract value are the only fields missing on more than one percent of rows, and neither is used to define treatment.

Table A7: Panel balance by year

Year	Municipalities	Firms	Donor firms
2014	381	37,968	462
2015	381	35,831	380
2016	381	33,080	304
2017	381	41,134	320
2018	381	39,136	281
2019	381	40,464	253
2020	381	37,690	202
2021	381	43,261	221
2022	381	34,591	178
2023	381	35,645	157

Notes: Observations and distinct firms by year in the firm–municipality–year panel. The panel is balanced on municipalities by construction and unbalanced on firms, since a firm enters in the years it bids and leaves in the years it does not.

Table A8: Descriptive statistics by donor status

	Mean	SD	Mean	SD
	Winner-side		Other	
mkt share	2.093	4.030	0.434	1.400
n bids	2.361	3.972	1.730	3.250
n wins	2.019	3.080	1.181	2.108
log capital	12.099	2.524	12.014	2.558
firm age	21.082	12.123	12.428	11.449
firm size	2.939	1.647	2.662	1.594

Notes: Means and standard deviations by donor status. Firm–municipality–year panel, 381 municipalities across *Minas Gerais*, *Paraná*, *Pernambuco* and *Paraíba*, 2014–2023, with donor status fixed by the 2012 mayoral election. Market share is the firm’s share of its municipality’s procurement, in percent. Donor firms are larger, older and better capitalized than the average bidder, which is why the estimating equation holds those characteristics fixed.

Table A9: Municipality profile, with and without donor firms

	With donors	Without	<i>p</i>
Population	51,303.04	45,467.23	0.706
Pre-ban HHI	0.06	0.07	0.232
Bids per firm-year	1.52	1.39	0.000
Market share (pp)	1.09	1.17	0.658
2012 victory margin	0.16	0.15	0.798
Bidding firms	390.45	332.00	0.223
Municipalities	329	52	

Notes: Municipality characteristics by whether the municipality clears the loser-reporting rule described above and therefore enters the panel. Differences are descriptive and are not adjusted for anything.

Table A10: Overview of the data

Statistic	Value
<i>Sample coverage</i>	
Municipalities	381
Minas Gerais	177
Paraná	121
Pernambuco	48
Paraíba	35
Unique bidding firms	152,245
Unique tenders	549,676
Bidder–tender observations	1,504,671
Pre-ban (2014–2016)	398,303
Post-ban (2017–2023)	1,106,368
Firm–municipality–year observations	868,528
Years	2014–2023
<i>Unique firms by donor status</i>	
Donor firms (donated to winning 2012 mayor)	710
Non-donor firms (all other bidding firms)	151,869
Donor in one municipality, non-donor in another	334
Loser-side donor firms (hedgers included)	302
Substitution donor firms (2016 cycle)	25
Substitution donor firms (2020 cycle)	21
<i>Bidder–tender observations by donor status</i>	
Donor firm observations	6,635
Non-donor firm observations	1,498,036
Donor share of observations (%)	0.44
<i>Municipality characteristics</i>	
Concentrated municipalities (above-median HHI)	190
Dispersed municipalities (below-median HHI)	191
Incumbent mayor municipalities	92
New mayor municipalities	289
<i>Pre-ban outcomes by donor status</i>	
Mean market share per firm, donors (%)	2.582
Mean market share per firm, non-donors (%)	0.538
Combined donor share of a municipality’s procurement (%)	2.661
Mean win rate, donor firms	0.863
Mean win rate, non-donor firms	0.681
<i>Tender characteristics</i>	
Competitive tenders	25
Discretionary tenders	200,069
Other or unclassified procedure	893
Average number of bidders per tender	2.74

3 Timing, Inference, and Sensitivity

The event study behind Figure 1, the Rambachan–Roth relative-magnitudes bound in closed form, the synthetic difference-in-differences estimate, inference procedures that do not rely on cluster-robust asymptotics, and the extended specification ladder.

Table A11: Event-study coefficients

	Market share (pp)	
	Full sample (1)	Matched on pre-ban path (2)
Donor	2.31*** (0.385)	1.48*** (0.349)
Donor $\times$ Year = 2014	-0.639 (0.333)	-0.374 (0.289)
Donor $\times$ Year = 2015	-0.764* (0.303)	-0.395 (0.242)
Donor $\times$ Year = 2017	-1.34*** (0.349)	-0.783* (0.326)
Donor $\times$ Year = 2018	-1.23*** (0.362)	-1.19** (0.445)
Donor $\times$ Year = 2019	-1.13*** (0.337)	-1.02** (0.334)
Donor $\times$ Year = 2020	-0.786* (0.343)	-0.858 (0.441)
Donor $\times$ Year = 2021	-1.39*** (0.367)	-1.07* (0.417)
Donor $\times$ Year = 2022	-1.15** (0.391)	-1.63* (0.777)
Donor $\times$ Year = 2023	-1.41*** (0.381)	-1.08** (0.355)
Observations	866,326	4,818
R ²	0.087	0.340
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Joint pre-ban p	0.04157	0.2517

Notes: Coefficients on Donor $\times$ 1[year] with 2016 as the reference year, from the specification with municipality and year effects. Firm–municipality–year panel, 381 municipalities across *Minas Gerais*, *Paraná*, *Pernambuco* and *Paraíba*, 2014–2023, with donor status fixed by the 2012 mayoral election. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A12: Sensitivity to violations of parallel trends, full sample

$\bar{M}$	CI at $h = 1$	CI at last horizon	Signed throughout
0.00	[-2.019, -0.652]	[-2.159, -0.667]	Yes
0.25	[-2.210, -0.461]	[-3.496, 0.670]	No
0.50	[-2.401, -0.270]	[-4.833, 2.007]	No
1.00	[-2.783, 0.111]	[-7.507, 4.681]	No
1.50	[-3.165, 0.493]	[-10.181, 7.354]	No
2.00	[-3.547, 0.875]	[-12.854, 10.028]	No
Breakdown $\bar{M}$	0.86		

Notes: Sensitivity of the headline estimate to violations of parallel trends, following Rambachan and Roth (2023), using the relative-magnitudes bound in closed form. $\bar{M}$ is the size of a post-ban departure from parallel trends, measured against the largest departure observed before the ban, at which the estimate ceases to be distinguishable from zero. Full sample.

Table A13: Sensitivity to violations of parallel trends, matched sample

$\bar{M}$	CI at $h = 1$	CI at last horizon	Signed throughout
0.00	[-1.421, -0.145]	[-1.773, -0.381]	No
0.25	[-1.520, -0.046]	[-2.464, 0.310]	No
0.50	[-1.618, 0.053]	[-3.155, 1.001]	No
1.00	[-1.816, 0.250]	[-4.538, 2.384]	No
1.50	[-2.013, 0.448]	[-5.920, 3.766]	No
2.00	[-2.211, 0.645]	[-7.302, 5.149]	No
Breakdown $\bar{M}$	0.37		

Notes: As in the preceding table, estimated on the sample in which each donor firm-municipality is matched to up to three non-donors with the closest pre-ban market-share path. The pre-ban joint test does not reject on this sample.

Table A14: Synthetic difference-in-differences

Estimator	Estimate	SE	Inference
TWFE, balanced panel	-0.984***	(0.144)	Clustered by municipality
DiD, same matrix	-0.984	—	—
Synthetic control	0.593	—	Diagnostic; $T_0 = 3$
Synthetic DiD	-0.696***	(0.137)	Municipality bootstrap, $B = 300$
jackknife SE, for reference	—	(0.132)	Treats pairs as independent
95% CI (bootstrap)	[-0.978, -0.428]		
Treated pairs	577		
Control pairs	25,000 of 145,146		
Pre-ban years	2014–2016		
Time weights	0.46 / 0.30 / 0.24		
Reweighting moves the estimate	-0.984 $\rightarrow$ -0.696		
Leave-one-pre-year-out range	[-0.769, -0.574]		

Notes: Synthetic difference-in-differences (Arkhangelsky et al., 2021), which reweights control firms and pre-ban years rather than imposing parallel trends. Firm–municipality–year panel, 381 municipalities across *Minas Gerais*, *Paraná*, *Pernambuco* and *Paraíba*, 2014–2023, with donor status fixed by the 2012 mayoral election. The estimator relies on different identifying assumptions from the headline specification, so agreement between the two is informative about robustness rather than about precision.

Table A15: Bootstrap and randomization inference

	Estimate	SE	p	95% CI
Clustered by municipality	-0.7003***	0.1857	0.0002	[-1.0642, -0.3364]
Randomisation inference	-0.7003	—	0.0000	—
Pairs cluster bootstrap	-0.7003	0.1812	—	[-1.0852, -0.3589]
Clusters	381			
Draws	1000 / 1000			

Notes: Inference procedures that do not rely on cluster-robust asymptotics. The pairs cluster bootstrap resamples municipalities with replacement; randomization inference permutes the donor flag within municipality. Both use 1,000 replications and seed 20260815. Reported because treatment is rare, at 710 donor firms against roughly 152,000 non-donors, which is the condition under which analytic cluster-robust errors are least reliable.

Table A16: Specification, extended

	Market share (pp)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Donor	1.80*** (0.235)	0.914*** (0.213)		1.79*** (0.234)	1.74*** (0.230)	1.23*** (0.268)	2.17*** (0.389)	1.84*** (0.457)
Donor $\times$ Post	-0.700*** (0.186)	-0.807*** (0.181)	-0.852*** (0.201)	-0.699*** (0.185)	-0.655*** (0.183)	-0.798** (0.261)	-0.908** (0.339)	-0.723* (0.364)
Observations	866,326	798,843	534,363	866,326	866,326	4,818	354,374	191,073
R ²	0.087	0.406	0.590	0.089	0.099	0.340	0.075	0.072
Municipality FE	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	No	No	No	No	No
Firm-Municipality FE	No	No	Yes	No	No	No	No	No
state-Year FE	No	No	No	Yes	No	No	No	No
Year $\times$ Municipality	No	No	No	No	Yes	No	No	No

Notes: The estimating equation with fixed effects and firm controls added in sequence, so that the movement of the coefficient across columns is visible. Firm–municipality–year panel, 381 municipalities across *Minas Gerais*, *Paraná*, *Pernambuco* and *Paraíba*, 2014–2023, with donor status fixed by the 2012 mayoral election. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A17: Alternative samples

	Market share (pp)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Donor	1.53*** (0.313)	1.79*** (0.246)	1.50*** (0.208)	2.56*** (0.325)	1.02*** (0.104)	1.67*** (0.246)	1.61*** (0.215)	1.62*** (0.216)
Donor $\times$ Post	-0.738** (0.241)	-0.685*** (0.193)	-0.485** (0.160)	-1.05*** (0.287)	-0.201** (0.074)	-0.334 (0.197)	-0.319 (0.163)	-0.518** (0.178)
Observations	631,658	805,748	723,969	437,603	866,326	866,326	866,326	801,399
R ²	0.078	0.083	0.089	0.083	0.146	0.087	0.087	0.085
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The headline specification re-estimated on alternative samples. Firm–municipality–year panel, 381 municipalities across *Minas Gerais*, *Paraná*, *Pernambuco* and *Paraíba*, 2014–2023, with donor status fixed by the 2012 mayoral election. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

4 Placebos and Falsification

Tests that should return nothing if the design is measuring what it claims. Municipal chamber procurement is not controlled by the mayor; pseudo-treatment dates fall inside the pre-ban period; loser-side donors are pseudo-treated as though they had backed the winner.

Table A18: Legislative-branch placebo

	Market share (pp) Legislative: market share (1)	Tenders entered Legislative: bids (2)	Market share (pp) Executive: market share (3)
Donor	3.39 (1.93)	0.054 (0.084)	1.80*** (0.235)
Donor $\times$ Post	-0.543 (2.11)	-0.056 (0.103)	-0.700*** (0.186)
Observations	46,273	46,273	866,326
R ²	0.409	0.054	0.087
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: Procurement run by the municipal chamber, which the mayor neither staffs nor administers, against executive procurement in the same municipalities. A null on the legislative side is the placebo passing. The legislative panel is small: it contains 122 donor bidder-rows across 119 tenders in 50 municipalities, which is why its standard errors are an order of magnitude larger than the executive ones. A pooled specification interacting donor status with branch returns -0.075 (SE 0.121) on the triple interaction, so the two branches cannot be formally distinguished, and this table is reported as consistent with a mayor-specific mechanism rather than as a test of one. Standard errors clustered by municipality in parentheses. $*p < 0.05$, $**p < 0.01$,

$***p < 0.001$.

Table A19: Pseudo-treatment dates

	Market share (pp) Pseudo 2015 Actual 2017 (1) (2)	
Donor	1.57*** (0.238)	1.80*** (0.235)
Donor $\times$ Post	0.226 (0.222)	-0.700*** (0.186)
Observations	208,131	866,326
R ²	0.111	0.087
Municipality FE	Yes	Yes
Year FE	Yes	Yes

Notes: The panel is restricted to the pre-ban years and the ban is assigned to an interior year, so that the actual reform falls outside the sample entirely and any effect the design finds must be a pre-existing trend. With three pre-ban years only one interior date is available. The final row repeats the estimate at the true date for comparison. Standard errors clustered by municipality in parentheses. $*p < 0.05$, $**p < 0.01$,

$***p < 0.001$.

Table A20: Close 2012 mayoral elections

	Market share (pp)			
	Margin j= 20 pp	Margin j= 10 pp	Margin j= 5 pp	All municipalities
	(1)	(2)	(3)	(4)
Donor	1.76*** (0.294)	2.17*** (0.389)	1.84*** (0.457)	1.80*** (0.235)
Donor $\times$ Post	-0.681** (0.236)	-0.908** (0.339)	-0.723* (0.364)	-0.700*** (0.186)
Observations	601,773	354,374	191,073	866,326
R ²	0.081	0.075	0.072	0.087
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Municipalities	286	180	97	381

Notes: The headline specification restricted to municipalities whose 2012 mayoral race was decided within the margin shown, so that which candidate won approaches arbitrary. Firm–municipality–year panel, 381 municipalities across *Minas Gerais*, *Paraná*, *Pernambuco* and *Paraíba*, 2014–2023, with donor status fixed by the 2012 mayoral election. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A21: Loser-side donors, pseudo-treated

	Market share (pp)		
	Loser-side, no controls	Loser-side, full controls	Winner-side (for scale)
	(1)	(2)	(3)
pseudo	0.731*** (0.202)	0.659** (0.199)	
pseudo $\times$ Post	0.150 (0.229)	0.158 (0.241)	
Log registered capital		0.010*** (0.002)	
Firm age (years)		0.002*** (0.0003)	
Firm size: small		0.034*** (0.006)	
Firm size: other		0.133*** (0.013)	
Donor			1.80*** (0.235)
Donor $\times$ Post			-0.700*** (0.186)
Observations	863,516	819,930	866,326
R ²	0.083	0.101	0.087
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm's industry FE	No	Yes	No
Legal form FE	No	Yes	No

Notes: Firms that donated to a *losing* 2012 mayoral candidate, pseudo-treated as though they had backed the winner. These firms selected a candidate, paid, and bore the risk, and differ from the treated group only in the outcome of an election they did not control, so a null here is what separates the effect of access from the effect of donating. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$,

*** $p < 0.001$.

Table A22: Loser-side donors, within-loser comparison

	Market share (pp)				
	(1)	(2)	(3)	(4)	(5)
Donor	0.623*	0.547*		0.340	
	(0.246)	(0.261)		(0.249)	
Donor $\times$ Post	-0.740**	-0.724*	-0.770*	-0.223	-0.996
	(0.272)	(0.308)	(0.375)	(0.262)	(0.611)
Log registered capital		-0.172*			
		(0.082)			
Firm age (years)		-0.001			
		(0.011)			
Firm size: small		0.581*			
		(0.254)			
Firm size: other		1.21**			
		(0.460)			
Observations	3,118	2,842	2,930	1,519	1,599
R ²	0.464	0.487	0.591	0.402	0.478
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Firm's industry FE	No	Yes	No	No	No
Legal form FE	No	Yes	No	No	No
Firm FE	No	No	Yes	No	No
Municipalities	374	374	374	113	261

Notes: Winner-side against loser-side donors. Firms that gave to both a winner and a loser are dropped, since they are neither cleanly treated nor cleanly control. The contrast is within municipality, so only the 113 municipalities holding firms of both kinds contribute to identification, and that rather than the 381 is the effective cluster count. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A23: Sector-by-year effects and the 2015–16 recession

	Market share (pp)				
	(1)	(2)	(3)	(4)	(5)
Donor	1.80***	1.77***	1.67***	1.67***	1.69***
	(0.235)	(0.234)	(0.219)	(0.219)	(0.248)
Donor $\times$ Post	-0.700***	-0.688***	-0.608***	-0.609***	-0.579**
	(0.186)	(0.185)	(0.171)	(0.172)	(0.218)
Observations	866,326	866,302	866,309	866,290	730,876
R ²	0.087	0.103	0.184	0.186	0.085
Municipality FE	Yes	Yes	No	No	Yes
Year FE	Yes	No	No	No	Yes
sector-Year FE	No	Yes	No	Yes	No
Municipality-Year FE	No	No	Yes	Yes	No
sector FE	No	No	Yes	No	No

Notes: Sector-by-year effects testing whether the 2015–16 contraction rather than the ban accounts for the decline. Sectors are two-digit CNAE. Firm–municipality–year panel, 381 municipalities across *Minas Gerais*, *Paraná*, *Pernambuco* and *Paraíba*, 2014–2023, with donor status fixed by the 2012 mayoral election.

Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A24: Alternative outcomes

	Tenders won n_wins (1)	Tenders entered n_bids (2)	Log value awarded log_total_value (3)	Market share, asinh asinh_share (4)
Donor	0.874*** (0.157)	0.835*** (0.207)	1.03*** (0.092)	0.566*** (0.055)
Donor $\times$ Post	-0.145 (0.127)	-0.084 (0.136)	-0.050 (0.088)	-0.115** (0.039)
Observations	868,528	868,528	644,226	866,326
R ²	0.030	0.057	0.132	0.169
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: The headline specification with alternative dependent variables: contracts won, logged awarded value, and tenders entered. Firm–municipality–year panel, 381 municipalities across *Minas Gerais*, *Paraná*, *Pernambuco* and *Paraíba*, 2014–2023, with donor status fixed by the 2012 mayoral election. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A25: Margins: bidding, tenders entered, and share

	Bid at all (1)	Tenders entered, all firm-years (2)	Tenders entered, if bidding (3)	Market share, all firm-years (4)	Market share, if bidding (5)	Won this tender (6)
Donor	0.212*** (0.012)	0.876*** (0.164)	0.901*** (0.197)	1.42*** (0.174)	1.70*** (0.225)	0.087** (0.033)
Donor $\times$ Post	-0.034* (0.016)	-0.377** (0.133)	-0.471** (0.156)	-0.988*** (0.144)	-0.816*** (0.184)	-0.018 (0.015)
Observations	1,457,358	1,457,358	366,833	1,457,358	366,833	1,504,671
R ²	0.135	0.035	0.065	0.036	0.116	0.186
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Margins of adjustment. Whether a firm bids at all, how many tenders it enters, and what share of the municipality’s procurement it wins. The first two are extensive margins and the third is the paper’s outcome. Firm–municipality–year panel, 381 municipalities across *Minas Gerais*, *Paraná*, *Pernambuco* and *Paraíba*, 2014–2023, with donor status fixed by the 2012 mayoral election. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

5 Mechanism Tests

Table 5 of the paper reports one row from each of the tables below. This section gives the full results, in the order the paper takes them: which unit purchased, which procedure it used, which firms bid, and which connected firms lost their advantage.

Table A26: Where the withdrawal occurred

	Estimate	SE	N
<i>Panel A. Purchasing unit</i>			
City hall	-1.217***	(0.341)	318,004
Other executive units	-0.524	(0.457)	560,491
Difference	<i>p</i> = 0.2340		
<i>Panel B. Procurement modality</i>			
Discretionary	-1.072	(1.426)	159,405
Competitive	-0.661**	(0.200)	730,857
Invitation only	-3.604	(3.435)	20,832
In-person auction	-1.346**	(0.462)	191,637
Discretionary vs competitive	<i>p</i> = 0.8185		
<i>Panel C. Did purchasing shift procedure?</i>			
Share discretionary, Post	0.0128	(0.0173)	—
Donor present × Post	-0.0001	(0.0339)	—

Notes: Panels report Donor $\times$ Post estimated separately by segment, with a formal test of the difference between segments. Market share is recomputed within each subset, so a firm's city hall share is its share of what city hall bought rather than of all municipal procurement. Firm–municipality–year panel, 381 municipalities across *Minas Gerais*, *Paraná*, *Pernambuco* and *Paraíba*, 2014–2023, with donor status fixed by the 2012 mayoral election. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A27: Procurement unit: city hall versus other executive

	mayor_share	Won this tender	
	Share bought by city hall	Won tender, city hall	Won tender, other units
	(1)	(2)	(3)
Post	-12.2*** (0.824)		
Donor		0.119*** (0.017)	0.067 (0.050)
Donor $\times$ Post		-0.012 (0.020)	-0.022 (0.020)
Observations	3,808	472,252	1,032,417
R ²	0.703	0.250	0.165
Municipality FE	Yes	Yes	Yes
Year FE	No	Yes	Yes

Notes: Purchases made by the *Prefeitura* itself against those made by subordinate executive units, with market share recomputed within each. The classification comes from the dictionary described above. The two estimates are not distinguishable from each other, so this table locates the effect without establishing a contrast between units. Standard errors clustered by municipality in parentheses. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table A28: Withdrawal by procedure

	Donor among bidders	
	Invitation-only	Other modalities
	(1)	(2)
Post	-0.014** (0.005)	-0.008*** (0.002)
Observations	9,996	539,660
R ²	0.114	0.029
Municipality FE	Yes	Yes
Pre-ban base	0.0260	0.0168
% decline	-54.2	-48.5

Notes: Donor $\times$ Post estimated separately by procurement procedure, with market share recomputed within each. Discretionary procedures are those in which competitive bidding may be waived (*dispensa*, *inexigibilidade*). Standard errors clustered by municipality in parentheses. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table A29: Win rate by procurement modality

	Won this tender								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Donor	0.057 (0.065)	0.188*** (0.052)	0.091*** (0.017)	-0.005 (0.027)	-0.009 (0.018)	0.099 (0.056)	0.219*** (0.044)	0.228*** (0.063)	0.087** (0.033)
Donor $\times$ Post	0.060 (0.042)	-0.052 (0.034)	-0.047** (0.016)	0.020 (0.030)	-0.005 (0.031)	-0.025 (0.112)	-0.293*** (0.055)	-0.056 (0.105)	-0.018 (0.015)
Observations	554,097	302,363	278,393	139,248	104,551	65,325	27,964	26,804	1,504,671
R ²	0.138	0.302	0.246	0.343	0.062	0.152	0.210	0.120	0.186
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Column order	(1) Reverse auction (pregão); (2) Electronic reverse auction; (3) In-person reverse auction; (4) Waived bidding (dispensa); (5) Sole source (inexigibilidade); (6) Mid-value tender (tomada de preços); (7) Open tender (concorrência); (8) Invitation only (convite); (9) Pooled								

Notes: Win rate by procurement modality, on bidder–tender records. Bidder–tender records for the 381 municipalities of the analysis panel, 2014–2023, with donor status fixed by the 2012 mayoral election.

Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A30: Competition in the tenders connected firms enter

	(1)	(2)	(3)	(4)
Donor	0.108** (0.042)	0.156*** (0.039)	-0.033 (0.049)	0.036 (0.048)
Donor $\times$ Post	-0.020 (0.033)	-0.004 (0.033)	0.022 (0.037)	-0.017 (0.037)
Observations	1,027,980	1,027,980	1,027,980	1,027,980
R ²	0.067	0.072	0.120	0.123
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Bidder–tender observations with a recorded bidder count. The first columns are indicators for the tender drawing one bidder and fewer than three; the last are indicators for the tender running under a procedure in which competitive bidding may be waived and under one in which it may not. *Donor* is the pre-ban level and *Donor $\times$ Post* the change after 2016. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A31: Tests of the favoritism mechanism

	Estimate	SE	N
<i>Panel A. Win rate, holding the contest fixed</i>			
Municipality and year FE	-0.0182	(0.0153)	1,504,671
Tender FE, contested tenders	-0.0412	(0.0273)	1,172,286
Tender FE, three or more bidders	-0.0596*	(0.0298)	1,045,170
<i>Panel B. What they chose to bid on</i>			
Log value of tenders entered	0.2021	(0.1861)	1,462,979
Log value of tenders won	0.0768	(0.1845)	1,013,947
<i>Panel C. Is the benchmark manipulated?</i>			
Log reference quote on Donor	0.0461	(0.0467)	507,504
<i>Panel D. Withdrawal by pre-ban win rate</i>			
Ever lost	-0.3291	(0.1697)	864,096
Won every bid	-1.3831***	(0.3246)	864,757
log_capital Lowest	-1.1478***	(0.3440)	864,462
log_capital Middle	-0.6679	(0.3797)	864,384
log_capital Highest	-0.2531	(0.1633)	864,267
firm_age Lowest	-0.8127*	(0.3469)	864,487
firm_age Middle	-0.7521**	(0.2863)	864,513
firm_age Highest	-0.4788	(0.2679)	864,350
firm_size Lowest	-0.7176*	(0.2896)	864,439
firm_size Middle	-0.9317**	(0.3537)	864,537
firm_size Highest	-0.4980	(0.2632)	864,382

Notes: Tests of the arrangement account. Panel A reports the post-ban change in the connected win rate holding the contest fixed; Panel B the logged value of tenders entered and won; Panel C the logged published reference quote regressed on donor status, which asks whether the benchmark itself was written high; Panel

D Donor $\times$ Post separately for connected firms grouped by their pre-ban bidding record and by firm characteristics fixed before the ban. Percentage points are not comparable across the Panel D groups, which held very different pre-ban positions, so each decline is also reported against its own group's pre-ban mean.

Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A32: Award margin versus access margin

	Prediction: award	Prediction: access	Estimate	p
Tender participation	Concentrated	Proportional	-0.0084***	0.0000
Win rate bidding	Falls	Unchanged	-0.0182	0.2345

Notes: The award margin against the access margin: whether the connection changed which firms bid, or only which of them won. Bidder–tender records for the 381 municipalities of the analysis panel, 2014–2023, with donor status fixed by the 2012 mayoral election. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A33: Withdrawal by tender type

	Donor among bidders						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post	-0.008*** (0.002)	-0.011*** (0.002)	-0.006*** (0.002)	-0.004* (0.002)	-0.011*** (0.002)	-0.007*** (0.001)	-0.012*** (0.004)
Observations	549,676	266,786	282,890	200,069	348,714	395,943	153,733
R ²	0.029	0.044	0.023	0.025	0.039	0.022	0.070
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-ban base	0.0171	0.0229	0.0116	0.0079	0.0224	0.0137	0.0266
% decline	-49.3	-46.9	-54.6	-46.8	-47.9	-53.7	-45.8

Notes: Donor $\times$ Post estimated separately by tender type, with market share recomputed within each subset. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

6 Market Structure

The moderator of Section 7, cut further, together with market-level outcomes that ask whether whole procurement markets changed rather than whether connected firms did.

Table A34: Effect by HHI tercile

	Market share (pp)		
	Lowest third	Middle third	Highest third
	(1)	(2)	(3)
Donor	0.578*** (0.102)	0.960*** (0.232)	4.64*** (0.626)
Donor $\times$ Post	-0.173 (0.089)	-0.040 (0.183)	-2.18*** (0.562)
Observations	470,634	240,211	155,481
R ²	0.071	0.084	0.067
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Equal across groups (p)		0.0015	

Notes: The effect by tercile of the pre-ban Herfindahl index of awarded value, computed over non-donor firms only so that a municipality cannot count as concentrated merely because its own donor firm is large there. Reported alongside the median split used in the paper, since the conditionality should not depend on where the line falls. Standard errors clustered by municipality in parentheses. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table A35: Market-level competition

	Estimate	SE	p
mean bidders	-0.0609	0.0458	0.1843
sh single	0.0077	0.0069	0.2685
Donor share of bidder pool		0.0064	

Notes: Market-level outcomes: the average number of bidders per tender and the share of tenders drawing a single firm. Reported because the tests were run and returned nothing. Donor firms are under one percent of the bidder pool, so their withdrawal cannot move a municipality-wide mean by an amount this design can resolve, and the null is therefore uninformative about the mechanism rather than evidence against it.

Standard errors clustered by municipality in parentheses. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

7 Item-Level Prices

Everything behind the price premium: the item sample, whether the benchmark itself is manipulated, and robustness to trimming, lot size, and sector.

Table A36: Item-level sample, Minas Gerais

Year	Items	Won by connected firms	Median discount	Above the quote
2014	479,693	9,946	0.098	0.149
2015	429,603	9,701	0.118	0.122
2016	376,584	6,775	0.112	0.115
2017	444,058	7,014	0.168	0.091
2018	410,001	6,108	0.186	0.059
2019	451,958	6,120	0.184	0.067
2020	360,824	4,023	0.143	0.084
2021	408,154	3,726	0.167	0.061
2022	413,563	3,480	0.168	0.050
2023	414,816	3,937	0.209	0.042
2024	311,673	2,031	0.244	0.029

Notes: Coverage of the item panel by year. Items purchased by the 177 *Minas Gerais* municipalities of the analysis panel, 2014–2024, carrying both an award price and a published reference quote. The clean-sample filter removes items whose price-to-quote ratio falls outside the range 0.01 to 100, which are data errors rather than extreme prices.

Table A37: Quote integrity

	Paid the quote exactly Paid exactly the quote (1)	Paid above the quote Paid above the quote (2)
Donor	0.010 (0.013)	0.008 (0.009)
Observations	4,500,927	4,500,927
R ²	0.071	0.070
Municipality FE	Yes	Yes
Year FE	Yes	Yes

Notes: Whether the published reference quote behaves as a benchmark should. Items purchased by the 177 *Minas Gerais* municipalities of the analysis panel, 2014–2024, carrying both an award price and a published reference quote. Standard errors clustered by municipality in parentheses. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table A38: Is the reference quote written higher?

	lq Log reference quote (1)	lp Log price paid (2)
Donor	0.046 (0.047)	0.110* (0.045)
Observations	507,504	507,504
R ²	0.945	0.946
Product-Municipality-Year FE	Yes	Yes

Notes: The logged published reference quote regressed on donor status. A municipality wishing to overpay a favored supplier could write the benchmark high and award at a price that looks reasonable against it; a null here means the premium is overpayment against an unmanipulated quote. Items purchased by the 177 *Minas Gerais* municipalities of the analysis panel, 2014–2024, carrying both an award price and a published reference quote. Standard errors clustered by municipality in parentheses. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table A39: Lot size

	Estimate	SE	p	Implied
Log lot size	0.7123***	0.1961	0.0004	104% larger

Notes: The premium by lot size, testing whether connected firms are paid more because they sell in smaller quantities. Items purchased by the 177 *Minas Gerais* municipalities of the analysis panel, 2014–2024, carrying both an award price and a published reference quote. Standard errors clustered by municipality in parentheses. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table A40: Item-level withdrawal

	Items won by connected firms
Average per year, before the ban	8,807
Average per year, after the ban	4,555
Fall	-48.3%
Fall in the bidding data	-49.3%

Notes: Item-level withdrawal after the ban, the item-panel analogue of the market-share result. Items purchased by the 177 *Minas Gerais* municipalities of the analysis panel, 2014–2024, carrying both an award price and a published reference quote. Standard errors clustered by municipality in parentheses. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table A41: Price premium by year

Year	Estimate	SE
2014	0.0217	0.0403
2015	-0.0176	0.0377
2016	0.0000	0.0000
2017	-0.0236	0.0394
2018	0.0239	0.0438
2019	0.0568	0.0393
2020	-0.0350	0.0513
2021	-0.0189	0.0379
2022	-0.0305	0.0681
2023	0.0001	0.0613
2024	-0.0121	0.0793

Notes: The price premium estimated year by year against 2016. Items purchased by the 177 *Minas Gerais* municipalities of the analysis panel, 2014–2024, carrying both an award price and a published reference quote. The outcome is the winsorized log of price over the published reference quote, so a positive value means the municipality paid more than it had said it expected to pay. Standard errors clustered by municipality in parentheses. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table A42: Price premium, robustness

	Log price over quote		Log price over quote, untrimmed	Discount off the quote	Log price over quote	
	(1)	(2)	(3)	(4)	(5)	(6)
Donor	0.104*** (0.018)	0.064* (0.030)	0.124*** (0.018)	-0.056 (0.036)	0.070 (0.061)	0.137*** (0.019)
Log registered capital	-0.009*** (0.002)					
Firm size class	-0.004 (0.002)					
Firm age (years)	0.002*** (0.0003)					
Observations	4,032,204	4,482,742	4,500,927	4,500,927	55,444	3,510,332
R ²	0.092	0.231	0.051	0.026	0.230	0.058
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm's industry FE	Yes	No	No	No	No	No
Legal form FE	Yes	No	No	No	No	No
Firm FE	No	Yes	No	No	No	No

Notes: The price premium under alternative trimming, product-cell thresholds and fixed-effect structures. Items purchased by the 177 *Minas Gerais* municipalities of the analysis panel, 2014–2024, carrying both an award price and a published reference quote. Standard errors clustered by municipality in parentheses.

$*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table A43: Exposure and sector splits

	Log price over quote		
	High-exposure towns	Construction	Premium after ban
	(1)	(2)	(3)
Donor share of bidders $\times$ Post	0.003 (0.002)		
Construction sector		0.254*** (0.020)	
Construction sector $\times$ Post		-0.002 (0.031)	
Donor			0.124*** (0.019)
Donor $\times$ Post			-0.002 (0.026)
Observations	1,944	3,565,769	4,500,927
R ²	0.439	0.119	0.056
Municipality FE	Yes	No	Yes
Year FE	Yes	No	Yes
Municipality $\times$ year FE	No	Yes	No

Notes: The premium split by pre-ban donor exposure and by sector, asking whether municipalities most exposed to connected firms saw a different price path. Items purchased by the 177 *Minas Gerais* municipalities of the analysis panel, 2014–2024, carrying both an award price and a published reference quote. Standard errors clustered by municipality in parentheses. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table A44: The cost of the premium

	Minas Gerais	All four states
Awarded procurement value	R\$ 52.02 billion	R\$ 202.28 billion
Won by connected firms	R\$ 820.8 million	R\$ 1.93 billion
Connected share of value	1.58%	0.95%
Price premium		6.7%
Excess paid	R\$ 51.6 million	R\$ 121.4 million
As a share of procurement	0.099%	0.060%
Per town-year, where present		R\$ 68,610
Untrimmed, for comparison	R\$ 52.7 million (0.059%)	R\$ 194.4 million (0.027%)

Notes: The excess paid to connected firms, obtained by applying the estimated premium to the value they won. This is an accounting of what connected firms were paid above comparable firms selling the same good in the same municipality and year, not an estimate of what the reform recovered. Items purchased by the 177 *Minas Gerais* municipalities of the analysis panel, 2014–2024, carrying both an award price and a published reference quote.

8 Alternative Explanations

The three rival accounts of the main result, each requiring firm-level ownership data or electoral records to evaluate.

Channel substitution

The ban prohibited corporate donations, but individuals remained free to give, and individual donations rose by nearly 50% in the first post-ban cycle, led disproportionately by senior executives of firms (Reis and Eduardo, 2019). If the owners of connected firms began donating in their own names, the ban closed nothing and the headline estimate captures a change in bookkeeping rather than a severed relationship.

I use Federal Revenue Service ownership records, matched to Electoral Court individual donation records through personal tax identifiers, to identify which connected firms' shareholders entered the individual donor pool after the ban. Among the 710 connected firms, 25 had a shareholder who donated to the 2016 winning mayor and 21 to the 2020 winning mayor, roughly 3% of the group. Counted as firm-municipality pairs, which is the unit the panel is built on, the 2016 figure is 26 of 734, since one firm substituted in two municipalities. Table A45 re-estimates the main specification with the donor side of the panel restricted to firms meeting each column's shareholder condition; non-donor firms remain the within-municipality control group throughout, so the restriction changes which donor firms enter the comparison without disturbing identification.

In the 2016 cycle the entire market share loss falls on connected firms whose shareholders did *not* donate to the incoming mayor (-0.76), while firms whose shareholders did donate lost nothing. Firms whose shareholders backed a losing candidate received no protection either, which indicates that what protects a firm is its relationship with the specific winning mayor rather than political activity in general. The 2020 cycle repeats the pattern (-0.74 among firms without substitution). These estimates rest on few firms and should be read with caution, though the caution runs in the same direction: the rarer substitution is, the less of the result it can explain.

Table A45: Market share by shareholder substitution status

	(1)	(2)	(3)	(4)	(5)
Donor	1.58*	1.81***	0.450	1.28*	1.81***
	(0.656)	(0.244)	(0.298)	(0.640)	(0.239)
Donor $\times$ Post	0.681	-0.764***	-0.069	0.683	-0.736***
	(0.633)	(0.192)	(0.212)	(0.950)	(0.188)
Observations	863,632	866,210	863,570	863,586	866,256
R ²	0.083	0.087	0.082	0.083	0.087
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Donor firms in column	26	686	18	21	689

Notes: The main specification with the donor side of the panel restricted to firms meeting each column's shareholder condition. Non-donor firms remain the within-municipality control group throughout, so the restriction changes which donor firms enter the comparison without disturbing identification. The substituting arm rests on 25 firms and should be read as imprecise. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

If contributions had moved from firms to their owners, some firm should have gained what the connected firms lost. Firms that never donated corporately but whose owners began donating personally to the incoming mayor are the natural candidates. Adding a second indicator for such firms to the main specification, interacted with the post-ban period, returns +0.01 (SE 0.25) for the 2016 cycle and +0.31 (SE 0.24) for 2020. They gained nothing, which indicates that the political channel narrowed rather than shifted. Of the 710 firms that lost the corporate channel, a few dozen had personal networks close enough to the incoming mayor to preserve their position, and the firms whose owners attempted to build such relationships from scratch gained nothing.

One case illustrates what substitution looks like when it occurs. Firm 75274423, in a small *Paraná* municipality, donated to the winning mayor in 2012. In 2016 two of its shareholders, members of the Gil family, donated personally to the winning candidate. In 2020 four Gil-family shareholders donated roughly 156,000 BRL to the winning mayor, who was himself a member of the family. Over a single decade the personal network rebuilt the relationship through three increasingly direct means: corporate donation, shareholder donation, and finally holding office itself. I do not read this as the average pattern. It marks the boundary of the reform, and such relationships were rare.

Firm failure

Connected firms might have lost share because they went out of business rather than because they lost a favor. Table A46 reports three exit margins. Connected firms were no more likely than comparable firms to stop bidding anywhere in the sample, and markedly less likely to

stop bidding in the municipality where they had been connected, which is the opposite of what firm failure would produce.

Table A46: Exit

	Stopped bidding in this town		Stopped bidding anywhere
	Left town (raw) (1)	Left town (footprint fixed) (2)	Left procurement (3)
Donor	-0.250*** (0.020)	-0.095*** (0.018)	-0.029 (0.019)
Observations	145,723	145,723	145,723
R ²	0.024	0.142	0.017
Municipality FE	Yes	Yes	Yes
Years active before the ban FE	No	Yes	No

Notes: Three exit margins: whether a firm stops bidding anywhere in the sample, whether it stops bidding in the municipality where it was connected, and whether its tax registration is closed. Firm–municipality–year panel, 381 municipalities across *Minas Gerais*, *Paraná*, *Pernambuco* and *Paraíba*, 2014–2023, with donor status fixed by the 2012 mayoral election. Standard errors clustered by municipality in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Mayoral turnover

If what mattered were a relationship with a particular officeholder rather than the donation itself, the effect should be larger where the 2012 mayor left office. Table A47 gives no sign of it. The estimate is -0.61 where the 2012 mayor was re-elected and -0.72 where a new mayor took over, or 25% and 28% of each group’s own pre-ban position. The difference, re-elected minus replaced, is $+0.11$ (SE 0.43, $p = .80$). That interval is wide enough to contain a gap the size of the headline effect, so I report the two estimates as similar rather than as established equals. What the comparison does rule out is the pattern the alternative predicts, which is a substantially larger loss where the mayor left. A firm that had bought favor kept losing it whether or not the mayor who sold it stayed in office.

Table A47: Market share by mayoral turnover

	Market share (pp)	
	2012 mayor re-elected	2012 mayor left
	(1)	(2)
Donor	1.75*** (0.470)	1.81*** (0.272)
Donor $\times$ Post	-0.613 (0.370)	-0.723*** (0.215)
Observations	245,865	620,461
R ²	0.106	0.081
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Difference	-0.1104 (SE 0.4283, $p = 0.797$)	

Notes: The headline specification estimated separately in municipalities where the 2012 mayor was re-elected in 2016 and where a new mayor took office. The two groups began from different pre-ban positions, so each estimate is also reported against its own group's pre-ban mean. The difference between them is +0.11 (SE 0.43), which is too imprecise to establish that the two are equal. Standard errors clustered by municipality in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

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