Politics and Markets:

Campaign Finance Reform and Favoritism in Public Procurement

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Abstract

Under which conditions do campaign finance reforms level the playing field in public procurement? I argue that how disruptive a corporate donation ban is depends on two conditions: market concentration, which sets the ceiling on how much change is possible, and political continuity, which determines how completely the donor-mayor relationship is severed when the ban takes effect. I test this using Brazil's 2015 corporate donation ban, linking five administrative data sources at the firm level to construct a panel of over one million bidder-tender observations across 329 municipalities from 2013 to 2020. Donor firms lose 0.85 percentage points of market share after the ban — a 34% decline relative to their pre-ban mean — while win rates remain unchanged, establishing that the ban reshaped procurement market structure through the extensive margin rather than through individual contract steering. Effects are largest in non-competitive markets under new mayors (−2.3pp) and near zero in competitive markets under reelected incumbents, where the personal relationship between donor and mayor survived the formal closure of the donation channel. Losses concentrate in city hall procurement, in competitive tenders, and under full political turnover; same-party transitions produce no effect, indicating that what sustained donor advantages was the specific mayor, not the party. The findings suggest that the relevant question for evaluating campaign finance reform is not whether favoritism falls on average, but whether the reform severs the specific political relationships that sustained it — a condition that depends on the timing of reform relative to electoral turnover and the market structure in which it lands.

Keywords: campaign finance reform, public procurement, political connections, market access, corporate donation ban, Brazil.

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1 Introduction

Public procurement accounts for roughly 10% to 25% of GDP in most countries (World Bank, 2020), making contract-award decisions one of the main channels through which political influence shapes the provision of local public goods and services. A large comparative politics literature has documented that decentralization often fails to deliver on its promise of more responsive local governance, in part because local politicians capture the discretion it creates (Bardhan 2002; Bardhan and Mookherjee 2006; Faguet 2012). Procurement is a central mechanism through which this capture operates: firms that donate to winning candidates receive more and larger contracts than firms that do not (Boas et al. 2014; Titl and Geys 2019; Dahlström et al. 2021), and the resulting distortions in how public money is spent have direct consequences for the quality of infrastructure, services, and goods that citizens receive (Olken 2007; Ferraz and Finan 2008). In response, many countries have adopted corporate donation bans — yet the same reform often leaves procurement markets largely unchanged in some places while reshaping them substantially in others (Baltrunaite 2020; Guerra et al. 2025). Under which conditions do campaign finance reforms level the playing field in public procurement?

I argue that donor firms' procurement advantages are politically conditioned, and that corporate donation bans reshape procurement market structure — but not equally across all municipalities. The ban disrupts the political environment that sustained donor firms' disproportionate market positions. How much it disrupts depends on two conditions: pre-existing market concentration, which sets the ceiling on how much change is possible, and political continuity, which determines how completely the donor-mayor relationship is severed when the ban takes effect.

In concentrated markets where a new mayor took office, both conditions reinforce one another: a ban removes the financial channel at the exact moment the personal relationship ends, and the market restructures sharply. In competitive markets under reelected incumbents, neither condition bites: donor positions were already modest, and the political

environment changes very little despite the formal closure of the donation channel. A ban can be the same everywhere, but the disruption it produces is not.

I test this argument using Brazil's 2015 corporate donation ban, which took effect with the 2016 municipal elections and applied simultaneously to all 5,570 municipalities. Because the ban coincided with an election, the same institutional moment produces both the policy shock and the variation in political continuity the argument requires: some mayors were reelected and the donor relationship survived; others were replaced and it ended. I link five administrative sources at the firm level via the Brazilian corporate tax identifier: (1) donation records, (2) municipal procurements, (3) firm registry, (4) electoral data, and (5) municipal characteristics. This produces a firm-municipality-year panel of over one million bidder-tender observations across 329 municipalities in four states from 2013 to 2020, restricted to municipalities where at least one donor firm participates in procurement — the sample where identification of the treatment effect requires within-municipality variation in donor status. The analysis sample contains 625 unique donor firms and over 133,000 non-donor firms. I estimate a difference-in-differences model comparing donor and non-donor firms before and after the ban, and test the heterogeneity predictions on subsamples defined by pre-ban market concentration and mayoral continuity.

Donor firms lose roughly 0.85 percentage points of market share after the ban on average — a 34% decline relative to their pre-ban mean of 2.5pp — with declines in contracts won and tenders entered but no change in win rates. In non-competitive markets the loss reaches 2.3 percentage points; in competitive markets it is indistinguishable from zero. Under new mayors the average loss is 0.9 percentage points; under reelected incumbents the coefficient is negative but insignificant. The two conditions interact: non-competitive markets under new mayors show losses of 2.1 percentage points, while competitive markets under incumbents show near-zero effects. Losses concentrate in city hall procurement and in competitive tenders, and are driven entirely by municipalities with full political turnover — same-party transitions produce no significant effect — indicating that what mattered was the specific

mayor, not the party.

This paper makes three contributions. First, it contributes to the comparative politics literature on corruption and local governance by showing that the effects of anti-corruption reform depend on the political and market conditions in which reform lands. A substantial literature has examined how institutional design shapes corruption at the local level (Ferraz and Finan 2008, 2011; Olken 2007; Hidalgo et al. 2016), but most studies treat reform as a uniform shock and estimate average effects. I show that a corporate donation ban reshaped procurement market structure rather than merely reducing individual contract favoritism, and that the magnitude of restructuring depended on two conditions — market concentration and political turnover — that prior work has not examined jointly. This finding speaks directly to the decentralization literature's concern that local discretion enables capture (Bardhan 2002; Faguet 2012): the ban was most effective precisely where mayoral discretion had done the most to structure markets around political relationships. Second, the paper contributes to the growing literature on the unanticipated consequences of incumbency and political continuity (Klašnja and Titiunik 2017; Lee 2008). I find that the ban's effect is near zero under reelected incumbents and concentrated entirely in municipalities with full political turnover, and that same-party transitions produce no effect — only the severance of the specific personal relationship between donor and mayor mattered. This result connects to work on how personal ties to officeholders, rather than partisan networks, sustain corruption and favoritism in procurement (Mironov and Zhuravskaya 2016; Fisman 2001), and it establishes that the ban's effect operates entirely on the extensive margin — participation, not capability — which reframes how we should evaluate campaign finance reform. Third, the paper constructs a novel substitution measure by matching firm shareholders to individual 2016 donation records via masked personal tax identifiers (CPF), tracing whether corporate donors re-entered politics through personal donations after the ban — a connection prior work has noted anecdotally but not traced at the firm level.

The paper proceeds as follows. Section 2 develops the theoretical argument, connecting

corporate donations to procurement market structure and deriving four hypotheses about when a ban should be most disruptive. Section 3 situates the Brazilian case, describing the institutional environment of municipal procurement and the 2015 corporate donation ban. Section 4 presents the research design, including data sources, variable construction, and the identification strategy. Section 5 reports the main results and heterogeneity analysis. Section 6 examines mechanisms — procurement unit, modality, and personal versus party continuity. Section 7 presents robustness checks, and Section 8 discusses the findings in light of the broader literature before concluding.

2 Campaign Bans and Procurement Markets

This section develops the theoretical argument in four steps. I first establish that corporate donations function as political investments in procurement access, drawing on evidence across institutional settings. I then examine why municipal mayors have both the incentive and the discretion to reward donors, and why firms respond strategically to the political environment this creates. Finally, I derive the conditions under which a corporate donation ban should disrupt this environment — market concentration and political continuity — and formalize four testable hypotheses.

2.1 Corporate Donations as Political Investments

The relationship between corporate donations and political influence is often framed in binary terms: either as a legitimate mechanism for interest representation or as a corrupting force that distorts democratic accountability. In reality, the relationship is more complex, shaped by the institutional environment, transparency norms, and enforcement capacity of each political system. While some argue that corporate donations enhance access to policymakers and allow firms to protect their interests through lobbying (Nownes 2015; Baumgartner et al. 2009), others highlight how wealthier actors gain disproportionate influence over policy

outcomes, exacerbating inequalities in political representation (Hacker and Pierson 2011). This concern is particularly pressing in contexts with limited transparency, where the true motivations and consequences of corporate donations remain obscured (Potter 2019).

The debate about whether money buys political influence has never been fully resolved, in part because the answer depends on what kind of influence one is looking for and under what institutional conditions. In comparative politics, this question connects to a broader literature on how economic elites translate wealth into policy influence and how institutional variation conditions whether they succeed (Grossman and Helpman 2001; Stokes et al. 2013). Some argue that firms invest in politicians because they expect a financial return: that donations buy access, influence, and favorable policy outcomes, and that firms expect a measurable return on that investment (Ferguson 1995; Gilens and Page 2014). Others are skeptical: the amounts involved are too small, the returns too uncertain, and the legal risks too high for donations to function reliably as a purchasing mechanism. On this view, firms give because they care about political outcomes, not because they expect a direct economic return (Ansolabehere, de Figueiredo, and Snyder 2003).

What makes this debate hard to settle is that both logics may be right, just in different contexts. Firms may donate for expressive reasons in some situations and strategic ones in others. What determines which logic prevails is the institutional environment: how much discretion officeholders have, how transparent the process is, how effectively political commitments can be enforced (Tollison and Congleton 1995; Witko 2011). Where officeholders control large and discretionary public resources, and where transparency is limited and enforcement weak, the conditions for the investment logic are most likely to be satisfied.

Public procurement is the setting where this logic of investment and its consequences are most directly observable. Unlike lobbying, where the relationship between money and policy outcomes is diffuse and difficult to trace, the relationship between donations and procurement contracts is bilateral and measurable: a firm donates to a candidate, the candidate wins office, and the firm subsequently receives contracts from the government that officeholder

controls. The evidence that this relationship substantial has accumulated across a wide range of institutional settings. Firms that donate to winning candidates receive more contracts, win larger ones, and face less competition than firms that do not (Boas et al. 2014; Titl and Geys 2019; Dahlström, Fazekas, and Lewis 2021), and the returns are large enough to rule out coincidence — in Brazil, Boas, Hidalgo, and Richardson (2014) estimate returns of at least fourteen times the donation value for firms that backed victorious federal candidates, while in Belgium, Titl and Geys (2019) find that connected firms receive contracts worth roughly 25% more than comparable non-connected firms. That this pattern holds across democracies with very different institutional arrangements suggests something general about how political relationships translate into procurement advantage, rather than something specific to any one country's particular circumstances.

2.2 Mayors' Incentives and Discretion

Understanding why donations translate into procurement advantages requires taking seriously the incentives of the politicians who receive them. Municipal governments are major purchasers of goods and services, and mayors typically control the allocation of contracts, set procurement priorities, and have broad discretion over which suppliers participate in which categories of spending — particularly through non-competitive modalities that require fewer procedural constraints. This discretion is not merely a formal power; it is an asset that mayors can deploy strategically to reward political allies and sustain the coalitions that keep them in office (Coviello and Gagliarducci 2017; Mironov and Zhuravskaya 2016).

Mayors' incentive to favor donor firms is driven, in large part, by the financial pressures of electoral competition. Running for and maintaining office is costly, and in contexts where corporate donations are legal and largely unregulated, firms become a natural source of campaign financing. The relationship that emerges is reciprocal: firms donate because they expect access, and mayors provide access because they need funds. This exchange does not require explicit quid pro quos — it operates through the informal expectations and repeated

interactions that a political relationship creates over time (Mironov and Zhuravskaya 2016; Witko 2011). Where oversight institutions are weak or themselves politically appointed, mayors face limited constraints on how selectively they distribute procurement opportunities, and the conditions for this exchange are most fully satisfied (Hidalgo, Canello, and Lima-de-Oliveira 2016). In Brazil, this constraint is particularly uneven: the federal government is monitored by the *Tribunal de Contas da União* (TCU), and some states maintain their own *Tribunais de Contas dos Estados* (TCEs), but the quality and independence of these audit courts vary substantially across states, and many municipalities face no consistent external oversight of procurement decisions (Ferraz and Finan 2008, 2011). This institutional environment means that the degree of mayoral discretion in procurement is not merely a function of formal procurement rules but also of how effectively — or ineffectively — those rules are monitored at the local level.

The strength of this incentive, however, is not constant across mayors. Electoral accountability conditions how much mayors are willing to risk in their procurement decisions. Ferraz and Finan (2011) show that mayors without reelection incentives — those serving their final term — misappropriate significantly more resources than those who face voters again, suggesting that the prospect of electoral punishment constrains behavior when it is present and removes that constraint when it is not. This has a direct implication for the relationship between mayors and donor firms: a mayor seeking reelection has an incentive to maintain the political relationship with donors carefully, balancing the benefits of access against the risks of visible favoritism. A mayor in a final term faces no such constraint, and the exchange between donations and procurement access may operate more freely. Beyond reelection, mayors also use procurement strategically to build and maintain the broader coalitions that sustain them in office — rewarding not just campaign donors but the network of political allies whose support extends across the electoral cycle (Mironov and Zhuravskaya 2016). Procurement discretion is valuable precisely because it can serve both purposes simultaneously: rewarding past donors and maintaining future political support, often through

the same allocation decisions.

2.3 Firms' Incentives and Market Entry

Whereas mayors had an incentive to favor donor firms, donor firms had an incentive to invest in this relationship, and non-donor firms had an incentive to stay out of markets where it operated. For donor firms, the political relationship with the mayor was an asset that extended beyond any individual contract — it sustained a market position that would have been difficult to maintain on capability alone, in an environment where the allocation of public resources was at least partly shaped by political considerations. The returns to this relationship were therefore not transactional but structural: what the donation purchased was not a specific contract but a political environment favorable to broad, sustained procurement participation. For non-donor firms, the same environment worked in reverse. Entering a market where a competitor holds a politically-sustained position is a different calculation than entering a genuinely open one — the expected returns are lower, the risks of investing in participation are higher, and the rational response is to compete elsewhere or not at all. The result is a procurement market structured not only by firms' productive capabilities but by the political relationships that condition who participates and how much of the market they can plausibly capture. The evidence is consistent across very different institutional settings: political connections shape who participates in procurement markets, not just who wins once they do (Coviello and Mariniello 2014); longer mayoral tenure produces more concentrated procurement markets with fewer bidders and higher contract values (Coviello and Gagliarducci 2017); and discretion in contracting generates systematic concentration around politically connected suppliers (Decarolis, Fisman, Pinotti, and Vannutelli 2025).

This distinction between the intensive and extensive margins of procurement advantage has a direct implication for how we think about the consequences of corporate donations. Win rates measure whether firms win tenders they enter — a capability question. Market share measures how much of the total market those firms capture — a participation question.

If political connections generate advantage primarily through sustained participation rather than individual contract steering, then the standard measure of donor advantage in the previous studies (win rates, contract values, award probabilities) systematically underestimates how much the political environment shapes market outcomes. A firm that dominates a procurement market not because it wins more often than its competitors, but because it participates more broadly and its competitors participate less, looks no different from a highly capable firm when evaluated on the intensive margin alone.

2.4 When a Ban Disrupts

Anti-corruption reforms targeting campaign finance are attempts to dismantle precisely the political environment described above. Corporate donation bans are not designed to make firms less capable or less competitive: they are designed to sever the financial relationship between donors and officeholders (Baltrunaite 2020; Avis, Ferraz, Finan, and Varjão 2022), and in doing so, to remove the political conditions that sustained donor firms' disproportionate market positions. But closing that formal donation channel is not the same as dismantling the environment that channel helped create. The political relationship between a donor firm and a mayor was built over time, through the donation and through whatever access and expectations it cultivated — and those do not dissolve automatically when the financial link is cut (Reis and Eduardo 2019). A ban that removes the formal channel but leaves the rest of that environment intact is a fundamentally different shock than one that removes it at the moment the personal relationship also ends. The question is therefore not whether a ban disrupts the political environment, but how completely it does so — and that depends on two conditions. I argue that corporate donation bans reshape procurement market structure rather than merely reducing individual contract favoritism, and that the magnitude of this restructuring depends on two conditions that prior work has not examined jointly.

The first condition is market concentration. In markets where donor firms held large shares and non-donors were largely absent before a ban, the political environment sustained

positions that were disproportionate to what capability alone would predict — there was more politically-sustained market share to lose and more room for restructuring. In competitive markets, where donor positions were already modest and non-donors were already active, the political premium was smaller to begin with, and a ban has less to disrupt. Market concentration therefore sets the ceiling on how much a ban can change.

The second condition is political continuity. The political environment that sustained donor firms' market positions was not impersonal — it was built around the relationship between the firm and the specific mayor, accumulated through the donation and whatever access and expectations it created. When that mayor is reelected, the formal financial channel closes but the rest of the environment survives: the same person is in office, with the same history and the same informal expectations intact. Other market participants face genuine uncertainty about whether the environment has truly changed, and the disruption is incomplete. When a new mayor takes office, both the financial channel and the personal relationship are severed at the same moment. The new mayor has no history with the old donors and no reason to maintain whatever arrangements had sustained their market positions. The signal to the market is unambiguous, and the disruption is complete.

In concentrated markets where a new mayor takes office, both conditions reinforce one another: a ban removes the financial channel at the exact moment the personal relationship ends, the space previously occupied by politically-sustained donor positions becomes genuinely contestable, and the market restructures sharply. In competitive markets under reelected incumbents, neither condition bites: donor positions were already modest, and the political environment changes very little despite the formal closure of the donation channel. The two intermediate cases reveal which condition does more of the work. In concentrated markets under reelected incumbents, the potential for disruption is large but the survival of the personal relationship limits how much of it materializes. In competitive markets under new mayors, the political break is clean but the ceiling on restructuring is low.

2.5 Hypotheses

H1: After the ban, donor firms lose market share in procurement markets.

If donor firms' market positions were politically conditioned rather than purely capability-driven, severing the donation channel should reduce their procurement market share. The ban disrupts the political relationships that sustained those positions, and firms that relied on them should lose ground. The key empirical implication is that the effect should appear on market share — a participation measure — rather than on win rates, which capture capability conditional on entry. If donations purchased a favorable environment for broad participation rather than an advantage on individual tenders, then a ban that removes the political channel should reduce how much of the market donor firms capture without changing how often they win when they do compete. A null win rate alongside a negative market share effect would rule out the alternative explanation that donor firms simply became less productive or less capable after the ban, and would establish that what the political relationship sustained was access, not competitive advantage in the narrow sense.

H2: The market share loss is larger in non-competitive markets, where political connections — rather than capability — were the primary driver of donor positions.

The magnitude of this loss depends on the pre-existing market structure. Market concentration sets the ceiling on how much a ban can change by determining how much of the donor firm's position was politically sustained in the first place. In non-competitive markets — where few firms compete and procurement spending is concentrated among a small number of suppliers — political connections were likely the primary driver of market position. When competition is thin, firms do not need to be the most capable, only the most connected, and the resulting market structure reflects political relationships more than productive efficiency (Coviello and Gagliarducci 2017; Decarolis et al. 2025). In competitive markets, donors had to compete on capability as well, so political connections explain less of their position and

the ban has less to disrupt. This prediction connects to the broader finding in the public economics literature that the distortionary effects of political connections are largest where discretion is high and competition is low (Bandiera, Prat, and Valletti 2009).

H3: The market share loss is larger under new mayors than under reelected incumbents.

The effect also depends on political continuity. The political environment that sustained donor firms' market positions was built around a specific personal relationship between the firm and the mayor, not around partisan or institutional ties. Under a new mayor, both the financial channel and the personal relationship are severed simultaneously: the new officeholder has no history with the old donors, no accumulated obligations, and no reason to maintain whatever informal access the donation had purchased. The signal to other market participants is unambiguous — the old political environment is gone — and the market should restructure accordingly. Under a reelected incumbent, the ban removes the formal donation channel but the personal relationship survives intact. The same mayor remains in office with the same history and the same informal expectations, and other firms face genuine uncertainty about whether the political environment has truly changed. This prediction is consistent with work showing that incumbency effects in weakly institutionalized settings operate through personal networks rather than party organizations (Klašnja and Titiunik 2017), and that the identity of the specific officeholder, rather than the party, drives procurement outcomes (Coviello and Gagliarducci 2017; Mironov and Zhuravskaya 2016).

H4: The market share loss is largest where non-competitive markets coincide with political turnover, and smallest where competitive markets coincide with incumbent continuity.

Market structure and political turnover are not independent in their effects but interact to determine the magnitude of disruption. Market structure sets the ceiling on how much change is possible: non-competitive markets have more politically-sustained share to lose.

Political turnover determines how completely the ban severs the relationships that sustained those positions: new mayors end the personal relationship at the same moment the financial channel closes. When both conditions hold simultaneously — a concentrated market under a new mayor — there is both more to lose and less to cushion the loss. The donor firm's market position was disproportionate to begin with, and neither the formal channel nor the personal relationship survives to sustain it. When neither condition holds — a competitive market under a reelected incumbent — the donor's position was already modest and the political environment barely changes despite the formal closure of the donation channel. The two intermediate cases are theoretically informative because they reveal which condition does more of the work. In non-competitive markets under reelected incumbents, the potential for disruption is large but the survival of the personal relationship limits how much of it materializes. In competitive markets under new mayors, the political break is clean but the ceiling on restructuring is low because the political premium was small to begin with. If H4 holds, it would suggest that anti-corruption reforms are not uniformly effective instruments but are instead conditional on the political and market environment in which they land — a finding with implications for how reformers think about the sequencing and targeting of institutional change.

Table 1: Predicted Market Share Loss After the Ban

	Reelected incumbent	**New mayor**
Competitive	Smallest loss	Moderate loss
Non-competitive	Large but limited	**Largest loss**

3 Local Government and Public Procurement in Brazil

In September 2015, Brazil's Supreme Court declared corporate campaign donations unconstitutional, and Electoral Reform Law (Lei 13.165/2015) formalized the campaign ban.

Starting with the 2016 municipal elections, all corporate donations to candidates and parties were banned. Campaign resources were limited to personal funds, individual donations with income-based caps, and inter-party transfers. The ban applied to all 5,570 Brazilian municipalities simultaneously, creating a common shock that affected every procurement market in the country at the same moment. Because it coincided with the 2016 municipal election, the same institutional moment produces both the policy change and the variation in political continuity that the argument requires: some mayors were reelected and the political relationship with their donors survived the ban while others were replaced and the relationship ended.

3.1 Municipal Procurement in Brazil

During the period under study, procurement in Brazil was governed by Law 8.666/1993 (*Lei de Licitações*), which established the rules under which governments could allocate contracts to private suppliers. The law created a hierarchy of procurement modalities ranging from open competitive processes to more restricted channels, each with different requirements for publicity, competition, and justification. The most important open process is the electronic auction (*pregão eletrônico*), which requires broad public notice, multiple bidders, and transparent price-based evaluation. At the other end of the spectrum, mayors can use invitation-only procedures (*convites*), price surveys (*tomadas de preços*), single-source contracts, exemptions (*dispensas*), and emergency procurements — each of which requires less competition and affords more discretion to the contracting authority.

These non-competitive channels are not marginal. Between 2009 and 2020, non-competitive tenders represented almost 40% of all tenders in major Brazilian states, and in 2020 they accounted for 62% of federal procurement spending (Dahis, Ricca, and Scot 2025). Municipal governments are particularly active procurers, especially in smaller municipalities where the local government is often the largest employer and the main source of demand for local goods and services. The scale of municipal procurement, combined with the discretion that

Law 8.666 affords to contracting authorities, makes the municipal level the natural arena in which political relationships between donors and mayors translate into market outcomes.

What makes municipal procurement particularly vulnerable to political influence is not just the scale of spending or the discretion embedded in procurement law, but the weakness and inconsistency of oversight institutions at the local level. At the federal level, the *Tribunal de Contas da União* (TCU) audits government accounts and procurement, and Ferraz and Finan (2008) show that the random audit program run by the federal comptroller's office (*Controladoria-Geral da União*, CGU) has measurable effects on electoral accountability. But this federal oversight apparatus covers only a fraction of municipal procurement activity. At the state level, *Tribunais de Contas dos Estados* (TCEs) are formally responsible for auditing municipal accounts, but their capacity, independence, and effectiveness vary enormously across states (Hidalgo et al. 2016). Some TCEs conduct regular, substantive audits; others are chronically underfunded, politically captured, or both. Many municipalities, particularly smaller ones, face no consistent external scrutiny of how procurement decisions are made or who benefits from them. This institutional unevenness means that the formal rules governing procurement — the modality requirements, the publicity thresholds, the competitive procedures — operate in very different enforcement environments depending on where a municipality is located. In municipalities where oversight is thin, the mayor's discretion over procurement is effectively unchecked, and the conditions for politically-conditioned market outcomes are most fully satisfied.

The distinction between competitive and non-competitive modalities is therefore central to understanding how political relationships operate in this setting. In open competitive processes, the mayor's influence is more constrained: prices are compared transparently and the lowest bidder typically wins. In non-competitive channels, discretion is considerably greater, and the scope for political relationships to shape who participates and who wins is correspondingly wider. This distinction maps directly onto the theoretical argument: if the ban changed the political environment in which procurement operates, the adjustment

should be visible in who participates in competitive processes — the extensive margin — rather than in who wins individual contracts once they enter.

3.2 Corporate Donations and Procurement in Brazil

Before the ban, corporate money was the dominant source of campaign funds in Brazilian municipal elections, and the connection between donations and procurement access was well understood by market participants. Brazil's experience illustrates a pattern documented across developing democracies: where decentralization transfers spending authority to local governments without commensurate strengthening of accountability institutions, the result is often not more responsive governance but more localized capture (Bardhan 2002; Faguet 2012). Donation records were published by the Electoral Court (TSE) and accessible to anyone, which meant that the political signal of who had donated to which mayor was public knowledge — a signal that matters for how non-donor firms read the market. The scale of the exchange between donations and contracts had been extensively documented. Operation Vampire (*Operação Vampiro*, 1990–2004) uncovered procurement fraud worth roughly USD 900 million. Operation Car Wash (*Lava Jato*) exposed how corporate contributions to winning candidates were exchanged for inflated contracts, estimated at nearly USD 10 billion, and its aftermath reshaped the political landscape in ways that made the 2015 ban both more politically viable and more consequential for the firms that had relied on the donation channel (Ferraz et al. 2023). The scale of these scandals reflected something systematic rather than incidental: corporate donations were not peripheral to Brazilian municipal politics but central to how campaigns were financed and how procurement markets were organized.[1]

The ban did not end the strategic use of money in politics — a finding consistent with a

[1] A growing literature documents the ban's effects on Brazilian politics and procurement. Guerra, Mattos, and Nakaguma (2025) find that more exposed sector-municipalities experienced increased entry and lower concentration after the ban. Dahis, Ricca, and Scot (2025) find that preferential payment to connected firms weakened after the reform. Avis, Ferraz, Finan, and Varjão (2022) show that spending caps introduced alongside the ban reduced incumbent reelection rates. None of these studies examine whether the ban changed procurement market structure at the firm level, or whether the answer varied with pre-existing market concentration and political turnover.

broader comparative literature showing that formal institutional reforms often redirect rather than eliminate the underlying demand for political access (Stokes et al. 2013). Individual donations surged by nearly 50% in the first post-ban electoral cycle (Reis and Eduardo 2019). A 2016 FGV-DAPP report found that 58 of the 59 largest individual donors to Rio de Janeiro mayoral candidates were senior executives at firms. By 2024, a single individual — Rubens Ometto, CEO of the Cosan group — had donated over 18 million Brazilian Reais to candidates across 104 municipalities. In the small Alagoan municipality of Igreja Nova, with a population of 22,000, Ometto financed seven of the eleven elected city councilors. What the ban changed was the formal channel through which corporate interests flowed into politics — not necessarily the underlying relationship between economic power and political access.

Figure 1 plots mean municipal procurement market share for donor and non-donor firms over time. The relevant comparison is not the level but the trend: donor mean market share rises from roughly 1.7pp in 2013 to a peak of 2.9pp in 2016, then falls sharply to 1.4pp in 2017 before partially recovering. Non-donor mean share remains flat throughout, hovering between 0.5pp and 0.8pp. Consider a municipality that spends R$10 million per year on procurement — a conservative figure for a mid-sized Brazilian town. A firm holding 2.5 percentage points of that market receives R$250,000 in annual contracts. A reduction of 0.8 percentage points means R$80,000 less per year — roughly the annual salary of two public school teachers, or the cost of resurfacing a neighborhood road. For a small supplier for whom public contracts represent a significant share of total revenue, this is not a small loss. In non-competitive markets, donor firms lose 2.3 percentage points on average — R$230,000 per year in a R$10 million municipality. The percentage point is a small number; what it represents is not.[2]

[2]The figure plots mean market share across all firm-municipality-year observations in the sample — a pool that includes nearly 285,000 non-donor firms, most of which participate in only one or two tenders per year and hold near-zero shares, alongside roughly 1,800 donor firms whose shares vary considerably by municipality size and market structure. When these are averaged together, the mean decreases substantially. A donor firm holding 15 percentage points of procurement in a small concentrated municipality receives the same weight as a donor firm holding 1 percentage point in a large competitive one. The mean of 2.5

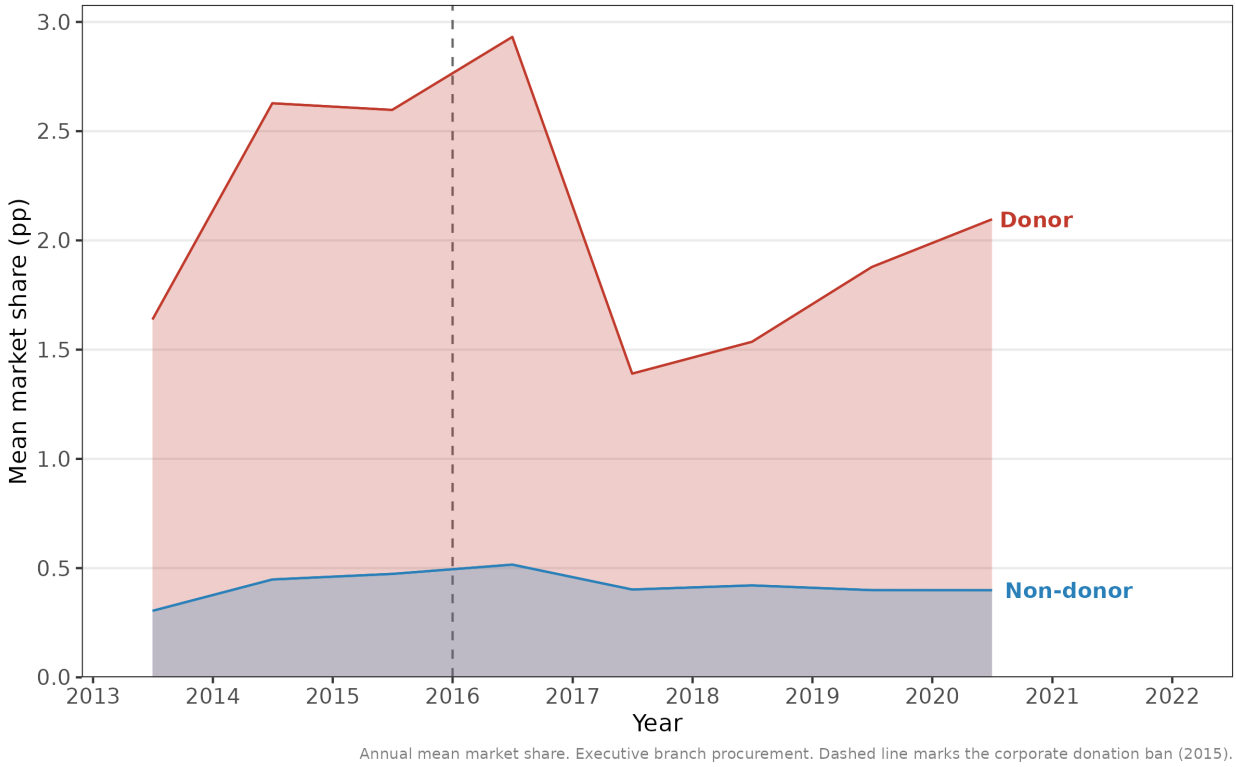


Figure 1: Mean annual market share for donor and non-donor firms, 2013–2020. Values are firm-municipality-year means across the full executive branch sample. Dashed line marks the corporate donation ban (2015).

If my argument is correct — that donor firms' market positions were politically conditioned, and that the ban's disruptive force depends on market concentration and political continuity — then the pattern in Figure 1 should not look the same everywhere. In concentrated markets where a new mayor took office, the ban removes both the financial channel and the personal relationship simultaneously, and donor market share should fall sharply. In competitive markets under reelected incumbents, the signal to the market is ambiguous, and the series should barely move.

percentage points is therefore best read as a lower bound on the economically relevant effect.

4 Research Design

This section describes the research design in five parts. I first introduce the five administrative data sources and the sample coverage. I then define the dependent variables, the treatment variable, and the two moderators that generate the heterogeneity predictions. Finally, I present the identification strategy, beginning with a discussion of the endogeneity concerns that motivate the empirical approach and proceeding through three progressive specifications. Details on data construction — including the municipality quality filter, firm matching, shareholder linkage, and branch classification procedures — are in Data Appendix 9.

4.1 Data Sources

I use five sources, linked at the firm level via CNPJ, the Brazilian corporate tax identifier analogous to a federal employer identification number. The first is the Electoral Court campaign donation registry (TSE), which I draw from twice: the 2012 corporate donation records to define treatment, and the 2016 individual donation records to test whether firm shareholders substituted personal donations after the ban. The second is the MiDES municipal procurement dataset (Dahis et al., 2023), which provides tender-level and participant-level records including procurement modality, contract values, and winner identities. The third is the Federal Revenue Service firm registry (Receita Federal), which I use for firm ownership structure, sector classification, firm-level controls, and shareholder tax identifiers. The fourth is TSE electoral data, from which I extract election results, party affiliation, and mayoral incumbency status. The fifth is the MAPE municipal characteristics dataset, used only in robustness checks as time-varying municipal controls.

Table 2: Overview of the Data

Statistic	Value
Sample coverage	
Unique municipalities	329
Minas Gerais	168
Paraná	103
Pernambuco	36
Paraíba	22
Unique bidding firms	133,585
Total unique tenders	418,926
Total bidder-tender observations	1,025,939
Pre-ban (2013–2016)	464,524
Post-ban (2017–2020)	561,415
Unique firms by donor status	
Donor firms (donated to winning 2012 mayor)	625
Non-donor firms (all other bidding firms)	133,258
Donor firms with shareholder substitution (2016)	21
Bidder-tender observations by donor status	
Donor firm observations	5,565
Non-donor firm observations	1,020,374
Donor share of observations (%)	0.54
Municipality characteristics	
Competitive municipalities (below-median HHI)	195
Non-competitive municipalities (above-median HHI)	134
Incumbent mayor municipalities	65
New mayor municipalities	264
Pre-ban outcomes by donor status	
Mean market share — donor firms (pp)	2.52
Mean market share — non-donor firms (pp)	0.45
Mean win rate — donor firms	0.84
Mean win rate — non-donor firms	0.69
Tender characteristics	
Competitive tenders	234,216
Non-competitive tenders	183,846
Average number of bidders per tender	9.14
Median number of bidders per tender	4
25th percentile of bidders per tender	1
75th percentile of bidders per tender	10
Maximum number of bidders per tender	605

Notes: Unit of observation is bidder-tender pair. Sample covers executive branch procurement in 329 municipalities across four Brazilian states (Minas Gerais, Paraná, Pernambuco, and Paraíba), 2013–2020. The sample is restricted to municipalities where at least one firm donated to the winning 2012 mayoral candidate and subsequently participated in procurement, since identification of the treatment effect requires within-municipality variation in donor status. Donor firms are defined as firms that made direct corporate contributions to the winning mayoral candidate in the 2012 municipal elections, prior to the ban enacted in 2015. Market structure is defined by the pre-ban (2013–2016) Herfindahl-Hirschman Index split at the median. Incumbent = same mayor elected in both 2012 and 2016. Shareholder substitution = at least one firm shareholder made a personal donation to the winning 2016 mayoral candidate.

The sample covers four states (Minas Gerais, Paraíba, Pernambuco, and Paraná), selected for data availability and quality. I restrict to executive branch procurement over 2013–2020, yielding a pre-ban period (2013–2016) and a post-ban period (2017–2020). The sample is restricted to municipalities that report both winning and losing participants in at least 5% of competitive tenders, ensuring that the participation data is meaningful.

4.2 Dependent Variables

The preferred outcome is **market share**, defined as firm i's share of total municipal procurement value in municipality m and year t, expressed in percentage points.[3] This measure captures the firm's overall footprint in the municipal market rather than its performance on any individual tender. It is the appropriate outcome for testing a market restructuring argument: if the ban changed who participates broadly rather than who wins individual tenders, the effect should appear here. For mechanism tests, I decompose market share by procurement source (city hall versus other executive agencies) and by modality (non-competitive versus competitive tenders), and I examine whether the relevant political relationship is personal or partisan by decomposing political continuity into same mayor, same party but different mayor, and full party turnover.[4]

4.3 Treatment Variable

The treatment variable of primary interest is **Donor**, D_{im}, equal to 1 if firm i made a campaign contribution to the winning mayoral candidate in municipality m in the 2012

[3]Market share is computed as the ratio of the total value of contracts awarded to firm i in municipality m in year t to total procurement spending in that municipality-year, multiplied by 100. A firm that does not win any contract in a given municipality-year has a market share of zero and remains in the sample.

[4]I also estimate the model on three secondary extensive-margin outcomes — log contract value, contracts won, and tenders entered — and on win rate as an intensive-margin falsification. Win rate is a binary variable equal to 1 if the firm won the tender conditional on having entered it. Under H1, win rates should be unaffected by the ban: donor firms' capabilities did not change, only the political environment in which they competed. A null win rate alongside negative market share effects rules out the alternative that donor firms simply became less productive. Results for all secondary outcomes are reported in the appendix.

elections, and 0 otherwise.[5] The ban is operationalized as **Post**, $P_t = \mathbf{1}[t \geq 2017]$, equal to 1 in years following the first post-ban electoral cycle and 0 otherwise. The ban applies uniformly to all municipalities, so P_t does not vary across firms or municipalities.

The regressor of interest is the interaction $D_{im} \times P_t$, whose coefficient $\hat{\delta}$ is the DiD estimator throughout. It captures the differential change in outcomes for donor firms relative to non-donor firms after the ban, within the same municipality. For the loser-donor falsification in Section 7, **Loser**, L_{im}, indicates donation to the losing 2012 mayoral candidate; both winner- and loser-donors are politically active firms, so this comparison isolates the winner-specific political advantage from general donor characteristics.

Finally, **Shareholder substitution** is an indicator equal to 1 if any shareholder of a donor firm personally donated to the 2016 winning mayoral candidate, constructed by linking Federal Revenue Service ownership records (Receita Federal) to Electoral Court 2016 individual donation data (TSE) via personal tax identifiers (CPF). This variable is used only in the substitution robustness check reported in the appendix.

4.4 Moderators

Two conditions define the heterogeneity subsamples. The first is **market concentration**, measured by the pre-ban Herfindahl-Hirschman Index (HHI) computed from procurement values over 2013–2016 and split at the median into competitive (below median) and non-competitive (above median) markets.[6] The second is **political continuity**, measured by whether the 2012 mayor was reelected in 2016. For the mechanism test on personal versus party relationships, political continuity is further decomposed into three categories: same mayor reelected, same party but different person elected, and full party turnover.

[5] Donations are recorded at the municipality level in the Electoral Court campaign finance registry (TSE). A firm that donated to the winning mayor in municipality A but also participates in procurement in municipality B without donating is classified as a donor only in A. This municipality-specificity is critical for the most demanding specifications, which exploit within-firm variation across municipalities with different donation histories.

[6] Higher HHI values indicate greater concentration, meaning fewer firms account for a larger share of total procurement spending. The median split produces two subsamples of roughly equal size. Robustness checks using tercile splits are reported in Section 7.

4.5 Identification

Before presenting the specifications, it is worth stating explicitly why a simple regression of procurement outcomes on donor status would be problematic. The core concern is that donor firms differ from non-donor firms in ways that independently predict procurement outcomes. Donor firms are, on average, larger, older, better-capitalized, and more experienced in public contracting — characteristics that would generate a positive correlation between donor status and market share even in the absence of any political advantage. A naïve comparison of donor and non-donor outcomes would therefore confound the political premium with these underlying differences in firm quality. Beyond firm-level selection, there is a municipality-level concern: municipalities where donor firms are active may differ systematically from those where they are not — they may be larger, wealthier, or have more active procurement markets, all of which could independently affect the outcomes. Finally, there is a temporal concern: the ban coincided with the 2016 municipal elections and with a period of significant macroeconomic contraction in Brazil, meaning that any post-2016 change in procurement patterns could reflect the recession, the election, or the ban itself. The progressive specification strategy below addresses each of these concerns sequentially: municipality fixed effects absorb all time-invariant differences across markets, year fixed effects absorb aggregate temporal shocks, and firm-level controls address the most salient observable differences between donor and non-donor firms within the same municipality.

The baseline specification includes no fixed effects or controls:

$$Y_{imt} = \beta\, D_{im} + \delta(D_{im} \times P_t) + \varepsilon_{imt} \tag{1}$$

where Y_{imt} is the outcome for firm i in municipality m in year t, D_{im} indicates whether firm i donated to the winning 2012 mayor in municipality m, and P_t indicates the post-ban period (2017–2020). This serves as a benchmark, capturing the full unconditional variation in the data.

The second specification adds municipality fixed effects (α_m) and year fixed effects (γ_t):

$$Y_{imt} = \beta\, D_{im} + \delta(D_{im} \times P_t) + \alpha_m + \gamma_t + \varepsilon_{imt} \tag{2}$$

Municipality fixed effects absorb all time-invariant differences across markets. Year fixed effects absorb P_t and any aggregate time trends. Both D_{im} and the interaction δ remain identified, with identification coming from variation in donor status across firms within the same municipality.

The third specification adds firm-level controls X_i:

$$Y_{imt} = \beta\, D_{im} + \delta(D_{im} \times P_t) + \alpha_m + \gamma_t + X_i'\varphi + \varepsilon_{imt} \tag{3}$$

X_i includes log capitalization, firm size category dummies (*porte*), legal form dummies (*natureza jurídica*), and firm age, drawn from the Federal Revenue Service firm registry (Receita Federal). These controls address the main threat to identification in the municipality fixed effects design: that donor firms may be systematically larger, better-capitalized, older, or differently organized than non-donors, and that these characteristics independently predict procurement outcomes. Because X_i is time-invariant, it does not absorb the treatment or the post period; it sharpens the comparison between donor and non-donor firms within the same municipality by holding observable firm characteristics constant. Standard errors are clustered at the municipality level throughout.

The Donor main effect $\hat{\beta}$ is identified in all three specifications. It captures the average pre-ban level difference in procurement outcomes between donor and non-donor firms within the same municipality, net of observable firm characteristics in column 3. As argued in Section 3.1, the persistence of a large, statistically significant $\hat{\beta}$ after controlling for size, capitalization, age, and legal form is itself evidence that the donor–non-donor gap was politically conditioned rather than purely capability-driven. The stability of $\hat{\beta}$ across specifications in Table 3 provides this evidence.

I test the hypotheses about market structure and political turnover by estimating Equation 3 on subsamples: competitive vs. non-competitive markets (H2), new mayor vs. incumbent (H3), and the four cells of their cross-tabulation (H4). This subsample approach avoids triple interactions, which are difficult to interpret and require strong functional form assumptions. The controls specification is the reference throughout; full progressive specifications for all heterogeneity subsamples are in Appendix 9.

5 Results

This section presents the empirical results in two parts. I first report the main difference-in-differences estimates for market share, establishing that donor firms lost roughly 0.85 percentage points of market share after the ban — a decline of more than 30% relative to their pre-ban mean — with the effect stable across progressive specifications and driven entirely by the extensive margin. I then decompose this average effect by pre-ban market concentration and political continuity, showing that the loss is concentrated in non-competitive markets (-2.3pp) and under new mayors (-0.9pp), with the interaction of both conditions producing the sharpest declines (-2.1pp in non-competitive markets under new mayors) and near-zero effects where neither condition holds.

5.1 Main Results

Table 3 presents the main difference-in-differences estimates for market share across three progressive specifications. Donor firms held significantly larger procurement shares prior to the ban — roughly 2.1 percentage points more than non-donor firms within the same municipality in the unconditional specification, and 1.8 percentage points after adding fixed effects and controls. The Donor $\times$ Post coefficient indicates that the ban reduced donor market share by roughly 0.8 percentage points, stable across all three specifications and

statistically significant at the 0.1% level throughout.[7]

The stability of the Donor × Post coefficient across specifications is informative in its own right. Moving from column (1), which includes no fixed effects, to column (2), which adds municipality and year fixed effects, changes the estimate only slightly — from −0.81pp to −0.78pp. Adding firm-level controls in column (3) produces an estimate of −0.85pp. The insensitivity to specification suggests that the result is not driven by compositional differences across municipalities or by observable firm characteristics. The Donor main effect — roughly 1.8 to 2.1 percentage points across all three columns — is itself telling. Even after controlling for firm size, capitalization, age, and legal form, donor firms held substantially larger market shares than non-donors in the same municipality before the ban. This gap is not explained by the observable characteristics that predict procurement success. It reflects the political environment in which both sets of firms operated: one that sustained donor firms' market positions beyond what their characteristics alone would predict, and one that the ban disrupted.

Figures 2 and 3 map the donor market share premium — the difference between donor and non-donor mean market share — across municipalities in each of the four sample states, before and after the ban. Blue municipalities are those where donor firms held larger shares than non-donors; red municipalities are those where non-donors held larger shares; grey municipalities had no donor firm activity in that period. The maps provide descriptive visual evidence consistent with the regression results. In Minas Gerais and Paraná, which had the densest donor firm activity pre-ban, the reduction in blue municipalities is visible and geographically broad — donor advantages were not confined to state capitals or large urban centres but dispersed across medium and smaller municipalities where procurement markets were more concentrated. Paraíba and Pernambuco show sparser donor activity

[7] I also estimate the model on three secondary extensive-margin outcomes — log contract value, contracts won, and tenders entered — and on win rate as an intensive-margin falsification. Win rates are null across all specifications, establishing that the adjustment is entirely on the extensive margin: donor firms lost market share without losing ground conditional on participation. This rules out the alternative that donor firms simply became less productive after the ban. Results for all secondary outcomes are reported in the Appendix.

overall, reflecting the smaller absolute number of donor firms in those states, but the post-ban pattern is directionally consistent: fewer and lighter blue municipalities after 2016.

Two features of the maps are worth noting. First, some municipalities shift from blue to red post-ban, meaning that non-donors came to hold larger shares than donors in those markets after the reform. This is the market restructuring the argument predicts: not just a reduction in donor market share but a reversal of the donor premium in some municipalities. Second, grey municipalities — those with no donor firm activity — are stable across periods, as expected from the sample balance checks. The variation in the maps reflects real changes in donor firm participation, not sample attrition.

Non-donor market share, shown in Appendix 9, is nearly identical pre and post-ban across all four states. The maps barely change. This is precisely what H1 predicts: the ban affected donor firm participation, not non-donor behavior. Non-donors did not surge into the space vacated by donors; donors simply retreated. The market restructured through withdrawal, not through competitive entry on the other side.

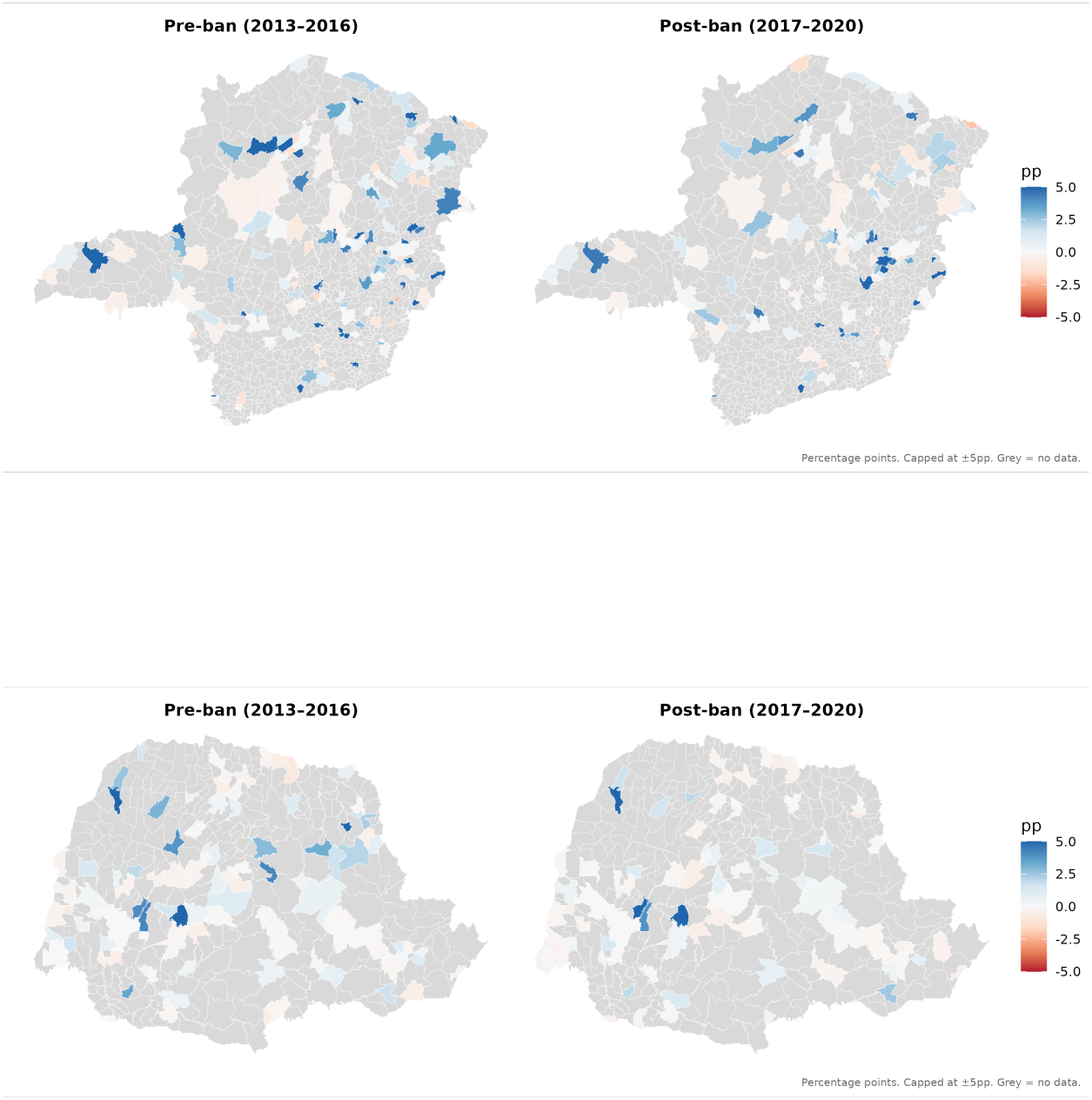


Figure 2: Donor market share premium (donor minus non-donor mean market share, pp) across municipalities in Minas Gerais (top) and Paraná (bottom), before and after the corporate donation ban. Capped at ±5pp. Grey = no donor firm activity.

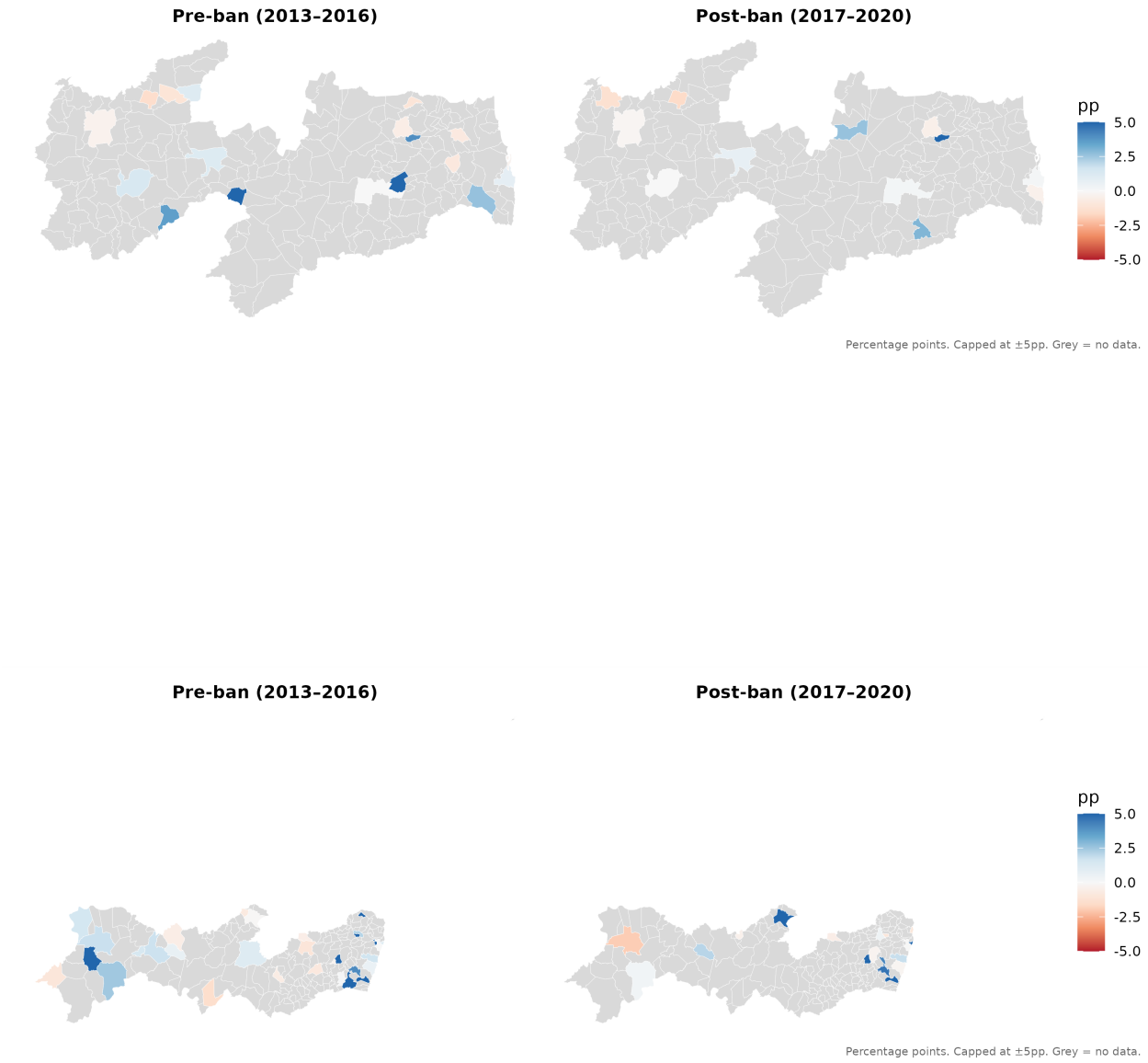


Figure 3: Donor market share premium (donor minus non-donor mean market share, pp) across municipalities in Paraíba (top) and Pernambuco (bottom), before and after the corporate donation ban. Capped at ±5pp. Grey = no donor firm activity.

Table 3: Progressive Specifications — Market Share

	(1)	(2)	(3)
Donor	2.075***	1.851***	1.820***
	(0.319)	(0.288)	(0.305)
Donor × Post	−0.807***	−0.784***	−0.845***
	(0.235)	(0.228)	(0.244)
Log Capital			0.008***
			(0.002)
Firm Size			0.040***
			(0.004)
Entity Type			−0.000
			(0.000)
Firm Age			0.001**
			(0.000)
Municipality FE	No	Yes	Yes
Year FE	No	Yes	Yes
Sector FE	No	No	Yes
Observations	565,833	565,833	477,843
R^2	0.006	0.086	0.112
Within R^2	—	0.005	0.008

Notes: Dependent variable is market share (pp). Unit of observation is firm-municipality-year. Sample restricted to the 329 municipalities where at least one donor firm participates in executive branch procurement. Post = 1 for 2017–2020. Donor = 1 if the firm donated to the winning 2012 mayoral candidate in that municipality. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

5.2 Heterogeneity by Market Structure and Political Continuity

Figure 4 summarizes the heterogeneity results across five subsamples. The ban's effect on market share is concentrated in non-competitive markets (−2.33pp, $p < 0.001$) and municipalities with new mayors (−0.90pp, $p < 0.01$), while the competitive and incumbent subsamples are not statistically significant. Full regression tables for all heterogeneity subsamples are in the Appendix.

The heterogeneity results tell a clear story. In non-competitive markets, donor firms held a pre-ban advantage of 4.3 percentage points relative to non-donors — more than twice the full-sample level — reflecting the degree to which concentrated procurement markets were

organized around political relationships. The ban reduced this advantage by 2.3 percentage points, a decline of more than 50% relative to the pre-ban donor level in those markets. In competitive markets, by contrast, the pre-ban donor advantage was already modest at 0.6 percentage points, and the post-ban coefficient is small and statistically indistinguishable from zero. The ban had nothing to disrupt where donor advantages were already thin. The political continuity results are similarly clean. Municipalities where a new mayor took office in 2016 show a loss of 0.9 percentage points, significant at the 1% level. Municipalities where the incumbent was reelected show a negative but insignificant coefficient of -0.6 percentage points. The personal relationship between donor firms and the specific mayor — not the formal donation channel alone — was what sustained the advantage, and it survived the ban wherever the same mayor remained in office. The gradient is clear: the non-competitive subsample drives the average effect, while the competitive subsample is statistically indistinguishable from zero. The incumbent mayor subsample spans zero, while the new mayor subsample sits firmly to the left.

Figure 5 decomposes these effects by interacting both dimensions. The largest loss occurs in non-competitive markets under new mayors (-2.12pp, $p < 0.01$), where both conditions for disruption were fully present. The smallest effect is in competitive markets under re-elected incumbents, where the coefficient is positive and insignificant — the ban produced no measurable change at all in those markets. The two intermediate cells reveal which condition does more of the work: non-competitive markets under reelected incumbents show a large but imprecisely estimated loss (-3.82pp, $p < 0.1$), driven by a small sample of 13,000 observations, while competitive markets under new mayors show a marginally significant loss (-0.20pp, $p < 0.1$). The gradient runs in exactly the direction H4 predicts: disruption is largest where both conditions hold, near zero where neither does, and intermediate otherwise.

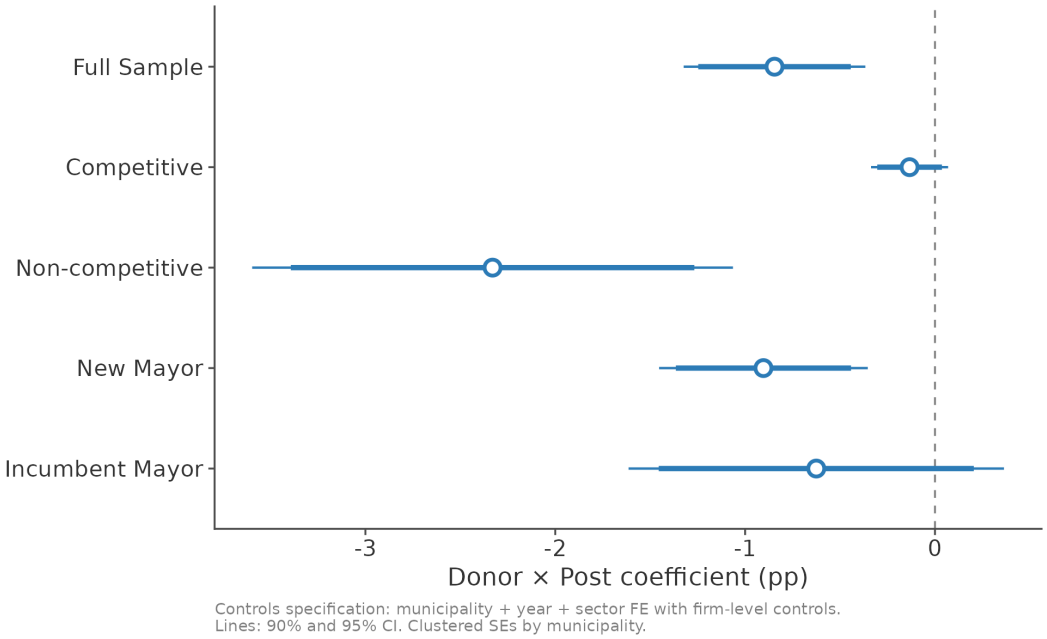


Figure 4: Donor × Post coefficients across heterogeneity subsamples. Controls specification: municipality + year + sector FE with firm-level controls. Lines show 90% and 95% confidence intervals. Standard errors clustered by municipality.

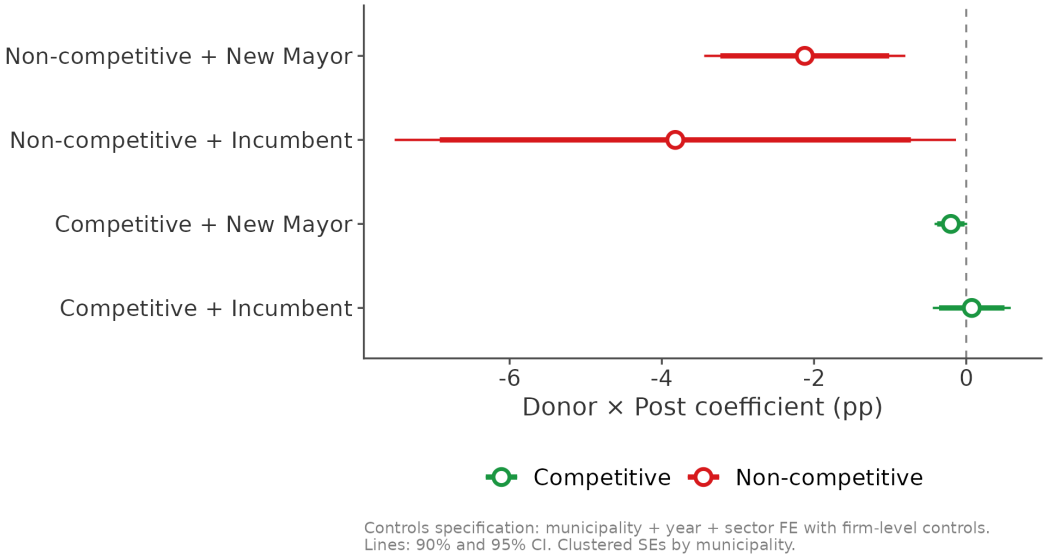


Figure 5: Donor × Post coefficients for the four 2×2 cells defined by market structure and political continuity. Controls specification: municipality + year + sector FE with firm-level controls. Lines show 90% and 95% confidence intervals. Standard errors clustered by municipality.

6 Mechanisms

The main results establish that donor firms lost market share after the ban, and that the loss was concentrated in non-competitive markets under new mayors. This section investigates the mechanisms through which this restructuring operated. I examine three dimensions: which procurement unit within the executive branch drove the effect (city hall versus other agencies), which procurement modality adjusted (competitive versus non-competitive tenders), and whether the relevant political relationship was personal or partisan (same mayor, same party, or full turnover). Together, these tests identify the specific institutional channel — mayoral discretion over city hall procurement, operating through the extensive margin of competitive tenders, and sustained by personal rather than partisan ties — through which the ban reshaped procurement markets.

6.1 Procurement Unit: City Hall vs. Other Executive

The ban's effect concentrates in city hall procurement. Donor firms lose roughly 1.7 percentage points of market share in city hall contracts after the ban (Table 4), while the coefficient for other executive-branch procurement is negative but statistically indistinguishable from zero. The pre-ban donor advantage was itself larger in city hall procurement (3.0pp versus 1.2pp for other executive agencies), consistent with donor firms' market positions being most concentrated in the procurement unit under direct mayoral control. This pattern is consistent with a mechanism rooted in personal relationships with mayoral offices rather than diffuse bureaucratic connections across executive agencies.

Table 4: Market Share: City Hall vs. Other Executive Branch

	City Hall	Other Executive
Donor	2.990***	1.161**
	(0.507)	(0.432)
Donor × Post	−1.728***	−0.450
	(0.464)	(0.420)
Log Capital	0.019***	0.011***
	(0.004)	(0.003)
Firm Size	0.076***	0.030***
	(0.007)	(0.006)
Entity Type	−0.000	0.000
	(0.000)	(0.000)
Firm Age	0.003***	0.000
	(0.001)	(0.000)
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Sector FE	Yes	Yes
Observations	185,561	299,931
R^2	0.092	0.473
Within R^2	0.014	0.002

Notes: Unit of observation is firm-municipality-year. Sample restricted to the 329 municipalities where at least one donor firm participates in executive branch procurement. Post = 1 for 2017–2020. Donor = 1 if the firm donated to the winning 2012 mayoral candidate in that municipality. Sector FE are two-digit CNAE dummies. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

6.2 Procurement Modality

The ban's effect on market share is concentrated in competitive tenders (−0.72pp, $p < 0.01$), while non-competitive procurement shows no significant change (Table 5). This is a counter-intuitive but informative result. If donor advantages operated through political steering of individual contracts, the effect should appear in non-competitive modalities, where mayoral discretion is greatest. Instead, the adjustment occurs in competitive tenders — the modality where participation decisions are most visible and where the signal of political access matters most for whether firms choose to enter. This points to participation and market entry as the margin of adjustment rather than political steering of individual contracts. Appendix

Tables 20 and 21 further explore these results by market structure and political continuity.

Table 5: Market Share by Procurement Modality

	Non-competitive	Competitive
Donor	2.120*	1.968***
	(1.001)	(0.331)
Donor × Post	−1.438	−0.724**
	(1.070)	(0.264)
Log Capital	0.070***	0.013***
	(0.012)	(0.002)
Firm Size	0.167***	0.049***
	(0.028)	(0.005)
Entity Type	0.001	−0.000**
	(0.001)	(0.000)
Firm Age	−0.004·	0.003***
	(0.002)	(0.000)
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Sector FE	Yes	Yes
Observations	105,099	390,642
R^2	0.218	0.125
Within R^2	0.006	0.010

Notes: Unit of observation is firm-municipality-year. Each column restricts to tenders of the indicated modality. Sample restricted to the 329 municipalities where at least one donor firm participates in executive branch procurement. Post = 1 for 2017–2020. Win rate by modality in Appendix 9. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

6.3 Individual vs. Party Continuity

Political connections appear to operate through individual relationships rather than party ties. The market share loss is concentrated in full-turnover municipalities (−0.99pp, $p < 0.001$), where neither the mayor nor the party continues. Same-party municipalities with a new mayor show no significant effect, and same-mayor municipalities likewise show no significant loss (Table 6). Together, these results suggest donor advantages reflect relationships with specific officeholders rather than party networks. The null result for same-party transitions is particularly informative: even when the incoming mayor belongs to the same

party as the outgoing one, the donor advantage does not transfer.

Table 6: Market Share by Political Continuity Type

	Same Mayor	Same Party	Full Turnover
Donor	1.794**	0.973**	1.879***
	(0.596)	(0.331)	(0.379)
Donor × Post	−0.625	0.240	−0.989***
	(0.504)	(0.476)	(0.297)
Log Capital	0.006*	0.029**	0.008**
	(0.002)	(0.008)	(0.002)
Firm Size	0.027**	0.067**	0.043***
	(0.009)	(0.022)	(0.005)
Entity Type	−0.000	0.001·	−0.000·
	(0.000)	(0.000)	(0.000)
Firm Age	0.001*	−0.001	0.001*
	(0.001)	(0.002)	(0.000)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Observations	122,006	21,193	334,643
R^2	0.140	0.113	0.103
Within R^2	0.009	0.008	0.008

Notes: Unit of observation is firm-municipality-year. Sample restricted to the 329 municipalities where at least one donor firm participates in executive branch procurement. Post = 1 for 2017–2020. Same Mayor = the same individual was elected in both 2012 and 2016. Same Party = a different candidate from the same party won in 2016. Full Turnover = a candidate from a different party won in 2016. Win rate by continuity type in Appendix 9. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

7 Robustness

This section subjects the main results to a series of robustness and falsification tests. I begin with event studies to assess pre-trend balance and the dynamic path of the treatment effect. I then restrict the sample to close elections to address selection on donor status, vary the market concentration cutoff using tercile splits, and test whether loser donors — firms that donated to the losing 2012 candidate — show any post-ban change. Finally, I estimate the main specification on legislative branch procurement as a placebo.

7.1 Event Studies

Table 7 reports pre-period event-study coefficients relative to 2016 for the incumbent and new-mayor subsamples. The 2013 interaction is negative and marginally significant in the incumbent subsample (-1.11pp, $p = 0.09$) and significant in the new-mayor subsample (-0.98pp, $p = 0.01$), indicating that donor firms gained market share relative to non-donors between 2013 and 2016. This pattern reflects a gradual accumulation of procurement relationships during the pre-ban period rather than a violation of parallel trends — donor and non-donor firms were trending apart before the ban, with donors consolidating their advantage through the final pre-ban year. The 2014 and 2015 coefficients are uniformly null in both subsamples, and the post-ban decline documented in the main results represents a reversal of this trajectory rather than a continuation of it.

Figure 6 plots the full event-study sequence for the full sample. The donor premium rises through 2014–2015, collapses at the reference year, and partially recovers post-ban — consistent with market restructuring rather than a permanent reversion to the pre-ban level. Figures 7 and 8 decompose this pattern by market structure and political continuity. In non-competitive markets, the pre-ban donor premium was substantially larger and the post-ban collapse sharper; in competitive markets the series is flat throughout. Under new mayors, the drop at 2016 is immediate and the recovery slower. Under reelected incumbents, the series barely moves.

Table 7: Pre-Trend Balance

	Incumbent	New Mayor
Donor	2.540*	2.029***
	(1.088)	(0.404)
2013 × Donor	−1.105·	−0.980*
	(0.642)	(0.400)
2014 × Donor	−1.133	0.010
	(0.872)	(0.521)
2015 × Donor	−0.584	−0.219
	(0.967)	(0.450)
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Reference year	2016	2016

Notes: Sample restricted to pre-ban years (2013–2016) and to the 329 municipalities where at least one donor firm participates in executive branch procurement. Dependent variable is market share (pp). Unit of observation is firm-municipality-year. Coefficients are interactions of year indicators with the donor indicator, relative to 2016. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

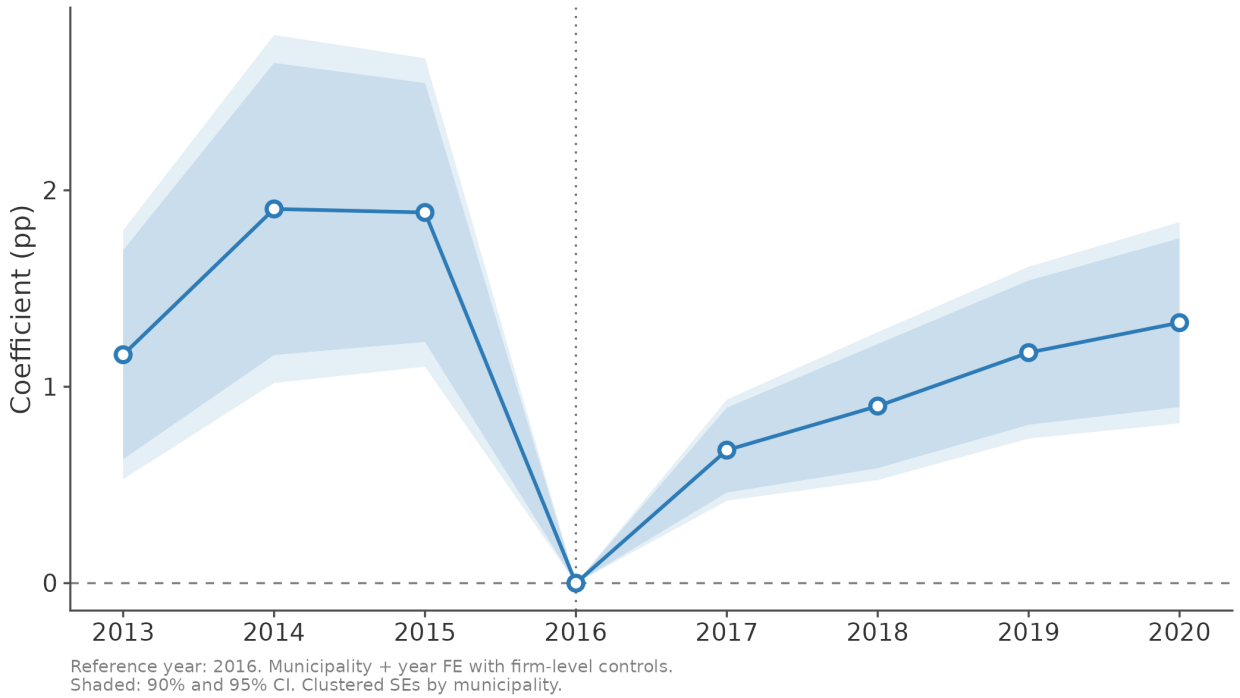


Figure 6: Event study estimates for donor market share premium, full sample. Reference year: 2016. Municipality and year fixed effects with firm-level controls. Shaded areas show 90% and 95% confidence intervals. Standard errors clustered by municipality.

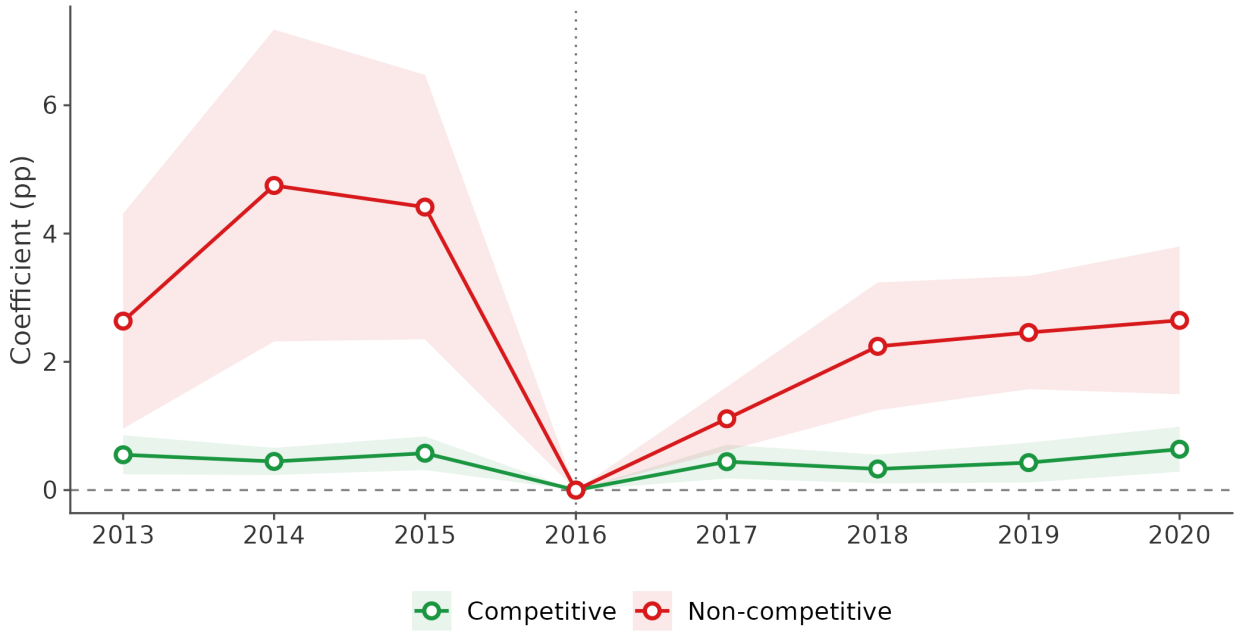


Figure 7: Event study estimates by market structure. Competitive = below-median pre-ban HHI; Non-competitive = above-median. Reference year: 2016. Municipality and year fixed effects with firm-level controls. Shaded areas show 95% confidence intervals. Standard errors clustered by municipality.

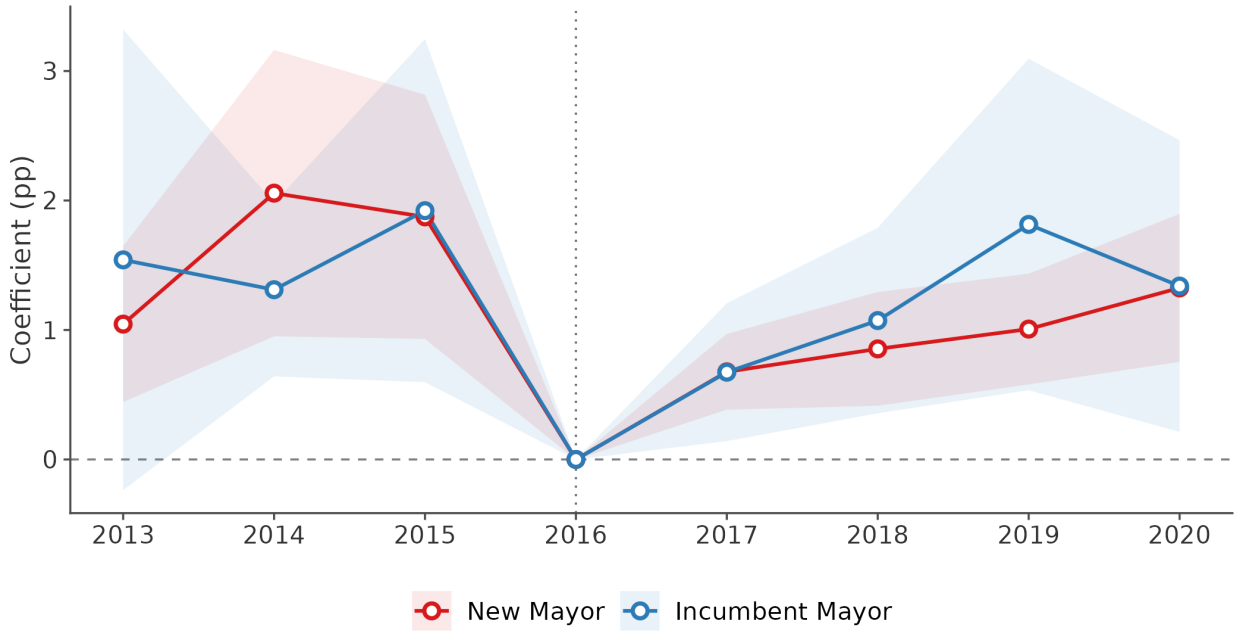


Figure 8: Event study estimates by political continuity. New Mayor = a different individual won the 2016 election; Incumbent Mayor = same individual reelected. Reference year: 2016. Municipality and year fixed effects with firm-level controls. Shaded areas show 95% confidence intervals. Standard errors clustered by municipality.

7.2 Close Elections

If donor status is correlated with unobserved municipal characteristics that also predict post-ban procurement, the main estimates may be biased. Restricting to municipalities where the 2012 race was decided by a narrow margin isolates quasi-random variation in whether a donor firm's candidate won. Table 8 reports estimates across four bandwidths. At 5 and 10 percentage points, the Donor × Post estimate is stable at −0.89pp ($p < 0.1$) and −1.10pp ($p < 0.01$), respectively — the latter larger than the full-sample coefficient, suggesting that the effect may be stronger in electorally competitive environments. At tighter bandwidths of 1 and 2.5 percentage points, the point estimates fall to near zero, but the number of municipality clusters drops to 12 and 36 respectively — well below the threshold at which cluster-robust inference is reliable. The loss of significance at narrow bandwidths therefore

reflects a power problem rather than evidence against the main result: the standard errors at 2.5pp (0.234) are comparable to those at 5pp (0.473), but the identifying variation comes from too few municipalities to estimate the effect precisely.

Table 8: Robustness: Close Elections

	Full Sample	≤1pp	≤2.5pp	≤5pp	≤10pp
Donor	1.820***	0.398	0.670*	1.782**	2.107***
	(0.305)	(0.377)	(0.326)	(0.533)	(0.459)
Donor × Post	−0.845***	−0.062	−0.061	−0.885·	−1.099**
	(0.244)	(0.233)	(0.234)	(0.473)	(0.396)
Log Capital	0.008***	0.013·	0.009·	0.009*	0.008*
	(0.002)	(0.007)	(0.006)	(0.004)	(0.003)
Firm Size	0.040***	0.035*	0.047***	0.048***	0.049***
	(0.004)	(0.013)	(0.010)	(0.008)	(0.006)
Entity Type	−0.000	−0.000	−0.000*	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Firm Age	0.001**	0.001	0.002·	0.001*	0.001
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes
Municipalities	329	12	36	83	152
Observations	477,843	25,007	48,705	103,572	191,087
R^2	0.112	0.140	0.145	0.104	0.106
Within R^2	0.008	0.008	0.007	0.009	0.010

Notes: Dependent variable is market share (pp). Unit of observation is firm-municipality-year. Sample restricted to municipalities where at least one donor firm participates in executive branch procurement. Post = 1 for 2017–2020. Close elections restrict to municipalities where the 2012 mayoral race was decided by the indicated margin or fewer. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

7.3 HHI Terciles

The main heterogeneity analysis uses a median split on pre-ban HHI. Table 9 replaces this with a tercile split to verify the result is not sensitive to the cutoff choice. The gradient is monotonic: Donor × Post is −0.22pp (marginal significance) in the lowest tercile, −0.15pp (null) in the middle, and −2.13pp ($p < 0.001$) in the highest. The concentration of losses in

non-competitive markets is robust to how market structure is measured.

Table 9: Robustness: HHI Tercile Subsamples

	Low HHI	Mid HHI	High HHI
Donor	0.490***	0.598**	4.159***
	(0.119)	(0.200)	(0.662)
Donor × Post	−0.220·	−0.148	−2.129***
	(0.111)	(0.151)	(0.619)
Log Capital	0.008***	0.009**	0.012*
	(0.002)	(0.003)	(0.005)
Firm Size	0.019***	0.028***	0.089***
	(0.003)	(0.006)	(0.010)
Entity Type	0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)
Firm Age	0.001	0.001·	0.001
	(0.000)	(0.001)	(0.001)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Observations	200,363	153,942	123,534
R^2	0.097	0.148	0.093
Within R^2	0.005	0.004	0.018

Notes: Dependent variable is market share (pp). Unit of observation is firm-municipality-year. Sample restricted to the 329 municipalities where at least one donor firm participates in executive branch procurement. Post = 1 for 2017–2020. Terciles are defined on pre-ban (2013–2016) HHI within each municipality. Sector FE are two-digit CNAE dummies. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

7.4 Legislative Branch Placebo

If donor advantages operate through the mayor's control over executive procurement, they should not appear in legislative branch procurement, where the mayor has no discretion. Table 10 reports Donor × Post estimates for legislative tenders across the four heterogeneity subsamples. All coefficients are null. The non-competitive subsample produces a positive point estimate (0.11), but this is not statistically significant and directionally opposite to the executive branch results. The absence of any systematic pattern in legislative procurement supports the interpretation that the main results reflect mayoral discretion rather than

unobserved firm characteristics.

Table 10: Falsification: Legislative Branch Procurement

	Competitive	Non-competitive	New Mayor	Incumbent
Donor	0.026	−0.046	−0.011	0.090$^{\cdot}$
	(0.031)	(0.070)	(0.036)	(0.049)
Donor × Post	0.025	0.110	0.045	0.031
	(0.031)	(0.110)	(0.042)	(0.053)
Log Capital	−0.003	−0.003	−0.005*	0.001
	(0.002)	(0.003)	(0.002)	(0.002)
Firm Size	0.007	0.019***	0.006*	0.018*
	(0.005)	(0.005)	(0.003)	(0.008)
Entity Type	0.000	0.000**	0.000*	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Firm Age	0.001**	−0.000	0.002***	0.000
	(0.001)	(0.001)	(0.000)	(0.001)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
Observations	22,732	4,949	20,041	7,640
R^2	0.216	0.308	0.235	0.229
Within R^2	0.002	0.007	0.003	0.005

Notes: Dependent variable is market share (pp). Unit of observation is firm-municipality-year. Sample restricted to the 329 municipalities where at least one donor firm participates in executive branch procurement. Post = 1 for 2017–2020. Sector FE are two-digit CNAE dummies. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

7.5 Loser Donors

Firms that donated to the losing 2012 candidate provide a direct falsification test: if the main effect reflects genuine donor-winner advantages rather than omitted firm quality, loser donors should show no post-ban change. Table 11 confirms this. The Loser × Post coefficient is null across all three specifications, while the positive level effect (0.44pp) simply reflects that politically active firms are larger and more competitive regardless of whether their candidate won.

Table 11: Falsification: Loser Donors — Market Share

	(1)	(2)	(3)
Loser Firm	0.418	0.542*	0.435·
	(0.283)	(0.236)	(0.246)
Loser Firm × Post	−0.037	−0.073	−0.085
	(0.207)	(0.201)	(0.214)
Municipality FE	No	Yes	Yes
Year FE	No	Yes	Yes
Sector FE	No	No	Yes
Firm Controls	No	No	Yes
Observations	565,833	565,833	477,843
Within R^2	—	0.000	0.003

Notes: Dependent variable is market share (pp). Unit of observation is firm-municipality-year. Sample restricted to the 329 municipalities where at least one donor firm participates in executive branch procurement. Loser Firm = firm donated to the losing 2012 mayoral candidate. Post = 1 for 2017–2020. Firm controls: log capital, firm size, entity type, firm age. Sector FE are two-digit CNAE dummies. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

8 Discussion

The central empirical finding of this paper, that the ban reduced market share without changing win rates, speaks directly to a long-standing debate about why firms donate to political campaigns. The consumption view holds that donations are expressive: firms give because they care about political outcomes, not because they expect a measurable economic return (Ansolabehere, de Figueiredo, and Snyder 2003). If this were true, removing the donation channel should leave procurement outcomes unchanged. It did not. Donor firms lost market share after the ban, and the losses were largest precisely where the political environment was most fully disrupted. The investment view, that donations purchase access and that access shapes economic outcomes (Ferguson 1995; Gilens and Page 2014), is consistent with this pattern. But the finding adds precision to what the investment bought. It was not individual contracts. Win rates did not move. What donations purchased was a market environment favorable to broad, sustained participation, one in which donor firms entered more markets, competed more consistently, and non-donor firms stayed out. The

ban disrupted that environment, and the extensive margin adjusted accordingly.

This result connects to how political connections shape procurement participation rather than individual contract outcomes. Coviello and Mariniello (2014) show that connections determine who enters procurement markets, not just who wins. Coviello and Gagliarducci (2017) show that longer mayoral tenure produces more concentrated procurement markets with fewer bidders and higher contract values. Decarolis, Fisman, Pinotti, and Vannutelli (2025) document systematic concentration around politically connected suppliers in contexts with high contracting discretion. The heterogeneity results in this paper are consistent with all three findings. The non-competitive markets where the ban's effect is largest are precisely the markets where political relationships had the most time and space to structure participation: concentrated, dominated by a small number of suppliers, with limited non-donor entry. The ban disrupted this structure most sharply where it was most entrenched. The competitive markets where the effect is near zero are those where the political premium was already modest and participation was already broader. There was less to disrupt because politics had done less of the structuring work to begin with.

The mechanism results, that losses concentrate in city hall procurement and under full political turnover rather than same-party transitions, speak to how political relationships operate at the individual level. Same-party transitions produce no significant effect while full turnover drives the result. This is consistent with Mironov and Zhuravskaya's (2016) account of informal relationships built through repeated interactions with specific officeholders rather than through institutional or partisan channels, and with Ferraz and Finan's (2011) finding that mayoral accountability is tied to the individual in office. What matters is not the party's procurement policy but the specific mayor's relationship with specific firms. The ban severed the financial channel everywhere, but the access built through past donations survived wherever the same mayor remained in office. Where it did not, where a new mayor with no history with the old donors took office, the relationship ended and the market restructured. Party affiliation carried none of this over.

Guerra, Mattos, and Nakaguma (2025) find that more exposed sector-municipalities experienced increased entry and lower concentration after the ban, consistent with the market restructuring documented here. But their sector-municipality level of analysis cannot identify which firms gained or lost, cannot trace the donor-mayor relationship at the firm level, and cannot test whether effects depend on whether the donated-to mayor was reelected. Dahis, Ricca, and Scot (2025) find that preferential payment to connected firms weakened after the reform, pointing to the payment channel rather than market structure. Avis, Ferraz, Finan, and Varjão (2022) document that spending caps introduced alongside the ban reduced incumbent reelection rates, establishing that the reform reshaped electoral competition but not its procurement consequences. The variation in reform effectiveness documented across these studies is not noise. It reflects the underlying conditions in which the ban landed, and those conditions are the two this paper identifies: market concentration and political continuity.

The evidence on individual donation substitution after the ban adds a cautionary note. Individual donations surged by nearly 50% in the first post-ban electoral cycle (Reis and Eduardo 2019), and FGV-DAPP data suggest that the largest individual donors were predominantly senior executives at firms, the same interests that had previously donated corporately. The substitution result in this paper, fragile as it is, points in the same direction: in markets dominated by a single donor firm, shareholder personal donations to the 2016 winning candidate are associated with sustained market share after the ban, while the effect is negative and significant everywhere else. These patterns suggest that the underlying demand for political access did not disappear when the corporate channel closed, it adapted. The market effects documented here are real, but they may capture a transition period rather than a permanent equilibrium. Whether donor advantages re-emerge as new informal relationships consolidate over longer time horizons is a question the current data cannot answer, and one that future work with post-2020 procurement records should address directly. What the findings do establish is the mechanism through which formal reform produces market change

when it does: not by making donor firms less capable, but by severing the specific personal relationship between a donor firm and a specific mayor at the moment the formal financial channel closes. Where that severance was complete, the market restructured. Where it was not, the ban changed very little.

The findings have broader implications for how we study and design anti-corruption reform. Campaign finance reform is often evaluated as a single intervention applied uniformly, and its effects are measured on average. This paper suggests that both the evaluation strategy and the design logic need to account for the political and market conditions in which reform lands. A ban that arrives alongside electoral turnover in concentrated markets is a structurally different intervention than one that does not, and treating them as equivalent obscures most of the variation that matters.

Future work should examine whether the conditions identified here, market concentration and political continuity, operate similarly in other reform contexts and other countries. It should also trace what happens over time, as informal relationships rebuild, new donors emerge, and procurement markets settle into new equilibria. The 2020 municipal elections in Brazil offer a natural next window: a second cycle of turnover, under a ban now five years old, in markets that have already partially restructured. Whether the patterns documented here persist, intensify, or reverse as the political landscape shifts is an open question.

9 Conclusion

A corporate donation ban removes the financial channel between firms and politicians. It does not remove the firms, their capabilities, or the mayors who control procurement spending. This distinction is the paper's starting point and its central finding: Brazil's 2015 corporate donation ban reduced donor firms' procurement market share by roughly 0.85 percentage points — a decline of more than 30% relative to their pre-ban mean — without changing their win rates conditional on participation. The ban changed who enters procure-

ment markets, not who wins once they do. Evaluating campaign finance reform through win rates alone, the dominant approach in the literature, misses where the advantage actually operates. Donor firms held disproportionate market positions not because they outcompeted rivals on individual tenders, but because the political environment sustained their broad participation and discouraged non-donors from entering. When the ban disrupted that environment, market share fell. Win rates did not.

That disruption, however, was not uniform, and the conditions that determined how much it occurred are themselves informative about how anti-corruption reform works. In concentrated markets where a new mayor took office, the ban removed both the financial channel and the personal relationship simultaneously, and market share fell by more than 2 percentage points. In competitive markets under reelected incumbents, the donation channel closed but the political environment changed very little, and the effect was near zero. This is not a story about some municipalities complying with the ban and others evading it. The ban was the same everywhere. What differed was the political and market context in which it landed. Non-competitive markets had more politically-sustained market share to lose; new mayors had no reason to maintain the access that old donations had purchased. Where both conditions held, the reform leveled the field. Where neither did, it barely moved it.

The mechanism results sharpen this picture. Losses concentrated in city hall procurement (-1.7pp) rather than other executive agencies, in competitive tenders (-0.7pp) rather than non-competitive channels, and in municipalities with full political turnover (-1.0pp) rather than same-party transitions. Each of these findings points to the same conclusion: what mattered was the specific relationship between the donor firm and the specific mayor, not a diffuse bureaucratic advantage, and not a party network. The ban was most effective precisely where it severed a personal relationship, and least effective where that relationship survived it.

Taken together, these findings suggest that the right question for evaluating campaign finance reform is not whether favoritism falls on average, but whether the reform dismantles

the political conditions that sustained donor advantages in the first place. A ban that coincides with political turnover in concentrated markets is a fundamentally different intervention than one that does not, even if the formal rule is identical. For reformers and policymakers, this has a practical implication: the sequencing and timing of anti-corruption reforms matter. A donation ban that takes effect alongside an election that brings new mayors to office will be more disruptive than one that does not. Targeting enforcement and monitoring resources toward concentrated procurement markets in municipalities undergoing political change is more likely to produce measurable restructuring than a uniform approach.

This paper has limitations that should inform how the findings are read. The sample covers 329 municipalities across four Brazilian states selected for data availability and quality, restricted to those where at least one donor firm participates in procurement, and the results may not generalize to other institutional contexts. The pre-trend tests show a significant 2013 coefficient in new-mayor municipalities, which I address in the robustness section but cannot fully resolve. The substitution result, which suggests that dominant donor firms may have sustained their market positions through shareholder personal donations after the ban, is suggestive but fragile and should be treated as indicative rather than conclusive. Finally, the four-year post-ban window (2017–2020) captures the immediate market response but cannot speak to whether the restructuring is permanent or whether donor advantages re-emerge over longer time horizons through new informal channels. The data end before the 2020 municipal elections, so the paper cannot trace what happens as a second cycle of new and reelected mayors takes office, a natural next test of whether the conditions identified here operate symmetrically over time, and whether markets that restructured after 2016 consolidate around new participants or revert as political relationships rebuild under different forms.

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Appendix

Table 12: Progressive Specifications — Secondary Outcomes

	Log Contract Value			Contracts Won			Tenders Entered			Win Rate (Falsification)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Donor	1.175***	1.275***	1.146***	1.020***	1.078***	0.951***	0.862*	1.171***	1.040***	0.155*	0.054	0.043
	(0.127)	(0.121)	(0.099)	(0.211)	(0.197)	(0.210)	(0.369)	(0.270)	(0.288)	(0.069)	(0.037)	(0.028)
Donor × Post	−0.084	−0.099	−0.084	−0.293·	−0.347*	−0.344*	−0.301·	−0.388*	−0.354*	−0.015	−0.017	−0.015
	(0.120)	(0.113)	(0.093)	(0.155)	(0.155)	(0.163)	(0.170)	(0.160)	(0.165)	(0.023)	(0.017)	(0.012)
Log Capital			0.088***			0.011			0.038**			−0.007***
			(0.013)			(0.007)			(0.015)			(0.002)
Firm Size			0.084***			−0.013*			−0.053***			0.011***
			(0.010)			(0.006)			(0.015)			(0.002)
Entity Type			0.000			−0.000**			−0.000			0.000
			(0.000)			(0.000)			(0.000)			(0.000)
Firm Age			−0.010***			0.007***			0.004*			0.002***
			(0.001)			(0.002)			(0.002)			(0.000)
Municipality FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Sector FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	446,619	446,619	367,631	565,833	565,833	477,843	565,833	565,833	477,843	1,025,939	1,025,939	901,639
R^2	0.002	0.108	0.231	0.001	0.022	0.039	0.001	0.051	0.073	0.002	0.153	0.223
Within R^2	—	0.001	0.017	—	0.001	0.002	—	0.000	0.001	—	0.000	0.004

Notes: Columns 1–9: unit of observation is firm-municipality-year. Columns 10–12: unit of observation is bidder (participant-level); win rate is a binary indicator equal to 1 if the firm won the tender. Sample restricted to the 329 municipalities where at least one donor firm participates in executive branch procurement. Post = 1 for 2017–2020. Donor = 1 if the firm donated to the winning 2012 mayoral candidate in that municipality. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

B Full Heterogeneity Specifications

Tables 13 and 14 report the full progressive specifications for market share across the heterogeneity subsamples and the 2×2 decomposition. The pattern of results is stable across all three specifications within each subsample. Tables 15 and 16 report the corresponding win rate falsification — coefficients are null throughout.

Table 13: Heterogeneity: Market Share — Progressive Specifications

	Full			Competitive			Non-competitive			New Mayor			Incumbent		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Donor	1.854***	1.825***	1.767***	0.611***	0.622***	0.562***	4.376***	4.301***	4.225***	1.856***	1.832***	1.775***	1.847**	1.796**	1.734**
	(0.323)	(0.288)	(0.305)	(0.152)	(0.116)	(0.119)	(0.688)	(0.666)	(0.697)	(0.375)	(0.332)	(0.354)	(0.625)	(0.577)	(0.592)
Donor × Post	−0.765**	−0.736**	−0.805**	−0.157	−0.145	−0.123	−2.022***	−1.990**	−2.257***	−0.852**	−0.808**	−0.868**	−0.436	−0.469	−0.586
	(0.234)	(0.228)	(0.244)	(0.102)	(0.097)	(0.104)	(0.608)	(0.604)	(0.644)	(0.266)	(0.259)	(0.281)	(0.508)	(0.492)	(0.503)
Log Capital			0.007***			0.008***			0.008**			0.007***			0.005·
			(0.001)			(0.001)			(0.003)			(0.001)			(0.003)
Firm Size			0.056***			0.032***			0.105***			0.058***			0.049***
			(0.003)			(0.002)			(0.006)			(0.003)			(0.007)
Entity Type			−0.000***			−0.000			−0.001***			−0.000***			−0.000**
			(0.000)			(0.000)			(0.000)			(0.000)			(0.000)
Firm Age			0.002***			0.002***			0.004***			0.002***			0.003***
			(0.000)			(0.000)			(0.001)			(0.000)			(0.001)
Municipality FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Sector FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	1,795,452	1,795,452	1,531,151	1,204,322	1,204,322	1,029,407	591,130	591,130	501,741	1,429,341	1,429,341	1,222,680	366,111	366,111	308,471
R^2	0.002	0.074	0.104	0.001	0.088	0.125	0.004	0.052	0.084	0.002	0.073	0.104	0.001	0.076	0.105
Within R^2	—	0.001	0.005	—	0.000	0.004	—	0.002	0.007	—	0.001	0.005	—	0.001	0.004

Notes: Unit of observation is firm-municipality-year. Post = 1 for 2017–2020. Competitive = below-median pre-ban HHI. Incumbent = same mayor elected in 2012 and 2016. Columns 1, 4, 7, 10, 13: no fixed effects. Columns 2, 5, 8, 11, 14: municipality + year FE. Columns 3, 6, 9, 12, 15: municipality + year + sector FE with firm-level controls. Sector FE are two-digit CNAE dummies. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 14: 2×2 Splits: Market Share — Progressive Specifications

	Competitive + New Mayor			Competitive + Incumbent			Non-competitive + New Mayor			Non-competitive + Incumbent		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Donor	0.581**	0.598***	0.531***	0.707**	0.691***	0.642**	4.011***	3.972***	3.909***	7.018***	6.660**	6.301**
	(0.189)	(0.140)	(0.143)	(0.223)	(0.187)	(0.196)	(0.705)	(0.682)	(0.721)	(2.077)	(2.091)	(2.101)
Donor × Post	−0.240*	−0.222*	−0.194·	0.102	0.093	0.083	−1.856**	−1.809**	−2.050**	−3.472·	−3.474·	−3.710·
	(0.110)	(0.105)	(0.110)	(0.268)	(0.252)	(0.260)	(0.623)	(0.619)	(0.670)	(1.939)	(1.959)	(1.936)
Log Capital			0.008***			0.007**			0.009**			0.004
			(0.001)			(0.002)			(0.003)			(0.008)
Firm Size			0.033***			0.027***			0.104***			0.111***
			(0.002)			(0.005)			(0.006)			(0.016)
Entity Type			−0.000*			0.000			−0.000**			−0.001***
			(0.000)			(0.000)			(0.000)			(0.000)
Firm Age			0.001***			0.002***			0.004***			0.005**
			(0.000)			(0.001)			(0.001)			(0.002)
Municipality FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Sector FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	934,481	934,481	803,384	269,841	269,841	226,023	494,860	494,860	419,293	96,270	96,270	82,445
R^2	0.001	0.082	0.121	0.001	0.109	0.140	0.004	0.054	0.086	0.004	0.046	0.078
Within R^2	—	0.000	0.004	—	0.001	0.004	—	0.002	0.007	—	0.003	0.009

Notes: Unit of observation is firm-municipality-year. Post = 1 for 2017–2020. Competitive = below-median pre-ban HHI. Incumbent = same mayor elected in 2012 and 2016. Columns 1, 4, 7, 10: no fixed effects. Columns 2, 5, 8, 11: municipality + year FE. Columns 3, 6, 9, 12: municipality + year + sector FE with firm-level controls. Sector FE are two-digit CNAE dummies. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 15: Heterogeneity: Win Rate (Falsification) — Progressive Specifications

	Full			Competitive			Non-competitive			New Mayor			Incumbent		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Donor	0.076$^{\cdot}$	0.063$^{\cdot}$	0.040	0.062$^{\cdot}$	0.048	0.017	0.122***	0.127***	0.115***	0.059	0.044	0.020	0.132***	0.134***	0.114***
	(0.039)	(0.035)	(0.029)	(0.049)	(0.044)	(0.033)	(0.026)	(0.023)	(0.022)	(0.047)	(0.044)	(0.033)	(0.041)	(0.038)	(0.016)
Donor × Post	−0.015	−0.012	−0.010	−0.007	−0.006	−0.002	−0.044*	−0.044*	−0.042*	0.000	0.001	0.000	−0.047$^{\cdot}$	−0.049$^{\cdot}$	−0.047$^{\cdot}$
	(0.012)	(0.012)	(0.012)	(0.014)	(0.014)	(0.014)	(0.022)	(0.021)	(0.021)	(0.013)	(0.013)	(0.013)	(0.028)	(0.028)	(0.028)
Log Capital			−0.010***			−0.008***			−0.014***			−0.010***			−0.007**
			(0.001)			(0.001)			(0.001)			(0.001)			(0.003)
Firm Size			0.012***			0.010***			0.016***			0.012***			0.010***
			(0.001)			(0.001)			(0.001)			(0.001)			(0.001)
Entity Type			0.000			0.000			0.000			0.000			0.000
			(0.000)			(0.000)			(0.000)			(0.000)			(0.000)
Firm Age			0.002***			0.002***			0.002***			0.002***			0.002***
			(0.000)			(0.000)			(0.000)			(0.000)			(0.000)
Municipality FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Sector FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	2,443,491	2,443,491	2,443,491	1,721,580	1,721,580	1,721,580	721,908	721,908	721,908	1,876,058	1,876,058	1,876,058	567,433	567,433	567,433
R^2	0.001	0.188	0.205	0.001	0.188	0.204	0.002	0.193	0.209	0.001	0.172	0.188	0.002	0.222	0.237
Within R^2	—	0.005	0.005	—	0.004	0.004	—	0.007	0.007	—	0.005	0.005	—	0.005	0.005

Notes: Unit of observation is bidder (participant-level). Win rate is binary, equal to 1 if the firm won the tender. Competitive = below-median pre-ban HHI. Incumbent = same mayor elected in 2012 and 2016. Columns 1, 4, 7, 10, 13: no fixed effects. Columns 2, 5, 8, 11, 14: municipality + year FE. Columns 3, 6, 9, 12, 15: municipality + year + sector FE with firm-level controls. Sector FE are two-digit CNAE dummies. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 16: 2×2 Splits: Win Rate (Falsification) — Progressive Specifications

	Competitive + New Mayor			Competitive + Incumbent			Non-competitive + New Mayor			Non-competitive + Incumbent		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Donor	0.032	0.007	−0.013	0.228**	0.088***	0.105***	0.156***	0.122***	0.109***	0.195***	0.175***	0.168***
	(0.072)	(0.054)	(0.036)	(0.080)	(0.015)	(0.018)	(0.018)	(0.024)	(0.024)	(0.034)	(0.045)	(0.048)
Donor × Post	0.005	0.002	0.014	−0.065	−0.041	−0.046	0.016	−0.033	−0.042·	0.024	−0.034	−0.041
	(0.028)	(0.024)	(0.018)	(0.052)	(0.041)	(0.031)	(0.018)	(0.021)	(0.023)	(0.033)	(0.041)	(0.041)
Log Capital			−0.009***			−0.005·			−0.014***			−0.013***
			(0.001)			(0.003)			(0.001)			(0.002)
Firm Size			0.011***			0.009***			0.015***			0.017***
			(0.001)			(0.001)			(0.001)			(0.002)
Entity Type			0.000·			0.000			0.000			−0.000
			(0.000)			(0.000)			(0.000)			(0.000)
Firm Age			0.002***			0.002***			0.002***			0.002***
			(0.000)			(0.000)			(0.000)			(0.000)
Municipality FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Sector FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	1,441,050	1,441,050	1,268,218	516,252	516,252	453,362	698,853	698,853	607,837	130,734	130,734	114,068
R^2	0.000	0.123	0.178	0.002	0.168	0.240	0.001	0.153	0.211	0.001	0.143	0.199
Within R^2	—	0.000	0.004	—	0.000	0.005	—	0.000	0.007	—	0.000	0.007

Notes: Unit of observation is bidder (participant-level). Win rate is binary, equal to 1 if the firm won the tender. Competitive = below-median pre-ban HHI. Incumbent = same mayor elected in 2012 and 2016. Columns 1, 4, 7, 10: no fixed effects. Columns 2, 5, 8, 11: municipality + year FE. Columns 3, 6, 9, 12: municipality + year + sector FE with firm-level controls. Sector FE are two-digit CNAE dummies. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

C Win Rate by Mechanism

Tables 17 and 18 report win rates by procurement modality and political continuity type. Win rates are null across all groups, preserving the intensive-margin falsification throughout the mechanism tests.

Table 17: Win Rate (Falsification) by Procurement Modality

	Non-competitive	Competitive
Donor	0.009	0.047
	(0.008)	(0.035)
Donor × Post	−0.031	−0.003
	(0.026)	(0.012)
Log Capital	0.001**	−0.009***
	(0.000)	(0.002)
Firm Size	0.002***	0.012***
	(0.001)	(0.001)
Entity Type	−0.000	−0.000
	(0.000)	(0.000)
Firm Age	0.000**	0.002***
	(0.000)	(0.000)
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Sector FE	Yes	Yes
Observations	419,207	2,020,020
R^2	0.233	0.222
Within R^2	0.001	0.003

Notes: Unit of observation is bid. Win rate = 1 if the firm won the tender. Each column restricts to tenders of the indicated modality. Post = 1 for 2017–2020. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 18: Win Rate (Falsification) by Political Continuity Type

	Same Mayor	Same Party	Full Turnover
Donor	0.114^{***}	-0.047	0.023
	(0.016)	(0.066)	(0.036)
Donor × Post	$-0.047^{\cdot}$	0.091	-0.005
	(0.028)	(0.068)	(0.013)
Log Capital	-0.007^{**}	-0.013^{***}	-0.010^{***}
	(0.003)	(0.001)	(0.001)
Firm Size	0.010^{***}	0.014^{***}	0.012^{***}
	(0.001)	(0.002)	(0.001)
Entity Type	0.000	0.000^{**}	0.000
	(0.000)	(0.000)	(0.000)
Firm Age	0.002^{***}	0.002^{***}	0.002^{***}
	(0.000)	(0.000)	(0.000)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Observations	567,433	230,189	1,645,868
R^2	0.237	0.185	0.189
Within R^2	0.005	0.006	0.005

Notes: Unit of observation is bid. Win rate = 1 if the firm won the tender. Post = 1 for 2017–2020. Same Mayor = same individual elected in 2012 and 2016. Same Party = different candidate from the same party won in 2016. Full Turnover = candidate from a different party won in 2016. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

D Substitution

Among municipalities where a donor firm held an outsized pre-ban share (`sh_donor`= 1), the Donor × Post coefficient is positive and marginally significant (+0.68pp), while the effect is negative and significant in all other municipalities (−0.83pp). The placebo using loser-candidate donors is null (Table 19). This asymmetry is consistent with a substitution story: in markets already dominated by a single donor firm, the ban reallocates contracts toward that firm's competitors, compressing rather than eliminating favoritism. The result is suggestive but fragile given the small dominant-donor sample (n = 1,963) and should be interpreted with caution.

Table 19: Market Share: Substitution Analysis

	Dominant Donor	Non-dominant	Placebo
Donor	−0.383	1.813***	−0.311
	(0.539)	(0.317)	(0.363)
Donor × Post	0.675·	−0.829**	−0.104
	(0.352)	(0.252)	(0.350)
Log Capital	−0.008	0.007***	0.046
	(0.084)	(0.001)	(0.051)
Firm Size	0.144*	0.056***	0.199·
	(0.061)	(0.003)	(0.105)
Entity Type	0.001	−0.000***	−0.003
	(0.002)	(0.000)	(0.002)
Firm Age	0.025·	0.002***	0.003
	(0.014)	(0.000)	(0.008)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Observations	1,963	1,529,037	1,410
R^2	0.516	0.104	0.476
Within R^2	0.010	0.005	0.005

Notes: Unit of observation is firm-municipality-year. Post = 1 for 2017–2020. Dominant Donor = municipalities where a single donor firm accounted for a disproportionate pre-ban share (`sh_donor`= 1). Non-dominant = all other municipalities with donor firms. Placebo = firms that donated to the losing 2012 candidate. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

E Modality Heterogeneity by Market Structure and Political Continuity

Tables 20 and 21 decompose the modality-level estimates from Section 6.2 by pre-ban HHI and political continuity. Invitation bid estimates are uniformly insignificant across all subsamples, consistent with thin cells and high variance. The in-person auction effect concentrates in non-competitive markets (-3.06pp, $p < 0.01$) and municipalities with new mayors (-1.92pp, $p < 0.01$), mirroring the main heterogeneity pattern.

Table 20: Invitation Bids: Market Share by Market Structure and Political Continuity

	Competitive	Non-competitive	New Mayor	Incumbent
Donor	8.220$^{\cdot}$	2.942$^{\cdot}$	5.551$^{\cdot}$	6.095
	(4.527)	(1.655)	(3.112)	(3.710)
Donor × Post	6.817	0.960	2.016	−4.790
	(8.781)	(6.109)	(6.154)	(3.513)
Log Capital	0.173**	0.131**	0.186***	0.020
	(0.054)	(0.046)	(0.040)	(0.072)
Firm Size	−0.096	−0.042	−0.085	0.000
	(0.088)	(0.064)	(0.059)	(0.114)
Entity Type	0.002	0.006**	0.005**	0.003
	(0.002)	(0.002)	(0.002)	(0.003)
Firm Age	−0.017	−0.019*	−0.020*	−0.007
	(0.011)	(0.009)	(0.008)	(0.016)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
Observations	30,145	44,945	61,447	13,641
R^2	0.189	0.163	0.170	0.197
Within R^2	0.001	0.001	0.001	0.000

Notes: Unit of observation is firm-municipality-year. Sample restricted to invitation bid tenders. Post = 1 for 2017–2020. Competitive = below-median pre-ban HHI. Incumbent = same mayor elected in 2012 and 2016. Sector FE are two-digit CNAE dummies. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 21: In-Person Auctions: Market Share by Market Structure and Political Continuity

	Competitive	Non-competitive	New Mayor	Incumbent
Donor	0.967***	6.139***	3.259***	4.882**
	(0.218)	(1.157)	(0.649)	(1.808)
Donor × Post	−0.235	−3.055**	−1.921**	−1.194
	(0.258)	(1.172)	(0.623)	(1.723)
Log Capital	0.011**	−0.024***	−0.004	−0.012
	(0.004)	(0.007)	(0.004)	(0.010)
Firm Size	0.081***	0.197***	0.129***	0.136***
	(0.006)	(0.014)	(0.007)	(0.021)
Entity Type	−0.000	−0.002***	−0.001***	−0.001
	(0.000)	(0.000)	(0.000)	(0.001)
Firm Age	0.007***	0.015***	0.009***	0.014***
	(0.001)	(0.001)	(0.001)	(0.002)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
Observations	297,842	222,629	435,950	84,520
R^2	0.125	0.110	0.124	0.136
Within R^2	0.009	0.011	0.008	0.011

Notes: Unit of observation is firm-municipality-year. Sample restricted to in-person auction tenders. Post = 1 for 2017–2020. Competitive = below-median pre-ban HHI. Incumbent = same mayor elected in 2012 and 2016. Sector FE are two-digit CNAE dummies. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

F Procurement Red Flags

Table 22 examines whether donor presence predicts tender-level irregularities before and after the ban. Prior to the ban, tenders with a donor firm present show fewer red flags overall — lower single-bidder rates, more bidders, and shorter publication windows — consistent with donors winning well-designed tenders rather than uncontested ones. Post-reform coefficients are largely null, suggesting the ban did not systematically alter procurement design. Table 23 restricts to winning bids and finds that donor winners were more likely to bid close to the budget ceiling and to be repeat winners pre-ban, but neither effect changes significantly post-ban.

Table 22: Tender-Level Red Flags

	RF Index	Single Bidder	N Bidders	Short Window	Quick Homolog
Donor Present	-0.588^{**}	-0.209^{***}	0.970^{***}	-0.134^{*}	-0.098
	(0.212)	(0.049)	(0.234)	(0.062)	(0.072)
Donor Present × Post	-0.101	$-0.058^{\cdot}$	0.782^{*}	0.028	0.038
	(0.113)	(0.034)	(0.309)	(0.037)	(0.037)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	418,926	418,926	418,926	274,733	208,065
R^2	0.223	0.050	0.033	0.233	0.168
Within R^2	0.002	0.003	0.001	0.001	0.001

Notes: Unit of observation is tender. Sample restricted to the 329 municipalities where at least one donor firm participates in executive branch procurement. RF Index = sum of five binary red-flag indicators. Single Bidder = only one firm participated. Short Window = fewer than 5 days between publication and opening. Quick Homolog = awarded within 7 days of opening. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 23: Red Flags Among Winning Bids

	Close to Budget	Repeat Winner	Log Value
Donor	0.127^{***}	0.226^{***}	-0.044
	(0.029)	(0.040)	(0.402)
Donor × Post	-0.061^{*}	-0.043	0.267
	(0.029)	(0.035)	(0.323)
Log Capital	$-0.002^{\cdot}$	0.011^{***}	0.070^{***}
	(0.001)	(0.001)	(0.015)
Firm Size	0.004	-0.010^{***}	0.099^{***}
	(0.003)	(0.002)	(0.018)
Entity Type	0.000	-0.000^{***}	0.001
	(0.000)	(0.000)	(0.000)
Firm Age	0.003^{***}	0.001^{***}	-0.011^{***}
	(0.000)	(0.000)	(0.002)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Observations	555,789	616,951	612,460
R^2	0.215	0.216	0.204
Within R^2	0.006	0.007	0.013

Notes: Unit of observation is winning bid. Sample restricted to the 329 municipalities where at least one donor firm participates in executive branch procurement. Close to Budget = contract value between 90% and 105% of the budget ceiling. Repeat Winner = firm won 3 or more contracts in the same municipality-year. Sector FE are two-digit CNAE dummies. Standard errors clustered by municipality in parentheses. $^{\cdot}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

G Non-Donor Market Share by State

Figures 9–12 show mean non-donor market share across municipalities in each state before and after the ban. The maps are nearly identical pre and post-ban, confirming that the ban's effect operated through donor firm withdrawal rather than non-donor expansion.

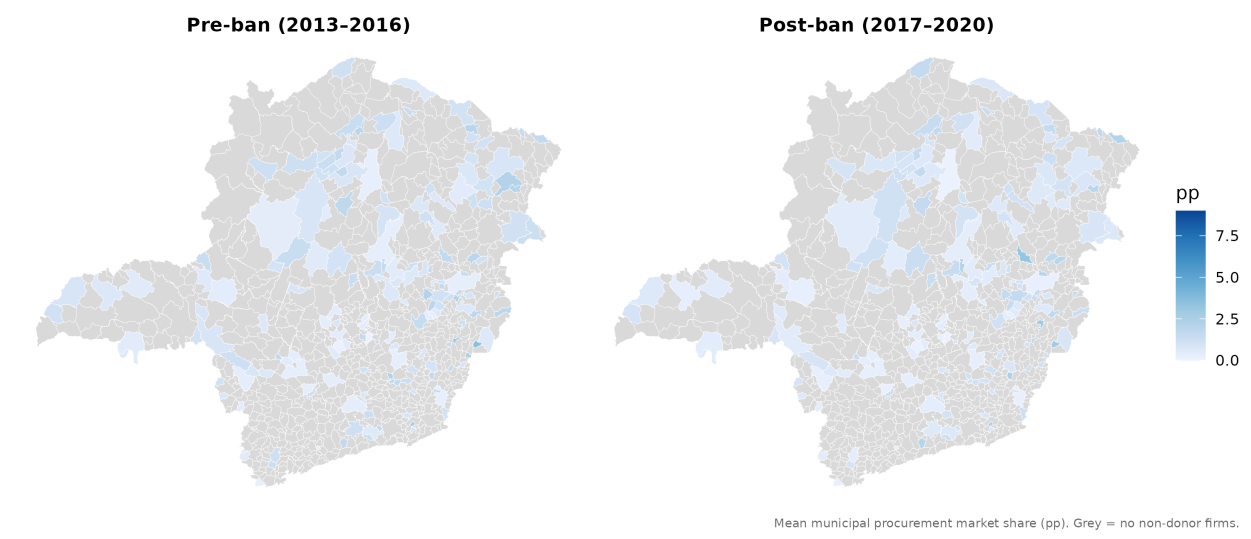


Figure 9: Mean non-donor market share (pp) across municipalities in Minas Gerais, before and after the corporate donation ban. Grey = no non-donor firm activity.

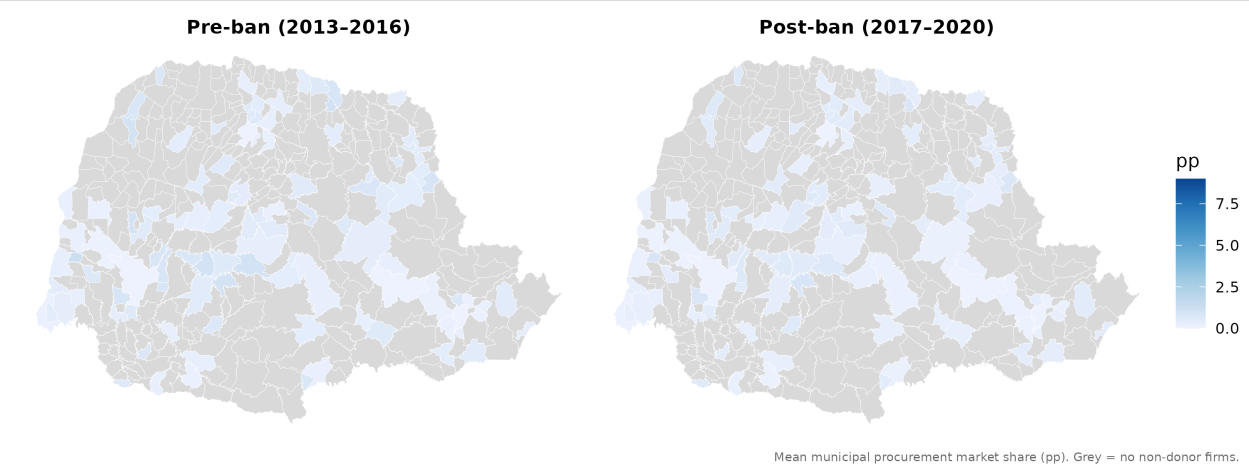


Figure 10: Mean non-donor market share (pp) across municipalities in Paraná, before and after the corporate donation ban. Grey = no non-donor firm activity.

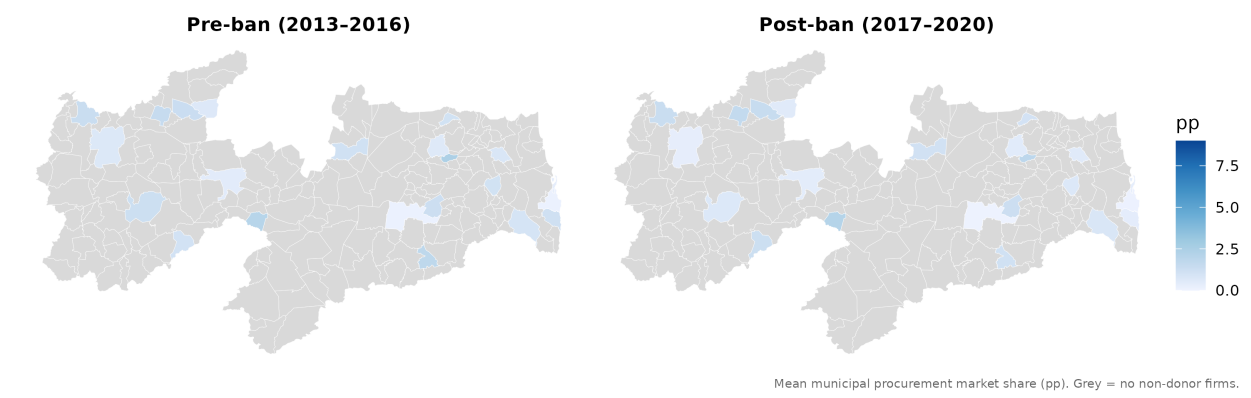


Figure 11: Mean non-donor market share (pp) across municipalities in Paraíba, before and after the corporate donation ban. Grey = no non-donor firm activity.

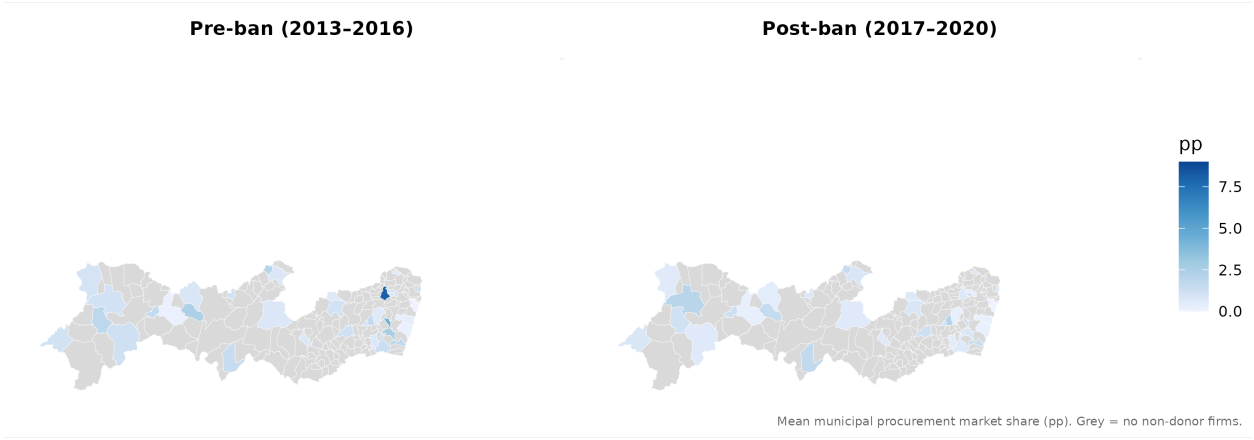


Figure 12: Mean non-donor market share (pp) across municipalities in Pernambuco, before and after the corporate donation ban. Grey = no non-donor firm activity.

H Data Construction

Linking the five data sources described in Section 4.1 requires several non-trivial steps.

Municipality quality filter. Reporting quality in MiDES varies across municipalities. Some record only winning bidders and omit losers entirely. Including these would artificially inflate win rates and bias any analysis using participation as an outcome. I flag each municipality by the share of competitive tenders that record at least one losing bidder and drop municipalities where this share falls below five percent. This retains 1,523 municipalities across the four states.

Firm matching. Brazilian CNPJs embed an establishment suffix in the final digits, so the same legal entity can appear under multiple identifiers across branches and datasets. I standardize firm identities using the first eight digits of the CNPJ, which identify the legal entity regardless of establishment. I apply this throughout: matching donors to procurement participants and linking firms to Receita Federal records.

Treatment assignment. I define treatment using 2012 corporate donations to the winning mayoral candidate in each municipality. I match TSE donor identifiers to MiDES procurement participants via the eight-digit firm code. I separately construct a loser-donor control group — firms that donated to a losing 2012 candidate in the same municipality — for robustness.

Shareholder matching. Brazil's 2015 ban prohibited corporate donations but left individual donations unrestricted. To test whether donor firms substituted through shareholder giving in the 2016 cycle, I match individual donors from TSE records to firm shareholders in the Receita Federal registry. Both datasets partially mask tax identifiers for privacy: TSE masks donor identifiers after the sixth digit and Receita Federal masks shareholder identifiers after the sixth digit from the other end, leaving six digits in common. I match on

those shared digits plus exact full-name agreement. I validate this procedure with a placebo test using shuffled identifiers, which produces near-zero matches against the several hundred exact matches in the real data. I also drop the twenty most common first names to guard against false positives from name frequency.

Branch and city hall classification. Donor relationships are with the executive branch — the mayor's office — not the legislature or judiciary. I classify each procurement record by issuing unit using string matching on the names of the procuring agency, separating executive, legislative, and judicial procurement. Within the executive, I further flag tenders issued directly by the mayor's office to isolate the procurement unit most directly under mayoral control. Legislative procurement serves as a placebo throughout.

Market structure. I compute the Herfindahl-Hirschman Index (HHI) for each municipality using the pre-ban period (2013–2016), weighting by contract value among winning firms in executive procurement. The HHI is a standard measure of market concentration: it equals one when a single firm wins all contracts and approaches zero as contracts spread evenly across many firms. I split municipalities at the median into competitive and non-competitive markets. This variable is fixed at the municipality level and does not vary post-ban, avoiding any endogeneity from post-treatment market restructuring.

Incumbency. I identify whether the 2016 mayoral winner is the same person as the 2012 winner by matching tax identifiers across TSE electoral records, linked to national municipality codes via a crosswalk. I classify municipalities into three continuity types: same mayor reelected, same party with a different mayor, and full turnover. The main heterogeneity split uses same-mayor reelection as the incumbent indicator.