Supporting Information

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

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1 Fieldwork Interviews

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

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

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

The tender is written before it is published

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

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

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

Why local governments

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

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

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

Donations

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

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

A senior official at the Office of the Comptroller General, who had audited state procurement and investigated bid-rigging cartels, described the exchange in one sentence: electoral competition "involves spending a great deal of money on campaigns, and it is the winners who are granted these [procurement] favors." A political scientist offered the longer genealogy, calling the Car Wash investigation the daughter of the earlier *Mensalão* scandal, both of them symptoms of a campaign finance model that was, in his word, rotten. Not all interviewees expected the ban to work. One anti-corruption official said that after 2015 "one tendency was to push things into illegality," with "unregistered payments that keep circulating" through individuals and intermediaries rather than disappearing. The shareholder-substitution analysis in Section 8 below is an attempt to test exactly that expectation, and finds it true of a few dozen firms and false of the rest.

Interviews

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

Composition of the sample

Table A1: Interviews by institution

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

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

Table A2: Interview roster

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

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

Interview questionnaire

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

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

Section I. Introduction.

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

Section II. Public procurement.

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

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

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

Section III. Bid rigging and political corruption.

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

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

Section IV. Corruption.

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

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

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

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

7. How do you assess the effectiveness of anti-corruption measures and reforms implemented within the public procurement system? What indicators or metrics are used to evaluate progress in reducing corruption risks?

8. What measures can oversight institutions recommend or advocate for to enhance transparency and integrity in public procurement? How do you engage with policymakers and other relevant stakeholders to promote these measures?

9. How can technology and data analytics be leveraged within oversight institutions to strengthen their ability to detect and prevent corruption in public procurement? Are there any innovative approaches or tools being utilized in this regard?

10. Based on your expertise and experience, what are the key areas or aspects within the public procurement process that require further attention and improvement to effectively combat corruption? What policy or procedural changes would you prioritize?

Section VI. Close.

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

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

Public administration and corruption.

1. Based on your research, what are the key characteristics of the public administration system in Brazil that can either contribute to or mitigate corruption in public procurement?

2. Are there specific administrative structures, processes, or practices that are more susceptible to corruption within the public procurement system? How can these be improved to minimize corruption risks?

3. In your view, what role does organizational culture play in shaping the propensity for corruption in public procurement? How can a culture of integrity and ethics be fostered within public administration?

4. Are there any successful examples or case studies where specific reforms or changes in the public administration system have led to a significant reduction in corruption in public procurement? What lessons can be learned from these examples?

5. How can the principles of transparency, accountability, and merit-based decision-making be better incorporated into the public administration of Brazil to enhance the integrity of public procurement processes?

6. Are there any specific challenges or obstacles within the public administration system that hinder effective anti-corruption measures in public procurement? How can these challenges be overcome?

7. In your opinion, what are the key competencies and skills that public administration officials should possess to effectively combat corruption in public procurement? How can these be developed or enhanced?

8. How can the use of technology, such as e-procurement systems or digital platforms, be leveraged to improve the efficiency, transparency, and accountability of public procurement processes within the public administration of Brazil?

9. What are the potential benefits of promoting interagency cooperation and coordination within the public administration system to combat corruption in public procurement? Are there any successful examples of such collaboration?

10. Based on your expertise, what are the most crucial reforms or changes that need to be prioritized within the public administration of Brazil to create a more resilient and corruption-resistant public procurement system?

Public procurement.

1. From your research and analysis, what are the key challenges and vulnerabilities in public procurement processes that can lead to corruption?

2. In your opinion, what are the main factors that contribute to bid rigging and corruption in public procurement? Are there any specific institutional or systemic issues that facilitate such practices?

3. Based on your expertise, what are some effective strategies or reforms that can be implemented to enhance transparency and integrity in public procurement?

4. Can you provide examples of successful anti-corruption measures implemented in other countries or regions that have significantly reduced corruption risks in public procurement? How applicable are these measures to the Brazilian context?

5. What role does academic research play in understanding and addressing corruption in public procurement? Can you share any notable studies or findings that shed light on this issue?

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

2 Sample and Data

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

Data sources

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

Table A3: Administrative sources

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

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

Extending the procurement record past 2020

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

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

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

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

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

Which municipalities enter the panel

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

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

loser, those that record one in fewer than 5% of competitive tenders, and those that record them routinely. The panel keeps the third group. This yields the 381 municipalities used throughout the paper—177 in *Minas Gerais*, 121 in *Paraná*, 48 in *Pernambuco* and 35 in *Paraíba*. The rule is a property of the reporting, not of the outcome: it is computed before any donor variable exists and before any tender value is examined, and it conditions on how a municipality publishes rather than on what it bought or from whom. Table A9 profiles the panel municipalities with and without donor firms.

Which purchases count as the mayor's

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

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

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

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

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

Identifying political connection

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

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

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

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

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

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

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

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

Three further constructions follow from the same matching machinery.

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

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

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

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

Firm attributes

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

Campaign finance across three release formats

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

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

Elections

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

The item panel

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

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

That retention rule changes the sample as well as the specification, and the two effects have to be separated. The item tables therefore report a bridge row: the paper's own specification, unchanged, estimated on the product subsample. If the estimate there sits near the full-sample figure, the movement between the two ladders is the product controls doing work; if it sits near the product rows, the movement is the change of sample, and the product rows are answering a different question about a different set of goods. Reporting the ladder

without that row would leave the two indistinguishable.

Retrieval at scale

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

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

What is checked, and what is reported

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

Table A4: Procurement activity per municipality-year

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

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

Table A5: Non-competitive procurement and political staffing

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

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

Table A6: Coverage and missingness

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

Notes: Share of bidder–tender records for which each field is missing, across the 381 municipalities of the analysis panel, 2014–2023. Registered capital and contract value are the only fields missing on more than 1% of rows, and neither is used to define treatment.

Table A7: Panel balance by year

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

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

Table A8: Descriptive statistics by donor status

	Mean	SD	Mean	SD
	Winner-side		Other	
Market share	2.093	4.030	0.434	1.400
Number of bids	2.361	3.972	1.730	3.250
Number of wins	2.019	3.080	1.181	2.108
Log registered capital	12.099	2.524	12.014	2.558
Firm age (years)	21.082	12.123	12.428	11.449
Firm size class	2.939	1.647	2.662	1.594

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

Table A9: Municipality profile, with and without donor firms

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

Notes: Municipality characteristics by whether the municipality has at least one donor firm. Differences are descriptive and are not adjusted for anything.

Table A10: Overview of the data

Statistic	Value
Sample coverage	
Municipalities	381
Minas Gerais	177
Paraná	121
Pernambuco	48
Paraíba	35
Unique bidding firms	152,245
Unique tenders	549,676
Bidder–tender observations	1,504,671
Pre-ban (2014–2016)	398,303
Post-ban (2017–2023)	1,106,368
Firm–municipality–year observations	868,528
Years	2014–2023
Unique firms by donor status	
Donor firms (donated to the winning 2012 mayor)	710
Non-donor firms (all other bidding firms)	151,869
Donor in one municipality, non-donor in another	334
Loser-side donor firms (hedgers included)	302
Substitution donor firms (2016 cycle)	25
Substitution donor firms (2020 cycle)	21
Bidder–tender observations by donor status	
Donor firm observations	6,635
Non-donor firm observations	1,498,036
Donor share of observations (%)	0.44
Municipality characteristics	
Concentrated municipalities (above-median HHI)	190
Dispersed municipalities (below-median HHI)	191
Incumbent-mayor municipalities	92
New-mayor municipalities	289

Table A10, continued

Statistic	Value
Pre-ban outcomes by donor status	
Mean market share per firm, donors (%)	2.582
Mean market share per firm, non-donors (%)	0.538
Combined donor share of a municipality's procurement (%)	2.661
Mean win rate, donor firms	0.863
Mean win rate, non-donor firms	0.681
Tender characteristics	
Competitive tenders	348,714
Discretionary tenders	200,069
Other or unclassified procedure	893
Average number of bidders per tender	2.74
Median number of bidders per tender	1
25th percentile of bidders per tender	1
75th percentile of bidders per tender	3
Maximum number of bidders per tender	285
Single-bidder tenders	332,385

Notes: Unit of observation is the bidder–tender pair unless stated otherwise. Donor firms are those that contributed to the winning 2012 mayoral candidate in that municipality. Because donor status is municipality-specific, 334 firms are donors in one municipality and non-donors in another, so the two firm counts sum to more than the total. Substitution donors are 2012 donor firms whose registered shareholders donated personally to the winning mayoral candidate in the cycle named. Concentration is the pre-ban Herfindahl index of awarded value, computed over non-donor firms only and split at the median.

3 Timing, Inference, and Sensitivity

The event study behind Figure 8 of the paper, the Rambachan–Roth relative-magnitudes bound in closed form, the synthetic difference-in-differences estimate, inference procedures that do not rely on cluster-robust asymptotics, and the extended specification ladder.

Table A11: Event-study coefficients

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

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

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

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

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

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

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

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

Table A14: Synthetic difference-in-differences

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

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

Table A15: Bootstrap and randomization inference

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

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

Table A16: Specification, extended

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

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

Table A17: Alternative samples

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

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

4 Placebos and Falsification

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

Table A18: Legislative-branch placebo

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

Notes: Procurement run by the municipal chamber, which the mayor neither staffs nor administers, against executive procurement in the same municipalities. A null on the legislative side is consistent with the placebo passing, but it is not decisive: the legislative market-share estimate is -1.99 with a 95% interval from -7.4 to $+3.4$, which does not exclude the executive effect of -0.700. Both market-share columns are shares of awarded value, so the two branches are measured on the same quantity. The legislative panel is small: it contains 122 donor bidder-rows across 119 tenders in 50 municipalities, which is why its standard errors are an order of magnitude larger than the executive ones. A pooled specification interacting donor status with branch returns -0.075 (SE 0.121) on the triple interaction, so the two branches cannot be formally distinguished, and this table is reported as consistent with a mayor-specific mechanism rather than as a test of one. Standard errors clustered by municipality in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A19: Pseudo-treatment dates

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

Notes: The panel is restricted to the pre-ban years and the ban is assigned to an interior year, so that the actual reform falls outside the sample entirely and any effect the design finds must be a pre-existing trend. With three pre-ban years only one interior date is available. The final column repeats the estimate at the true date for comparison. Standard errors clustered by municipality in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A20: Close 2012 mayoral elections

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

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

Table A21: Loser-side donors, pseudo-treated

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

Notes: Firms that donated to a *losing* 2012 mayoral candidate, pseudo-treated as though they had backed the winner. These firms selected a candidate, paid, and bore the risk, and differ from the treated group only in the outcome of an election they did not control, so a null here is what separates the effect of access from the effect of donating. Standard errors clustered by municipality in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

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

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

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

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

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

Notes: Sector-by-year effects testing whether the 2015–16 contraction rather than the ban accounts for the decline. Sectors are two-digit CNAE. Firm–municipality–year panel, 381 municipalities across *Minas Gerais*, *Paraná*, *Pernambuco* and *Paraíba*, 2014–2023, with donor status fixed by the 2012 mayoral election. Standard errors clustered by municipality in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A24: Alternative outcomes

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

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

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

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

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

5 Mechanism Tests

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

Table A26: Where the withdrawal occurred

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

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

Table A27: Procurement unit: city hall versus other executive units

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

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

Table A28: Withdrawal by procedure

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

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

Table A29: Win rate by procurement modality

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

Notes: Win rate by procurement modality, on bidder–tender records. Bidder–tender records for the 381 municipalities of the analysis panel, 2014–2023, with donor status fixed by the 2012 mayoral election. Standard errors clustered by municipality in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A30: Competition in the tenders donor firms enter

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

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

Table A31: Tests of the favoritism mechanism

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

Notes: Tests of the arrangement account. Panel A reports the post-ban change in the donor win rate holding the contest fixed; Panel B the logged value of tenders entered and won; Panel C the logged published reference quote regressed on donor status, which asks whether the benchmark itself was written high; Panel D Donor × Post separately for connected firms grouped by their pre-ban bidding record and by firm characteristics fixed before the ban. Percentage points are not comparable across the Panel D groups, which held very different pre-ban positions, so each decline is also reported against its own group's pre-ban mean. Standard errors clustered by municipality in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A32: Award margin versus access margin

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

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

Table A33: Withdrawal by tender type

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

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

6 Market Structure

The paper's moderator, pre-ban market concentration, cut further, together with market-level outcomes that ask whether whole procurement markets changed rather than whether donor firms did.

Table A34: Effect by HHI tercile

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

Notes: The effect by tercile of the pre-ban Herfindahl index of awarded value, computed over non-donor firms only so that a municipality cannot count as concentrated merely because its own donor firm is large there. Reported alongside the median split used in the paper, since the conditionality should not depend on where the line falls. The equality test is estimated in percentage points; measured in proportion to each tercile's own pre-ban donor mean the ordering survives but is no longer significant ($p = .231$). Standard errors clustered by municipality in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A35: Market-level competition

	Estimate	SE	p
Mean bidders per tender	-0.0609	0.0458	0.1843
Share of single-bidder tenders	0.0077	0.0069	0.2685
Donor share of bidder pool		0.0064	

Notes: Market-level outcomes: the average number of bidders per tender and the share of tenders drawing a single firm. Reported because the tests were run and returned nothing. Donor firms are under 1% of the bidder pool, so their withdrawal cannot move a municipality-wide mean by an amount this design can resolve, and the null is therefore uninformative about the mechanism rather than evidence against it. Standard errors clustered by municipality in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

7 Item-Level Prices

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

Table A36: Item-level sample, Minas Gerais

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

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

Table A37: Quote integrity

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

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

Table A38: Is the reference quote written higher?

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

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

Table A39: Lot size

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

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

Table A40: Item-level withdrawal

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

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

Table A41: Price premium by year

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

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

Table A42: Price premium, robustness

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

Notes: The price premium under alternative trimming, product-cell thresholds and fixed-effect structures. Items purchased by the 177 *Minas Gerais* municipalities of the analysis panel, 2014–2024, carrying both an award price and a published reference quote. Standard errors clustered by municipality in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A43: Exposure and sector splits

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

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

Table A44: The cost of the premium

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

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

8 Alternative Explanations

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

Channel substitution

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

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

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

Table A45: Market share by shareholder substitution status

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

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

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

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

Firm failure

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

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

Table A46: Exit

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

Notes: Three exit margins: whether a firm stops bidding anywhere in the sample, whether it stops bidding in the municipality where it was connected, and whether its tax registration is closed. Firm–municipality–year panel, 381 municipalities across *Minas Gerais*, *Paraná*, *Pernambuco* and *Paraíba*, 2014–2023, with donor status fixed by the 2012 mayoral election. Standard errors clustered by municipality in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Mayoral turnover

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

Table A47: Market share by mayoral turnover

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

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

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