# Pay-to-Play: Political Contributions and Long-term Distortions in Public Procurement*

Saad Gulzar,† Juan Felipe Ladino,‡ Juan Felipe Riaño§

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*Abstract:* How do political donations distort the allocation of public procurement contracts? By linking the universe of public contractors and the mandatory report of political donations by individuals and firms in Colombia, we demonstrate long-term distortions in the contract allocation process attributable to political donations: (1) Donors to political campaigns are more likely to be awarded public contracts, often of greater value, than non donors. (2) These donors tend to continue receiving public contracts even after losing direct political connections in subsequent election cycles, suggesting that their donations grant them extended access beyond immediate reciprocation. (3) Pointing to potential inefficiencies, we find that contracts secured by donors frequently exceed budgetary limits and tend to benefit riskier firms and inexperienced individuals. Building on these results, we conduct a Randomized Control Trial (RCT) in collaboration with investigative journalists, NGOs, and the Office of the Inspector General in Colombia to study how new mayors respond to deterrence messages about contract scrutiny for their donors. Using this intervention and our data, we estimate a structural model that captures selection in donations and public sector entry, helping rationalize the reduced-form results. We then use this model to evaluate counterfactual policies aimed at reducing the influence of political contributions on public procurement outcomes.

**Keywords:** Public Procurement Allocation, Campaign Contributions, Donor Selection.

**JEL codes**: D72, H57, P00

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†Department of Politics and SPIA, Princeton University. Email: gulzar@princeton.edu

‡Department of Economics, Stockholm University. Email: juan.felipe.ladino@su.se

§Department of Economics, Georgetown University. Email: juan.felipe.riano@georgetown.edu

# 1 Introduction

Money plays a central role in electoral politics, acting both as a driver of democratic competition and a potential source of political and economic distortions. A growing body of research, particularly in advanced industrialized contexts, demonstrates that lobbying and campaign contributions shape electoral outcomes and public policy.[1] However, less is known about how campaign contributions distort the allocation of public resources—particularly through mechanisms of selection and persistence in public procurement, a sector that accounts for approximately 25% of total government expenditures globally (World Bank, 2020; OECD, 2019).

This paper addresses this gap in the literature by examining whether political campaign donations serve as implicit exchanges for preferential treatment in public procurement, and how selection and persistence factor into this process.[2] To examine this question, we employ a comprehensive empirical approach that integrates three empirical strategies: a reduced-form analysis, a nationwide randomized controlled trial, and a structural model. Leveraging a novel dataset that links firm- and individual-level donation records to the universe of over four million public procurement contracts across three mayoral election cycles in Colombia, this framework allows us to isolate the causal effect of donations on procurement outcomes while also accounting for self-selection into political donations and procurement participation.

Our approach helps us revisit and address two persistent puzzles in the money-in-politics literature. First, hidden entry costs and procurement frictions may explain why, despite the high perceived return on investment from donations, not all individuals and firms choose to contribute to political campaigns. Second, variations in fringe benefits and operating costs among donors reveal the presence of what we term a *"pay-to-play"* mechanism, wherein contributions are made to sustain privileged access to the public procurement process even long after political connections fade.

To motivate our empirical analysis, we begin by documenting four key data patterns that illustrate the relationship between political donations and public procurement allocations in Colombia. First, we find that donor firms and individuals systematically differ from non-donors across multiple observable characteristics, exhibiting weaker financial indicators, stronger bureaucratic connections, and less labor market experience. Second, political contributions are closely linked to a higher likelihood of entering the public procurement system as elections approach, suggesting strategic

[1] See Bombardini and Trebbi (2020) and Ansolabehere, De Figueiredo and Snyder Jr (2003), for reviews.

[2] The effect of political donations on allocation efficiency is ambiguous. Donations may enable favoritism, benefiting politically connected firms while reducing competition. Conversely, they may signal firm quality or alignment, improving contractor selection and project execution. Distinguishing these channels is essential to understanding their impact on resource allocation and overall welfare.

incentives that intensify near election periods. Third, the composition of donors shifts significantly across electoral cycles, with minimal continuity between successive elections. Finally, contract values for both donors and non-donors exhibit bunching just below the threshold for discretionary (rather than open) tender allocation, a pattern that is especially pronounced among donors. These empirical patterns inform our subsequent analyses, which explicitly account for selection into either donating, entry into the procurement system, or both, allowing us to disentangle the mechanisms through which political donations influence contract allocation.

The second part of the paper continues the investigation by using our unique datasets to confirm and expand on reduced-form findings from previous work.[3] First, we employ a stacked difference-in-differences design, comparing the evolution of public contract outcomes for donors and non-donors before and after the 2015 and 2019 mayoral elections. By focusing on ever contractors and expanding the pure control group to the universe of individuals within municipalities (i.e., by holding constant the selection into the public procurement system), we show that individuals and firms who donate to political campaigns are more likely to receive public procurement contracts than non-donors, and that the contracts they receive tend to be of higher value. Second, we provide evidence of the efficiency costs of this preferential treatment. Contracts awarded to donors exhibit higher cost overruns, and seem to be disproportionately awarded through discretionary channels, allowing donors to extract rents from their political connections. Finally, we extend these results by documenting a new pattern of procurement allocation. We find that donors tend to continue receiving public contracts even after *losing* their direct political connections, indicating that their donations grant them extended access beyond immediate reciprocation, a *pay-to-play* mechanism. These results combined with the motivating facts suggest that political donations provide an important pathway for long-term access to public contracting, even for those who may not be the most qualified contractors *ex-ante* nor the most efficient *ex-post*.

Building on these findings, the third part of the paper aims to evaluate how different forms of enhanced oversight by various authorities could help curb the patterns of favoritism and inefficiency previously documented within municipalities that have donors. To achieve this, we carry out a nationwide randomized controlled trial (RCT) in collaboration with anti-corruption NGOs, investigative journalists, and Colombia's Office of the Inspector General. Launched in May 2024, the intervention informs newly elected mayors (sworn in in late 2023) about existing legislation and the legal risks associated with awarding contracts to their donors. By analyzing within-donor and non-donor variation over time (i.e., by holding constant the selection into donation), we find

[3] See, for instance, Ansolabehere, De Figueiredo and Snyder Jr (2003); Ovtchinnikov and Pantaleoni (2012); IDEA (2014); Powell and Grimmer (2016); Kalla and Broockman (2016); Titl and Geys (2019); Kim, Stuckatz and Wolters (2020); Bombardini and Trebbi (2020); Ruiz (2021); Gulzar, Rueda and Ruiz (2022); Teso (2023).

that the intervention systematically reduces kickbacks, depending on the identity of the oversight agent and the specific information provided about donors. Our randomization scheme allows us to causally show that when the government oversight agency, the Inspector General's office, is informed *and* the list of donors is attached, there is a significant and sustained reduction in favorable treatment toward donors. The results highlight that deterrence messages only work when the threat of punishment is *credible* and *actionable*, as treatments involving journalists or only NGO oversight authority without a donor list did not produce similar effects.

Since each methodological approach presented thus far focuses on only one margin of selection at a time, and given the correlation between donation and entry decisions, we propose a dynamic discrete choice model that integrates both donation and entry decisions. This structural model leverages government demand shifters from the RCT and the richness of our administrative data to disentangle the extent to which *pay-to-play* dynamics are driven by entry barriers, operational costs, and market forces. By analyzing these dynamics and the rest of our reduced-form patterns, the model allows us to estimate both entry costs into the public procurement system and the sunk costs of political donations that rationalize our results. Ultimately, this model seeks to (1) explain the emergence of the *pay-to-play* mechanism, (2) clarify why, despite substantial returns from donations, not all firms and individuals engage in this practice, and (3) evaluate counterfactual policies aimed at further reducing the influence of political donations on public procurement outcomes in Colombia.

Our paper contributes to multiple strands of the political economy literature. Our first contribution is to the growing literature on the role of money in politics that shows how politicians in power reward their donors through public contracts employing predominantly regression discontinuity methods (Boas, Hidalgo and Richardson, 2014; Titl and Geys, 2019; Ruiz, 2021). Complementing this work, our contractor level difference-in-differences analysis allows us to first focus on the decision to donate or not and the consequences this carries for kickbacks, thereby investigating both the *extensive* and *intensive* margin effects of political donations on public procurement outcomes. Second, our analysis deepens connections with the discretion in public procurement literature (Palguta and Pertold, 2017; Coviello, Guglielmo and Spagnolo, 2018; Szucs, 2023), revealing that donors disproportionately benefit from longer and more complex *discretionary* contracts, facilitating the extraction of private rents (Gulzar, Rueda and Ruiz, 2022). Our key contribution to this literature that estimates reduced form effects is to investigate and present evidence for an under-examined "*pay-to-play*" mechanism that postulates that an important reason to donate is to get ongoing access to the public procurement system regardless of electoral outcomes of the supported candidates.

Second, our paper contributes to the literature on accountability and monitoring of public sector institutions and politicians (Acharya, Lipnowski and Ramos, 2024; Duggan and Martinelli, 2017).

Previous studies like those by Ferraz and Finan (2008); Avis, Ferraz and Finan (2018) have shown the effect of increasing auditing risk in deterring illegal behavior from politicians. The authors demonstrate that random audits in Brazilian municipalities decrease corruption. We extend this strand of literature by implementing an RCT to prevent corruption by breaking the influence of political donations, which can be a source of illegal behaviors. This RCT not only allows us to test how the identity of the oversight principal and the nature of the disclosed information about donors affect compliance with current laws but also how these factors influence accountability and efficient allocation of public funds. To the best of our knowledge, we are the first to conduct a study incorporating these elements, providing new insights into the role of oversight mechanisms in shaping the allocation of public procurement contracts.

Finally, our paper also contributes to the theoretical and empirical literature on the distortions and misallocation of public resources stemming from political connections (Auriol, Straub and Flochel, 2016; Mironov and Zhuravskaya, 2016; Huneeus and Kim, 2018; Schoenherr, 2019; Baltrunaite et al., 2021; Brugués, Brugués and Giambra, 2024). Specifically, we develop and estimate a structural model based on administrative data and the RCT intervention, which not only accounts for the "*pay-to-play*" mechanism but also rationalizes the rest of our reduced-form results. This model enables us to investigate the economic determinants behind firms' decisions to make political contributions and participate in the public procurement system. By incorporating factors such as differential marginal production costs, the sunk costs of donating, and the endogenous evolution of market structure, our framework provides a tractable, estimable, and exportable approach to analyzing political donations in local elections and competition within discretionary public procurement systems. Crucially, this model allows us to evaluate counterfactual policies aimed at curbing the influence of political donations on public resource allocation, offering valuable insights into future public policy reforms.

The paper proceeds as follows: Section 2 describes the mayoral elections in Colombia and details on public procurement procedures. Section 3 describes the available data used in this paper. Section 4 documents the motivating data facts. Section 5 reports the main empirical strategy and results from the returns to donate to a political campaign. It also analyzes the changes in connections to politicians and explores the efficiency consequences of donating. Section 6 presents the design for the randomized controlled trial to decrease kickbacks in public procurement, while Section 7 presents the structural model and the details for its estimation. Finally, Section 8 provides a preliminary discussion and conclusion.

# 2 Institutional Background

**Electoral organization** Our focus is on mayoral elections in Colombia that occur every four years. Winning candidates cannot be reelected, but can contest in other races or run again after one cycle of elections. As the highest municipal authority, mayors are tasked with executing municipal budgets, which gives them a high capacity to define where and how to allocate public resources, for which usually public procurement is used as the allocation mechanism. Runner-up candidates also obtain a position in the municipal council, granting them access to a budget and other sources of public resources.

Elections are coordinated by the National Electoral Commission, which is an independent agency that administers electoral regulations, including campaign finance. Candidates can finance their campaigns through bank loans, close relatives contributions, and private donations. In practice, about 90% of financing comes from family/relatives' resources and private contributions (MOE, 2019). Yet, private donations are subject to a limit of 10% of the total allowed budget for the municipality.[4] Usually, such limits are imposed to curb the influence of money in politics (IDEA, 2014; Gulzar, Rueda and Ruiz, 2022; Avis et al., 2022) and to prevent private interests from influencing policy. Failing to comply with the regulations might carry legal punishments for the candidate and the donors.

**Public procurement** Public procurement amounted close to 13.3% of the GDP in 2022, representing nearly a third of government spending, illustrating its importance in economic activity and the allocation of public resources (OECD, 2020). The National Agency for Public Procurement is the main office in charge of issuing public procurement regulations and administrating public contracts. As a national-level agency, it also supports smaller jurisdictions, although procurement is not a centralized procedure but an autonomous one for each territory.

Contracts can be awarded to both firms and individuals in different procurement categories, each governed by specific rules and conditions, mainly involving the competitiveness of the selection process. Appendix A.1 describes each procurement category. Mayoral offices enjoy significant discretion for assigning contractors when issuing contracts for bureaucratic or administrative tasks and small-value contracts.

The behavior of the agents interacting in public procurement transactions is officially overseen by

[4]There is no limit on the amount of family resources as long as they do not exceed the spending limit. Candidates can also obtain public financing from the government in the form of money refund whenever the candidate has more than 4% of votes.

the Inspectors General Office, which is the national government agency in-charge of keeping track of potential misbehavior of public servants, including elected officials, public workers, and contractors. The Inspector General Office can intervene and investigate whenever there is a risk of misbehavior, such as the misuse of public resources, and start prosecution processes if required. Thus, this office is responsibel for ensuring procurement procedures performed by municipal governments are efficient and do not violate any law.

Civil society organizations can also oversee procurement procedures to exert social control. In particular, national-level media outlets usually regularly cover corruption cases, which induce pressure on respective authorities to revise and punish officials as required. Coverage by these organizations' investigative groups additionally provide independent identification of potentially corrupt practices in procurement procedures. Such scandals usually appear around elections, when mayors are receiving donations, or after donations, when donors are awarded public contracts.

# 3 Data

**Donations to political campaigns** We gathered data on political donations from the web application "Cuentas Claras" developed by the Colombian counterpart of the NGO Transparency International.[5] The tool is used by the National Electoral Commission to improve the reporting of candidates campaign-related income and expenses, and increasing the accountability of campaign financing. Its use became mandatory in 2011, so any candidate failing to comply with the reporting can overcome sanctions, ranging from monetary punishments to jail sentences. According to the National Electoral Commission, close to 92% of candidates report financing information. We collected data for three mayoral elections held in October 2015, 2019, and 2023. The data allow us to track the names and national IDs of the donors (firms and individuals), the candidates they donated to, and the donated value following (Ruiz, 2021; Gulzar, Rueda and Ruiz, 2022; Gulzar, Purroy and Ruiz, 2022).

Table 1 presents summary statistics for the population of donors. Panel A includes all donors. On average, each donor contributes only to one candidate. The average value of the donation is close to 8 million COP (about $2,600 USD), approximately eight times the minimum legal monthly wage in the country. Nevertheless, the values have high heterogeneity, with a standard deviation of 16.4 million COP, and a maximum of 500 million COP. The value of donations was higher in 2023, being approximately 9 million COP.

[5] See https://www.cnecuentasclaras.gov.co/, https://transparenciacolombia.org.co/

**Table 1:** Summary statistics of donors

| | Mean (1) | Max (2) | Sd (3) | Obs (4) |
|---|---|---|---|---|
| **Panel A: All donors** | | | | |
| Supported candidates (#) | 1.008 | 4 | 0.103 | 21,065 |
| Donated in three elections (%) | 0.011 | 1 | 0.104 | 21,065 |
| Had contract before (%) | 0.169 | 1 | 0.375 | 21,065 |
| Had contract after (%) | 0.162 | 1 | 0.369 | 21,065 |
| Donated value (million COP) | 7.994 | 500 | 16.401 | 21,065 |
| **Panel B: Donors with contracts after elections** | | | | |
| Contracts after elections (#) | 4.641 | 208 | 8.346 | 3,421 |
| Contracts supported muni (%) | 0.490 | 1 | 0.491 | 3,421 |
| Contracts supported region (%) | 0.880 | 1 | 0.321 | 3,421 |
| Donated value (million COP) | 6.321 | 253 | 11.594 | 3,421 |

*Notes:* This table shows the summary statistics for the sample of donors who donated in each election (2015, 2019, 2023). Each observation is a donor. Panel A shows summary statistics for all donors. Panel B includes only donors who had contracts in the legislative period.

**Public procurement** We also collect procurement data from the "Electronic System for Public Procurement" – SECOP (from its acronym in Spanish) – managed by the National Agency for Public Procurement. SECOP was established in September 2011 to collect all the information about public contracts in the country, including information on the value, duration, contractor name and national ID, contract description, and other characteristics of the contract, contractor, and public agency issuing the contract.

The national ID identifies the same person or firm across any national data. Thus, we use it to connect public contracts with information about donations to political campaigns following (Ruiz, 2021; Gulzar, Rueda and Ruiz, 2022; Gulzar, Purroy and Ruiz, 2022). As both legal persons and firms have unique national IDs, we can separately identify contracts awarded directly to each legal entity. Furthermore, in the case of contracts assigned to firms, our data also allows us to identify the ID of the individuals managing the firm enabling us to also link individual donors with contractor firms. See Figure A1 for an illustration of how the matching between donors and contractors was performed.

Between January 2012 and December 2024, more than 6 million contracts were registered in SECOP. Panel A in Table 2 reports descriptive statistics for all the public procurement contracts in our data. The average value of a contract is 21 million COP, which accounts for approximately 20 times the minimum legal monthly wage in Colombia. Yet, the median contract value is 8.4 million COP, suggesting fewer high-value contracts exist. Such heterogeneity is not found in the duration, which has an average of 4 months, with a median of 3 months, so differences in the contract value can not be explained by the time of contracts. 10% of all contracts have reported having an overrun. 76% of

contracts were awarded to individuals, and 24% to firms. Panel B reports the descriptive statistics for contracts awarded to non-donors, and Panel C reports on contracts awarded to donors. Donors report a higher average contract value compared to non-donors. The median contract value is also higher for donors, with a similar standard deviation, suggesting an overall distribution towards higher valued contracts with same heterogeneity. Donors also have a lower share of the awarded contracts reporting an overrun and a lower average duration.

**Table 2:** Summary statistics of public contracts

| | Mean (1) | Median (2) | Sd (3) | Max (4) | Obs (5) |
|---|---|---|---|---|---|
| **Panel A: All contracts** | | | | | |
| Contract value (million COP) | 21.212 | 8.462 | 61 | 1,010 | 6,829,966 |
| Contract overruns (%) | 0.098 | 0.000 | 0 | 1 | 6,829,966 |
| Duration (months) | 3.994 | 3.000 | 4 | 205 | 6,829,966 |
| **Panel B: No donors** | | | | | |
| Contract value (million COP) | 21.107 | 8.456 | 61 | 1,010 | 6,730,909 |
| Contract overruns (%) | 0.099 | 0.000 | 0 | 1 | 6,730,909 |
| Duration (months) | 4.003 | 3.000 | 4 | 205 | 6,730,909 |
| **Panel C: Donors** | | | | | |
| Contract value (million COP) | 28.321 | 8.920 | 79 | 1,009 | 99,057 |
| Contract overruns (%) | 0.057 | 0.000 | 0 | 1 | 99,057 |
| Duration (months) | 3.372 | 2.000 | 5 | 180 | 99,057 |

*Notes:* This table shows the summary statistics for the public contracts signed between Jan/2012-December/2024 separating between All contracts (Panel A), contracts to non-donors (Panel B) and contracts awarded to ever donors (Panel C). Each observation is a contract.

# 4 Who Donates? Who Enters? Facts on Selection Margins

Estimating the impact of political donations on public procurement posits a key empirical challenge: firms and individuals who selects into donating are unlikely to be randomly drawn from the broader pool of potential government contractors and may systematically differ in characteristics. Ignoring the selection dynamics can lead to biased estimates by conflating the effect of political contributions with pre-existing individual characteristics. Moreover, failing to account for the strategic incentives to enter the public procurement system through political donations may obscure the extent to which these contributions function as implicit exchanges for preferential treatment.

To address these concerns, we document key empirical patterns that illustrate how selection into donating and procurement participation relates to the allocation of public contracts in Colombia. These patterns underscore the necessity of explicitly accounting or modelling selection mechanisms to disentangle the causal relationship between political donations and procurement outcomes.

**Selection into donation** We use the universe of ever-contractors to characterize the type of individuals and firms who self-select into donating. We aggregate our data at the contractor $\times$ election level and define the donation status independently for each election. We then estimate equation 1, a linear model using as outcome $D_{i,s}$, a dummy variable equal to one if the contractor $i$ is a donor in election $s$. We use as an explanatory variable a vector of pre-election contractor characteristics $\mathbf{X}_{i,s}$, such that the coefficients show the extent to which each covariate predicts the donation status. We include $\gamma_s$ for event-specific fixed effects to ensure comparisons within electoral races and cluster our standard errors at the contractor level.

$$D_{i,s} = \delta_s + \mathbf{X}_{i,s} + \varepsilon_{i,s} \tag{1}$$

We gather firm characteristics from the ORBIS Global Database (van Dijk, 2024), which contains information on variables like corporate ownership structures and financial details on private companies worldwide. Using the national ID, we are able to match 18.5% of the firms in our dataset.[6] We recover individual characteristics using administrative data built by Riaño (2021) on the career paths of public servants in Colombia. The dataset is based on the CVs of all the public servants between 2011 and 2017 and includes information on experience and education. We also have a measure of connectedness to a family member already in the public sector, used by Riaño (2021) to estimate the effects of nepotism. We can match 34% of the individuals in our dataset.[7]

Figure 1 reports the results from predicting the donation status based on contractor characteristics. Panel (a) shows the results for firms. We use as predictors performance measures, all defined as dummies equal to one if the firm is above the median, and zero otherwise. The donation status is predicted by firms having lower performance: i) a lower current ratio reflects less financial capacity, ii) a lower solvency ratio indicates less financial stability, and iii) a lower return on equity suggests less capacity to generate new investments.

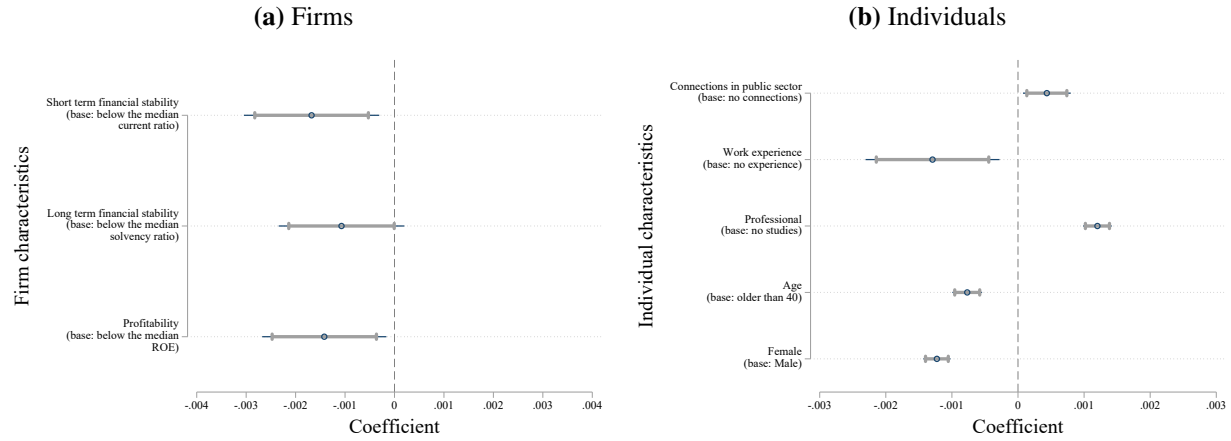
Panel (b) reports for individuals. Donors are more likely to have a family member in the public sector. If nepotism plays a role, it does so indirectly. The connection might have been used as an indirect measure of nepotism to facilitate more intense access to the candidate, thus increasing the returns of donating. It can also suggest that the individual donating has been more exposed to the public sector, so it uses the donation to ensure a public sector career as well. Donors are also are

[6] ORBIS dataset contains information on relatively large firms. Because some firms in our data are relatively small and do not report key financial information, they are not covered by ORBIS. Our main results throughout the document hold for the sample of matched firms.

[7] See Riaño (2021) for further details. We do not recover all the contractors for various reasons. First, the data only covers some of the periods of our sample. Second, public servants CVs only considers contractors performing specific tasks within a public office. Some services provision would not require the CV of the contractor to be uploaded. Our main results throughout the document hold for the sample of matched public servants.

more likely to have less overall experience despite being more likely to have formal studies. Thus, donations might be driven by donors looking to consolidate their careers in the public sector.

**Figure 1:** Contractor selection into donation

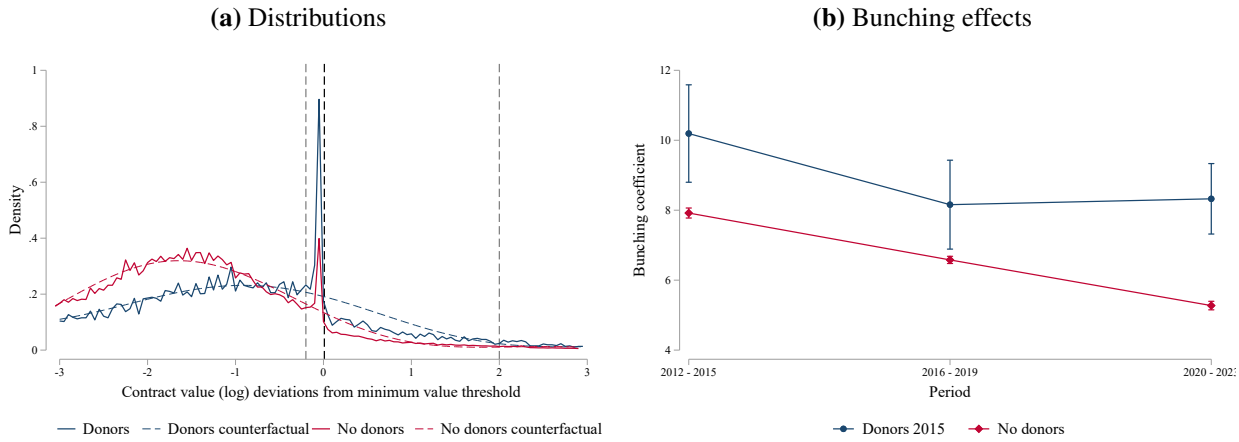



*Note:* The figure reports the point estimate and the 95% standard error from a linear probability model using as outcome whether a contractor donated or not, as explanatory variables the characteristics displayed in the figure and election-specific fixed effects. Panel (a) report the results for firm characteristics. All explanatory variables are defined as dummies for whether a firm was above the median of the distribution of each variable or below. Panel (b) report the results for individual contractors. Connected family equals one if the contractor have ever had a connection with a family member working in the public sector. Education levels are defined relative to *No formal studies*. Experience variables are defined as zero if *No experience*. Female equals one if the contractor reported its gender as female. Age is categorized relative to being older than 40 years. Standard errors are clustered at the contractor level.

**Strategic behavior for decreasing competition** We analyze the extent to which the relationship between mayors and donors reflects strategic behavior to easily award contracts. We analyze arbitrary thresholds below which contracts can be assigned under less competitive procedures. Previous literature suggests that contractors manipulate contract values to avoid competition (Coviello, Guglielmo and Spagnolo, 2018; Szucs, 2023). We test if contracts to donors bunch below thresholds at even higher rates, suggesting that mayors manipulate contract values to assign work to their preferred contractors.

Procurement types mainly differ in the level of competition required to award a contract. Except for bureaucratic contracts, for which the mayor has full discretion, all procurement procedures require some level of competition (see Appendix A.1). For those requiring competition, the *Minimum value* is the one that requires the lowest level of competition. Specifically, other procedures require the presentation of multiple bids and are made public for more extended periods. On the contrary, minimum value contracts can be awarded with at least one bid and are made public only for a day. The decision rule for using a minimum value procedure consists of a value threshold below which a

**Figure 2:** Bunching around discretionary contract thresholds




*Note:* Panel (a) reports the distribution of (log) deviations from the minimum value threshold, excluding contract assigned via direct procurement. The blue (red) line plots the observed distribution for donors (non-donors). Dashed lines plot the predicted distribution from estimating equation 2 without the data within the excluded range. The gray lines at both sides of the threshold indicates the minimum and maximum value of the excluded range. Panel (b) shows the estimate for the bunching mass around the excluded range following Kleven and Waseem (2013), for donors and non-donors in each mayoral period.

mayor can decide to use such a process. Around the threshold, contracts are similar, so mayors have incentives to use the more discretionary procedure.

Panel (a) of Figure 2 shows the distribution of contracts assigned relative to deviations from the (log) minimum value threshold. We plot deviations from the threshold in bins of size 0.05 log deviations. The blue line displays the distribution of contracts assigned to donors and in red for non-donors. The black dashed vertical line indicates the threshold. The figure suggests that donors and non-donors bunch just below the threshold. We formally estimate the bunching size for each population following Kleven and Waseem (2013); Kleven (2016). First, we fit a flexible polynomial to the observed distribution, excluding the data around the threshold using the following equation:

$$D_j = \sum_{l}^{p} \beta_j \cdot (z_j)^i + \sum_{j=-z}^{z+} \gamma_j \cdot \mathbb{1}[z_j = l] + \varepsilon_j \tag{2}$$

Where $D_j$ is the number of individuals in the bin $j$, $z_j$ is the deviations from the threshold in bin $j$, $z-$ and $z+$ consist of the excluded range, and $p$ is the order of the polynomial to be fitted. The counterfactual distribution is obtained by estimating equation 2 without the bins in the exclude range. The excess bunching estimate is calculated by comparing the counterfactual and observed distributions in the excluded range. Grey vertical lines in Panel (a) indicate the excluded range,

which consists of the parts of the distribution where the bunching is occurring.[8].

Panel (b) shows the bunching estimates for 2015 donors and non-donors for contracts assigned in different mayoral periods. First, we note that the bunching estimates for donors are larger than those of non-donors. Also, the downward trend in the assignment of discretionary contracts stops for 2015 donors during the mayoral period of 2020-2023, which is the one immediately after the one they donated to. The difference between both suggests mayors better manipulate contract values to ensure an easier assignment process. As such, manipulating the threshold requires donors and mayors to interact to agree on a price such that the bid value is as close to the threshold as possible without additional competition frictions.

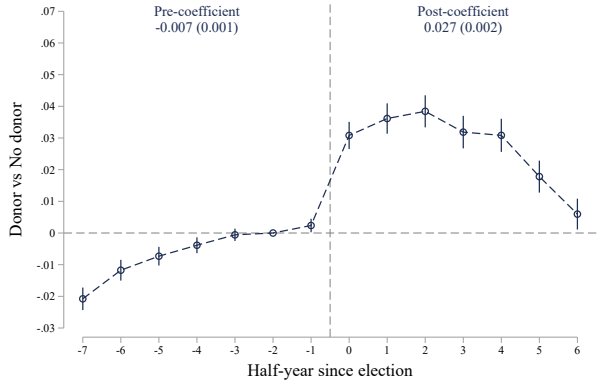
**Strategic timing of donation and entry** A relevant feature of our context is that individuals and firms donate to campaigns only in one electoral race. Figure 3b shows the overlap between donors across the electoral cycles in our data. Less than 4% of donors in the 2015 (blue) and 2019 (red) mayoral elections donated again in the following electoral periods, suggesting a very limited overlap in the donor population across election cycles. Such limited overlap might be explained by donors benefiting from donations through entry to the procurement system. As shown in Table 1, only 15% of donors had contracts before the elections in which they donated, while close to 20% had public contracts after the elections where they donated.

We explore the extent to which the timing of donation correlates with entry to the procurement system by comparing the entry rates at the half-year level between donors and non-donors. If donation is a strategic decision to get preferential treatment, then donors should report larger entry rates after the election in which they donated. Figure 3a shows the event study coefficients comparing donors and non-donors before and after the elections following the empirical strategy described in Section 5. The outcome equals one after the first entry of the individual into the procurement system. The coefficients at the top of the figure report the pre and post-average coefficients. The pattern suggests that donors increase their entry rate the closer to the elections. However, there is a jump in the entry probability immediately after the election relative to non-donors. Such an increasing pattern remains until the third half-year after the election when the difference in entry rates between donors and non-donors starts decreasing. Yet, donors report a larger probability of entry to the procurement system for the electoral period after the donation.

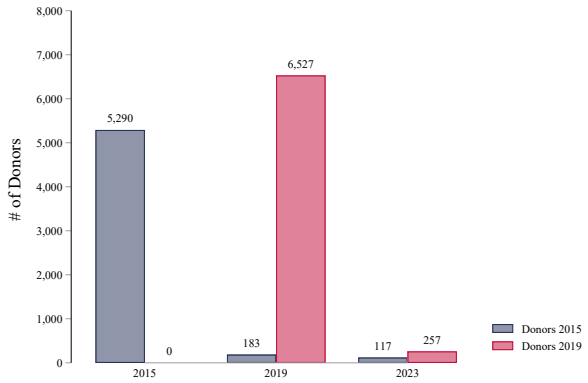
[8]The excluded range on both sides of the threshold is asymmetric, as it comes from a notch that changes the level of the distribution around the threshold (Kleven, 2016).

**Figure 3:** Strategic timing of donation and entry

**(a)** Differential Entry




**(b)** Overlap




*Note:* Panel a) The figure reports the point estimate and the 95% standard error from estimating an event study version of equation 3 for the overall population of potential contractors. It also reports the pooled coefficients for the periods before and after. Standard errors clustered at the individual level. The outcome equals one for the periods where the individual obtained the first public contract and remains constant after. Panel b) reports the number of donors for the 2015 and 2019 electoral cycles, separating between those that initially donated in the 2015 elections (blue) and those donating for the fist time in the 2019 mayoral elections (red).

# 5 Political Donations and Public Contract Allocation

In this section, we assess the impact of donating to a political campaign on public procurement outcomes. We estimate the returns from donating as well as the mechanisms by which donors obtain returns from their support. We then assess the extent to which the returns distort the allocation of contracts in the public procurement system.

## 5.1 Empirical strategy

The differences documented in Section 4 pose a significant treat to identification, as direct comparisons of contractors who are donors and non-donors can lead to biased estimates from the pre-existing differences we document. To address this concern we rely on a difference-in-differences empirical strategy that accounts for time invariant differences between the two groups under two assumptions: a parallel trends assumption, which ensures that, in the absence of elections, procurement outcomes for donors and non-donors would have followed similar trajectories, and a no-anticipation assumption, which posits that elections do not alter expected contract allocations beforehand.

However, this exercise might still yield a biased estimate because of potentially different selection into contractor registration across donors and non-donors. We require information on donors

and non-donors regardless of whether they ever received a public contract. From our data, we can observe donors ever receiving and not receiving contracts and non-donors receiving public contracts. What remains is the group of non-contractor non-donors. To recover this population of non-donors never receiving a contract, we rely on the 2018 National Census produced by the National Statistics Department to simulate the number of eligible individuals never receiving a public contract. We count the number of households registered in each municipality, and assume that each household has at least one individual eligible for a public contract. As all national citizens above the legal age are eligible, we consider such an assumption plausible. We then subtract the number of registered contractors from the number of households. The resulting number represents the number of individuals not donating and not receiving a contract.

**Data structure** After defining the sample as above, we aggregate our public procurement data at the individual-by-half-year level using all donors and non-donors between 2012-2023. We create a balanced panel such that the outcomes for individuals not receiving a contract in a half-year are coded as zero. Because there is almost no overlap between donors across mayoral races (see Table 1), we can sharply separate donors for the 2015 and 2019 mayoral campaigns. For the few cases where there is overlap, we assign donors to the first mayoral campaign they donated to.

Appendix Figure A2 describes the sample composition. From our data, 1,555,990 individuals are contractors, and 12,000 are donors. 4,490 are donors and contractors, representing less than 1% of the contractors population and 37% of the donors population. Overall, our data allow us to observe 1,563,500 unique individuals. From the 2018 National Census, after subtracting the observed individuals by municipality, we identify 12,671,500 simulated individuals eligible for having a contract but never receiving one. Thus, our dataset consists of a balanced panel at the individual-by-half-year level of almost 14 million individuals. The procurement outcomes for the simulated individuals and donors never receiving a contract are always zero.

**Estimating equation** To identify the returns of donating to a political campaign, we estimate a *stacked difference-in-differences* (Cengiz et al., 2019; Baker, Larcker and Wang, 2022; Dube et al., 2023).[9] We consider each electoral period as an event and create event-specific datasets centered around the electoral period using never-donors as control units. The resulting dataset stacks each event-specific dataset, so an effect can be recovered conditional on indexing individual and time-fixed effects with indicators for each event-dataset. Thus, our setting compares individuals that donate to a political campaign to those that do not before and after each election. Figure A3

[9]We thank Gulzar, Purroy and Ruiz (2022) for their work on an early version of this analysis.

illustrates the stacking procedure. The following equation describes the empirical strategy:

$$P_{i,t,s} = \gamma_{i,s} + \lambda_{t,s} + \beta \cdot (Donor_{i,s} \times Post_{t,s}) + \mathbf{X}_{i,s,t} + \varepsilon_{i,t,s} \tag{3}$$

Subscripts $i$, $t$, and $s$ are for a individual, half-year and election-event panels, respectively. $Donor_{i,s}$ equals one if the individual donated to a political campaign in election $s$. $Post_{t,s}$ equals one after the election $s$. $\beta$ captures the causal effect of donating on outcome $P_{i,t,s}$. $\gamma_{i,s}$ is for individual fixed effects to capture time-invariant characteristics of individuals. $\lambda_{t,s}$ is for time $\times$ municipality fixed effects to control by time trends in each municipality, as procurement behavior might have differential trends by region. Both fixed effects terms are saturated with an indicator $s$ for each event panel to account for the stacked structure of the data. $\mathbf{X}_{i,s,t}$ is a vector that controls by fixed characteristics interacted with time dummies to account for pre-existing differences, including the number of half-years of experience in the public sector and the cohort of entry to the public sector (equal to 0 if entering after elections). $\varepsilon_{i,t,s}$ is the error term clustered at the individual level.

$P_{i,t,s}$ refers to outcomes, including the probability of getting a contract (extensive margin) and the total contract value (intensive margin). We also explore the effect on overruns, as a measure of contract efficiency. All outcomes are coded as zero for not-recipients in a half-year. We use log-transformation of the continuous outcomes as the benefits of donating to a political campaign have decreasing returns to scale, so that higher returns would be observed by a higher contract value and by a larger relative increase in the value. For instance, given a limited budget, a mayor might find it easier to double the value of a contract when the original value is relatively small.

We follow Chen and Roth (2022) to account for the problems in log transformations for non-negative outcomes. Specifically, we address combining the intensive and extensive margin effects for zero-valued outcomes. We first normalize positive values relative to the minimum non-zero value to decrease the sensibility to the measurement units. Then, we log-transform normalized positive values, and assign a value $-x = -1$ to zero-valued outcomes. $-x$ measures the importance of the extensive margin, so a change from not receiving a contract to receive one accounts for $100x$ log points (approximately percent change) in the intensive margin. Thus, the interpretation of $\beta$ would be of approximately percent change in the intensive margin.

Finally, we drop the half-years when the elections occur (2015-2 and 2019-2). First, this is because starting in July of the election year, a procurement ban decreases the amount of contracts awarded by the outgoing mayoral team. The ban remains in effect until October once the elections have occurred. Second, during November and December of the election year, after the ban on procurement is lifted, donors can receive contracts for which we can not disentangle whether they were given directly by

the current mayor or indirectly by the incoming mayor.

## 5.2 Returns of donating to a political campaign

Table 3 reports the average effect ($\beta$) from estimating equation 3. In the extensive margin, we find that the probability of receiving a contract after the elections is 2.8 percentage points higher for donors relative to non-donors. In the unconditional intensive margin, we find that donors receive contracts with 17% greater value relative to non-donors after the elections. The coefficients support the hypothesis that donors receive significant returns from their contributions.

**Table 3:** Effect of donating to a political campaign

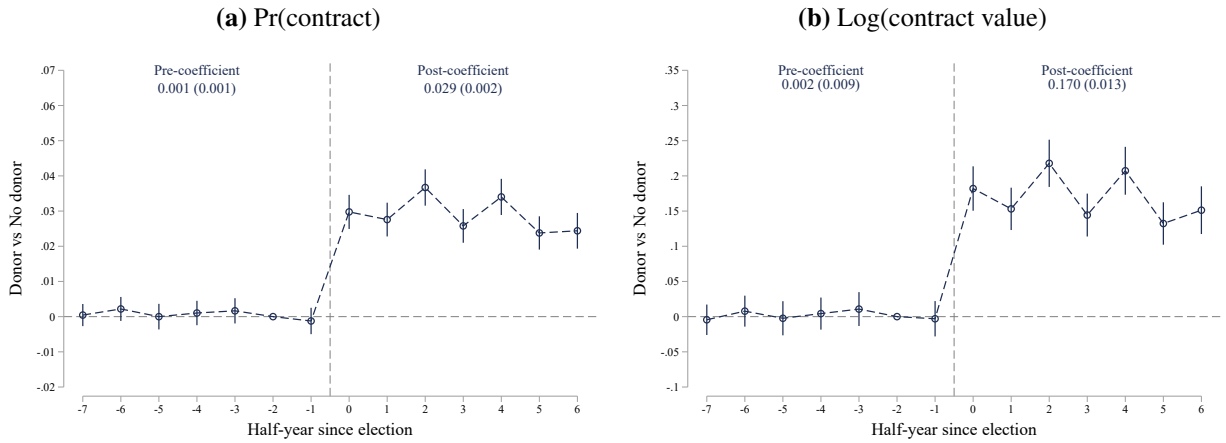
| Dep. Variable | 1(contract) | Log(contract value) |
|---|---|---|
| | (1) | (2) |
| Donor×Post | 0.028*** | 0.17*** |
| | (0.0015) | (0.0097) |
| Number of observations | 398,412,056 | 398,412,056 |
| Population | 14,235,289 | 14,235,289 |
| Non-donor Mean Dep. Var. before (unlogged) | 0.0077 | 0.52 |
| Individual FE | Yes | Yes |
| Municipality × Time FE | Yes | Yes |
| Controls | Yes | Yes |

*Notes:* Observations are at the individual-month level. *Donor* equals one for individuals / firms donating for a political campaign. *Post* equals 1 for the periods after the elections. The contract value is log-transformed. The intensive margin of the log transformation is defined following Chen and Roth (2022) and set to $x = -1$. The mean of the contract value is measured for non-donors in the pre-period in million COP. Pr(contract) equals one if the individual reported having at least one contract per period. Standard errors clustered at the individual level are in parentheses. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

We also take an event study approach in Figure 4 to observe the evolution of the effects. At the top of each figure, we report the pooled coefficients for the pre and post periods. Panel (a) shows the results on the probability of receiving a contract. Panel (b) reports on the log-transformed contract value. Both cases show non-significant pre-coefficients (individually and pooled) providing evidence for the parallel trends assumption. Instead, the figures show an immediate jump after the election, suggesting the returns from donating are perceived immediately after the mayoral period starts and remain across the mayoral period.[10]

[10]These results are robust to variations of the intensive margin value in the log-transformation (Figure A4).

**Figure 4:** Effect of donating to political campaign




*Note:* The figure reports the point estimate and the 95% standard error from estimating an event study version of equation 3 for the overall population. It also reports the pooled coefficients for the periods before and after. Standard errors clustered at the individual level. The contract value is log-transformed. The intensive margin of the log transformation is defined following Chen and Roth (2022) and set to $x = -1$ . Pr(contract) equals one if the contractor reports having at least one contract per period.

## 5.3 Pay-to-play and long-term access to the procurement system

Does access to the public sector continue after the connection with the politician is lost? As mayors cannot be reelected, persistent returns after the mayoral period has ended might signal that, beyond the typically postulated quid pro quo relationship between donors and politicians, donations can also act as investments that enable access to the procurement system, the fruits of which can be enjoyed over the long run. We label this mechanism "pay-to-play."

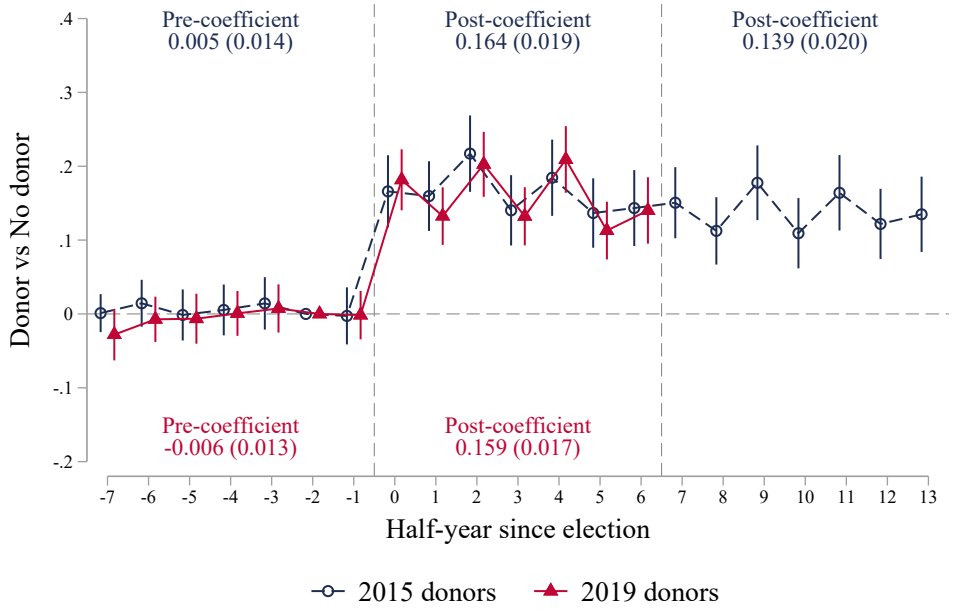
To test for this, we estimate effects separately for the 2015 and 2019 elections, extending both event panels to all available post-periods.[11] For 2015 donors, this allows us to explore the longer-term consequences of donating. For 2019 donors, this exercise enables us to explore if there are differences in the returns across mayoral races.

Figure 5 reports the results. Blue coefficients are for 2015 donors, and red for 2019 donors. The figure shows that 2015 donors perceive returns beyond the mayoral election after the donation. The coefficient for the first mayoral period shows an increase of 16.4% in the contract value relative to non-donors. The returns decrease to 13.9% in the second mayoral period but remain significant. The longer-term results suggest donors stay connected with the public sector once introduced to it and do not require continued connection with the mayor to whom they donate. The loss of

[11] By separating the estimation by event-panel, we drop the indicators for each event-panel from equation 3. Because we do not have staggered timing, we estimate the effects through two-way fixed effects.

political connection might explain the slight decrease in the returns, so the quid-pro-quo mechanism still explains the benefits perceived by donors. Yet, the pay-to-play mechanism ensures there are longer-term benefits. The coefficient for the 2019 donors shows an increase of similar magnitude to that of 2015 donors just after the first election.

**Figure 5:** Pay-to-play: extended effect of donating on log(contract value)




*Note:* The figure reports the point estimate and the 95% standard error from estimating an event study based on equation 3 independently for each election sub-panel, and extending for each the respective post periods. Blue coefficients are for the 2015 elections. The red coefficients are for the 2019 elections. It also reports the pooled pre and post coefficients. Standard errors clustered at the individual level. The contract value is log transformed. The intensive margin of the log transformation is defined following Chen and Roth (2022) and set to $x = -1$.

**Political turnover** The effects can also explained by party turnover. Titl and Geys (2019) show that donors of winning parties receive higher-valued public contracts. One interpretation is that donors who obtained a contract by supporting a political party might be punished if the party leaves office in the next election. We explore these dynamics by estimating the extended effect of donating in the 2015 elections, separating between municipalities that change and those that do not change the party in power in the 2019 elections. We proxy party turnover by measuring ideology turnover using data on political party characterization produced by Cabra-Ruiz et al. (2023). Figure A5 shows that the dynamics between these two samples are similar, suggesting that even if the party that the donors initially supported loses its position, the returns of the donation are long-lasting.

**Degree of access to political power** Ex-ante, it is expected that the returns would be larger when donating to the candidate with the highest access to public resources. Previous work typically examines this by studying the difference between contracts received by donors to winners vs donors to runner-ups (Boas, Hidalgo and Richardson, 2014; Gulzar, Rueda and Ruiz, 2022; Ruiz, 2021). Our data allow us to separate the treatment effects by the electoral position of the person to whom money was donated. Figure A6 presents these results by decomposing the treatment group between donors to the winner, the runner-up, and the remaining candidates, always comparing against the non-donating group. The figure shows that winning donors receive public contracts at a higher rate in the short and long term. The effect is smaller for donors of non-winning candidates.

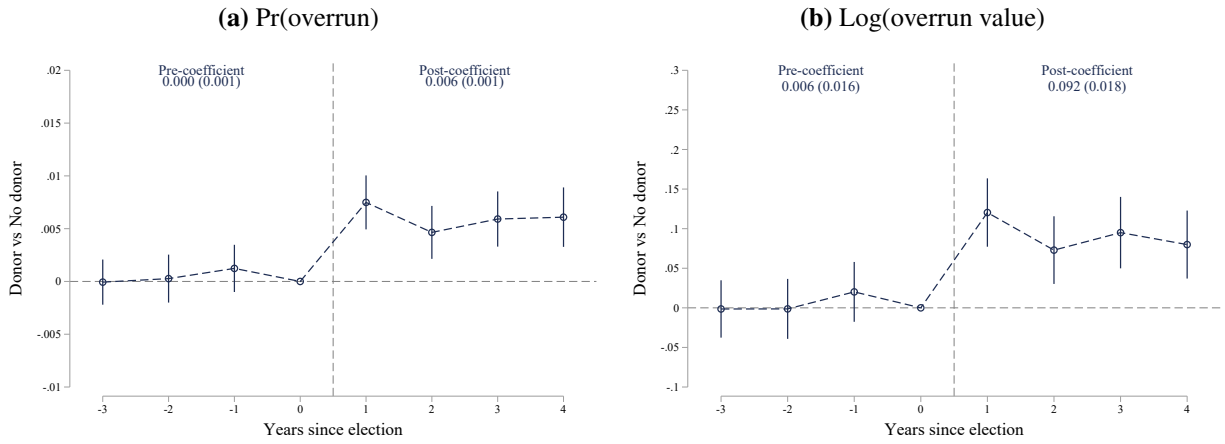
## 5.4 Efficiency consequences of political donation

Having showed in section 4 that contractors selected into donation are worse than those non-donors, and are more likely to bunch barely below discretionary contract thresholds to avoid competition, we proceed to study the efficiency of contracts considering that donors disproportionately reporting less efficient contract execution would suggest that the returns from donating lead to potential misallocation of public resources (Mironov and Zhuravskaya, 2016; Schoenherr, 2019; Baranek and Titl, 2020; Ruiz, 2021). We estimate equation 3 on two proxies of inefficient behavior following Gulzar, Rueda and Ruiz (2022) – the probability of cost overrun and value of the overrun –. Considering an overrun is a rare event (8%, see Table 2), we aggregate our data at the contractor-year level for each event-study analysis.

Figure 6 report the results from estimating an event study based on equation 3. The results are positive and significant for the years after the election. Panel (a) shows that donors have a 1 percentage point higher probability of reporting an overrun, while Panel (b) shows that donors report close to 10% higher overrun value than no donors. The results suggest that donors extend their contracts beyond the originally specified date more than non-donors, allowing them to increase their returns from donating. Figure 7 explores the "pay-to-play" mechanism. Donors of the 2015 elections perceive similar overrun value across mayoral periods, suggesting that the increased overrun is not driven by the political connection but by the donors being connected to the procurement system. Donors of the 2019 elections report similar increase in overruns as that of 2015 donors.

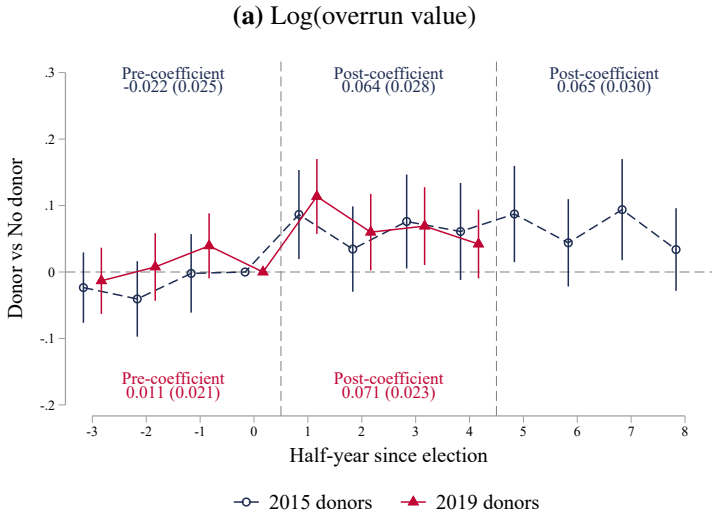
Our efficiency measures approximate inadequate behavior but do not ultimately capture it. For instance, well-planned contracts might still have overruns from factors external to the contractor. Even more, some contract extensions are allowed if the public agency wants the contractor to keep executing specific tasks. Yet, differences in the extended value are significant and major in

**Figure 6:** Effect of donating to a political campaign - efficiency of contracts




*Note:* The figure reports the point estimate and the 95% standard error from estimating an event study version of equation 3 for the overall population. Standard errors clustered at the individual level. The contract value of the overrun is log transformed. The intensive margin of the log transformation is defined following Chen and Roth (2022) and set to $x = -1$. Pr(overrun) equals 1 if the contractor had at least one contract with an overrun per period.

**Figure 7:** Effect of donating to a political campaign - Pay-to-play in efficiency of contracts




*Note:* The figure reports the point estimate and the 95% standard error from estimating an event study based on equation 3 independently for each election sub-panel, and extending for each the respective post periods. Blue coefficients are for the 2015 elections. The red coefficients are for the 2019 elections. It also reports the pooled pre and post coefficients. Standard errors clustered at the individual level. The contract value is log transformed. The intensive margin of the log transformation is defined following Chen and Roth (2022) and set to $x = -1$. Pr(overrun) equals 1 if the contractor had at least one contract with an overrun per period.

magnitude. Thus, interpreting the contracts assigned to donors as inefficient also captures the ability of donors to keep extracting benefits from their connections.

To further test the ability of donors to capture rents, we separate the contracts between discretionary and non-discretionary, as higher discretion can lead to the misallocation of public resources (Baltrunaite et al., 2021; Bosio et al., 2022; Ruiz, 2021; Gulzar, Rueda and Ruiz, 2022). Figure A7 in the appendix reports the results by dividing the contractors between those receiving/not receiving discretionary contracts.[12] The figure shows that the returns for donors come from discretionary contracts, consistent with the opacity to which they can be assigned. We interpret the returns from donation leading to an inefficient use of public resources, as donors can capture more rents.

# 6 Curbing influence of political donations

The results so far show that political donations cause significant distortions in the public procurement system: i) donors are negatively selected and have a higher capacity to exploit discretionary contract assignment, and ii) receive higher returns from public contracts, which are held in the longer-run, and iii) contracts report higher overruns, suggesting an inefficient execution through which donors extract larger rents. In this section, we study the extent to which the influence of money in politics can be curbed to ensure the contracts given to donors are efficient in their execution and use of resources. We take advantage of the latest mayoral election to implement a randomized controlled trial that intends to decrease the influence of money politics in Colombian public procurement by increasing the risk of oversight of public contracts. We specifically explore whether mayors respond to informational and deterrence messages about public contracts scrutiny.

## 6.1 Intervention and experimental design

The intervention takes place in the latest mayoral period, consisting of mayors elected for the 2024-2027 time span. Elections were held between July and October of 2023, and elected mayors took position into office in January 2024. We partnered with an international NGO – Open Contracting Partnership (OCP) – a local Anti-Corruption NGO – Instituto Anticorrupcion (IA) –, and the Colombian office of the Inspector General to send letters to recently elected candidates five months after mayors assume office.[13] The letters use publicly available data on public procurement and political campaign donations to highlight Colombian regulation about benefiting political campaign donors via public procurement contracts and to inform mayors about a greater oversight of the public procurement outcomes associated with their donors.

[12] Discretionary includes Direct and Minimum value procurement categories. Non-discretionary includes Tender, Contest, and Selection. See appendix A.1 for details on each category

[13] See Appendix B.1 for a description of each partner organization.

In the experiment, elected mayors in municipalities were randomized into receiving correspondence that is signed by OCP and IA. Figure 8 presents the design of the experiment and includes in parentheses the number of mayors (municipalities) included in each treatment arm. We focus on municipalities where at least one candidate reported receiving a campaign donation before the election. The control municipalities consist on two groups. Mayors in the **Pure Control group** do not receive any letter. Mayors in the **Placebo letter** control group, receive a letter describing the NGOs with which we partner to test whether mayors react to the warning or just *any* communication that they might perceive as relevant. Because the NGOs are related to anti-corruption activities and improvement of public procurement, a mayor might be induced to improve its procurement behavior without mentioning their donors.

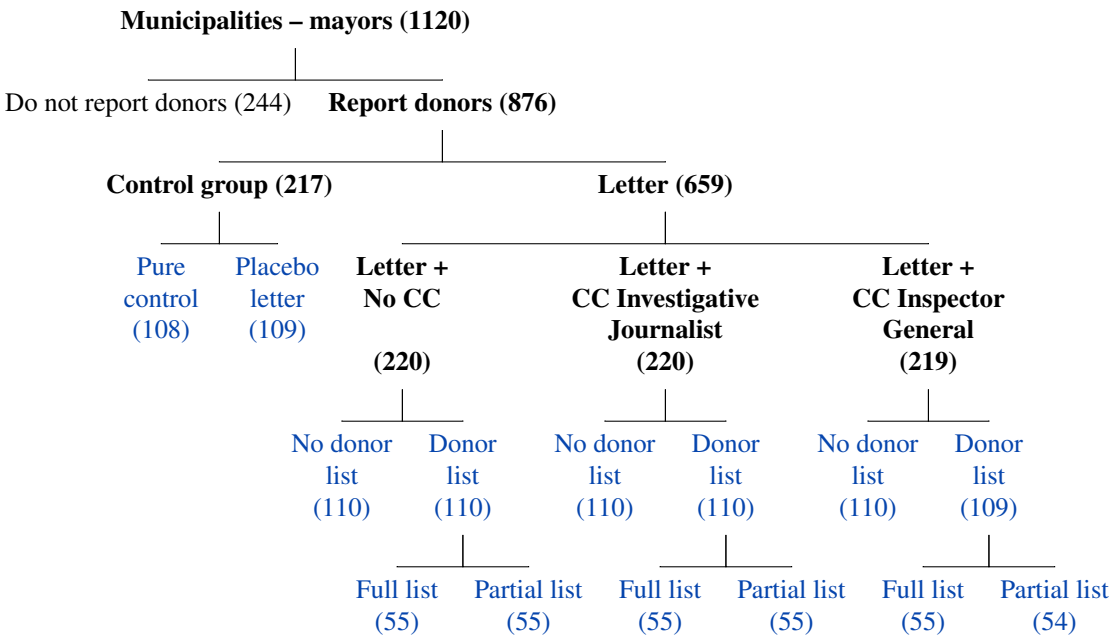
The base treatment – **Letter treatment** – informs elected mayors about current regulations relating to campaign finance and greater oversight of their donor's procurement behavior. Beyond this basic letter, we also vary whether the letter is CCed to agents with oversight capacity: we expect mayors to have higher incentives to reduce contract assignment to donors when a relevant actor that can punish misbehavior is CCed. The **Investigative Journalist** and **Inspector General** treatment arms add this information in the letters. The letters inform mayors that donors' procurement outcomes will be shared with investigative journalists or the Inspectors General Office, which is the public office responsible for investigating misbehavior of public officials. The **Letter + No CC** treatment arm does not carry any mention of any agent.

Finally, we also divide these treatments into three subgroups, that vary the degree to which:

- **No donor list**: the mayor receives only the letter.
- **Full donor list**: the mayor receives the letter including an attachment with the list of all the donors to any candidate in the last elections in the municipality.
- **Partial donor list**: the mayor receives the letter including an attachment with the list of donors to any candidate in the last elections in the municipality, randomly omitting some of them.

Including the donor's information in an attached list allows us to test whether mayors react to highlighting the regulation for public procurement or explicitly to the knowledge that their procurement behavior is being observed. Consider that a mayor might respond to the letter because it makes it easier to follow the regulation or because there is a risk of being caught in misuse of resources as long as a relevant actor can track their donor's procurement information. In addition, by omitting a random donor from the list, we can study whether mayors react strategically by assigning contracts to donors not tracked.

**Figure 8:** Experimental design - treatment arms




*Note:* The figure illustrates the treatment arms structure of the experimental design. The number of municipalities (i.e., mayors) included in each treatment arm is reported in parentheses.

**Randomization** The experimental design described in Figure 8 consists of 876 municipalities randomized across 11 treatment arms. Municipalities are placed in 57 blocks based on the number of unique contractors, the total contracted value, and the share of contracted value assigned to donors, all measured by municipality in the previous mayoral period (2020-2023). We build equally sized blocks by sorting the three variables based on consecutive observations. These blocks serve as randomization strata and all treatments are randomized at the municipal level within each block. The size of each block is 16 municipalities (except for one block, which is of size 28).

For the 164 municipalities where the mayors will receive a partial list of donors as an attachment, we randomly select the donors who will be excluded. We build blocks within each municipality based on whether the candidate reporting donors was the winner, the runner-up, or any other candidate. A donor is randomly excluded for each candidate's rank within the municipality. On some occasions blocks were formed with observations across municipalities because of data sparsity in each sub-group. In practice, we obtained 1,189 blocks of at least two donors, one of which is randomly excluded from the attachment sent to mayors.

**Balance** We test for balance on covariates across randomized municipalities using variables related to procurement history, electoral results, donors behavior and general municipal characteristics. Table B1 in the appendix show the results from estimating a linear regression comparing each

treatment arm relative to the control. All models include randomization block fixed effects. No coefficient is significant. We report the p-value from a joint significance test, which in all cases does not reject the null of all treatment arms be jointly equal to zero.

We also test for balance in observable donor characteristics in Table B2. We test balance for their total donated value, the contracts awarded in the previous mayoral period, the existence of a registry in the procurement system, and the ranking of the donor relative to other donors of the same candidate based on the donated value. We estimate the same linear regression used for the municipal characteristics balance, but cluster the standard errors at the municipal level, which is the relevant randomization unit. No variable is significant relative to the constant, and the joint significance test for the difference across treatment arms is never significant, suggesting balance in all the variables.

Finally, we test for balance across omitted and included donors. Only 164 municipalities effectively receive the treatment of receiving a list of donors with a omitted individual, yet in practice we performed the randomization for all treatment arms to ensure global balance. Table B3 show the the results from a linear regression comparing the omitted and included donors with the constant, including donor-block randomization fixed effects. Because the blocks were built based on the municipality where the donation happened, it already accounts for the difference in treatment arms. No coefficient is significant and the p-value for the joint significance test also suggest that there is balance across omitted and included donors for all observed variables.

**Implementation details** Figure B1 illustrates the differences between letters across treatment arms. In all cases, the letters start with an introduction mentioning the intention of the partner NGOs to remind mayors about the relevant legal framework related to public procurement and donors to political campaigns, followed by a short description of such legal framework. In municipalities randomized into receiving the list of donors, the letter follows with a paragraph mentioning that the letters come with an attachment including relevant information about procurement outcomes for all donors in the municipality in the elections held for 2024-2027. Figure B2 show an example of the donors list attached to the letter. In municipalities randomized into mentioning a relevant agent (journalist or government), the letter concludes, with a mention that the information about donors' procurement outcomes will be shared with the respective agent. The header of the letter also mentions the respective oversight agent.

E-mails and physical letters where sent to the addresses of the mayoral office. Information of mayoral e-mails and physical addresses is public and was compiled from the *National Information*

*System of Public Employment*.[14] Relevant to our case is that any communication received by a public office should be re-directed to the individual to whom it is addressed. Thus, we can be confident that the e-mail and physical letters would reach the mayor's mailbox. All e-mail letters were sent on May 15, 2024, almost five months after the mayors started their governing period. We tracked the interaction with the e-mail and found that 100% of the letters were received and opened. Physical letters were sent on July 24, 2024, two months after the e-mails were sent, as a reminder. Overall, 80% arrived at their destined municipality within the first week. After three weeks, 95% of the letters were effectively delivered, and 5% of them did not reach their destination.

## 6.2 Estimating equation

To study whether our intervention decreases the distortions induced by political contributions and what type of oversight can effectively curb the influence of money in politics, we follow the public procurement outcomes for donors and non-donors (contractors or not contractors as described in Figure A2) before and after the letters have been sent. We specifically observe whether someone received a public contract and the value of the contract.

Ex-ante, if informing mayors that their recruitment is being monitored decreases the returns of political donations, we expect to observe a lower probability of receiving contracts for donors of the informed mayors compared to donors of other mayors. Even if donors of informed candidates received similar number of contracts, we expect the value to be lower than those of donors of other mayors. Similarly, the decrease in the returns of donating can be reflected in the distribution of contracts towards other non-donor contractors, suggesting there might be a system-level effect where the resources allocated to donors are now given to other participants of the procurement system. Alternatively, mayors can make a system-level decision to award fewer contracts to decrease the probability of being caught in any misuse of resources, and to reduce spurious contracting.

To test these hypotheses, we estimate the effects of our intervention for donor and non-donor contractors. We estimate reduced-form equations of the form

$$P_{i,t} = \gamma_i + \lambda_{m(i)\times donor\times t} + \sum_j^J \beta_{donor}^j \left(\mathbb{1}[\text{Letter}_{m(i),donor} = j] \times \text{Post}_t\right) + \sum_j^J \beta_{nondonor}^j \left(\mathbb{1}[\text{Letter}_{m(i),nondonor} = j] \times \text{Post}_t\right) + \varepsilon_{i,t}, \quad (4)$$

[14] See here the directory for public offices information. In cases where the e-mail or physical address was incorrect, we obtained the information from the institutional web pages of the municipalities.

where $P_{i,t}$ is the procurement result for the donor or non-donor $i$, and month $t$. Crucially, $\mathbb{1}[\text{Letter}_{m(i),donor} = j]$ equals one for donors in municipality $m$ where the mayor received the letter of type $j$, and zero otherwise. Similarly, $\mathbb{1}[\text{Letter}_{m(i),nondonor} = j]$ equals one for non-donor in municipality $m$ where the mayor received the letter of type $j$. $\text{Post}_t$ equals one for the periods after the letters were sent in May and zero for periods before. Subscript $j$ refers to a single treatment arm, such that the set $J$ in the summation terms consists of all mutually exclusive treatment arms. Our coefficients of interest are the $\beta^j$s. $\beta^j_{donor}$ would capture the effect of informing mayors about greater oversight when awarding contracts to donors in municipalities with letter type $j$. If the coefficient associated with $\beta^j_{donor}$ is negative for the population of donors, we would conclude that treatment $j$ decrease the influence of political donations in public procurement compared to the control group. As such, $\beta^j_{nondonor}$ would show us any deviation in the allocation of contracts to non-donor.

We also take advantage of the panel structure of the data to include $\gamma_i$ as a fixed effect for the donor or non-donor, controlling for time-invariant characteristics. We include $\lambda_{m(i)\times donor\times t}$ as a municipal randomization block $\times$ $\mathbb{1}[Donor]$ $\times$ time fixed effects. Because the randomization and donation status are constant for individuals, they are absorbed by the individual fixed effects. Yet, by including $\lambda_{m(i)\times donor\times t}$, we are able to account for specific differential time trends across randomization structures and by donation status. $\varepsilon_{i,t}$ is for the standard errors clustered at the municipal level, which is the main randomization level.

In our preferred specifications, we consider for the effect on donors procurement outcomes municipalities that received an attached list as a single treatment group, regardless of whether there was an omitted donor. In this case, $Letter_{m,donor}$ consist of six independent dummies, that correspond to mayors receiving the letter with *List* and *No list*, by each of the mentioned oversight agents *Inspector General*, *Investigative Journalist*, and *No CC* ($2 \times 3$ coefficients). For the non-donor contractors effect, because the only relevant treatment is whether the mayor received or not a letter, we consider the aggregate effects from the oversight agents ($1 \times 3$ coefficients). Thus, we analyze nine coefficients, six for donors, and three for non-donors. For the donor population we will also present the most disaggregated effects when separating the list treatment by *omitted* and *included* donor. The main control group is the combination of the pure control and the placebo letter, but we report robustness to considering the placebo letter as a separate treatment.

$P_{i,t}$ procurement outcomes are mainly the probability of receiving a contract (extensive margin) and the total contract value received by the donor (intensive margin). We define both variables as zero for non-recipients in a period. For the total contract value, we analyze the log transformation following Chen and Roth (2022). Specifically, we normalize the outcome relative to the minimum contract value in the data, and then we log-transform the normalized outcomes for all recipients.

Last, we define the intensive margin as $x = -1$ for non-recipients. Thus, the coefficients associated with the log transformation are interpreted in (approximate) percent change.

## 6.3 Impact of increased risk of oversight on procurement outcomes

Table 4 describes the results. Columns 1-2 report on the probability of receiving a contract. Columns 3-4 report on the log-transformed total contract value. The first column for each outcome reports the effect of the aggregate treatment (columns 1 and 3), regardless of the inclusion of the donor list. The second columns for each outcome (columns 2 and 4) report the coefficients when separating the treatment between no list and list recipients.

We find significant effects when the letter treatment CCs the Inspector General's office (government arm) *and* includes an attachment with the list of donors. The effect for the *Inspector General + list* treatment is of a 1 percentage point lower probability of a donor receiving a contract, and a decrease in contract value of 4.8%. The bottom of the table presents the p-values from linear combinations comparing the Inspector General treatment coefficients with the other treatment arms. The difference is significant comparing against the investigative journalist treatment arm and the non-donors, suggesting that the effect for the *Inspector General + list* is sizable. Yet, we cannot reject significant difference against the No CC treatment arm. We can conclude then that communications mentioning the government as an oversight agent, attaching the list of donors as a way to signal specific oversight capacity, was effective in decreasing the short-term returns from contracts awarded to to donors. We do not see any movement on non-donors.

When further separating the effects by *omitted donor* and *included donor*, Table B4 in the Appendix shows that both coefficients are negative for the *Inspector General + list*. The effect is only significant for the *included donor*, yet the p-value for the difference with the *omitted donor* is not statistically significant. The negative effect of the omitted donor treatment for the Inspector General letter suggests that mayors do not strategically react to incomplete information so long as the threat of oversight is strong enough.

Additionally, we estimate the effects for each post-period to explore the extent to which the effects might be decreasing over time. Figure 9 shows the results for the treatment arm mentioning the Inspector General, using as the base period April, just before the letters were sent. The Figure 9 is for the treatment when the letter includes the mention of the government, Panel (a) show the result for the list treatment and Panel (b) report the results for the letters without a list of donors. Overall the coefficients after the intervention was implemented are significant only for the treatment letter including the list of donors. The effects are sustained throughout the year and remain negative for

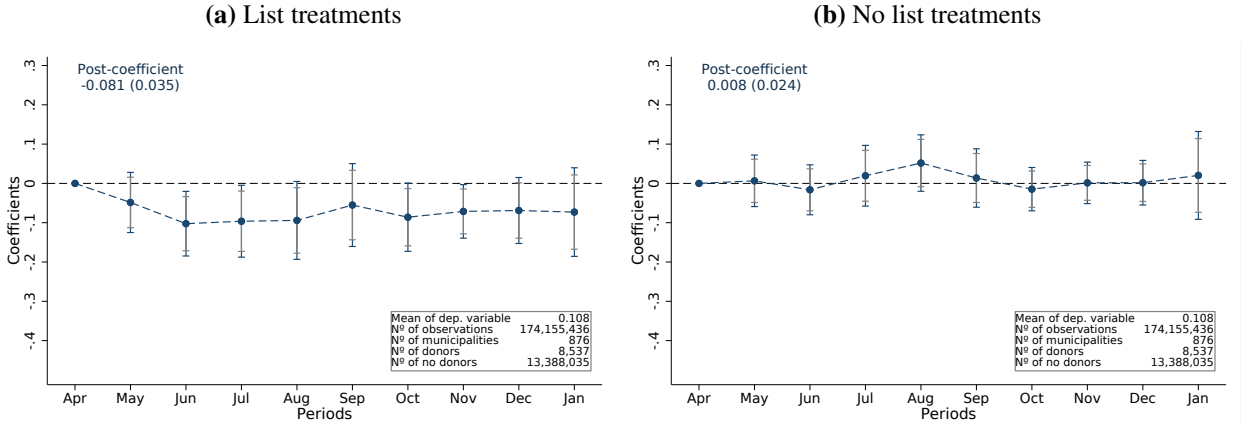
**Table 4:** Impact on contractors procurement outcomes

| Dep. Variable | Pr(contract) | | Log(contract value) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Donor* | | | | |
| A. Inspector General×Post | -0.003 | | -0.020 | |
| | (0.002) | | (0.014) | |
| **Inspector General + no list**×Post | | 0.001 | | 0.005 |
| | | (0.002) | | (0.015) |
| **Inspector General + list**×Post | | -0.008** | | -0.048** |
| | | (0.003) | | (0.019) |
| B. Investigative Journalist×Post | -0.002 | | -0.013 | |
| | (0.002) | | (0.014) | |
| **Investigative Journalist + no list**×Post | | -0.001 | | -0.012 |
| | | (0.003) | | (0.018) |
| **Investigative Journalist + list**×Post | | -0.002 | | -0.014 |
| | | (0.003) | | (0.016) |
| C. No CC×Post | -0.002 | | -0.019 | |
| | (0.002) | | (0.015) | |
| **No CC + no list**×Post | | -0.001 | | -0.015 |
| | | (0.003) | | (0.019) |
| **No CC + list**×Post | | -0.003 | | -0.022 |
| | | (0.003) | | (0.018) |
| *Non-donors* | | | | |
| A. Inspector General + NO donor×Post | -0.000 | -0.000 | -0.001 | -0.001 |
| | (0.000) | (0.000) | (0.001) | (0.001) |
| B. Investigative Journalist + NO donor×Post | 0.000 | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.001) | (0.001) |
| C. No CC + NO donor×Post | 0.000 | 0.000 | 0.002 | 0.002 |
| | (0.000) | (0.000) | (0.003) | (0.003) |
| *Linear combinations P-values* | | | | |
| A. Inspector General - B. Investigative Journalist | .582 | | .619 | |
| A. Inspector General - C. No CC | .843 | | .957 | |
| **Inspector General + list - Inspector General + no list** | | .012 | | .007 |
| **Inspector General + list - Investigative Journalist + list** | | .1 | | .09 |
| **Inspector General + list - No CC + list** | | .289 | | .24 |
| A. Inspector General + donor - A. Inspector General + NO donor | .216 | | .18 | |
| **Inspector General + list + donor - Inspector General + NO donor** | | .023 | | .014 |
| Mean of dependent variable | .003 | .003 | .111 | .111 |
| N. of obs. | 174,155,436 | 174,155,436 | 174,155,436 | 174,155,436 |
| Number of municipalities | 876 | 876 | 876 | 876 |
| Number of donors | 8,537 | 8,537 | 8,537 | 8,537 |
| Number of non donors | 13,388,035 | 13,388,035 | 13,388,035 | 13,388,035 |
| Contractor FE | Yes | Yes | Yes | Yes |
| Block × $\mathbb{1}[Donor]$ × Time FE | Yes | Yes | Yes | Yes |

*Notes:* The unit of analysis is the individual-month, estimating equation 4 on procurement outcomes. *Inspector General* is for the treatment letter carbon copying the relevant public office that oversight public procurement. *Investigative Journalist* is for the treatment letter carbon copying investigative journalist. *No CC* is for municipalities receiving the treatment letter without carbon copy additional organizations. *No list* is for treatment letters not including the donor list as attachment. *List* group the treatment letter including the full and partial list of donors as attachment. *Pr(contract)* equals 1 if the donor received a contract in $t$, 0 otherwise. *log(contract value)* is the value of contracts. The log transformation follows Chen and Roth (2022) and define the intensive margin as $x = -1$. *Post* equal 1 for periods after the letter were sent to mayors. The mean of the outcomes is measured in the pre-period and is reported in million COP for the contract value. *Individual FE* is for individual fixed effects. *Block* × $\mathbb{1}[Donor]$ × *Time FE* is for randomization block by donation status by month fixed effects. Standard errors clustered at the municipal level are in parentheses. $^{*}p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

treated donors relative to donors in control municipalities. Figures for the other oversight agents copied in the letters received by mayors are in Figure B3 in the appendix.

**Figure 9:** Impact on donor procurement outcomes - monthly coefficients on log(contract value)




*Note:* The unit of analysis is the individual-month. The figure shows point estimates and the 90% and 95% confidence intervals from estimating equation 4 on procurement outcomes. Fixed effects for randomization block $\times$ [*Donor*] $\times$ month are included. Standard errors clustered at the municipality level. The point estimates represent the effect of the treatments relative to the *Placebo* and *Control* groups post-intervention. The outcome is the *Log(contract value)*. The log transformation for the value follows Chen and Roth (2022), defining the intensive margin as $x = -1$ for non-recipients.

**Impact by rank of supported candidate** We might expect treated mayors to respond by changing the contracting outcomes of their own donors if they anticipate that their direct link is most likely to be scrutinized. Conversely, they might take more strict action against donors of other candidates. We estimate a variation of equation 4, where we separate each treatment arm for donors into a treatment arm by supported candidate (winner / non-winners). We then include donor randomization block fixed effects instead of individual fixed effects as the donors blocks were built within municipality for each candidate rank, ensuring that we are comparing donors supporting the same rank of candidate across municipalities. For non-donors, the donor block fixed effects ensures the comparison of non-donor contractors across treatment arms. We also include time-fixed effects.

Table 5 shows the results. The coefficients show that the effect is negative and significant for the donors of the winner (columns 1 and 3). When separating by *list* or *no list* some effects stop being significant but all coefficients remain negative. Consistently, the coefficient in the *Inspector General + list* treatment remains significant. The p-values at the bottom of the table do not show significant differences between the *list* treatments in for the winning candidate. The effects are consistent with donors having a direct relationship with individuals in public positions, for which the punishment might be stronger. In the case of donors to non-winner candidates, the coefficients are smaller in magnitude and significance.

**Table 5:** Impact on contractors procurement outcomes - by candidate ranking

| Dep. Variable | Pr(contract) | | Log(contract value) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Supported candidate: Winner* | | | | |
| A. Inspector General + Winner×Post | -0.009** | | -0.047** | |
| | (0.004) | | (0.022) | |
| **Inspector General + no list + Winner**×Post | | -0.005 | | -0.027 |
| | | (0.005) | | (0.028) |
| **Inspector General + list + Winner**×Post | | -0.012** | | -0.069** |
| | | (0.006) | | (0.031) |
| B. Investigative Journalist + Winner×Post | -0.008** | | -0.048** | |
| | (0.004) | | (0.022) | |
| **Investigative Journalist + no list + Winner**×Post | | -0.010* | | -0.061* |
| | | (0.006) | | (0.034) |
| **Investigative Journalist + list + Winner**×Post | | -0.005 | | -0.037 |
| | | (0.004) | | (0.026) |
| C. No CC + Winner×Post | -0.007* | | -0.046** | |
| | (0.004) | | (0.023) | |
| **No CC + no list + Winner**×Post | | -0.006 | | -0.049 |
| | | (0.005) | | (0.032) |
| **No CC + list + Winner**×Post | | -0.007 | | -0.043 |
| | | (0.005) | | (0.028) |
| *Supported candidate: Non-winners* | | | | |
| A. Inspector General + Non-winners×Post | 0.001 | | -0.002 | |
| | (0.003) | | (0.017) | |
| **Inspector General + no list + Non-winners**×Post | | 0.006** | | 0.028* |
| | | (0.003) | | (0.016) |
| **Inspector General + list + Non-winners**×Post | | -0.004 | | -0.034 |
| | | (0.004) | | (0.024) |
| B. Investigative Journalist + Non-winners×Post | 0.002 | | 0.009 | |
| | (0.002) | | (0.013) | |
| **Investigative Journalist + no list + Non-winners**×Post | | 0.004 | | 0.018 |
| | | (0.003) | | (0.016) |
| **Investigative Journalist+ list + Non-winners**×Post | | 0.000 | | 0.000 |
| | | (0.002) | | (0.015) |
| C. No CC + Non-winners×Post | 0.000 | | -0.002 | |
| | (0.003) | | (0.016) | |
| **No CC + no list + Non-winners**×Post | | 0.002 | | 0.004 |
| | | (0.003) | | (0.020) |
| **No CC + list + Non-winners**×Post | | -0.001 | | -0.007 |
| | | (0.003) | | (0.020) |
| *Linear combinations P-values* | | | | |
| A. Inspector General: Winner - Non-winners | .032 | | .072 | |
| **Inspector General Winner: List - No list** | | .294 | | .284 |
| **Inspector General Non-winners: List - No list** | | .012 | | .007 |
| **Inspector General List Winner - Investigative Journalist List Winner** | | .284 | | .384 |
| **Inspector General List Non-winners - Investigative Journalist List Non-winners** | | .247 | | .149 |
| **Inspector General List Winner - No CC List Winner** | | .458 | | .513 |
| **Inspector General List Non-winners - No CC List Non-winners** | | .429 | | .3 |
| Mean of dependent variable | .003 | .003 | .111 | .111 |
| N. of obs. | 174,155,436 | 174,155,436 | 174,155,436 | 174,155,436 |
| Number of municipalities | 876 | 876 | 876 | 876 |
| Number of donors | 8,537 | 8,537 | 8,537 | 8,537 |
| Contractor FE | Yes | Yes | Yes | Yes |
| Block × $\mathbb{1}[Donor]$ × Time FE | Yes | Yes | Yes | Yes |

*Notes:* The unit of analysis is the individual-month, estimating equation 4 on procurement outcomes. Panel for non-donors estimated but not reported. See Table 4. *Inspector General* is for the treatment letter carbon copying the relevant public office that oversight public procurement. *Investigative Journalist* is for the treatment letter carbon copying investigative journalist. *No CC* is for municipalities receiving the treatment letter without carbon copy additional organizations. *No list* is for treatment letters not including the donor list as attachment. *List* group the treatment letter including the full and partial list of donors as attachment. *Winner*, and *Non-winners* are for donors supporting each candidate. *Pr(contract)* equals 1 if the donor received a contract in $t$, 0 otherwise. *log(contract value)* is the value of contracts. The log transformation follows Chen and Roth (2022) and define the intensive margin as $x = -1$. *Post* equal 1 for periods after the letter were sent to mayors. The mean of the outcomes is measured in the pre-period and is reported in million COP for the contract value. *Donor block FE* is for donor randomization block fixed effects. *Time FE* is month fixed effects. Standard errors clustered at the municipal level are in parentheses. $^{*}p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

**Impact on Pay to play donors** The results in section 5 showed that the distortions in the allocation of public procurement contracts might be held in the long-term, as donors of a mayoral period can receive contract even after their political connection leaves office, suggesting that donors "pay" to get into the system, and once registered, they can keep developing their career. We explore the extent to which treatments impact the incentives of mayors to assign contracts to previous election donors. Because there is no overlap in donors across mayoral races, the list of donors never includes a donor from the previous mayoral cycle (2020-2023). Also, because there is no reelection in consecutive mayoral races, the newly elected candidate does not necessarily need to have a direct connection with a donor from a previous election.

We estimate equation 4 further separating from the non-donors the group of pay-to-play donors, which is defined as donors of the previous mayoral election (2020-2023). Table 6 shows the results. We can observe that pay-to-play donors do not perceive significant changes on their procurement outcomes for none of the treatment arms. As such, despite the results suggesting that communications coming from the government including a specific list for oversight can break the influence of money in politics for new donors, it does not appear to be the case that it affects contracts to previous election donors, who may in fact be the beneficiaries of further contracts. It also does not suggest strategic behavior of displacement of procurement to different types of donors.

**Municipal procurement behavior** A possible unintended consequence of the letter might be that mayors can reduce the overall amount of resources assigned to public procurement as a mechanism to avoid any type of complaint or scandal related to the use of public resources, which might, in turn, affect municipal performance despite effectively decreasing the influence of money in politics.

We aggregate our individual-by-month data at the municipality-by-month level to analyze the aggregate behavior of public procurement. We estimate a reduced version of equation 4 with a single $\beta^j$ for each treatment arm at the municipality level. We use municipality fixed effects to control by constant municipal characteristics and block $\times$ month fixed effects to account for differential specific trends across municipal randomization blocks. Table B5 in the appendix shows for the (log) total amount of resources –the value of contracts– that there are no significant differences across the coefficients of the treatments on any of the columns. More importantly, there are no negative and significant effects for non-donors, suggesting that mayors did not react by decreasing the overall amount of resources in the municipality.

**Table 6:** Impact on donors, non-donors and previous donors (pay to play) on procurement outcomes

| Dep. Variable | Pr(contract) | | Log(contract value) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Donor* | | | | |
| A. Inspector General×Post | -0.003 | | -0.020 | |
| | (0.002) | | (0.014) | |
| **Inspector General + no list**×Post | | 0.001 | | 0.005 |
| | | (0.002) | | (0.015) |
| **Inspector General + list**×Post | | -0.008** | | -0.048** |
| | | (0.003) | | (0.019) |
| B. Investigative Journalist×Post | -0.002 | | -0.013 | |
| | (0.002) | | (0.014) | |
| **Investigative Journalist + no list**×Post | | -0.001 | | -0.012 |
| | | (0.003) | | (0.018) |
| **Investigative Journalist + list**×Post | | -0.002 | | -0.014 |
| | | (0.003) | | (0.016) |
| C. No CC×Post | -0.002 | | -0.019 | |
| | (0.002) | | (0.015) | |
| **No CC + no list**×Post | | -0.001 | | -0.015 |
| | | (0.003) | | (0.019) |
| **No CC + list**×Post | | -0.003 | | -0.022 |
| | | (0.003) | | (0.018) |
| *Donors in previous elections (pay to play)* | | | | |
| A. Inspector General + previous election donor×Post | 0.003 | | 0.013 | |
| | (0.003) | | (0.016) | |
| **Inspector General + no list + previous election donor**×Post | | 0.003 | | 0.013 |
| | | (0.004) | | (0.022) |
| **Inspector General + list + previous election donor**×Post | | 0.002 | | 0.014 |
| | | (0.003) | | (0.017) |
| B. Investigative Journalist + previous election donor×Post | 0.001 | | 0.002 | |
| | (0.002) | | (0.013) | |
| **Investigative Journalist + no list + previous election donor**×Post | | 0.003 | | 0.018 |
| | | (0.002) | | (0.014) |
| **Investigative Journalist + list + previous election donor**×Post | | -0.001 | | -0.012 |
| | | (0.003) | | (0.018) |
| C. No CC + previous election donor×Post | -0.000 | | -0.002 | |
| | (0.003) | | (0.018) | |
| **No CC + no list + previous election donor**×Post | | -0.002 | | -0.011 |
| | | (0.004) | | (0.022) |
| **No CC + list + previous election donor**×Post | | 0.001 | | 0.004 |
| | | (0.003) | | (0.022) |
| *Linear combinations P-values* | | | | |
| A. Inspector General + donor - A. Inspector General + previous election donor | .099 | | .124 | |
| A. Inspector General + donor - A. Inspector General + NO donor | .099 | | .124 | |
| **Inspector General+ list + donor - Inspector General + previous election donor** | | .023 | | .014 |
| **Inspector General + list + donor - Inspector General + NO donor** | | .016 | | .015 |
| Mean of dependent variable | .003 | .003 | .111 | .111 |
| N. of obs. | 174,155,436 | 174,155,436 | 174,155,436 | 174,155,436 |
| Number of municipalities | 876 | 876 | 876 | 876 |
| Number of donors | 8,537 | 8,537 | 8,537 | 8,537 |
| Number of previous donor | 13,611 | 13,611 | 13,611 | 13,611 |
| Contractor FE | Yes | Yes | Yes | Yes |
| Block × Donor × Time FE | Yes | Yes | Yes | Yes |

*Notes:* The unit of analysis is the individual-month, estimating equation 4 on procurement outcomes. Panel for non-donors estimated but not reported. See Table 4. *Inspector General* is for the treatment letter carbon copying the relevant public office that oversight public procurement. *Investigative Journalist* is for the treatment letter carbon copying investigative journalist. *No CC* is for municipalities receiving the treatment letter without carbon copy additional organizations. *No list* is for treatment letters not including the donor list as attachment. *List* group the treatment letter including the full and partial list of donors as attachment. *Pr(contract)* equals 1 if the individual received a contract in $t$, 0 otherwise. *log(contract value)* is the value of contracts received. The log transformation follows Chen and Roth (2022) and define the intensive margin as $x = -1$. *Post* equal 1 for periods after the letter were sent to mayors. The mean of the outcomes is measured in the pre-period and is reported in million COP for the contract value. *Individual FE* is for individual fixed effects. *Block × Donor × Time FE* is for randomization block by donation status by month fixed effects. Standard errors clustered at the municipal level are in parentheses. $^{*}p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

# 7 A model of political donations and public procurement

In this section, we introduce a model to interpret our reduced-form findings presented earlier. Our model addresses two key questions: What economic factors determine firms' and individuals' political contributions and their decisions to participate in public procurement, and how do these factors explain our empirical observations?

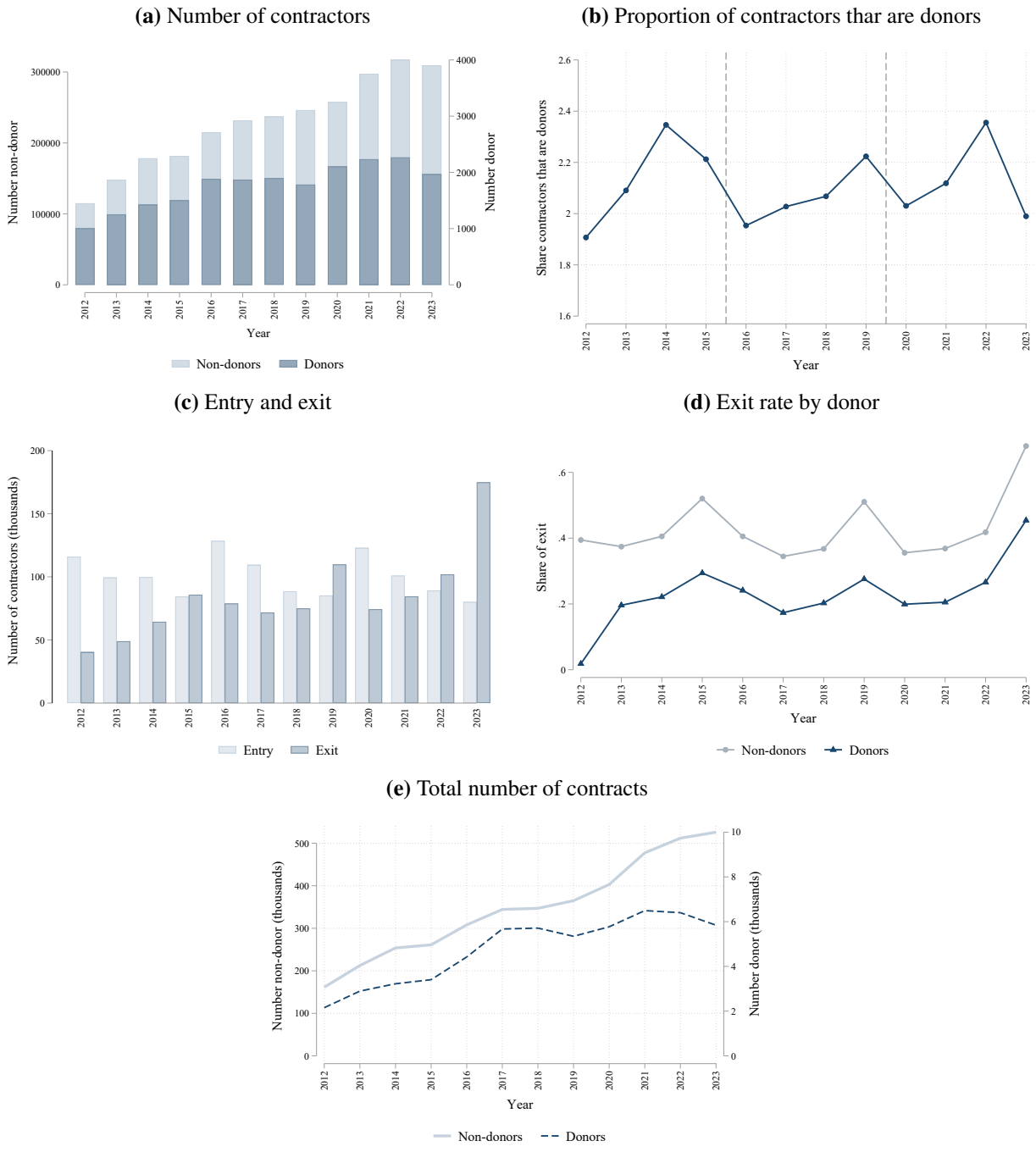
Although our reduced-form analysis identifies the existence of a "*pay-to-play*" distortion, it cannot uncover the underlying economic mechanisms that generate this distortion or jointly explain how decisions regarding donations and procurement participation interact. Notably, our data indicate that only a small fraction of contractors contribute politically, despite the apparent high returns from donations in public procurement. To understand this puzzle, we move beyond average treatment effects and explicitly model entry and exit decisions, as well as cost differences between donating and non-donating entities.

## 7.1 Preliminaries

**Evolution of the market structure** To ensure our structural model captures the full set of empirical regularities beyond the reduced-form findings, we seek to match additional data patterns that describe the long-run evolution of public procurement markets. We focus on three additional patterns that are essential for refining the model's representation of entry, persistence, and competition in public contracting. First, Panel (a) in Figure 10 illustrates the steady expansion of the contractor pool over time, yet it also underscores that donors consistently represent a small fraction of all contractors. This observation highlights the existence of substantial barriers to entry into procurement markets, especially for firms that seek privileged access through political contributions. Panel (b) further refines this insight by showing that the proportion of contractors who are donors fluctuates cyclically, peaking in election years. This pattern suggests that donation behavior is inherently dynamic, with firms strategically adjusting their status based on political cycles. To match these patterns, our model must incorporate the costs and benefits of entry, account for firms' endogenous decisions to donate, and capture the role of sunk costs in sustaining persistent donor advantages.

Second, the dynamics of contractor participation are captured in Panels (c) and (d), which depict entry, exit, and persistence in the procurement market. Panel (c) shows that contractor entry and exit are highly volatile, with turnover increasing significantly in the years following elections. This suggests that the decision to participate in public procurement is not purely static but is driven

**Figure 10:** Evolution of public procurement market structure by donor and non-donors




*Note:* The panels in the figure describe the market structure of public procurement contracts. Panel (a) describes the total number of contracts by year and donor / non-donor. Left axis correspond to non-donors, right axis to donors. Panel (b) shows the proportion of contractors that are donors for each year. Panel (c) describes the number of contractors entering and exiting the procurement system –in terms of receiving or not a contract– by year. Panel (d) describes the average share of exit rate of donors and non-donors. Panel (e) show the time trends of the total number of contracts assigned to donors and non-donors by year. Left axis correspond to non-donors, right axis to donors.

by shifting political and institutional conditions. Panel (d) further differentiates the exit patterns of donors versus non-donors, revealing that donors exhibit systematically lower exit rates. This persistence is a hallmark of the pay-to-play mechanism we document—donors, once embedded in the system, appear to retain access even beyond the direct tenure of the politicians they originally supported. Our structural model must account for these dynamics by allowing for long-term persistence effects among donors, capturing the differential exit risks between donors and non-donors, and incorporating dynamic firm decisions regarding continued participation in public procurement.

Lastly, Panel (e) tracks the total number of contracts assigned to donors and non-donors over time, revealing a key allocative rigidity: while the volume of contracts awarded to non-donors steadily increases, donor firms maintain a disproportionately high share of contracts relative to their representation in the contractor pool. Despite fluctuations in the overall number of donors, the flow of contracts to donor firms remains relatively stable, suggesting that procurement markets exhibit structural persistence—once firms gain privileged access via political contributions, their contract volume remains insulated from competitive pressures. To match this pattern, our model must allow for donor firms to have preferential access to discretionary contracts, face lower uncertainty in bidding, or maintain a cost advantage that allows them to persist over multiple election cycles. By ensuring that our structural model replicates these additional empirical moments, we aim to provide a comprehensive representation of the long-term distortions in public procurement markets driven by political contributions.

## 7.2 Model Structure

**Timing** Time is discrete with a finite horizon $t = 0, 1, 2, \ldots, T$. There are a finite number of incumbent firms within the public procurement system, indexed by $i$.[15] At any period $t$, each incumbent firm is in one of four states, $d_{it} \in \{\text{OO, OD, DO, DD}\}$. The state of the public procurement system at time $t$ is represented by the aggregation, $d_t \equiv \{d_{it}\}_i = (N_t^{OO}, N_t^{OD}, N_t^{DO}, N_t^{DD})$, where $N_t^{OO}$ denotes the number of firms that did not donate in the previous election and are not donating in the upcoming campaign, $N_t^{OD}$ represents the number of firms that did not donate in the previous election but are donating in the upcoming campaign, $N_t^{DO}$ represents the number of firms that donated in the previous election but are not donating in the upcoming campaign, and $N_t^{DD}$ represents the number of firms that donated in both the previous and upcoming elections.

The model begins in period 0 with $N_0^{OO} \geq 0$ and $N_0^{DO} \geq 0$ incumbents who are non-donors in this

[15] We take individual contractors as sole proprietorship firms, as is usually assumed in the public procurement system.

electoral cycle, and zero firms donating for the upcoming election ($N_0^{OD} = 0, N_0^{DD} = 0$). At the start of each period, a potentially infinite number of entrants consider entering and competing in the public procurement system. They enter sequentially until the expected value of entry falls below the entry cost $\kappa^{ent}$. We assume entry firms are not yet donors for the current election cycle, but they could have been donors in the past. In each period, incumbents may exit permanently, continue operating without donating to the upcoming election, or operate and donate to the upcoming election by paying a sunk cost $\kappa$ to procure goods and services at a different (most likely lower)[16] production cost in the next electoral cycle. A post-donation incumbent decides whether to exit or remain in the public procurement system.[17]

The following features of the game are common knowledge to all firms:

- Each period $t$ starts with free sequential and costly entry, followed by a competitive period where each firm earns period profit $\pi_t(d_{it}, d_{-it})$ based on public sector demand and cost conditions.
- After the competition period, $N_t^{OO}$ and $N_t^{DO}$ incumbent firms draw i.i.d private cost shocks $\varepsilon_{it}^{OO} = (\varepsilon_{it}^{1,OO}, \varepsilon_{it}^{2,OO}, \varepsilon_{it}^{3,OO})$, $\varepsilon_{it}^{DO} = (\varepsilon_{it}^{1,DO}, \varepsilon_{it}^{2,DO}, \varepsilon_{it}^{3,DO})$ and simultaneously choose actions $a_{it}^{OO}, a_{it}^{DO}$ both in the choice set {exit, stay, stay & donate}.
- Once these actions are observed, $N_t^{OD}, N_t^{DD}$ donating firms, (excluding those non-donor firms which just decided to donate) draw i.i.d private cost shocks $\varepsilon_{it}^{OD} = (\varepsilon_{it}^{1,OD}, \varepsilon_{it}^{2,OD})$, $\varepsilon_{it}^{DD} = (\varepsilon_{it}^{1,DD}, \varepsilon_{it}^{2,DD})$, respectively; and simultaneously choose actions $a_{it}^{OD}, a_{it}^{DD}$ in the choice set {exit, stay}

Private cost shocks arise from each firm's idiosyncratic but transient conditions. We focus on anonymous, type-symmetric pure strategies that convert these cost fluctuations into discrete choices, similar to a static entry game with private information. For simplicity and for estimation purposes, we assume all $\varepsilon_{it}$ shocks are i.i.d type-1 extreme value.

[16] The marginal cost difference reflects the direct benefit of donating. We will use this for identification: The greater the market share of donor firms relative to non-donor firms, the lower the implied marginal cost for donors compared to non-donors. This cost advantage might stem from reduced market competition uncertainty or decreased operational risk due to the insurance given by the government.

[17] Our model builds on and extends Igami (2018), which examines the industry dynamics of offshoring. Similar to offshoring decisions, political donations entail significant sunk costs aimed at securing a competitive advantage in public procurement contracts. Firms in the hard disk drive industry, as studied by Igami (2018), incurred substantial relocation costs to reduce production expenses and sustain market viability. Likewise, political donations can be seen as a strategic investment, positioning firms favorably in the procurement process—much as offshoring serves as a cost-reducing investment essential for competitiveness in a global oligopoly.

On top of variable profits $\pi_t(d_{it}, d_{-it})$, active firms might additionally incur in either fixed costs or fringe benefits of operation denoted by $\phi$, which are state specific.

**Per-period profit** Each period the public sector demand $D_t$, production-cost conditions $C_t$, the firm's own state $d_{it}$, and the other firms' status $d_{-it}$ completely determine the form's period profit: $\pi_{it} = \pi(D_t, C_t, d_{it}, d_{-it})$. The demand $D_t$ establishes the link between the aggregate value and quantity of contracts, while the cost function represents the relationship between each firm's output and production costs, taking into account its donor status. As we explain below, $D_t$ and $C_t$ will be recovered from the data.

We also assume Cournot competition among firms in each period and analyze an anonymous, type-symmetric Nash equilibrium. As a result, the market structure, represented by the industry state $d_t$, which includes $d_{it}$, along with $D_t$ and $C_t$, fully determines each firm's equilibrium profit during the competition period. This method allows us to succinctly address the dynamic game of donation and entry/exit decisions within a compact state space, despite the large number of firms observed in the data.

**Dynamic Optimization 1: Active firms' problem:** Firms make dynamic discrete choices regarding entry, exit, and donations to maximize their expected values. They discount their future profit streams by a factor $\beta \in (0,1)$, maintaining rational expectations about both the endogenous evolution of market structure and the exogenous evolution of demand and production costs. The dynamic programming problems of active firms at different states are

$$V_t^{OO}(d_t, \varepsilon_{it}^{OO}) = \pi_t^{OO}(d_t, \cdot) + \max_{a \in \mathscr{A}(s)} \begin{cases} \varepsilon_{it}^{1,OO}, & \text{if a = exit} \\ \phi^{OO} + \beta \mathbb{E}\left[V_{t+1}^{OO}(d_{t+1}, \varepsilon_{it+1}^{OO}) | d_t, \varepsilon_{it}^{OO}\right] + \varepsilon_{it}^{2,OO}, & \text{if a = stay} \\ \phi^{OD} + \beta \mathbb{E}\left[V_{t+1}^{OD}(d_{t+1}, \varepsilon_{it+1}^{OD}) | d_t, \varepsilon_{it}^{OO}\right] - \kappa + \varepsilon_{it}^{3,OO} & \text{if a = stay, donate} \end{cases}$$

$$V_t^{DO}(d_t, \varepsilon_{it}^{DO}) = \pi_t^{DO}(d_t, \cdot) + \max_{a \in \mathscr{A}(s)} \begin{cases} \varepsilon_{it}^{1,DO}, & \text{if a = exit} \\ \phi^{DO} + \beta \mathbb{E}\left[V_{t+1}^{DO}(d_{t+1}, \varepsilon_{it+1}^{DO}) | d_t, \varepsilon_{it}^{DO}\right] + \varepsilon_{it}^{2,DO}, & \text{if a = stay} \\ \phi^{DD} + \beta \mathbb{E}\left[V_{t+1}^{DD}(d_{t+1}, \varepsilon_{it+1}^{DD}) | d_t, \varepsilon_{it}^{DO}\right] - \kappa + \varepsilon_{it}^{3,DO} & \text{if a = stay, donate} \end{cases}$$

$$V_t^{OD}(d_t, \varepsilon_{it}^{OD}) = \pi_t^{OD}(d_t, \cdot) + \max_{a \in \mathscr{A}(s)} \begin{cases} \varepsilon_{it}^{1,OD}, & \text{if a = exit} \\ \phi^{OD} + \beta \mathbb{E}\left[V_{t+1}^{OD}(d_{t+1}, \varepsilon_{it+1}^{OD}) | d_t, \varepsilon_{it}^{OD}\right] + \varepsilon_{it}^{2,OD} & \text{if a = stay} \end{cases}$$

$$V_t^{DD}(d_t, \varepsilon_{it}^{DD}) = \pi_t^{DD}(d_t, \cdot) + \max_{a \in \mathscr{A}(s)} \left\{ \begin{array}{ll} \varepsilon_{it}^{1,DD}, & \text{if a = exit} \\ \phi^{DD} + \beta \mathbb{E}\left[ V_{t+1}^{DD}(d_{t+1}, \varepsilon_{it+1}^{DD}) | d_t, \varepsilon_{it}^{DD} \right] + \varepsilon_{it}^{2,DD} & \text{if a = stay} \end{array} \right\} \quad (5)$$

All subject to the perceived law of motion governing $d_t$. The expectations are over the other firms' choices, and hence over the realizations of their private cost shocks.

**Dynamic Optimization 2: New entrants:** Normalizing the outside option for all entrants to 0, the problem of each one of them becomes: $\max\{0, V_t^{\cdot}(d_t) - \kappa^{ent}\}$, which by free entry condition implies that $V_t^{\cdot}(d_t) \leq \kappa^{ent}$.

**Equilibrium** We address the finite-horizon sequential-move dynamic discrete game with private information by seeking a Perfect Bayesian Equilibrium (PBE) or a Sequential Equilibrium. Our analysis is concentrated on type-symmetric pure choice strategies. Considering the sequential nature of entry, the presence of transient private independent and identically distributed (i.i.d.) information costs, and the finite-horizon scope of the problem, we can ensure the uniqueness of our equilibrium.

## 7.3 Estimation

To estimate the empirical model we proceed in four steps. First, we focus on individual municipalities and only periods around the last two electoral cycles. We solve the model treating each municipality as a single market. Second, we estimate the public sector demand for contracts in that municipality by using public procurement transaction data on number of contracts and contract values. For this step we use the RCT intervention as a shifter force for the demand for contracts. Third, we recover the marginal costs of production of donors and non-donors in each state by using the estimated demand and the observed market shares of donors and non-donors. Based on this information we calculate per-period profits for each type of firm in each year under any market structure $d_t$. Finally, we embed these period profit into the dynamic discrete game of donation and entry/exit, which we solve to estimate the sunk costs of donating ($\kappa$), the cost of entry ($\kappa^{ent}$), and the different fringe benefits or fixed costs of operation ($\phi^{OO}, \phi^{DO}, \phi^{OD}, \phi^{DD}$).

## 7.4 Preliminary Results: A one electoral cycle model

We start by estimating a simplified version of the model with only one electoral cycle and, therefore, a reduced set of parameters. In this single-cycle setting, we collapse the game so that each

firm makes a one-time decision whether to donate (and thus become a "donor") or remain a non-donor. This approach allows us to focus on identifying the sunk cost of donating and the difference in operating (marginal or fixed) costs between donor and non-donor firms. By leveraging variation from the RCT—particularly the exogenous increase in perceived oversight and reputational risks—we can then pinpoint how changes in the expected cost-benefit ratio of donating affect these entry and donation decisions. Despite its parsimonious structure, this one-cycle model provides preliminary estimates that reveal a sizable sunk cost of becoming a donor and a large entry cost to the procurement system. In turn, these estimates serve as a key input for the full multi-period model, where firms face repeated decisions about whether to donate across multiple electoral cycles.

**Demand estimation and marginal cost** To estimate the demand, we start by aggregating our data using a combination of donation status and contract type. From each industry/municipality, we calculate the total market share and the average contract value as the price faced by the respective sector. Following Igami (2018), we estimate the demand parameter by regressing the log-transformed share of contracts on the average contract value. We use the RCT treatment assignment described in Section 6 as a demand shifter to induce exogenous variation in prices and obtain a causal estimate for the demand of contracts. Specifically, we interact the treatment assignment for the *list* and *no list* treatments with a dummy equal to one for 2024 (the year of the shock to prices) and zero before the year of the RCT implementation. We estimate the IV through two-step generalized method of moments. Table 7 displays the demand estimates. Column (1) shows the OLS estimate, and column (2) the IV estimate. Both coefficients are negative and statistically significant. We use the IV coefficient to calculate the marginal cost.

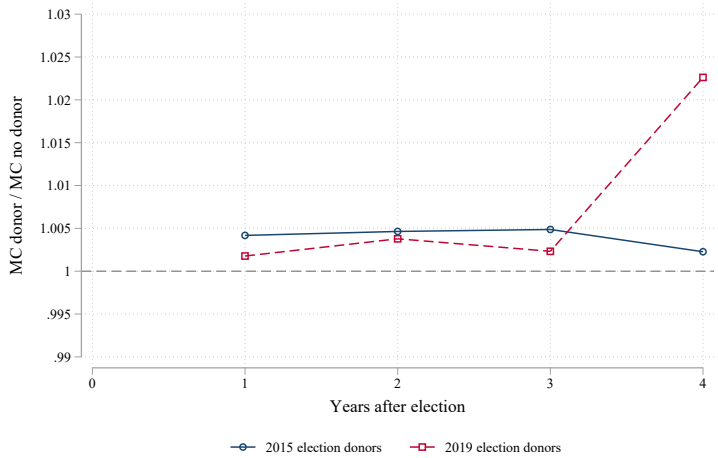
**Table 7:** Demand estimates

| Dep. Variable | Log(share of contracts) | |
|---|---|---|
| | OLS | IV |
| | (1) | (2) |
| Contract price (million COP) | -0.007*** | -0.131** |
| | (0.001) | (0.058) |
| N. of obs. | 61,035 | 61,035 |
| Municipality FE | Yes | Yes |
| Year FE | Yes | Yes |

*Notes:* The unit of analysis is the municipality-type of contract-year, estimating a linear regression using as outcome the log share of market shares and as explanatory variable the price of the average contract value by municipality-type of contract-year. We include municipality and year fixed effects. For the column (2) use as instrument the treatment assignment from the RCT described in section 6, interacted with a dummy for the year 2024, which is the period when the RCT was implemented, generating an exogenous shock to procurement market. The IV estimate is obtained through two-step GMM. Standard errors clustered at the municipality-type of contract level in parentheses. $^{*}p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

To calculate the marginal cost, we study each electoral round independently. Using the IV estimates for the demand, the observed market shares, and the firms' first-order conditions from Cournot competition, we calculate the marginal costs of production for donors and non-donors, separating for periods around the 2015 elections (2012-2019), and periods around the 2019 elections (2016-2023). All our analysis from now on differentiate between these two electoral cycles. Figure 11 show the average ratio between marginal costs of donors and non-donors across municipalities, starting the year immediately after an election. For all the periods after the elections, donors report a larger marginal cost, suggesting that donor tend to be less efficient in contract execution.

**Figure 11:** Marginal cost ratio donors vs non-donors




*Note:* The figure reports the ratio between the implied marginal cost of donors relative to the implied marginal cost of non-donors. The marginal cost is calculated using data on prices, quantity shares and the estimated demand parameter, based on data aggregated at the municipality-by-type of contract-by-year.

**Cost of donation and entry to the procurement system** To estimate the model we decrease the dimensionality of our set of contractor by aggregating our data for a representative agent-year level. We define a representative agent based on the donation status (donor / non-donor), and whether the contractor is a firm or an individual. The parameters then are estimated based on the representative agents ever having a contract within a municipality. The entry, exit, and donation decisions are also based on the representative agent.

Table 8 shows the average maximum likelihood estimates of the base sunk cost of donation $\kappa$, the fixed cost of operation $\phi$, and the logit scaling parameter $\rho$. Panel A show the estimates for the 2015 elections, covering the period between 2012 and 2019, while Panel B report on the 2019 elections, covering the period 2016-2023. The base sunk cost of donation and cost of operation are in million COP. Our estimates calibrate $\beta = 0.8$ following the literature and set $\delta = 1.05$. Our

decision of $\delta$ is based on the sensitivity analysis and fit in Table 9. Because we are aggregating our data by municipality at the donor/no donor $\times$ individual/firm level to decrease dimensionality, the parameters are estimated in a smaller sample of municipalities for which such representative agents report variation in entry, exit, and donation decisions.

**Table 8:** Estimates of dynamic parameters

| | Mean | Median | Std. Dev. | Min. | Max. | Obs. |
|---|---|---|---|---|---|---|
| **Panel A: 2015 elections (2012-2019)** | | | | | | |
| Base sunk cost of donation ($\kappa$) - million COP | 1.918 | 1.627 | 1.443 | 0.001 | 10.657 | 287 |
| Fixed cost of operation ($\phi$) - million COP | 0.075 | 0.101 | 0.335 | -2.917 | 1.383 | 287 |
| Log scaling parameter ($\rho$) | 0.934 | 0.755 | 0.676 | 0.000 | 6.317 | 287 |
| Log likelihood | -14.578 | -13.734 | 5.349 | -30.332 | -3.915 | 287 |
| **Panel B: 2019 elections (2016-2023)** | | | | | | |
| Base sunk cost of donation ($\kappa$) - million COP | 1.843 | 1.341 | 1.753 | 0.000 | 11.087 | 336 |
| Fixed cost of operation ($\phi$) - million COP | 0.019 | 0.076 | 0.419 | -2.666 | 2.028 | 336 |
| Log scaling parameter ($\rho$) | 0.945 | 0.707 | 0.894 | 0.000 | 6.245 | 336 |
| Log likelihood | -14.322 | -13.789 | 4.379 | -31.005 | -3.657 | 336 |

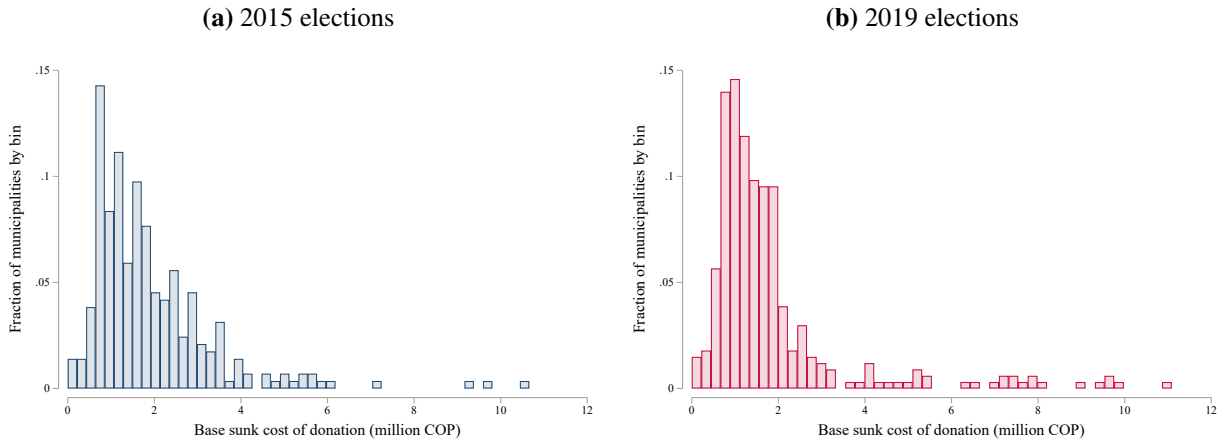
*Notes:* The table show the average across municipalities of the parameters of the model estimated using Maximum Likelihood Estimation under different decisions of the cost of donation $\delta$. Panel A report on the 2015 elections, covering the period 2012-2019. Panel B report on the 2019 elections, covering the period 2016-2023. $\kappa$ and $\phi$ are in million COP.

On average, the base sunk cost of donation is approximately 2 million COP in both elections, ranging from no cost up to 10.6-11 million COP. Figure 12 plots the distribution of the $\kappa$ estimates. $\phi$ is on average positive in both elections, although very close to zero. Nevertheless, across municipalities it can range from negative 2.6-3 million COP (fixed cost) up to 0.9-1.4 million COP (fixed profit). Figure 12 shows the distribution the $\kappa$ estimates by municipality and by election sample. Both distribution display a similar pattern.

Table 9 show sensitivity analysis for the estimated parameters to changes in the rate of change of the donation cost $\delta$. The estimates for the base sunk cost of donation decrease with higher values of $\delta$, consistent with a higher cost of donation over time, thus the model setting a lower cost to match the data. The fixed cost estimates increase slightly but remain in less than a tenth of a million COP. We define our preferred specification to have $\delta = 1.05$ as, the largest log likelihoods are reported for $\delta = 1.05$ and $\delta 1.10$ for both elections. Nevertheless, the larger for 2015 elections is reported for $\delta = 1.10$, while for 2019 elections is $\delta = 1.05$. As a conservative decision and to hold constant $\delta$ across electoral cycles we use $\delta = 1.05$. For such parameter value it is also the case that the base sunk cost of both elections are closer to each other.

The estimates in Figure 13 suggest that donors face substantial barriers to participating in public procurement markets. Panel (a) indicates that the sunk cost of donation, given by $\delta^t \times \kappa$, grows at

**Figure 12:** Distribution of sunk cost of donation - $\hat{\kappa}$




*Note:* The figure plots the distribution of the municipal estimates of $\kappa$. Panel (a) report for the 2015 elections, covering the period 2012-2019. Panel (b) reports on the 2019 elections, covering 2019-2023.

**Table 9:** Sensitivity analysis to $\delta$

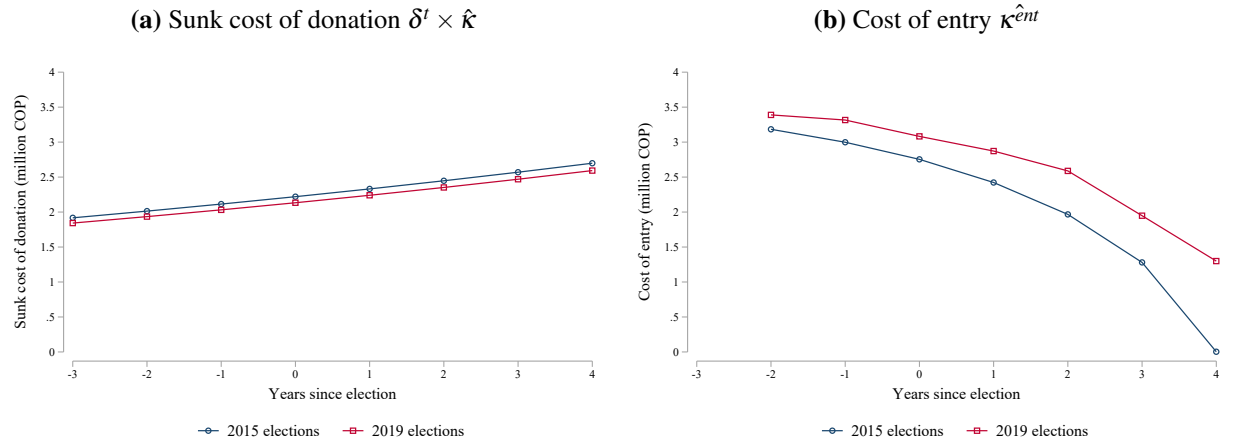
| | $\delta = 0.90$ | $\delta = 0.95$ | $\delta = 1.00$ | $\delta = 1.05$ | $\delta = 1.10$ |
|---|---|---|---|---|---|
| **Panel A: 2015 elections (2012-2019)** | | | | | |
| Base sunk cost of donation ($\kappa$) | 2.744 | 2.692 | 2.268 | 1.918 | 2.203 |
| Fixed cost of operation ($\phi$) | 0.045 | 0.053 | 0.094 | 0.075 | 0.082 |
| Log scaling parameter ($\rho$) | 0.843 | 0.905 | 0.920 | 0.934 | 1.026 |
| Log likelihood | -14.678 | -14.598 | -14.830 | -14.578 | -14.346 |
| **Panel B: 2019 elections (2016-2023)** | | | | | |
| Base sunk cost of donation ($\kappa$) | 2.286 | 1.998 | 1.965 | 1.843 | 1.778 |
| Fixed cost of operation ($\phi$) | 0.012 | 0.028 | 0.022 | 0.019 | 0.049 |
| Log scaling parameter ($\rho$) | 0.888 | 0.828 | 0.938 | 0.945 | 1.002 |
| Log likelihood | -14.674 | -14.508 | -14.613 | -14.322 | -14.459 |

*Notes:* The table show the average across municipalities of the parameters of the model estimated using Maximum Likelihood Estimation under different decisions of the cost of donation $\delta$. Panel A report on the 2015 elections, covering the period 2012-2019. Panel B report on the 2019 elections, covering the period 2016-2023. $\kappa$ and $\phi$ are in million COP.

an annual rate of 5% (assuming $\delta = 1.05$) and reaches approximately 2.5 million COP by the end of the mayoral term. In contrast, Panel (b) shows that the average cost of entry across municipalities begins between 3 and 3.5 million COP and subsequently declines over time. Taken together, these two components imply that the overall cost of procurement participation—comprising the donation (2.5 million COP) plus entry costs (3 million COP)—is approximately 5.5 million COP at baseline. As Table 2 reveals, this amount constitutes nearly 60% of the median contract value eventually awarded to donors, highlighting the substantial portion of potential returns consumed by entry barriers.

Further evidence from Figure 12 reveals that many donors contribute amounts exceeding the

**Figure 13:** Evolution cost of donation and cost of entry




*Note:* The figure displays the average parameter estimates across municipalities over time. Panel (a) reports the average sunk cost of donation $\kappa$. Panel (b) report on the average estimate cost of entry $\kappa^{ent}$. The blue line reports for the estimates for the 2015 elections, covering the period 2012-2019. The red line reports on the estimates for the 2019 elections, covering the periods 2016-2023.

baseline cost, suggesting that the realized cost of entry can be even higher in practice. These excess costs likely reflect a variety of "hidden" frictions—ranging from administrative to logistical requirements—that dissuade many prospective donors from participating, despite the attractive expected payoffs of securing a public contract. While some individuals or firms may be willing to bear these burdens as a cost of doing business, others are deterred by the financial and bureaucratic hurdles.

Figure 13 also illustrates the evolution of these costs over the election cycles. Panel (a) shows a steady increase in sunk donation costs, $\delta^t \times \kappa$, for both the 2015 and 2019 cohorts. This upward trend reflects the cumulative nature of recurring financial obligations. Meanwhile, Panel (b) presents a decline in the cost of entry, $\delta^t \times \hat{\kappa}^{ent}$, which appears more pronounced during the 2015 election cycle than in 2019. These patterns suggest that procurement markets across municipalities may adjust over time—lowering entry barriers in some contexts—yet the degree and speed of this adjustment vary by election cycle and donor status. For instance, the persistently higher cost of entry for the 2019 cohort may stem from factors such as intensified competition, changes in regulatory oversight, or adverse market conditions that impede donors' ability to recoup their initial expenditures.

Collectively, these findings underscore the role of substantial sunk and ongoing costs in shaping firms' incentives to enter public procurement. Barriers on both the donation and entry margins not only limit participation but also have critical implications for market structure and competition,

underscoring the importance of understanding how policy changes, administrative reforms, or evolving market conditions can amplify or attenuate these costs.

**Potential Policy Counterfactuals** We preliminarily propose the following counterfactuals to evaluate the effectiveness of different strategies aimed at curbing political distortions in public procurement. First, we consider imposing a cap or ban on repeat contractors who were donors, directly targeting the persistence channel identified in the pay-to-play mechanism. By preventing firms that have donated in past election cycles from participating again, this policy could reduce the long-term insider advantage and encourage greater contractor turnover. Second, we introduce a cool-off period that bars donors from receiving public contracts for a fixed number of years after making a political contribution. This measure would increase the cost of donating by delaying any potential returns, discouraging firms from using contributions as a short-term investment strategy to access procurement contracts. Third, we simulate a scaling-up of credible oversight, akin to permanently institutionalizing the most effective arm of our RCT—the Inspector General treatment with donor list disclosure. This counterfactual increases the perceived threat of detection and sanction for mayors who allocate contracts to donors, thereby shifting the relative value of donor versus non-donor status in equilibrium. Fourth, we evaluate a centralized procurement reform, where certain categories of contracts—especially high-value or discretionary ones—are allocated through national-level procedures or randomized selection. This would reduce local discretion, weaken the payoff from cultivating political ties, and potentially improve efficiency.

# 8 Conclusion

This paper studies whether political campaign donations operate as a form of pay-to-play in Colombian public procurement, generating long-term distortions in the allocation of public contracts. Our evidence is drawn from a comprehensive dataset linking donations to mayoral campaigns with the universe of public procurement contracts in Colombia. The data reveal that donors not only receive more (and higher-valued) contracts in the mayoral period immediately following an election but also continue to secure public contracts in subsequent administrations, after the original political connection is lost. This persistent effect suggests that donations effectively confer long-term "insider" status in the public procurement system, rather than merely short-term reciprocity from the winning politician.

The reduced-form evidence also indicates that donors frequently exhibit weaker ex-ante performance and are more likely to manipulate procurement processes—both by clustering bids just below

competitive thresholds and by incurring higher contract overruns relative to non-donors. Together, these behaviors underscore efficiency and resource-allocation costs associated with pay-to-play dynamics, as donors appear able to extract rents through additional discretion while delivering lower-quality services.

To address these distortions, we implemented a large-scale nationwide randomized controlled trial in partnership with Colombia's Office of the Inspector General, investigative journalists, and NGOs specialized in anti-corruption. Our intervention informs newly sworn mayors about the legal risks of awarding contracts to donors and varies both the identity of the oversight authority and the degree of donor-specific information disclosed. The findings indicate that monitoring can curb kickbacks only when there is a credible threat of punishment and clear information on the identity of donors. Specifically, informing mayors that the Inspector General's office is copied on the communication, along with a detailed list of donors, significantly reduces the probability and value of donor contracts. In contrast, treatments that lack either (i) a credible legal enforcer or (ii) explicit donor details do not yield measurable reductions in contract favoritism.

Moreover, donors who contributed in past electoral cycles—and already acquired "insider" status—continue to capture substantial public contracts under new administrations, regardless of the politician in power. Thus, while the deterrence messages effectively reduce kickbacks to *new* donors, they do not dislodge entrenched contractors with prior donation histories. This pattern is consistent with a pay-to-play mechanism in which the largest benefit from donating is the long-run entry into and persistence in government contracting, rather than a one-shot quid pro quo.

Motivated by these findings, we develop a structural model of pay-to-play, incorporating contracting frictions, sunk donation costs, and donor versus non-donor cost differentials. By leveraging exogenous demand shifts induced by the RCT, we recover key parameters such as the entry cost into contracting and the sunk cost associated with donations. The model highlights how high returns to donation can coexist with relatively few donors: not all firms may find the up-front costs—both monetary and hidden—to be worthwhile. Furthermore, once a firm becomes a donor, it retains privileged access that does not disappear even if a favored candidate is later voted out of office.

Two sets of policy lessons emerge. First, traditional approaches that rely on general public monitoring or vague legal threats often prove insufficient when kickbacks hinge upon *specific* donors and require *credible* legal liability. Oversight efforts are most effective when the monitoring entity has the authority to impose sanctions *and* has immediate visibility into donor identities—underscoring the role of targeted transparency in constraining corruption in procurement. Second, our preliminary estimation of the structural model and RCT results both underscore that pay-to-play might be

rooted in persistent distortions linked to donors' cost of entry into the public contracting system. Policy reforms aimed solely at curtailing quid pro quo in a single administration will have limited effectiveness unless they also disrupt the long-run persistence channel. Examples might include bans or stricter limits on repeat donations by previously successful contractors, improved cross-administration data sharing on contractor performance, or more robust ex-ante eligibility screening for new entrants.

Overall, our paper highlights that favoritism in procurement through campaign contributions is both substantial and persistent, with robust evidence of allocative inefficiencies. Nonetheless, relatively low-cost innovations in oversight—particularly from a legally empowered agency with direct information on donor identities—can curb pay-to-play for new donors. Future work could explore whether more sweeping reforms—such as corporate donation bans, stricter campaign finance limits, or the introduction of comprehensive digital auditing—could eliminate these distortions for good.

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# Online Appendix

# A Political donations and public procurement

## A.1 Public procurement categories

Public procurement in Colombia is governed by Law 80 of 1993, establishing the General Contracting Statute of the Public Administration. The law defines the public procurement categories and the requisites for public agencies to use each type of procedure. Law 1150 of 2007 further introduced efficiency and transparency measures to Law 80 of 1993, most of which consisted of stronger requisites for participating in competitive procedures. Below we describe each procurement.

**Direct (discretionary)**: contracts with a maximum of one year to support bureaucratic/administrative tasks. The public agency can define the technical requirements and assign the contractor with complete discretion as long as the selected contractor meets the requirements defined for the contract execution. The contractor finishing a contract can receive another through direct procurement immediately after, and there are no value limits.

**Minimum value and short selection (discretionary)**: open bid contracts whose value is lower than 10% of the yearly budget of the public agency and are assigned in less than a day to the offer with the minimum value. Public offices issuing minimum value contracts do not require the establishment of a necessary level of experience or technical skills.

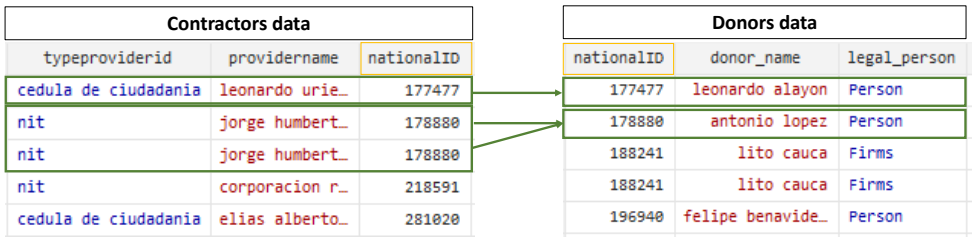
**Merits contest**: assigned based on open-bid competition. A technical proposal is evaluated, and the best-suited for the specific requirement is chosen. Contest assigned contracts are usually used for consultancy or technical assistance in public program execution.

**Open tender**: open-bid competition for contracts with large values and long-term execution. Usually used for infrastructure projects. Proposals are scored based on different technical aspects, with price being the main factor defining the assignment.

**Special procurement regime**: some public agencies are not subject to the regulations established by Law 80 of 1993, so they do not need to follow the categories described above but instead have their own procedures, adjusted as required depending on their needs. Public universities and hospitals are part of the special procurement regime.

## A.2 Additional details

**Figure A1:** Link donors and contractors with National ID



**Contractors data**

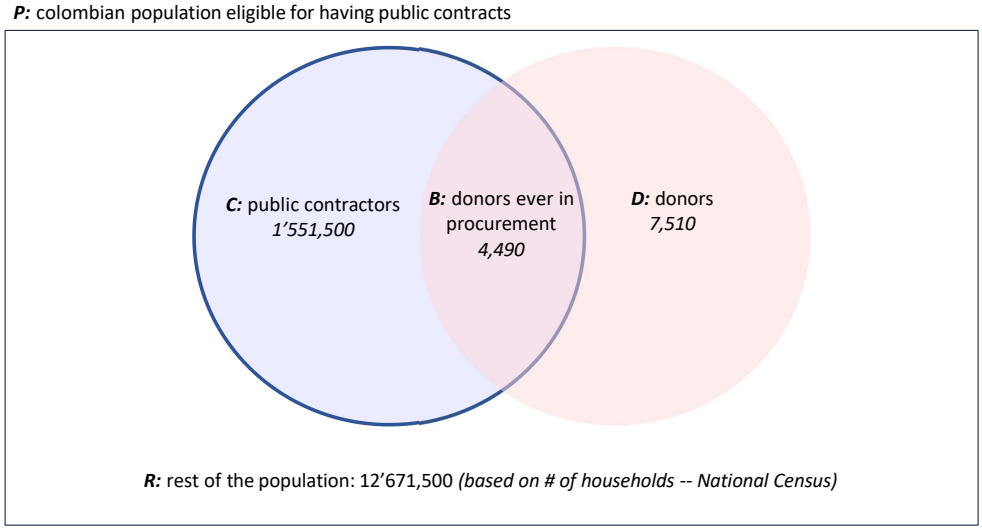
| typeproviderid | providername | nationalID |
|---|---|---|
| cedula de ciudadania | leonardo urie… | 177477 |
| nit | jorge humbert… | 178880 |
| nit | jorge humbert… | 178880 |
| nit | corporacion r… | 218591 |
| cedula de ciudadania | elias alberto… | 281020 |

**Donors data**

| nationalID | donor_name | legal_person |
|---|---|---|
| 177477 | leonardo alayon | Person |
| 178880 | antonio lopez | Person |
| 188241 | lito cauca | Firms |
| 188241 | lito cauca | Firms |
| 196940 | felipe benavide… | Person |

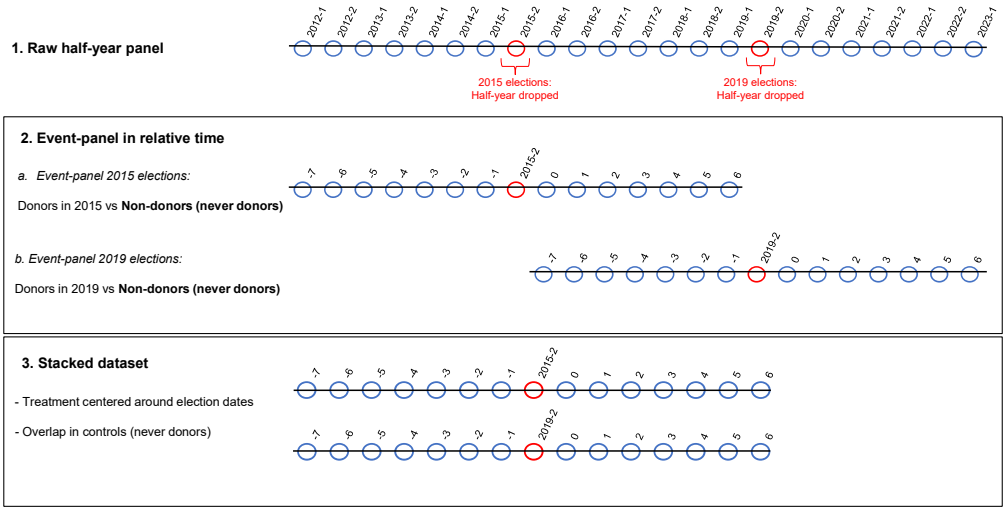
*Note:* The figure exemplifies how the data is merged between donors and contractors dataset. Note that the national ID coincides for the same individuals in both datasets. In the contractors dataset, the column *typeproviderid* takes the value of "cedula de ciudadania" and "nit", which are the names of the ID for individuals and firms respectively. Thus we are able to identify if the contract was awarded to a firm or an individual. In the donors data, the column *legal_person* identifies whether the donor was a person or a firm. The first match between the two datasets correspond to a Person-person match, so the same individual donating is receiving the contract. The second match in the donors sample is merged with two observations. The ID in the donors data is identified as person, while the column *typeproviderid* in the contractors identifies the two observations as firms. The merged firms in this case are identified as donors a long as the national ID coincide with a person donating, which usually corresponds to the CEO or manager. In cases where the national ID in the donors is identified as firm in both datasets, firms are also defined as donors.

**Figure A2:** Sample description




*Note:* The figure illustrates the sample that is being used for the estimation exercises. Our data consist of the population of contractors (blue circle) and population of donors (red circle), both of which are a subset of the Colombian population. From the red circle, we only observe outcomes for those that have ever being in the procurement system (intersection between blue and red). Thus, outcomes for donors not receiving contracts are coded as zero. We simulate the population of non-donors never registered in the procurement system (rest of the population) based on the number of households reported by the 2018 National Census. We assume that each household represents at leas one individual who would be eligible for obtaining a public contract.

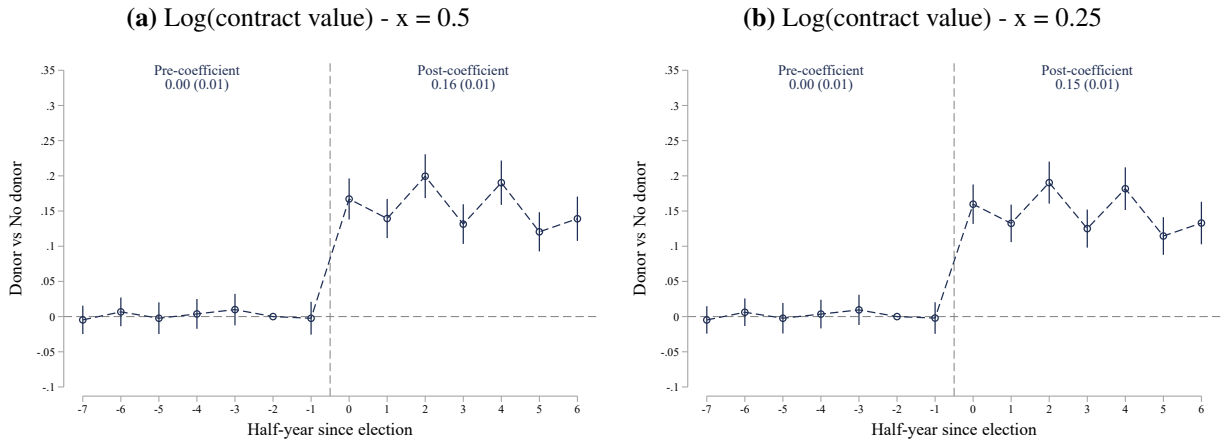
**Figure A3:** Stacking procedure description




*Note:* The figure describes the stacking procedure. The first panel illustrates the structure of the raw data. Blue half-years will be included in the stacking. Red half-years include the month of elections (October) and are dropped from the sample as: i) there is a ban in procurement between July and October, which might affect the contract distribution, and ii) during November and December of the election year, just before the winning candidate take position into office, it is not possible to disentangle whether contracts awarded come directly from the outgoing mayor or indirectly through the incoming mayor. The second panel describes for each event (election) the normalized periods before and after the electoral half-year. Note that post-periods for the 2015 elections dataset overlap with the pre-periods of the 2019 elections dataset. For each event, donors are compared with never donors. The third panel show how both event-datasets are stacked one on top of the other. As shown in the second panel, the stacking imply that there is overlap in the control units. Equation 3 include fixed effects interacted with indicators for each event-dataset to account for overlap in control units

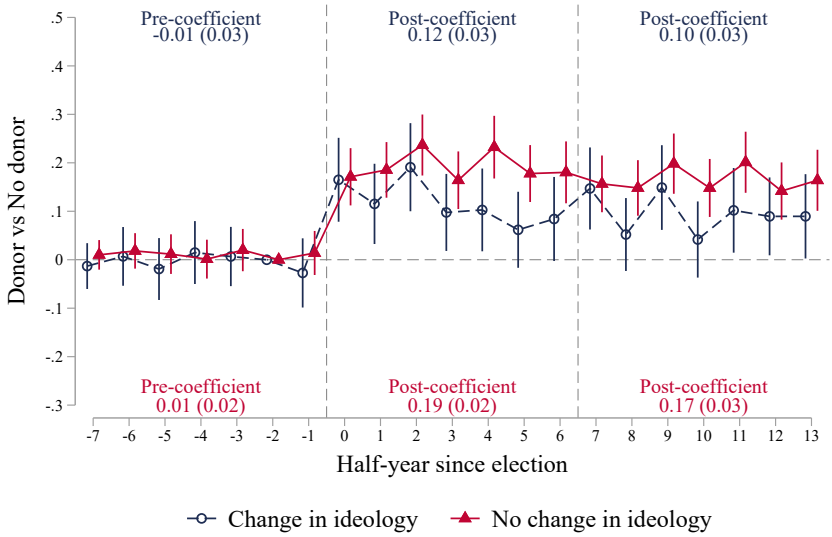
## A.3 Additional results

**Figure A4:** Effect of donating - log transformation robustness



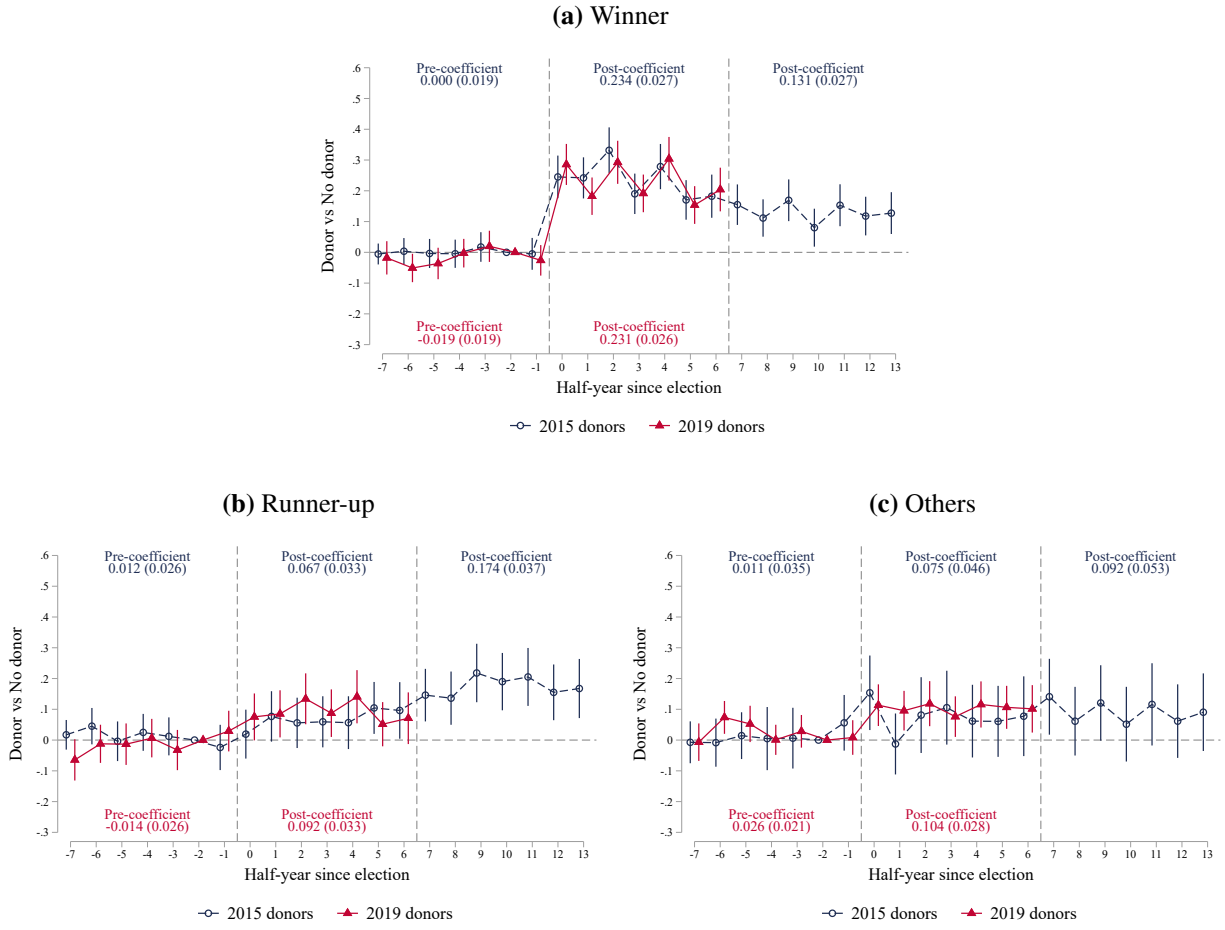

*Note:* The figure reports the point estimate and the 95% standard error from estimating an event study based on equation 3 for the overall population on the log transformed outcomes. $x$ accounts for the value of the intensive margin following Chen and Roth (2022). It also reports the pooled coefficients for the periods before and after. Standard errors clustered at the individual level.

**Figure A5:** Effect of donating on log(contract value) - change in ideology



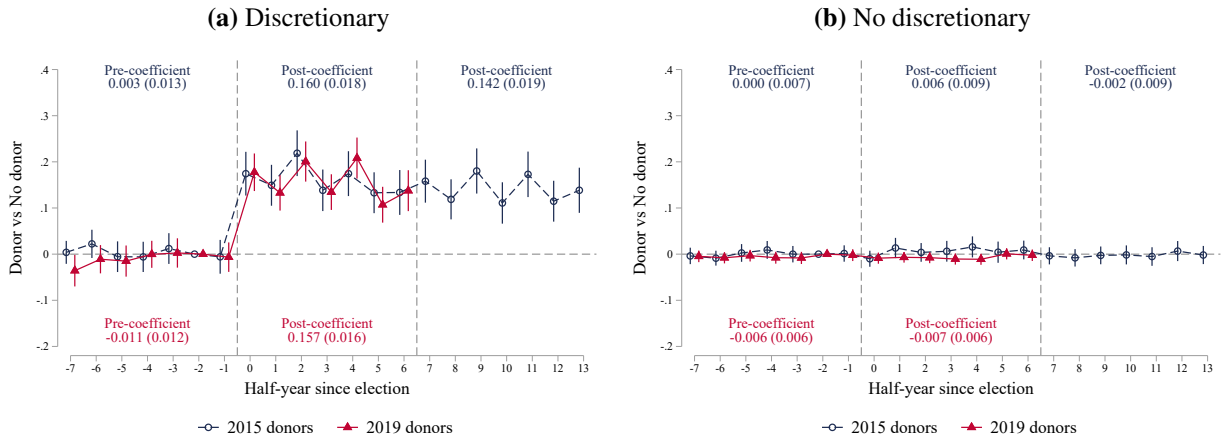

*Note:* The figure reports the point estimate and the 95% standard error from estimating an event study based on equation 3 comparing 2015 donors against non-donors, and separating between those receiving contracts in municipalities that in 2019 elections changed / not changed ideology of the political party in power. Blue coefficients are for the changing municipalities. The red coefficients are for not changing municipalities. It also reports the pooled pre and post coefficients. The contract value is log-transformed. The intensive margin of the log transformation is defined following Chen and Roth (2022) and set to $x = -1$.

**Figure A6:** Effect of donating to a political campaign on log(contract value) - supported candidate




*Note:* The figure reports the point estimate and the 95% standard error from estimating an event study based on equation 3 independently for each election sub-panel, and extending for each the respective post periods, and separating the donors' group by the position of the supported candidate in the elections, while leaving the non-donors group constant. It also reports the pooled pre and post coefficients. Standard errors clustered at the individual level. The contract value is log transformed. The intensive margin of the log transformation is defined following Chen and Roth (2022) and set to $x = -1$.

**Figure A7:** Effect of donating to political campaign on log(contract value) - procurement type




*Note:* The figure reports the point estimate and the 95% standard error from estimating an event study based on equation 3 separating by type of procurement. It also reports the pooled pre and post coefficients. Standard errors clustered at the individual level. The average contract value is log transformed. The intensive margin of the log transformation is defined following Chen and Roth (2022) and set to $x = -1$. *Discretionary:* include Direct and Minimum Value. *Non-discretionary:* include Contest, Tender and Selection.

# B Curbing kickbacks to donors of political campaigns

## B.1 Partner organizations

**Open contracting partnership – NGO**: It is an independent non-profit organization that supports government, businesses, and civil society to implement reforms and innovations aimed at better and more transparent public procurement. The organization works in over fifty countries, including Colombia. Open Contracting Partnership supports the project by allowing us to send communications (e-mail and physical) under their name.

**Anti-corruption Institute (Instituto Anticorrupción) – NGO**: it is a Colombian non-profit organization founded in 2018 with the intention of developing research and analysis on the causes and effects of corruption, as well as potential solutions to it. As partners, the Anti-corruption Institute supports the project by allowing us to send communications (e-mail and physical) under their name.

**Inspector's General Office (Procuraduría General de la Nación) – Government agency**: it is the national level government office overseeing public sector workers and presenting the required legal actions whenever a public servant is found in unlawful behavior. Part of the office's responsibility is to investigate inefficient procurement as long as the municipal mayor, contractor, and other relevant actors may be involved in corruption or other types of misbehavior. The Inspector General Office is related to the project only by being knowledgeable about the letters that will mention them as receptors of the data. We consider such coordination might increase the policy implications of the intervention, as it might increase the efficiency by which the office identifies probable unlawful behavior.

## B.2 Letter draft

Figure B1 shows a draft of the letter received by mayors. Highlighted in yellow, there is a paragraph that mentions that attached to the letter, there is the list of donors of the latest mayoral campaigns in the municipality. For the treatment arms that do not receive an attachment with the letter, there paragraph is not included. Highlighted in red there are the parts of the letter that mention an specific authority to share the donors procurement data with. It can mention the Inspector General Office (government), or multiple investigative media outlets (journalist). In cases were no authority is mentioned, the letter does not include the red highlighted text. The rest of the text remains regardless of the treatment arm.

Figure B2 shows an example of the donor list received by mayors . The first and second column mention the national ID and name of the donor, respectively. The third column mentions the name of the candidate that was supported by the donor, while the fourth mention the total donated amount. The rest of the column report whether the donor has already received contracts with the new mayoral administration, and the value of such contracts if they have received. Similarly, it also reports if the donor received contracts under the previous mayor, and the total amount of the contracts received.

**Figure B1:** Draft letter for elected mayors

Bogotá, D.C., May 1st, 2024

Dear Doctor,

**<<NAME>> <<LAST NAME>>**
Mayor of the municipality of **<<MUNICIPALITY NAME>>**

**Subject:** Political donations and public procurement in your municipality
**Copied to:** ***<<Contact government organizations or other civil society organizations>>***

As part of an independent research study, **Open Contracting Partnership (OCP)** in partnership with the **Anti-Corruption Institute (IA)** wants to remind you of the existing and specific legal restrictions on the allocation of public contracts to political campaign donors in your municipality.

As you may be aware, Colombia has adopted strict measures to promote transparency in public procurement, particularly regarding political campaign donors.

- **The Law 1474 of 2011**, in its second article, establishes that natural or legal persons who have made contributions to the political campaign of the mayor-elect ***may not receive contracts*** when such donations exceed the limits established by this law. In addition, it extends this restriction to family members of natural persons and to the legal representatives, partners, and members of the board of directors of legal entities.
- **The Statutory Law 1475 of 2011,** in its twenty-seventh article, ***prohibits political donations*** from individuals whose main income in the previous year comes from contracts with the public sector.
- Likewise, **Law 80 of 1993,** in its eighth article, establishes a detailed list of ***inabilities and incompatibilities*** for contracting with the state.

Attached to this letter you will find the list of donors to the political campaigns in your municipality during the last elections of 2023, as well as the values donated by each of them. We have included information on public procurement and donation amounts based on information available on the *Clear Accounts in Elections* page and on the *Open Data portal* as **of April 30, 2024**.

We hope that this legal framework and detailed cross-checking of information will be valuable for you to manage the public resources of your municipality and, above all, useful for complying with all the restrictions provided for by law. We remind you that as part of our process of empowering local actors and with the aim of surveillance and monitoring the contracting process, all the information contained in this letter, as well as the attached documents and databases, are being shared with ***<<Contact government organizations or other civil society organizations>>***

If you have any doubts regarding this communication or its content, we invite you to consult the portal for resolved questions: https://www.open-contracting.org/studies/donnors_study

Kind regards

The Open Contracting Partnership Colombia Team
The Anti-Corruption Institute Team.

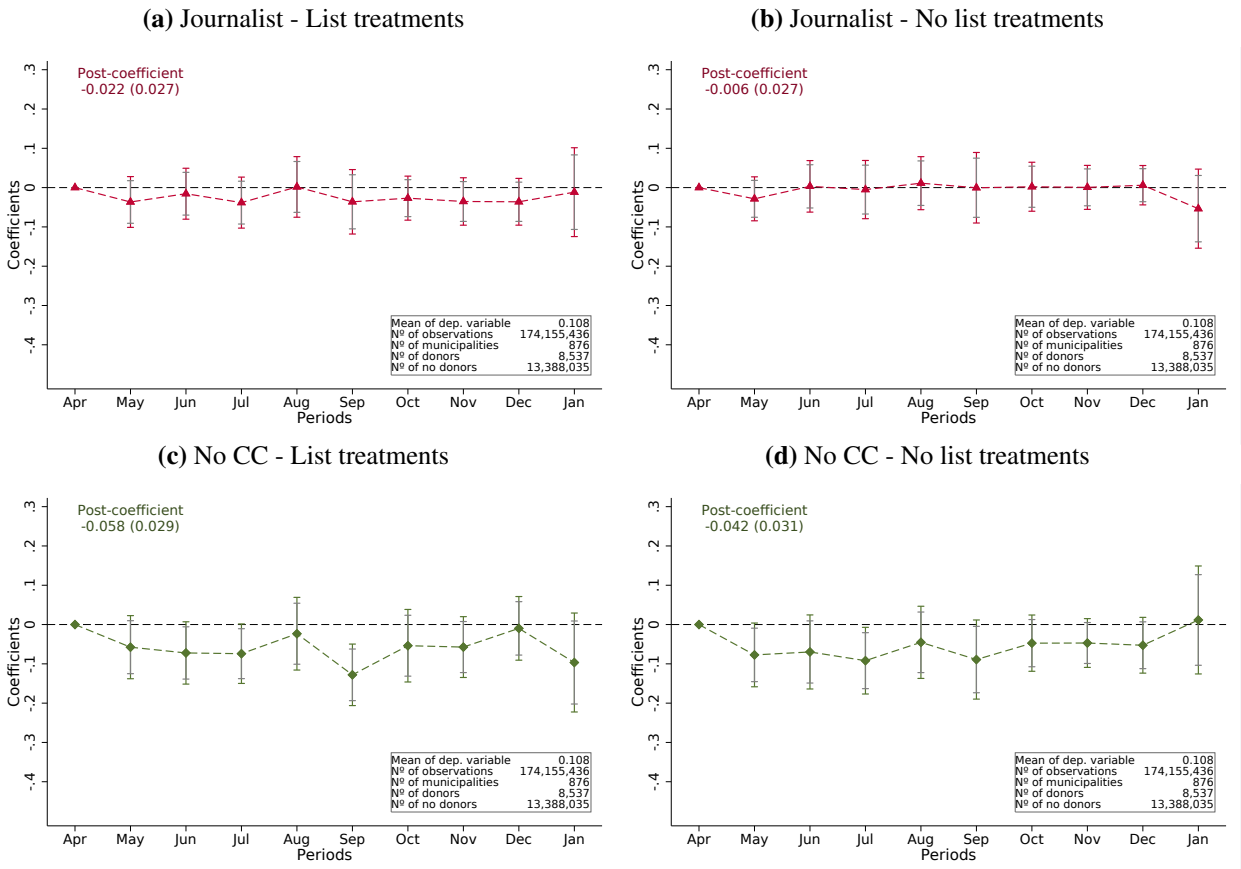
This communication is addressed solely and exclusively to the public office or person to whom reference is made. All the information mentioned and attached to this communication comes from official public sources of the Colombian Government. Open Contracting Partnership, OCP, is committed to the legal, lawful, confidential, and secure treatment of your data, so any additional information outside of what is available in the public domain will be treated in accordance with Colombian regulations on data management.

**Figure B2:** Attached donor list

| ID del donante (Cedula o NIT) | Nombre completo del donante | Dono al candidato... | Valor donado | Recibio contratos durante la actual administracion con corte al 30/04/2024 | Valor total contratado durante la actual administracion con corte al 30/04/2024 | Recibio contratos durante la administracion pasada | Valor total contratado durante la administracion pasada |
|---|---|---|---|---|---|---|---|
| 30341103 | Myriam Yolanda Rojas Mafla | Campo Elias Ramirez Padilla | $30.000.000 | No | - | No | - |
| 91179414 | Robinson Rodriguez Sarmiento | William Mantilla Serrano | $51.000.000 | No | - | Si | $120.205.904 |
| 1098610878 | Yuly Andrea Mantilla Mantilla | William Mantilla Serrano | $37.000.000 | No | - | Si | $125.232.376 |
| 9007546468 | Partido Politico Centro Democratico | William Mantilla Serrano | $10.000.000 | No | - | No | - |
| 13512558 | Sabal Mantilla Rivero | Dalida Prada Villamizar | $4.500.000 | No | - | No | - |
| 70119713 | Mario De Jesus Osorio Zora | Blanca Azucena Rodriguez Romero | $5.813.745 | No | - | No | - |
| 1095806423 | Laura Tatiana Florez Prada | Blanca Azucena Rodriguez Romero | $290.000 | No | - | No | - |
| 1095920214 | Jose Luis Prada Rueda | Mauricio Gomez Nino | $40.000.000 | No | - | No | - |
| 13826970 | Oscar Clavijo Camargo | Mauricio Gomez Nino | $7.500.000 | No | - | No | - |
| 37727297 | Jackeline Gomez Carreno | Mauricio Gomez Nino | $6.000.000 | No | - | Si | $96.072.696 |
| 1095920583 | Cristian Mauricio Gomez Perez | Mauricio Gomez Nino | $6.000.000 | No | - | No | - |

## B.3 Additional results

**Figure B3:** Impact on donor procurement outcomes - monthly post-treatment coefficients on log(contract value)




*Note:* The unit of analysis is the individual-month. The figure shows point estimates and 95% confidence intervals from estimating equation 4 on procurement outcomes. Fixed effects for randomization block $\times$ [*Donor*] $\times$ month are included. Standard errors clustered at the municipality level. The point estimates represent the effect of the treatments relative to the *Placebo* and *Control* groups post-intervention. Top panel report the coefficients in the municipalities were mayors received the list. Bottom panel show the results for municipalities that did not received the list. The bottom labels specify the *Letter* treatment copying the *Inspector General*, *Investigative Journalist*, or *No CC*. The outcome is the *Log(contract value)*, with non-recipients assigned a value of 0. The log transformation for the value follows Chen and Roth (2022), defining the intensive margin as $x = -1$ for non-recipients.

# Table B1: Balance on municipal covariates

| **Panel A: municipal procurement covariates** | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Dep. Variable | # contractors | | | # contracts | | | # discretionary | | | Contract value | | |
| Electoral cycle | 2012-2015 | 2016-2019 | 2020-2023 | 2012-2015 | 2016-2019 | 2020-2023 | 2012-2015 | 2016-2019 | 2020-2023 | 2012-2015 | 2016-2019 | 2020-2023 |
| Control | 2.126 | 3.432 | 3.510 | 0.758 | 1.754 | 1.631 | 0.379 | 0.541 | 0.754 | 34.371 | 69.746 | 99.349 |
| | (1.830) | (3.120) | (2.894) | (0.613) | (1.566) | (1.368) | (0.270) | (0.471) | (0.667) | (65.331) | (99.983) | (93.836) |
| Placebo letter | 7.282 | 8.342 | 6.123 | 1.605 | 1.897 | 1.602 | 1.004 | 0.970 | 0.973 | 65.687 | 127.594 | 173.566 |
| | (6.514) | (7.588) | (5.621) | (1.420) | (1.727) | (1.617) | (0.866) | (0.928) | (1.061) | (86.189) | (138.959) | (172.924) |
| Letter + no CC + no list | -1.604 | -2.114 | -2.338 | -0.386 | -0.737 | -0.984 | -0.188 | -0.280 | -0.548 | -19.913 | 84.367 | -66.466 |
| | (1.658) | (2.208) | (1.976) | (0.385) | (0.645) | (0.759) | (0.240) | (0.339) | (0.537) | (65.714) | (144.315) | (51.764) |
| Letter + no CC + full list | -1.940 | -2.939 | -3.837 | -0.438 | -0.635 | -1.450 | -0.328 | -0.526 | -1.216 | -87.962 | -96.199 | -90.310 |
| | (1.984) | (2.696) | (2.663) | (0.461) | (0.808) | (1.028) | (0.283) | (0.425) | (0.764) | (57.707) | (63.265) | (67.949) |
| Letter + no CC + partial list | 1.111 | 3.682 | 5.303 | 0.245 | 1.213 | 3.173 | 0.204 | 1.068 | 2.904 | 235.007 | 56.835 | 114.378 |
| | (2.378) | (5.084) | (5.723) | (0.566) | (1.667) | (3.259) | (0.355) | (1.114) | (2.818) | (267.630) | (82.520) | (120.273) |
| Letter + CC journalist + no list | -2.191 | -3.239 | -2.942 | -0.519 | -1.134 | -1.136 | -0.296 | -0.496 | -0.689 | 30.742 | -84.647 | -82.144 |
| | (1.696) | (2.233) | (2.021) | (0.401) | (0.670) | (0.789) | (0.244) | (0.329) | (0.557) | (110.911) | (51.077) | (53.395) |
| Letter + CC journalist + full list | -2.352 | -3.587 | -4.044 | -0.540 | -1.171 | -1.587 | -0.306 | -0.446 | -0.955 | -84.033 | -94.288 | -86.011 |
| | (2.107) | (2.946) | (2.846) | (0.492) | (0.893) | (1.108) | (0.297) | (0.449) | (0.774) | (56.495) | (63.967) | (67.915) |
| Letter + CC journalist + partial list | -1.218 | -2.486 | -2.752 | -0.263 | -0.694 | -1.026 | -0.140 | -0.311 | -0.773 | -59.323 | -84.801 | -76.443 |
| | (2.017) | (2.808) | (2.585) | (0.453) | (0.763) | (0.953) | (0.288) | (0.415) | (0.701) | (56.343) | (59.689) | (62.038) |
| Letter + CC government + no list | -1.112 | -1.245 | -0.445 | -0.287 | -0.382 | -0.207 | -0.182 | -0.224 | -0.162 | -46.844 | -40.481 | -19.466 |
| | (1.594) | (2.096) | (1.986) | (0.370) | (0.619) | (0.745) | (0.226) | (0.305) | (0.548) | (44.997) | (50.574) | (52.745) |
| Letter + CC government + full list | -1.173 | -0.989 | -0.776 | -0.270 | -0.397 | -0.168 | -0.090 | -0.080 | -0.139 | -72.418 | -77.161 | -46.766 |
| | (1.868) | (2.449) | (2.281) | (0.439) | (0.704) | (0.900) | (0.291) | (0.393) | (0.658) | (53.032) | (58.117) | (57.800) |
| Letter + CC government + partial list | -0.968 | -1.672 | -1.161 | -0.211 | -0.544 | -0.609 | -0.108 | -0.253 | -0.360 | -46.743 | -3.743 | -22.995 |
| | (1.104) | (1.575) | (1.370) | (0.266) | (0.490) | (0.574) | (0.167) | (0.217) | (0.391) | (38.321) | (55.883) | (38.260) |
| Number of municipalities | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 |
| Joint significance p-value | 0.746 | 0.746 | 0.574 | 0.828 | 0.755 | 0.568 | 0.697 | 0.719 | 0.608 | 0.818 | 0.603 | 0.696 |

| **Panel B: municipal elections and donations covariates** | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Dep. Variable | # votes | | % votes | | # candidates | # donors per candidate | | | # total donors | Donated value | | |
| Candidate rank | winner | runner-up | winner | runner-up | | winner | runner-up | other cand. | | winner | runner-up | other cand. |
| Control | 7.965 | 2.295 | 0.467 | 0.045 | 1.951 | 3.647 | 1.026 | 0.824 | 5.495 | 28.811 | 15.281 | 4.182 |
| | (5.564) | (1.458) | (0.097) | (0.019) | (0.281) | (0.788) | (0.392) | (0.716) | (1.442) | (9.619) | (10.406) | (5.200) |
| Placebo letter | 14.403 | 5.392 | 0.466 | 0.040 | 1.978 | 3.963 | 0.872 | 1.185 | 5.910 | 26.672 | 3.031 | 21.171 |
| | (11.896) | (4.943) | (0.099) | (0.019) | (0.283) | (0.767) | (0.389) | (0.765) | (1.332) | (9.780) | (5.639) | (18.350) |
| Letter + no CC + no list | 0.712 | 0.616 | 0.503 | 0.035 | 1.959 | 2.655 | 0.266 | 0.540 | 3.481 | 13.367 | -5.532 | -0.531 |
| | (3.590) | (1.906) | (0.098) | (0.019) | (0.295) | (0.757) | (0.367) | (0.781) | (1.372) | (8.971) | (4.353) | (7.407) |
| Letter + no CC + full list | -0.867 | -0.003 | 0.460 | 0.050 | 2.004 | 3.928 | 0.648 | -0.060 | 4.517 | 20.734 | 0.150 | -10.455 |
| | (3.976) | (1.974) | (0.101) | (0.022) | (0.325) | (0.884) | (0.457) | (0.875) | (1.607) | (10.300) | (6.719) | (9.462) |
| Letter + no CC + partial list | 4.806 | 3.980 | 0.450 | 0.058 | 1.877 | 3.219 | 0.703 | 0.376 | 4.263 | 41.081 | -6.233 | 8.480 |
| | (4.752) | (3.301) | (0.104) | (0.023) | (0.299) | (0.833) | (0.505) | (0.593) | (1.299) | (24.111) | (5.330) | (13.714) |
| Letter + CC journalist + no list | 0.460 | 0.294 | 0.453 | 0.046 | 2.004 | 3.446 | 1.530 | 0.131 | 5.117 | 19.323 | 8.408 | -4.018 |
| | (3.363) | (1.685) | (0.098) | (0.019) | (0.296) | (0.799) | (0.509) | (0.559) | (1.244) | (8.927) | (6.600) | (6.626) |
| Letter + CC journalist + full list | 0.567 | 0.771 | 0.424 | 0.053 | 2.022 | 4.165 | 1.084 | -0.042 | 5.154 | 22.930 | 9.672 | -1.414 |
| | (4.170) | (2.064) | (0.100) | (0.023) | (0.313) | (1.023) | (0.535) | (0.679) | (1.510) | (10.613) | (7.683) | (6.982) |
| Letter + CC journalist + partial list | 1.785 | 1.313 | 0.421 | 0.025 | 2.131 | 3.728 | 1.466 | 1.522 | 6.772 | 32.931 | 9.141 | 10.454 |
| | (3.693) | (1.820) | (0.101) | (0.021) | (0.339) | (0.849) | (0.681) | (1.071) | (1.823) | (10.600) | (9.177) | (11.121) |
| Letter + CC government + no list | -0.051 | 0.809 | 0.461 | 0.048 | 1.813 | 3.501 | 0.630 | 0.349 | 4.563 | 21.683 | -2.858 | -2.360 |
| | (3.418) | (1.755) | (0.098) | (0.019) | (0.280) | (0.820) | (0.355) | (0.671) | (1.314) | (9.678) | (4.518) | (7.187) |
| Letter + CC government + full list | 0.947 | 1.874 | 0.543 | 0.064 | 1.531 | 3.274 | 0.721 | -0.696 | 3.390 | 20.766 | -1.266 | -6.573 |
| | (3.665) | (2.011) | (0.104) | (0.023) | (0.299) | (0.956) | (0.595) | (0.756) | (1.762) | (9.884) | (7.254) | (8.012) |
| Letter + CC government + partial list | 1.071 | 0.578 | 0.470 | 0.040 | 2.025 | 3.259 | 0.730 | 0.842 | 4.773 | 17.852 | -0.772 | 4.521 |
| | (2.790) | (1.624) | (0.103) | (0.021) | (0.322) | (0.832) | (0.460) | (0.888) | (1.430) | (8.861) | (4.605) | (6.425) |
| Number of municipalities | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 |
| Joint significance p-value | 0.932 | 0.644 | 0.663 | 0.754 | 0.605 | 0.663 | 0.516 | 0.809 | 0.839 | 0.435 | 0.359 | 0.764 |

| **Panel C: general municipal characteristics and other variables** | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Dep. Variable | Population | Area | Height | Dist. capital | Poverty | Andean | Caribbean | Pacific | Orinoquian | Amazonian | # teachers | # students |
| Control | 30.112 | 1685.766 | 549.599 | 108.131 | 60.410 | 0.310 | 0.355 | 0.140 | -0.023 | 0.218 | 2.504 | 6.158 |
| | (18.408) | (761.879) | (206.726) | (20.096) | (5.300) | (0.128) | (0.132) | (0.091) | (0.019) | (0.103) | (1.367) | (3.196) |
| Placebo letter | 68.859 | 1599.301 | 638.729 | 104.643 | 62.885 | 0.323 | 0.336 | 0.138 | 0.013 | 0.190 | 6.286 | 12.853 |
| | (59.396) | (748.374) | (203.900) | (20.208) | (5.146) | (0.125) | (0.131) | (0.091) | (0.024) | (0.106) | (5.191) | (10.169) |
| Letter + no CC + no list | 4.665 | 1391.553 | 652.448 | 105.225 | 62.625 | 0.300 | 0.398 | 0.173 | -0.033 | 0.162 | 0.909 | 2.246 |
| | (16.626) | (745.405) | (202.306) | (19.916) | (5.215) | (0.125) | (0.133) | (0.092) | (0.017) | (0.101) | (1.539) | (3.200) |
| Letter + no CC + full list | -9.087 | 1670.762 | 507.357 | 104.198 | 62.787 | 0.345 | 0.371 | 0.091 | 0.012 | 0.181 | -0.549 | -0.039 |
| | (19.851) | (868.139) | (213.740) | (20.318) | (5.668) | (0.134) | (0.136) | (0.094) | (0.034) | (0.110) | (1.771) | (3.690) |
| Letter + no CC + partial list | 33.672 | 2088.980 | 522.157 | 89.837 | 61.633 | 0.291 | 0.353 | 0.109 | 0.048 | 0.199 | 3.241 | 5.847 |
| | (32.110) | (816.164) | (214.542) | (21.652) | (5.258) | (0.132) | (0.136) | (0.096) | (0.040) | (0.104) | (2.734) | (4.444) |
| Letter + CC journalist + no list | -2.709 | 1394.526 | 640.694 | 103.629 | 62.125 | 0.318 | 0.407 | 0.091 | 0.012 | 0.171 | 0.042 | 0.984 |
| | (15.947) | (740.017) | (203.291) | (19.686) | (5.162) | (0.129) | (0.131) | (0.089) | (0.023) | (0.102) | (1.410) | (2.991) |
| Letter + CC journalist + full list | -5.234 | 1612.344 | 463.230 | 108.979 | 64.157 | 0.272 | 0.426 | 0.128 | -0.006 | 0.181 | -0.149 | 0.642 |
| | (21.096) | (797.320) | (230.117) | (20.951) | (5.595) | (0.132) | (0.138) | (0.105) | (0.030) | (0.104) | (1.866) | (3.813) |
| Letter + CC journalist + partial list | 6.392 | 1335.708 | 313.903 | 105.195 | 64.309 | 0.272 | 0.426 | 0.128 | -0.006 | 0.181 | 0.814 | 2.568 |
| | (18.293) | (740.067) | (213.718) | (20.154) | (5.503) | (0.136) | (0.138) | (0.105) | (0.031) | (0.104) | (1.690) | (3.443) |
| Letter + CC government + no list | 8.767 | 2732.271 | 614.603 | 100.123 | 60.293 | 0.345 | 0.353 | 0.118 | 0.003 | 0.181 | 1.066 | 3.056 |
| | (15.983) | (969.994) | (205.579) | (20.020) | (5.180) | (0.127) | (0.130) | (0.090) | (0.023) | (0.106) | (1.435) | (3.058) |
| Letter + CC government + full list | 1.056 | 1318.762 | 685.194 | 106.918 | 59.934 | 0.309 | 0.371 | 0.091 | 0.012 | 0.217 | 0.600 | 2.021 |
| | (17.926) | (740.963) | (219.354) | (21.256) | (5.491) | (0.135) | (0.139) | (0.093) | (0.034) | (0.106) | (1.581) | (3.302) |
| Letter + CC government + partial list | 5.834 | 2095.609 | 387.015 | 107.028 | 64.599 | 0.319 | 0.355 | 0.131 | -0.004 | 0.199 | 0.850 | 2.642 |
| | (12.565) | (821.055) | (219.673) | (20.097) | (5.567) | (0.138) | (0.135) | (0.097) | (0.032) | (0.105) | (1.035) | (2.396) |
| Number of municipalities | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 | 876 |
| Joint significance p-value | 0.882 | 0.215 | 0.269 | 0.737 | 0.899 | 0.998 | 0.876 | 0.912 | 0.717 | 0.751 | 0.876 | 0.908 |

*Notes:* The unit of analysis is the municipality. The table shows the average municipal characteristics per treatment arms. All regressions include randomization block fixed effects and robust standard errors in parentheses. The bottom of the table reports the p-value of a joint significance test for the difference of each arm treated arm relative to the control group. Panel A show balance in municipal procurement outcomes by electoral cycle. *# contractors* measures the number of unique contractors in hundreds. *# contracts* and *# discretionary* measure the number of contracts and discretionary contracts, respectively, in thousands. *Contract value* report the total value in hundred million COP. Panel B report balance on electoral and donation covariates in the last electoral cycle (2020-2023). *# votes* per winner and runner-up candidate in thousands. *% votes* per winner and runner-up candidate relative to overall votes in municipality. *# candidates* is for the number of participating candidates. *# donors* reported per winner, runner-up and other candidates. *# donors* is the sum of all reported donors in the municipality. *Donated value* per winner, runner-up and other candidates in million COP. Panel C report on general municipal characteristics. *Population* in thousand inhabitants as reported by the 2005 national census. Municipal *Area* in squared kilometers ($Km^2$). *Height* reports the distance relative to the sea level. *Distance capital* in linear kilometers. *Poverty* is the share of municipal households that reported unmet basic needs in 2012. *Andean*, *Caribbean*, *Pacific*, *Orinoquian* and *Amazonian* measure the share of municipalities in each regions. *# teachers* in hundreds and # students in thousands, measured in 2012.

**Table B2:** Balance on donor characteristics by municipal treatment arms

| Dep. Variable | Donated val. last election (1) | # contracts 2020-2023 (2) | Contracted val. 2020-2023 (3) | Had contract before 2024 (4) | Had contract since 2024 (5) | Rank donor =1 if highest (6) | Donated above threshold (7) |
|---|---|---|---|---|---|---|---|
| Control | 6.681 | 0.429 | -74.448 | 0.163 | 0.041 | 3.094 | 0.535 |
| | (1.927) | (0.157) | (92.977) | (0.062) | (0.024) | (0.598) | (0.103) |
| Placebo letter | 7.839 | 0.429 | 632.409 | 0.140 | 0.039 | 2.462 | 0.515 |
| | (2.957) | (0.194) | (533.182) | (0.060) | (0.024) | (0.539) | (0.102) |
| Letter + no CC + no list | 5.003 | 0.409 | -56.538 | 0.191 | 0.033 | 2.049 | 0.509 |
| | (1.809) | (0.160) | (81.553) | (0.061) | (0.023) | (0.647) | (0.101) |
| Letter + no CC + full list | 5.401 | 0.394 | 5.690 | 0.155 | 0.035 | 2.279 | 0.532 |
| | (1.765) | (0.184) | (48.710) | (0.063) | (0.024) | (0.535) | (0.106) |
| Letter + no CC + partial list | 8.336 | 0.558 | -56.161 | 0.204 | 0.034 | 1.984 | 0.582 |
| | (3.322) | (0.209) | (97.796) | (0.068) | (0.024) | (0.662) | (0.102) |
| Letter + CC journalist + no list | 6.159 | 0.574 | -18.145 | 0.188 | 0.049 | 3.160 | 0.486 |
| | (1.732) | (0.176) | (54.410) | (0.061) | (0.024) | (0.643) | (0.101) |
| Letter + CC journalist + full list | 6.624 | 0.216 | 25.839 | 0.186 | 0.042 | 3.270 | 0.571 |
| | (1.891) | (0.167) | (73.390) | (0.061) | (0.024) | (0.649) | (0.107) |
| Letter + CC journalist + partial list | 7.404 | 0.456 | -20.625 | 0.149 | 0.029 | 2.645 | 0.508 |
| | (1.765) | (0.168) | (69.464) | (0.060) | (0.023) | (0.740) | (0.102) |
| Letter + CC government + no list | 5.350 | 0.426 | -52.795 | 0.171 | 0.039 | 2.955 | 0.506 |
| | (1.766) | (0.171) | (77.219) | (0.063) | (0.023) | (0.662) | (0.102) |
| Letter + CC government + full list | 5.935 | 0.706 | -70.872 | 0.185 | 0.061 | 3.337 | 0.496 |
| | (1.795) | (0.245) | (115.682) | (0.066) | (0.026) | (0.987) | (0.103) |
| Letter + CC government + partial list | 5.271 | 0.248 | -42.474 | 0.137 | 0.044 | 2.106 | 0.570 |
| | (1.801) | (0.162) | (81.997) | (0.061) | (0.024) | (0.568) | (0.107) |
| N. of donors | 8,560 | 8,560 | 8,560 | 8,560 | 8,560 | 8,560 | 8,560 |
| N. of municipalities | 876 | 876 | 876 | 876 | 876 | 876 | 876 |
| Joint significance p-value | 0.719 | 0.284 | 0.995 | 0.408 | 0.534 | 0.574 | 0.304 |

*Notes:* The unit of analysis is the donor for the electoral period 2024-2027. The table shows the average donor characteristics across municipal treatment arms relative to the constant. All regressions include block-fixed effects and standard errors clustered at the municipal level. The bottom of the table reports the p-value of a joint significance test for the difference of each arm treated arm relative to the control group. *Donated value* in million COP is for the total amount donated by an individual donor to a single candidate. *# contracts* and *Contracted value* for 2020-2023 measures the procurement characteristics of donors in the mayoral period before the donation. *Contracted value* in million COP. *Had contracts before 2024* measures whether the donors were already registered contractors at the time of the mayoral period for which they donated started. *Had contracts since 2024* measures if the donor received a contract between January and May of 2024. *Rank donor* measures the ranking of the donor by candidate and municipality based on the amount donated. *Donated above threshold* measures if the total amount donated by an individual or firm is above the legal limit for being allowed to participate in public procurement procedures.

**Table B3:** Balance on donor characteristics by omitted and included donor

| Dep. Variable | Donated val. last election (1) | # contracts 2020-2023 (2) | Contracted val. 2020-2023 (3) | Had contract before 2024 (4) | Had contract since 2024 (5) | Rank donor =1 if highest (6) | Donated above threshold (7) |
|---|---|---|---|---|---|---|---|
| Include donor | 14.800 | 0.330 | 5.088 | 0.256 | 0.026 | 20.002 | 0.026 |
| | (3.127) | (0.232) | (4.117) | (0.075) | (0.027) | (1.942) | (0.027) |
| Omitted donor | 14.799 | 0.452 | -6.620 | 0.272 | 0.029 | 19.923 | 0.021 |
| | (3.144) | (0.256) | (22.588) | (0.077) | (0.028) | (1.942) | (0.029) |
| N. of donors | 8,560 | 8,560 | 8,560 | 8,560 | 8,560 | 8,560 | 8,560 |
| Joint significance p-value | 1 | .27 | .61 | .26 | .61 | .42 | .68 |

*Notes:* The unit of analysis is the donor for the electoral period 2024-2027. The table shows the average donor characteristics across omitted or included donor treatment arms. All regressions include donor block-fixed effects and robust standard errors. The bottom of the table reports the p-value of a joint significance test for the difference of each arm treated arm relative to the control group. *Donated value* in million COP is for the total amount donated by an individual donor to a single candidate. *# contracts* and *Contracted value* for 2020-2023 measures the procurement characteristics of donors in the mayoral period before the donation. *Contracted value* in million COP. *Had contracts before 2024* measures whether the donors were already registered contractors at the time of the mayoral period for which they donated started. *Had contracts since 2024* measures if the donor received a contract between January and May of 2024. *Rank donor* measures the ranking of the donor by candidate and municipality based on the amount donated. *Donated above threshold* measures if the total amount donated by an individual or firm is above the legal limit for being allowed to participate in public procurement procedures.

**Table B4:** Impact on contractors procurement outcomes - omitted and included donors

| Dep. Variable | Pr(contract) (1) | Log(contract value) (2) |
|---|---|---|
| *Donor* | | |
| A. Inspector General×Post | | |
| **Inspector General + no list**×Post | 0.001 | 0.005 |
| | (0.002) | (0.015) |
| **Inspector General + list** + Omitted donor×Post | -0.015 | -0.083 |
| | (0.009) | (0.051) |
| **Inspector General + list** + Included donor×Post | -0.007** | -0.045** |
| | (0.003) | (0.020) |
| B. Investigative Journalist×Post | | |
| **Investigative Journalist + no list**×Post | -0.001 | -0.012 |
| | (0.003) | (0.018) |
| **Investigative Journalist + list** + Omitted donor×Post | 0.006 | 0.038 |
| | (0.006) | (0.033) |
| **Investigative Journalist + list** + Included donor×Post | -0.002 | -0.018 |
| | (0.003) | (0.016) |
| C. No CC×Post | | |
| **No CC + no list**×Post | -0.001 | -0.015 |
| | (0.003) | (0.019) |
| **No CC + list** + Omitted donor×Post | 0.003 | 0.020 |
| | (0.010) | (0.057) |
| **No CC + list** + Included donor×Post | -0.004 | -0.026 |
| | (0.003) | (0.018) |
| *Linear combinations P-values* | | |
| Inspector General + list: Omitted donor - Included donor | .389 | .479 |
| Mean of dependient variable | .003 | .111 |
| N. of obs. | 174155436 | 174155436 |
| Number of municipalities | 876 | 876 |
| Number of donors | 8537 | 8537 |
| Contractor FE | Yes | Yes |
| Block × $\mathbb{1}[Donor]$ × Time FE | Yes | Yes |

*Notes:* The unit of analysis is the individual-month, estimating equation 4 on procurement outcomes. Panel for non-donors estimated but not reported. See Table 4. *Inspector General* is for the treatment letter carbon copying the relevant public office that oversight public procurement. *Investigative Journalist* is for the treatment letter carbon copying investigative journalist. *No CC* is for municipalities receiving the treatment letter without carbon copy additional organizations. *No list* is for treatment letters not including the donor list as attachment. *List* group the treatment letter including the full and partial list of donors as attachment. *Pr(contract)* equals 1 if the donor received a contract in $t$, 0 otherwise. *log(contract value)* is the value of contracts. The log transformation follows Chen and Roth (2022) and define the intensive margin as $x = -1$. *Post* equal 1 for periods after the letter were sent to mayors. The mean of the outcomes is measured in the pre-period and is reported in million COP for the contract value. *Individual FE* is for individual fixed effects. *Block* × $\mathbb{1}[Donor]$ × *Time FE* is for randomization block by donation status by month fixed effects. Standard errors clustered at the municipal level are in parentheses. $^{*}p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

**Table B5:** Impact on municipal procurement outcomes

| Dep. Variable | Log(contract value) | | | | | |
|---|---|---|---|---|---|---|
| Sample | Total | | Donor | | No donor | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| A. Inspector General×Post | 0.022 | | -0.124 | | 0.027 | |
| | (0.069) | | (0.098) | | (0.069) | |
| **Inspector General + no list**×Post | | -0.014 | | -0.083 | | -0.001 |
| | | (0.073) | | (0.116) | | (0.074) |
| **Inspector General + list**×Post | | 0.059 | | -0.166 | | 0.055 |
| | | (0.087) | | (0.126) | | (0.087) |
| B. Investigative Journalist×Post | -0.020 | | -0.162 | | -0.025 | |
| | (0.084) | | (0.103) | | (0.083) | |
| **Investigative Journalist + no list**×Post | | 0.088 | | -0.082 | | 0.069 |
| | | (0.087) | | (0.115) | | (0.087) |
| **Investigative Journalist + list**×Post | | -0.128 | | -0.242* | | -0.120 |
| | | (0.119) | | (0.141) | | (0.118) |
| C. No CC×Post | 0.119* | | -0.131 | | 0.120* | |
| | (0.069) | | (0.093) | | (0.069) | |
| **No CC + no list**×Post | | 0.144 | | -0.138 | | 0.143 |
| | | (0.091) | | (0.100) | | (0.091) |
| **No CC + list**×Post | | 0.093 | | -0.125 | | 0.096 |
| | | (0.070) | | (0.123) | | (0.070) |
| *Linear comb. P-values* | | | | | | |
| A. Inspector General - B. Investigative Journalist | 0.575 | | 0.714 | | 0.487 | |
| A. Inspector General - C. No CC | 0.103 | | 0.936 | | 0.118 | |
| **Inspector General + list - Inspector General + no list** | | 0.377 | | 0.557 | | 0.504 |
| **Inspector General + list - Investigative Journalist + list** | | 0.137 | | 0.636 | | 0.160 |
| **Inspector General + list - No CC + list** | | 0.668 | | 0.783 | | 0.603 |
| Mean of dependent variable | 1982.147 | 1982.147 | 12.645 | 12.645 | 1969.502 | 1969.502 |
| Number of obs. | 11,388 | 11,388 | 11,388 | 11,388 | 11,388 | 11,388 |
| Number of municipalities | 876 | 876 | 876 | 876 | 876 | 876 |
| Municipality FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Block × Time FE | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* The unit of analysis is the municipality-month. Results obtained from estimating equation 4 on municipality procurement outcomes. *Inspector General* is for the treatment letter carbon copying the relevant public office that oversight public procurement. *Investigative Journalist* is for the treatment letter carbon copying investigative journalist. *No CC* is for municipalities receiving the treatment letter without carbon copy additional organizations. *log(contract value)* is the total value of contracts issued in the municipality. The log transformation follows Chen and Roth (2022) and define the intensive margin as $x = -1$ for municipalities not reporting any outcome. Both variables are reported separately for the Total, Donors and Non donors, as indicated in the top of the columns. *Post* equal 1 for periods after the letter were sent to mayors. The mean of the outcomes is measured in the pre-period and is reported in million COP for the contract value. *Municipal FE* is for municipal fixed effects. *Block ×Time FE* is for randomization block by month fixed effects. Standard errors clustered at the municipal level are in parentheses. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.