

The Power of Money. The Consequences of Electing a Donor Funded Politician.

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Abstract

This paper examines the impact of electing donor-funded politicians and campaign finance-limits using a novel dataset that uniquely links campaign donors and recipients of public contracts during a mayor's incumbency period in Colombia. Evidence shows that electing a donor-funded politician more than doubles the probability of donors receiving contracts and incumbents receiving disciplinary sanctions. Donor contracts are assigned under a minimum-value modality where there is less screening, and have a higher price compared to similar non-donor contracts. Campaign limits, lead to lower participation of donor- funding in campaigns, and as a result reduce the effect of favoring donors with contracts.

Keywords: Campaign finance, elections, political selection, corruption, public procurement, campaign limits

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1 Introduction.

The role of money in politics has been center in the study of political economy. For instance [Bakunin \(1882\)](#) argued that elected leaders in democracies have a limited ability to improve the conditions of working people if they are beholden to the power of private money. Nobel prize winner [Arrow \(1978\)](#), argued that economic inequality and the presence of powerful elites who influence the political system produce a form of democracy that represents the few rather than the majority, he proclaimed: “*democratic government is inevitably something of a sham*” ([Arrow, 1978](#), pg.479).

Recent events have placed the study of money in politics at the forefront of the policy debate. In the 2016 US election, Hillary Clinton was perceived to be too close to donors, which some analysts argue affected her chances of winning.¹ Peru’s ex-president, Alejandro Toledo, was recently found guilty of receiving money from the Brazilian construction giant Odebrecht, and then awarding the company a big contract to build a transoceanic road, that came under public scrutiny for its poor construction and cost overruns. It was initially forecast to cost \$800M USD and ended up costing around \$2,000M. According to Peru’s Ministry of Interior, the president received \$20M dollars, which would imply the contract was *100 times larger* than the money received.² Therefore, do privately donor-funded politicians benefit donors disproportionately?

Voters are often suspicious of donor funded politicians, because they could represent donor interests rather than the interests of the general constituency. According to the formal literature, donor funded politicians could give favors to donors in exchange of donations ([Snyder Jr, 1990](#); [Grossman, 2002](#); [Coate, 2004](#); [Ashworth, 2006](#)), and these favours could be provided in a nontransparent way, reducing competition, and potentially increasing the costs of providing goods to society. On the other hand, donor-funded politicians could be more competent if donations go to the most prepared politician. Moreover, donations could help increase campaign spending which could reveal the competence of the politician to voters ([Coate, 2004](#)), allowing for a more informed voting decision. Donor-funded politicians, could also favour the best companies, at both selecting candidates via donations and contracting, improving the allocative efficiency in the economy.

There is extensive research in the theoretical literature, yet progress in the empirical literature is difficult due to severe data limitations: it is difficult to observe systematically how politicians benefit donors. Furthermore it is difficult to disentangle whether donors benefited from giving money to politicians, or whether their success is due to other potential explanations.³ Despite the lack of evidence, countries have enacted limits to money

¹BBC News, “Is Wall Street a problem for Hillary Clinton?”, April 14, 2016. Available [here](#).

²BBC News, “Juez de Perú ordena el arresto del expresidente Alejandro Toledo por caso Odebrecht,” February 10, 2016. Available [here](#).

³It is difficult to assess whether these companies would still have benefited if their funded politicians had *lost* the election. For example, companies could benefit from the economic stability produced by an election, or their economic success may happen to coincide with the timing of an election. Recent papers that address this concern are: [Boas, Hidalgo, and Richardson \(2014\)](#); [Szakonyi \(2016\)](#). However, compared to those papers, this paper focuses on the selection of donor-funded politicians vs. non-donor

in politics -in particular- campaign finance contributions, with the premise that they will reduce the benefits to donors and the conflict of interest. But, are they effective?

I make progress on these issues by constructing a novel dataset that links campaign donors and contractors during a mayor's incumbency period in Colombia (2012–2015) using unique national identifiers (IDs). I also make progress by exploiting close races between politicians who received campaign donations by private donors and those who *did not*⁴ using a regression discontinuity design (RDD). I also exploit arbitrary campaign limits (which jump discontinuously at arbitrary voter cut-offs)⁵ as an instrument for the candidates' proportion of donor funds using a fuzzy RDD. The latter allows me to study the effects of campaign limits and disentangle the effect of campaign donations, from the politician personal characteristics associated with being donor funded. Using detailed contracting data, I study the procurement process for donors and how they can enjoy a price premium for the same type of contract executed. Therefore, this paper aims to respond two closely linked questions: **i)** What are the consequences of electing a donor-funded politician? (Consequences in terms of contract assignment, economic policy, corruption, procurement process and contract cost). **ii)** Can campaign contribution limits reduce the influence of money in politics?

Answering the first question provides –to the best of my knowledge– the first causal estimates of the effects of selection of a donor-funded politicians, and could further inform voter decisions. Results show that the electoral success of a donor-funded politician (over a non-donor-funded politician) more than doubles the probability of donors receiving contracts from a mean of 5.9% to 15.5% (an increase of 9.6%). The total value of contracts awarded to donors when their funded politician wins is, on average, *13.75 times higher* than the amount donated. Moreover, the evidence shows that electing a donor-funded politician does *not* lead to better economic outcomes in the municipality, lower budget deficits or more investment. However, it *does* increase the probability that the mayor will be investigated and/or sanctioned by the procurement watchdog. Contracts for donors have short duration which increases the likelihood they are paid during the mayors incumbency term. Compared to regular contracts, contracts for donors tend to be given for supplies ensuring their quicker pay. Moreover, they tend to be awarded under a minimum value modality, where it is only required to publish the call for bidders only for 24 hours or more, it is not required to publish the call in the national on-line system, the sole criteria for awarding contracts is the lowest bidder, and there is no need of a committee to evaluate proposals. This modality allows the allocation of contracts to specific individuals in a less transparent way compared to regular contracts.⁶ Compared to

funded, rather than the effects of general electoral victory.

⁴Not all politicians are recipients of campaign donations

⁵For example at 25,000 registered voters cut-off, the campaign limits increase from 58M to 110M COP. 1M COP is equivalent to \$350 USD. Each donor can contribute up to 10% of the maximum campaign total.

⁶This is in sharp contrast with a regular bid, where it is required to publish call for applications between 5 to 10 working days, this call has to be in the on-line system an evaluation committee can be used, and the award of the contract has to be justified publicly.

regular contractors, I find that donors on average receive more minimum value contracts, contract in less number of municipalities (are more local), and contract in more sectors of the economy, lacking specialization. Finally using text analysis, I match similar contracts for donors and non-donors within donor-funded incumbencies, and I find that donors enjoy a price premium of 2 M COP, which is equivalent to approximately 2 average monthly wages in the municipality.⁷

In the case of evaluating campaign limits, evidence shows that loosening campaign limits from 58M to 110M has no effects on political selection of elected politicians, however it does increases the participation of donor campaign funds by 20%, which in turn leads to an increase in the number of contracts given to donors. This is an important finding, because this is the first paper -to best of my knowledge-, to causally estimate the effect of campaign limits (donations) on benefiting donors, which could be considered the main purpose of campaign limits: limiting the influence of donations on public procurement.⁸

In sum, this paper provides novel evidence which can reveal the consequences of electing a donor funded politician; Electing a donor funded politician is associated with an increased probability of donors being awarded contracts. Most importantly these practices can be costly because they result in procurement processes that are less transparent, and can limit competition by other bidders. Also because donor contracts receive a price premium compared to non-donor contracts, however the size of the distortion does not affect the overall budget deficit or investment in the municipality. Donor funded mayors are associated with mayors being more likely to receive disciplinary sanctions. Although not conclusive, the suggestive evidence shows that sanctions are related to the level of corruption associated with the contracting transaction. For policy purposes, the paper also shows that campaign limits can be a useful public policy tool to reduce the benefits for donors, as a result of participating in campaigns.

2 Related literature

According to [Witko \(2011\)](#), there is a broad range of anecdotal evidence regarding how donating money to election campaigns has led to politicians benefiting the interests of donors, which in turn has resulted in corruption cases. However, campaign donations have the potential to play a positive role: they could serve as a mechanism to select the most prepared politicians for office, and those politicians may in turn select the donor companies that could be effective at execution.

Studying how politicians reward donors is challenging, because it is difficult to measure what the exact benefits is. According to [Stratmann \(2005\)](#) there have been two common

⁷However, a potential limitation of just looking at price, is that the price differential could be justified by a quality differential. However, the lack of specialization in an economic sector of donors, could imply that it is less likely that quality of goods provided by donors are of a higher quality.

⁸[Avis, Ferraz, Finan, and Varjão \(2017\)](#) estimates the effects of campaign limits on the number of candidates and how competitive the election is, but not if the donor-funded politicians benefit their donors or general corruption.

approaches in the literature: looking at politicians' roll-call votes and the financial performance of donor companies. Regarding the former, the premise is that politicians will vote in ways that benefit their donors. However, a large survey conducted by [Ansolabehere, De Figueiredo, and Snyder \(2003\)](#) shows that most studies find no effects of campaign contributions on voting.⁹ A potential issue with using roll-call votes is that it is difficult to assess whether campaign contributions go to candidates who are ideologically aligned with a company, or if the donations truly changed a politician's position.¹⁰ Another difficulty of using roll-call votes to measure benefits to donors is that voting for legislation in favor of donors does not necessarily produce immediate benefits. There could be a lag before the legislation is implemented and the company realizes any benefits. Therefore, it is uncertain when benefits (if any) will materialize, and when they should be measured.

The other approach to quantifying the benefit to donors has been using a company's actual financial performance. For example [Johnson and Mitton \(2003\)](#) uses the stock value of politically connected companies or [Szakonyi \(2016\)](#) uses the profit margin. Alternatively, [Ansolabehere, Snyder, and Ueda \(2004\)](#) study the effects of soft money given to political parties on firms' excess rate of returns. The premise is that companies that gave soft money may have received contracts or competitive advantages that could be reflected in their profits and stock value. A potential concern with using companies' financial performance is that performance can depend on many other determinants beyond the political connection, and it will take some time for the benefits to materialize (if any). So timing of the measurement can also be difficult. Due to these potential drawbacks, I focus on the effect of donations on contract assignment as a directly observable benefit to companies.

Empirically there is another paper with identification that use contracts as measurement of benefits: [Boas et al. \(2014\)](#), find that electoral victory increases the probability of conferring contracts for candidate's corporate donors. However, my paper concentrates in a substantially different research question: the effects of electing a donor-funded politician vs. a non-donor-funded politician – that is, the political selection of a certain *type* of politician. In a related paper [Szakonyi \(2016\)](#) studies election of firm directors in Russia, and find that politicians firms enjoy higher profits. This represents a different politician type to the one in my sample, which is politicians funded by companies/individuals, rather than company owners running for office themselves.

This paper contributes to the literature that shows that elected leaders *do* make a difference (see [Jones and Olken, 2009](#)). Most of the studies focus on the effects of diverse politician types: education of leaders ([Besley, Montalvo, and Reynal-Querol, 2011](#); [Freier and Thomasius, 2012](#)), women ([Ferreira and Gyourko, 2014](#); [Bagues and Campa, 2017](#); [Chattopadhyay and Duflo, 2004](#)), their professional background ([Matter and Stutzer,](#)

⁹[Stratmann \(2005\)](#) uses an alternative methodology to perform a meta-analysis on studies of campaign contributions and roll-call votes, and shows that campaign contributions affect politicians' voting records.

¹⁰To address this concern, [Stratmann \(2002\)](#) has used within-politician variations in contributions, and [Ansolabehere et al. \(2003\)](#) has employed an instrumental variable approach.

2015), and their minority status (Pande, 2003). This is the first paper to causally estimate the effect of electing a politician who is donor-funded.

2.1 Donors and candidates

Why do individuals or companies donate to politicians? Ansolabehere et al. (2003) argue that campaign contributions can be considered a consumption activity in which individuals mostly gain from participating in the democratic process. Most formal models (for example see Snyder Jr, 1990; Grossman, 2002; Coate, 2004; Ashworth, 2006) assume campaign contributions are used to influence politicians' policy stances or obtaining favors.¹¹ From a politician's perspective, accepting donations could allow them to further advertise their campaign and qualifications for office (see Ashworth (2006) or Coate (2004)), which could increase their chances of winning the election. If donations can potentially benefit both donors and candidates, why are there candidates that are non-donor funded?

One potential explanation is that, although accepting a donation can help a candidate pay for more campaign advertisement, it could damage a candidate's reputation. Voters may perceive donor-funded candidates to be less trustworthy, and more likely to represent special interests instead of their constituents. Ashworth (2006) argues that rational voters can infer that donor funding could lead to policies contrary to their interests. Moreover, Coate (2004) argues that informed voters are less willing to be convinced by additional advertising if they know donors provided the funds to pay for the ads. Therefore, the cost of accepting donations could offset the benefits for politicians if a large proportion of voters are informed about the sources of campaign financing.

Another potential explanation is that donors only give money to candidates who are ideologically aligned with them. For example, donors may prefer financing a pro-enterprise candidate rather than a leftist candidate who would prefer a higher taxation rate. In my case, I find that more right-wing¹² politicians are donor funded. This is in line with US evidence reviewed in Stratmann (2005), in which campaign donors fund candidates who share their ideological preferences.

Elected donor-funded politicians could be willing to give contracts to their donors in exchange for support given during elections (Snyder Jr, 1990). For the incumbent, awarding contracts is low cost since it is public money and in the case of Colombia there can be low risk of punishment given the lack of enforcement. Moreover, since mayors serve only a single term in Colombia, they do not worry that their actions while in office will harm their chances of re-election. An immediate source of benefits for elected politicians, is that they could ask a % of the conferred contract directly to the donor/contractor as payback. There is anecdotal evidence that shows that elected politicians do so.¹³

¹¹Which could entail moving to a certain policy stance closer to the donor's interests, or directly conferring contracts to donors.

¹²For a classification of ideology see Fergusson, Querubin, Ruiz, and Vargas (2017))

¹³This is informally known as "the bite", see discussion here: La Silla Vacía, "Santos, su Ñoño y su Musa". Available [here](#).

Moreover, after holding office, politicians could be employed by donor companies.¹⁴

A final piece of the puzzle is, why citizens would vote for a donor-funded politician if they know he/she may be beholden to donors after the election? An explanation given by Coate (2004), is that donor-funded politicians, using donor funds and advertisement, could convincingly persuade non-partisan uninformed voters of their qualifications, while informed partisan voters may be less willing to be convinced with additional advertisement. Another potential explanation is that although rational voters know a candidate accepted donations, they may believe that being donor funded could indicate a candidate's competence. They could think donor-funded politicians can do the job more effectively than a non-donor-funded politician who was less skillful at raising funds.

3 Context

3.1 Colombian institutions and electoral context

Colombia recently experienced political and economic decentralization (Bushnell, 1993). In 1986 there was introduction of elections for local mayors in municipalities. This allowed for first-past-the-post elections, in which every party could put one candidate name forward. In 1991, a new constitution allowed increased social spending and decentralization of fiscal resources to the regions. The new constitution made municipalities and departments jointly responsible for the provision of basic public services (Faguet and Sanchez, 2008). It also allowed new political parties to play a greater role through an increased presence at the local level (Hoyos, 2005). In recent years there has been an increase in the number of parties participating in local elections.¹⁵

Mayors are currently responsible for designing the budget and implementing an annual development plan in the municipality. Although most municipalities receive transfers from the central government that are tied to specific expenses or the central government spends directly in the municipality, mayors have discretion over an average of 20.24% of all local spending.¹⁶ According to Martinez (2017), most discretionary resources come from property tax revenues, which are used for the provision of education, health insurance, water, and sanitation projects as well as the functioning of the municipality. Most public services at the municipality level are provided by contracting third parties. There are three main forms of contracting in Colombia: 1. An open-bid process in which applicants submit their proposals. The call for applicants have to be opened for 5 to 10 working days, and has to be published in an on-line reporting system. A committee can select the winner, and the award of the contract has to be justified publicly. 2. Minimum value modality contracts. This applies when the size of the contract is below 10% of the total municipality budget. It is required to publish the call for applicants only for a day or

¹⁴Evidence by Eggers and Hainmueller (2009) for the UK shows that politicians can be hired by companies once they leave office.

¹⁵An average of 4.4 parties contested the 2011 mayoral elections.

¹⁶Data from 2012-2015 incumbency period.

more, it is not required to publish the call in the national on-line system, the sole criteria for awarding contracts is the lowest bidder, and there is no need of a committee to evaluate proposals. 3. A non-bid process that involves specific waivers that need to be formally justified¹⁷. In practice, 83% of all contracts during the 2011 mayoral incumbency period were given by the non-bid modality: 53% using waivers and 30% were the minimum value threshold.

In order to limit the influence of money in politics, Colombian law¹⁸ establishes limits for both total campaign contributions and individuals contribution size. The National Electoral Commission sets the campaign limits for each election. These limits jump discontinuously at arbitrary registered voter cut-offs, for example at 25,000 registered voters the campaign limit increases from 58M to 110M COP.¹⁹ In addition, individual donors cannot give more than 10% of the total campaign limit.²⁰ Limits are announced before campaigning starts. For the 2011 elections campaigning was allowed from the 30th of July; voting took place on 30th October 2011, and mayors did not take office until 1st January 2012, therefore I measure the outcomes during the 2012–2015 incumbency period.

4 Empirical strategy and data

4.1 Data

I use electoral data compiled by Pachón and Sánchez (2014), gathered from the Colombian national electoral authority, the *Registraduría Nacional del Estado Civil*. This data contains the results for mayoral elections for all municipalities in Colombia for 2011. Additionally, I gathered data reported by candidates to the the National Electoral Commission, on sources of income and expenditures of political campaigns. This comes from a new campaign reporting system financed by the US Agency for International Development in collaboration with Transparency International. This data is available publicly and its intention is to increase transparency to the public in campaign finance sources. In order to increase compliance, campaign finance reporting is mandatory by law since 2009.²¹ Parties have to electronically submit this information within two months after the election²², and subsequently the physical evidence of each source of campaign income and spending. The National Electoral Commission fines candidates or parties that do not

¹⁷The waiver list applies to: 1. The acquisition or supply of goods and services of uniform technical characteristics and common use by entities; 2. Contracting in which the tender process has been declared abandoned; 3. Contracts for the provision of health services; 4. Goods produced by or intended for agricultural purposes, offered on legally constituted product exchanges; 5. The contracting of goods and services required for defense and national security; and 6. Disposal of assets.

¹⁸Article 28 of Law 130 of 1994.

¹⁹Subsequently at 50,000 registered voters the limit jumps to 330M COP; at 100,000 registered voters the limit jumps to 659M COP; at 250,000 the limit jumps to 745M COP; at 500,000 the limit jumps to 1,318M COP. For the capital city of Bogotá the limit is 1,646M COP.

²⁰According to article 23 of Law 1475 of 2011.

²¹Resolution 1094 of 2009.

²²Article 25, Law 1475 of 2011

comply with the reporting requirements.²³ The data reports the donors' unique national ID numbers²⁴, which allows me to link candidates with publicly available information on contracting.

Table A1 illustrates the types of sources of campaign revenue reported in campaign reporting finance forms. For example, candidates can self-fund their campaigns, obtain personal bank loans, the party can organize fund-raisers, receive state funding, and/or receive donations by companies and/or people. In order to generate the counterfactual, I separate candidates who received campaign donations (code 102 in Table A1) from those who *did not*.²⁵

There are 1,098 municipalities in Colombia, in order to implement the RDD, I limit the sample to races in which the mayoral winner and runner-up candidates report their campaign income (966).²⁶ In order to implement the RDD, I must first ensure that I have enough power – i.e. that enough races are decided between a candidate who had donors and one who did *not*. Out of the 996 municipalities that report information on campaign financing sources, there are 408 such races, while there are 209 races between donor-funded politicians and 379 between non-donor-funded politicians. I concentrate on the first group since I am interested in comparing two different *politician types*. One potential concern is that these close races are clustered in a certain region of the country, but this is not the case. Figure A-2, provides a map of the distribution of municipalities, contested between non-donor and donor funded politicians, and those contested in a 6.5% narrow sample %.²⁷ Furthermore, in the sample of a narrow margin I find no spatial auto-correlation. The Moran I index, available in Figure A-3, indicates that municipalities included in the RDD estimate are randomly distributed. Another potential concern is that municipalities that are contested between donor-funded candidates and non-donor-funded candidates (408) are not representative of the entire country. I check with a broad range of municipality covariates (see Table A3), and there are no statistically significant differences between the municipalities included in the sample and those that are excluded, which strengthens the external validity.²⁸

Comparing sources of financing between donor-funded politicians and non-donor funded politicians (see Table 1), winners without donors finance on average 95% of their cam-

²³Compliance is fairly high: out of 4,460 mayoral candidates in 2011, 89% reported campaign information. However, the commission's capacity to fine candidates who do not submit the information was removed in late 2012, which could limit the compliance after for the 2015 electoral period.

²⁴In Colombia a unique national ID is assigned when a person turns 18 and is used for many purposes such as getting a mobile phone line, obtaining health care, or a loan. IDs are also assigned to companies, and their assignment is mandatory to conduct any business. When a person owns a company, the same ID is used for the individual and for the company.

²⁵As a robustness check I separated code 101, and coded family members as donors, but the results remain unchanged.

²⁶Table A2 shows that the places where there is no reporting by the top two candidates, are more remote, are more rural, and have more unmet basic needs. The lack of reporting can be associated with the level of development in the municipality.

²⁷For a discussion of bandwidth choice see the RDD set-up section

²⁸This result is not surprising, considering that the Morans I spatial autocorrelation index shows that the municipalities in the narrow sample are randomly distributed.

paign using their own resources. When there are donor funds involved, on average they represent 38% of the campaign financing, thus reducing the burden on the candidate and their families. It is important to note that state financing is almost non-existent in both groups, which increases candidates' reliance on donor funds and self-financing. Also parties, on average, contribute only 3% of campaign funds.²⁹ In addition, having donors increases the amount of disposable income available by campaigns by over 8.7M COP, and are able to finance more public events (see Table A5).

In order to uniquely link campaign donors to contracts assigned in a municipality, I obtained detailed data on contracting, which was gathered in order to increase transparency in public procurement.³⁰ This data reports the entity in charge of contracting, the contractor (and their unique ID), under which modality the contract was made, the broad sector of the economy, the size of the contract, the detailed purpose of the contract, the length of the contract, whether it was completed, and/or overrun in costs. I dropped contracts that were assigned by the national government, and examine only those under the municipality's jurisdiction since mayors are not in charge of the contracts executed by the national government. A summary of the contract data descriptive statistics can be found in Panel B of Table A4.

A key aspect of this research is linking the donor and checking whether it is the same person/company getting a contract. In Colombia, two types of legal entities can contract with the State: individuals and companies, both with unique IDs. If an individual donates money to a candidate and then receives a contract, the same unique ID is used; when an individual gives a donation and his/her company receives a contract, I can also link them uniquely since the same number is used for the person and their company. This unique feature of Colombia allows people to be linked with their companies even if they have different names. The only link that cannot be made is between individuals and public companies or companies with multiple owners: It could be the case that one of the owners gives a donation and then the company receives the contract. However contracts for multiple owner companies represent only 9.9% of all contracts across municipalities. Figure A-1 illustrates which links were found.

Using the links above, I calculated the probability of *any donor* – for the winner or runner-up candidate in the mayoral election – receiving a contract. This is to take into account the fact that donors of non-winner candidates also get contracts, since they can bid for contracts. If contracts are given strictly to the most competitive contractor, the probability of receiving a contract would be orthogonal to the contractor making a donation and their funded candidate winning the election. However, it is not the case: Table 4 shows that when a donor funded politician is elected 11.71% donors of the winner politician obtain contracts, while only 1.13% runner-up “donors”³¹ obtained a contract. Therefore

²⁹Hangartner, Ruiz, and Tukiainen (2017) discuss the weak party system. They interviewed candidates for local councils, and in many instances were told that parties just lent their credentials so candidates could run in the election.

³⁰I am grateful to *datos abiertos* online portal, for posting the contracting data.

³¹These are contracts for the candidate runner-up or their immediate families

if I find that electing a donor-funded politician has an effect on awarding contracts for donors, it would mostly be driven by contracts awarded to the donors of donor-funded politicians. In the case when non-donor politician funded is elected, 3.58% of donors receive contracts, driven by contracts given to donors of the runner-up. Interestingly even when donating to the loser politician, companies hold a positive probability of obtaining a contract. Why? donor companies of the opposing candidate could still be competitive in winning public procurement contracts, or they could receive contracts by the elected politician to attract support for the party for the following election. However, given the lack of reelection and weak party system in Colombia the latter is less plausible.

In order to measure politicians underlying characteristics, I made an extensive effort to obtain candidate-level data. The registry office only holds data on the gender of candidates. I obtained data on disciplinary sanctions from *Procuraduría*, which is the main public watchdog in charge of prosecuting corruption charges. Via a formal requirement, I requested the entire history of disciplinary *sanctions* for all mayoral candidates in the 2011 election and the date the sanction was executed, which allowed me to code whether the candidate had been sanctioned before or after holding office. On average, only 2.5% of the mayors in the sample were found guilty of disciplinary sanctions after they entered office. Disciplinary sanctions can happen for a variety of reasons, for example if a mayor does not reply to a formal information request by citizens, running for office without having the legal entitlement to do so, contracting improperly, or any extended violation of the law. In order to code whether the sanctions and investigations were related to contracting, I followed the methodology by Martinez (2017) and web-scraped from the official Procuraduría website³² all the public bulletins of investigations or sanctions after the 2011 period. Using the mayor's name, I organized all bulletins associated with a particular mayor. Then using QDA miner and WordStat and stemming analysis, I searched all bulletins of investigations or sanctions that contained the root of the word "contract" and coded as sanction/investigation related to contract if the root of the word contract appeared in the news bulletin. I further refined the coding by manually reading the contract-related bulletins and verifying that the investigation/sanction was indeed related to contracting.

In addition to the disciplinary sanctions, I obtained information on candidates voting registration available at *Registraduría del Estado Civil* and I coded if the candidate was registered to vote and if he/she had previously illegally registered to vote.³³ In order to obtain further individual covariates, I requested all 1,098 mayoral election ballots for the 2011 election, which contain politicians' pictures, from the National Registry Office. A sample can be found in Figure A-4. Using a Python facial detection API algorithm, I obtained an estimate of gender, age, and race from the politicians' pictures. In order to

³²<https://www.procuraduria.gov.co/portal/>

³³That is, the person either used a dead person's ID to vote, changed his or her registration ballot to another municipality in exchange for money, or tried to vote while underage. The most common fault is moving to another municipality to vote.

validate these results, I compared the self-reported gender to the gender predicted by the algorithm and there was less than a 3% difference. For the sample, descriptive statistics in panel D Table A4 show that less than 11% of mayors were female during the study period, 12% were categorized with indigenous background,³⁴ and 5% are black. The average politician age is 44. I used the coding used in Fergusson et al. (2017) to measure ideology. In the study sample, 22% of mayors were classified as right-wing, while only 10% were classified as left-wing. I also coded candidates' experience in politics, since it could be the case that donors choose to give money to more experienced candidates. To do so, I exploit the fact that all Colombians have two last names which helps uniquely identify candidates³⁵, and code the number of times the candidate participated in elections and held office. In order to account for the possibility of homonyms in full names, I coded any names that appeared twice as candidates in the same electoral year as missing, since it is only legally possible to be candidate for a single mayoral post. Only two candidates had exactly the same name and both last names in the 2011 election.

Finally, to obtain municipality-level predetermined covariates, and potential policies enacted by mayors, I obtained a municipality-level panel (2011–2015) with a broad range of economic, social and institutional covariates, thanks to a pre-existing effort by *CEDE* at *Universidad de los Andes*³⁶. This has municipality fiscal data, such as the total municipality budget, expenditure, and the sources of revenue. This data is organized by the National Department of Planning. I created averages during the incumbency period of different fiscal variable, and measured predetermined covariates in 2011, before the Mayor term starts in 2012.

Extended descriptive statistics of the base sample of 408 municipalities are available in Table A4. On average, 17.2% of donors are awarded contracts. Of the contracts given in a municipality, on average 40% were given directly with justified waivers and 50% by minimum value modality, while only 10% are open to competitive bid. On average, municipalities invest 86% of their budget; most of the remaining income is for administrative operations.³⁷

4.2 RD Design

The outcomes I want to study here – contract assignment, policies, and corruption – could be determined by a broad range of constituency characteristics. For example, bigger municipalities likely have larger budgets, which could lead to more contracting. Mayors' wages could also play a role: according to Besley (2004), higher wages could make politicians act more congruently with voter preferences, and ensure better behavior. Furthermore, according to Martinez (2017), municipality income from natural resources,

³⁴There algorithm predicted 12% Asians, but there are few Asians in Colombia. A further investigation of the pictures showed that they were actually Colombians with a distinct indigenous background.

³⁵All descendants from the Spanish colony use two last names. One from each parent.

³⁶*Centro de Estudios sobre Desarrollo Económico*

³⁷This expenditure is for the functioning of the internal administrative apparatus, such as municipal public employees.

rather than taxation, can lead to more corruption.

Given all the potential explanations of the outcomes, it is difficult to disentangle the effect of electing a politician from other constituency characteristics. To address this challenge, I employ a quasi-experimental design: a RDD that examines close elections in 408 Colombian municipalities between donor *and* non-donor funded politicians.³⁸ The premise is that within a narrow electoral margin, municipality characteristics are very similar *except* the type of mayor who won. In order to test this premise, I check whether other municipality characteristics jump discontinuously at the cut-off.

Since the campaign reporting system has only been in place since 2009, I can only use the 2011–2015 election period. I check the mayors’ funding sources as well as the effects on: contract assignment to donors, disciplinary sanctions, whether the investigations/sanctions are related to contracting, the types of policies implemented. I also look at contract level data to check the difference contract modality assignment for donors and non-donors during the 2012–2015 incumbency period.

A potential concern with this analysis is that there is systematic misreporting of donors that leads to misclassification. Candidates with donors are most likely to *under-report* donations, and therefore be misclassified as non-donor-funded politicians, while I would not expect candidates to report donors who did not donate³⁹. Therefore, I would expect several control units to be classified as treatment. If I was able to classify these candidates correctly, I would expect to find even bigger effects of electing a donor-funded politician because I would observe more potential links between donors and contractors, so the effect I am finding represents a lower bound.

This section focuses on *close* races in which the donor-funded (DF) candidate wins or is the runner-up.⁴⁰ I count the winner and runner-up votes and let: X_i = be the vote share of the DF politician in municipality i minus the vote share of non-DF. X_i = is centered around 0, so $DF_{it} = 1$ if $(X_{it} > 0)$. In particular:

$$DF_i = \begin{cases} DF_i = 1 & \text{if } X_i > 0 \\ DF_i = 0 & \text{if } X_i < 0 \end{cases} \quad (1)$$

Note that when the donor-funded politician *loses*, this implies that the non-donor-funded politician *wins*: If $DF_i = 0$ then $nonDF_i = 1$. So I compare across municipalities that are similar except for the identity of the winning candidate.

If there is no manipulation of the electoral outcome near the margin (i.e., it is not the case that donor-funded politicians always barely win), and if there are no other predetermined factors that vary discontinuously when donor-funded politicians win, the RDD

³⁸Here is important to note that I am estimating the effect of a type of politician -a bundle of characteristics-, rather than donations themselves. In order to estimate the effect of donations I do so in the following empirical section.

³⁹There is no plausible gain from adding non-existent donors with their unique ID to the reporting form, while doing this could increase scrutiny.

⁴⁰Since Colombia has a first-past-the-post system, I am only interested in close races between the winners and runners-up.

allows me to estimate the causal effect of electing a donor-funded politician:

$$\alpha = \lim_{x \downarrow c} E[Y_1 | X = c] - \lim_{x \uparrow c} E[Y_0 | X = c], \quad (2)$$

where c is the winning cut-off that has been centered around 0. Eq (2) holds when a narrow margin h is close enough ([Lee and Lemieux, 2010](#)). A first approximation estimates the effect of electing a donor-funded politician on the outcomes studied here:

$$y_i = \alpha + \beta_1 DF_i + \beta_2 f(X_i) + \beta_3 DF_i \times f(X_i) + \varepsilon_i \quad (3)$$

For a close h bandwidth, I estimate $f(X_{it})$ non-parametrically with a variety of polynomials to make sure it is not the functional form determining the evidence of discontinuity at the c cut-off, so I use a polynomial that may vary for $DF = 0$ or $DF = 1$.

I estimate α non-parametrically to make sure it is not the functional form driving the result. A very narrow margin h , can ensure that municipality characteristics do not vary discontinuously at the cut-off ([Lee and Lemieux, 2010](#)), however this can lead to larger standard errors and imprecise estimates due to the small sample. In order to account for the trade-off between efficiency and bias, in addition to reporting the conventional estimates I employ the optimal bandwidth, bias correction, and robust standard errors proposed by ([Calonico, Cattaneo, and Titiunik, 2014](#)). Evidence by [Hyytinen, Merillinen, Saarimaa, Toivanen, and Tukiainen \(2017\)](#) shows that this type of bias correction produces RDD results that are similar to the experimental estimate. Following [Gelman and Imbens \(2014\)](#), I estimate α for first- and second-order polynomials to avoid using higher-order polynomials.

A potential concern with this design is that politicians who received campaign donations may have personal characteristics that are different to those of politicians who did not receive campaign donations, and those characteristics themselves determine the probability of giving contracts to donors. In the results section, I test for differences in individual covariates across treatment and control group and find that donor-funded politicians tend to be more right-wing and have more political experience than their non-donor-funded counterparts. To test whether ideology or political experience alone increases the probability contracts awarded to donors, I control for these characteristics and the main result holds. However, these characteristics could be considered as post-treatment, so this specification has to be interpreted with caution. In addition, I employ a RDD using close elections between right-wing (politically experienced) and non-right-wing (politically inexperienced) candidates, and find that neither has a statistically significant effect. This result is intuitive, since it is difficult to find a reason why either ideology or political experience would cause politicians to award contracts to specific donors, except for the fact that donors provided a financial contribution.

Finally, it is important to note that the current RDD estimate shows the effect of electing a certain *type* of politician – being a donor-funded politician (a bundle of characteristics) – rather than donations themselves. While my fuzzy RDD estimate aims to

isolate the effect of donations from individual characteristics, see section 4.4.

4.3 Text analysis and contract price comparison

The paper aims to determine whether there was a price reward for donor contracts compared to non-donors contracts, for similar contracts. To maintain comparison with the RDD design, I compare contracts within donor funded incumbencies that were elected in a narrow margin. The narrow electoral margin allows the comparison to be made within similar municipalities, since their predetermined covariates are smooth across the cut-off (see Table A6). Moreover, focusing on donor-funded incumbencies, helps capture differential conditions for donors and non-donors, that the donor-funded mayor may be giving. Similarly, I compare contracts for donors of the runner-up vs. regular contracts, in non-donor funded incumbencies⁴¹ to check whether there is a punishment in contract terms for donors of the runner-up candidate.

A first approximation involved comparing the value of contracts for the exactly the same purpose contracts and in the same municipality, but the sample was limited. However, there could be several contracts that differ by one or two words but have the same purpose. In order to look for these, I used a natural language toolkit in Python, described in [Bird, Klein, and Loper \(2009\)](#). First, I removed the stopwords from the description of the purpose of the contract – i.e., words that are not relevant to explaining the contents of the contract (i.e., is, the, by, or, with). A complete list of stopwords used is available upon request. Then, I compared the same wording (except stopwords) for the purpose of the contract in the same municipality. This increased the sample size of comparison, but still there could be several contracts that are similar in purpose but slightly differ in words not identified in the stopwords. In order to make this comparison I employed a composite index that contains: 1) similarity in the sequence of wording that describes the purpose of the contract and 2) the Jaro-Winkler edit distance⁴² between the two contract purpose descriptions. I used this to construct an index of similarity that ranges between 0 and 1, where 1 is exactly the same wording of the contract except stop-words. The following table illustrates the process:

⁴¹Note that the only type of donors, in the non-donor funded incumbencies are donors for the runner-up.

⁴²For example, between the word *car* and *cart* there is an edit distance of 1.

Example of text analysis	
Donor-funded contract purpose	Non-donor-funded contract purpose
Support the administration in the adequate management, organization, collection and classification of the archives and all its components, of the archive of the administration of the municipality of Betania	Support administration in the management, organization, adequate collection and classification of the files and all its components of the municipal administration file
<i>Stopwords removed</i>	<i>Stopwords removed</i>
support administration adequate management organization collection classification archives components archive administration municipality betania	support administration management organization adequate collection classification files components municipal administration file

Similarity score

0.942

Since it is difficult to establish what is the minimum similarity score to be able to ensure a comparison, I calculated the difference in contract value for a similarity score from 0.9 or above to 1 in short intervals to ensure the result holds. Another potential concern is that contracts in different municipalities could have a different real value. Wages, vary considerably across regions in Colombia.⁴³ To scale the value of contracts relative to local wages, I calculated the average wage during the contracting period (2012–2105) in each municipality.⁴⁴

4.4 Fuzzy RDD Set-up

The main treatment in the first empirical design (i.e., having received donor funds) aims to estimate the effect of electing a donor-funded politician (a bundle of characteristics), but not the effect of donations themselves. To isolate the effect of money, given a set of personal characteristics, I use looser campaign limits as an instrument for the proportion of money received by candidates:

Second stage:

$$Y_i = \alpha + \beta_1 \widehat{DFP}_i + \beta_2 f(V_i) + \beta_3 DFP_i \times f(V_i) + \varepsilon_i. \quad (4)$$

Where DFP which is the percent of the winning candidate's campaign funds that came from donors. Y_i is the probability of a donor being awarded a contract, and the number of contracts awarded to donors.

In order to estimate the effect of DFP , I employ a fuzzy RDD, that instruments the proportion of donor funds received with campaign limits that jump discontinuously at

⁴³During 2012–2015 the average wage in municipalities ranged from 0.5 M COP, (equivalent to \$197 USD) to 1.9 (M COP)

⁴⁴Based on Harvard's Colombian Atlas of Economic Complexity, which gathered wage data from the Integrated Report of Social Security Contributions, managed by the Colombian Ministry of Health.

arbitrary registered voter thresholds. These cut-offs do not coincide with other public policy cut-offs, since they use the registered voters' cut-offs instead of population cut-offs. This is important because according to Grembi, Freier, Eggers, and Nannicini (2017), some studies use population cut-offs where several policies vary at the cut-off, which would imply a compound treatment. Such approach makes it difficult to disentangle the effect of one of the policies varying at the cut-off. Using this cut-offs I estimate the first stage:

First stage:

$$DFP_i = \beta_1 CampaignLimit_i + \beta_2 f(V_i) + \beta_3 CampaignLimit_i \times f(V_i) + \varepsilon_i. \quad (5)$$

Where *CampaignLimits* is a dummy variable taking a value of 1 if the municipality is over the 25,000 registered voters' cut-off that implies 110M COP campaign limit, 0 under the 25,000 registered voters which implies 58M COP campaign limit. I am unable to use other campaign limit cut-offs due to power restrictions, since most of the municipalities are under around the 25,000 registered voters (see Figure 1). Therefore, the sample is limited to municipalities with registered voters under 50,000, where campaign limits jump discontinuously again.⁴⁵ V_i = is the forcing variable, that is the number of registered voters around the 25,000 voters' cut-off, from 0 to 50,000. V_{it} = is centered around 0 that is $V_{it} = \text{Registered Voters}-25,000$, so $CampaignLimit_i = 1$ if $(V_i > 0)$. In particular:

$$CampaignLimit_i = \begin{cases} CampaignLimit_i = 1 & \text{if } V_i > 0 \\ CampaignLimit_i = 0 & \text{if } V_i < 0 \end{cases} \quad (6)$$

The premise is that the instrument is determined by an arbitrary institutional cut-off, and in a narrow margin h its assignment is uncorrelated with both municipality covariates and winning candidate characteristics. As with other RDD designs in a narrow margin of the campaign limit, it would be expected that there are smooth covariates on both sides of the cut-off.

Exclusion restriction

There could be other potential effects of campaign limits affecting the exclusion restriction. According to the formal model by Avis et al. (2017), fewer candidates may participate in elections where there are stricter campaign finance limits. Yet, Table 17 shows that when there are looser campaign limits, there are no differences in the total number of candidates participating in elections or the total number of donors. Another potential concern is that when there are more registered voters, municipalities are bigger, and more covariates could change, like politicians' salaries or the income of the municipality which could in turn affect the probability of giving a contract to a donor. A further concern is that with looser campaign limits there could be a political selection effect. For example, donors may prefer certain types of candidates (i.e., more right-wing), or

⁴⁵At 50,000 registered voters the limit jumps to 330M COP, from 110 M COP

certain types of candidates could be motivated to participate in elections if there is more campaign money to be raised.

In order to empirically check this potential violation of the exclusion restrictions, I check whether any municipality or individual-level covariates jump discontinuously at the cut-off. The results show that across 7 municipality covariates (See Table A12), and 10 individual-level covariates (See Table 15), when there are looser campaign limits there is 0.2% total less income from natural resource royalties in the municipality and a lower proportion of winners from an indigenous background. These differences could be by chance, since I am testing for numerous covariates, or potentially correlated with the outcome. As an additional robustness check I include the fuzzy RDD specifications both with and without these individual and municipality controls, and the results hold.

5 Results

5.1 Effects of electing a donor-funded politician

This section compares the effect of electing a donor-funded politician vs. a non-donor-funded politician. Before running this RDD, I check if there is manipulation of the electoral outcome. It could be the case that mayors who receive campaign donations systematically barely win (or lose), which would indicate that there is electoral manipulation. Running the McCrary test (McCrary, 2008) (see Figure 1) to check if there is any discontinuity in the density of vote share around the winning cut-off shows no discontinuity in the distribution at the winning cut-off.⁴⁶ As an additional robustness check, I test whether other measures of electoral manipulation jump discontinuously when the donor-funded politician wins. For this, I used measures of vote buying and turnout suppression used in Rueda and Ruiz (2017). It could be the case that the additional funding received by donor-funded politicians is used for vote buying in order to win elections. In addition, I use the measurements of violence developed by Restrepo, Vargas, and Spagat (2003), which identify violent attacks by armed groups. Violent groups could be pressuring the population to vote for a certain group, and then ask for contracts once their favored candidate wins. The results in Table A7 show that there is no difference in vote-buying reports, turnout suppression reports, or actual violence, which indicates that in a narrow margin, when the donor-funded candidate won, there is no evidence of electoral manipulation.

In order to conduct a valid RDD estimate, a potential threat to identification is that there are other covariates that jump discontinuously when a donor-funded candidate wins. Running a RDD estimate with the predetermined covariates as a dependent variable, I show that there is no difference among treatment and control groups in a broad range of covariates (see Tables 3 and A6). Municipality total income, mayors' salaries, and income

⁴⁶Similarly, for the arbitrary campaign cut-offs, there is no sorting of registered voters around the 25,000 voters' cut-off (see Figure 1).

from royalties do not vary discontinuously at the cut-off.

Another potential explanation for the outcome is that in races in which the donor-funded politician won, there were more donors in the election, and that is why more contracts were awarded to donors. However, results in Table 3, show this is not the case: a similar number of donors appear in the races whether the donor-funded candidate wins or loses. The same results holds for the size of the donations, and the number of contracts available in the municipality. Of the 16 covariates, I checked to see if they jumped discontinuously at the cut-off, there is only a systematic difference in the size of the local council (see Table 3). This could be a potential concern, since giving more contracts to donors could be explained by buying support of the local council via awarding contracts. In order to address this concern, I control for council size to determine whether results change.⁴⁷

What is expected to be different across treatment and control groups are the individual characteristics of politicians, given that receiving donor funding implies potential differences in characteristics. The results on individual characteristics (reported in Table 2) show that there is a difference in ideology: 28% of donor funded mayors are right-wing in a narrow electoral margin.⁴⁸ This could imply that donors prefer candidates who are ideologically aligned with them; they could prefer more pro-enterprise candidates, who tend to be more right-wing. Another individual covariate that jumps at the cut-off is political experience (having participated in an election before). It could be argued that candidates become more skilled at raising funds the more they participate in elections, or that donors prefer candidates with more experience in elections.

To further investigate whether ideology or experience affects whether contracts are awarded to donors I ran a regression including these characteristics as controls, and the result holds (see Table 5). In addition, I ran two separate RDDs in which the treatment is barely electing a right-wing politician or barely electing a politician who has political experience.⁴⁹ The results (available in Table 6) show that electing a right-wing candidate does not result in awarding more contracts to donors. Moreover, the proportion of donor-funded politicians remains constant, and donor income as a percent of the total do not vary discontinuously. Therefore, given the same level of donor involvement, electing a right-wing candidate by itself does not lead to more contracts given to donors. Table 7 shows similar results: electing a candidate with previous political experience does not lead to more contracts given to donors, with similar level of funding at both sides of the cut-off. As discussed earlier this result is intuitive given that it is difficult to find a reason why either ideology or political experience would cause politicians to award contracts to

⁴⁷This unbalanced covariate could be explained by chance given that I tested for discontinuities for 16 covariates and one could be significant.

⁴⁸This finding is in line with the US evidence reviewed in Stratmann (2005), which shows that campaign donors select candidates with the most experience who best align with their ideological preferences (Snyder, 1992).

⁴⁹Evidence of smooth covariates around the winning cut-off and the McCrary test for these RDDs is available upon request.

donors, except for the fact that donors provided a financial contribution.

Estimating equation (3) in Table 5, both with polynomials of order 1 and 2, and using both the conventional RDD estimate and the bias-corrected estimate proposed by Calonico et al. (2014), I find that when a donor-funded politician wins, the probability that he or she will award a contract to a donor increases by 9.6%. This result is robust to including the unbalanced covariate, council size in column (2) and the individual characteristics in column (3). However, the later specification is only indicative given the individual characteristics of donor funded politicians, are covariates that are part of the treatment (post-treatment). Nevertheless, the effect ranges from around 103–164% of the mean, which means that the probability of receiving a contract more than doubles when the donor-funded candidate wins. The results (displayed on Figure 2), show that the effect is slightly stronger close to the winning cut-off point, which could suggest that donor-funded politicians are more willing to award contracts when they win by a narrow margin. This could be to buy support for the next election, while a secure win may indicate less need to repay donors.⁵⁰ In order to check if the result holds for a variety of bandwidths, I estimate the result for bandwidths in small intervals and find that the results also hold across larger bandwidths (see Figure 3).

In addition to measuring the outcome the probability of receiving a contract, I measure the number of contracts obtained by donors. The results (presented in Table A8) show that donor-funded politicians give 5.9 more contracts to donors. However it is important to note that the result in total number of contracts is not significant using robust estimates nor for higher order polynomials, which indicated that electing a donor funded politicians, increases the probability of obtaining a contract, but does not increase the total number of contracts for donors, which would show the effect is on the extensive margin. The results so far show that electing donor-funded politicians indeed leads to higher probability for campaign donors to obtain a contract. But does this result necessarily entail more corruption and waste?

Table 8 shows that electing a donor-funded politician does not lead to having a higher income during the incumbency period, more tax collection, less operational expenditure or more investment in the budget, or a lower deficit. However, Table 9 shows that the probability of a mayor receiving a disciplinary sanction increases considerably when a donor funded politician wins, from an average of 2.45% to 9–11.6%. This result is robust to a variety of bandwidth sizes (see Figure 3) and different functional forms in Table 9. In order to check if the sanctions were related to contracting, I use the coding of whether the sanction or investigation was related to contracting as the dependent variable. Due to power restrictions, I pool sanctions and investigations related to contracts.⁵¹ The results, shown in Table 10, show that electing a donor-funded politician increases sanctions or investigations related to contracting by 8.6%, which is a sizable effect compared to the

⁵⁰However, the farther I go from the cut-off, the fewer balance covariates there is, and identification is likely to break.

⁵¹There were only 13 cases of sanctions related to contracting.

6.1% mean. This result is only suggestive because it is not robust to using a higher-order polynomial, or including controls, however it is significant when the biased corrected estimate is used. The result is valid only for bandwidths up to 10% (see Figure 3), yet the coefficient remains positive across larger bandwidths. This result is suggestive that the type of corruption taking place in the municipality is related to contracting. But what exactly are the mayors doing to benefit their donors?

5.2 Mechanism for benefiting donors: underlying contract characteristics

One way of understanding how donor-funded politicians may benefit their donors is to compare the contracts awarded to donors to regular non-donor contracts. When a donor funded politician wins, I expect to find that contractors who were funders will get more beneficial contract terms than those who were not funders. Similarly, when a non-donor-funded politician wins, they could be less likely to reward the donors of the runner-up, or may provide less favorable contract terms as punishment. Table 11 reports the conditions under which the contracts were given.

According to Table 11, when the donor-funded politician wins, contracts are mostly awarded to donors using the minimum-value modality 60.9% of the time, and using waiver contracts only 27% of the time. The proportion of minimum-value contracts for non-donors is lower, and this difference is statistically significant. This modality can provide a significant advantage for donors; it is only required to publish the call for applicants only for a day or more, it is not required to publish the call in the national on-line system, the sole criteria for awarding contracts is the lowest bidder, and there is no need of a committee to evaluate proposals.⁵²

A potential drawback of employing this modality of contracting for donors is that the contract value has to be under a minimum threshold which is 10% of the municipality income. However, a way to circumvent this, used by donor-funded politicians is to issue several contracts under minimum value modality. In Table 12, I check difference in donor characteristics, vs. regular contractor characteristics. Effectively, on average donors receive over 5 minimum value contracts, while regular contractors obtain 1.23 contracts. Moreover, the donor contracts in more sectors of the economy compared to a regular contractor, evidencing lack of specialization.

Looking at aggregate evidence of contracts, in Table 11, for donors, the average duration is much shorter, 60.5 days compared to 102.7 days for non-donors, and 83.5% of donor contracts finished execution by September 2016⁵³ while only 40% of contracts for non-donors finished by September 2016. Moreover, contracts for donors are signed earlier

⁵²This is in sharp contrast with a regular bid, where it is required to publish call for applications between 5 to 10 working days, this call has to be in the on-line system an evaluation committee can be used, and the award of the contract has to be justified publicly. And is in contrast to contracts assigned via waivers where it is necessary to legally justify and prove with evidence why the contract was given

⁵³Date when the dataset was obtained

in the incumbency term, mostly in the second year of incumbency, while regular contracts are given in the last two years of incumbency. Given the short duration and earlier assignment it is more likely that contracts finish during a Mayor incumbency term, compared to regular contracts as evidenced in Table 11. Moreover donors who are contractors tend to be individuals, rather than established companies. Interestingly, if a non-donor Mayor is elected, there is no statistically significant difference in how contracts are assigned to donors compared to non-donors, so there is no evidence of punishment to donors of the runner-up. Rather donors of the loser candidate face the same contracting conditions as any other contractor in the municipality. Moreover donors who are able to obtain a contract despite being a donors of the runner-up tend to be more companies rather than contracts for individuals.

Table A9 shows the sector of the economy of contracts for donors and non-donors under different mayor types. When the donor-funded politician wins and donors are awarded contracts, 44% of these contracts are given in the personal services sector⁵⁴. Interestingly, donor-funded contracts have a larger proportion of supplies of materials and machinery, as well as supplies for the municipality. These types of contracts can be quickly executed (and paid off). Surprisingly, non-donors tend to be awarded more construction contracts (15.6%, compared to 12.5% to donors). This discrepancy may be because construction contracts take longer to execute, and therefore they are less likely to finish during an incumbency period. Moreover, they tend to be larger contracts, and are therefore legally required to open up for competitive bids, and therefore more likely to be under scrutiny. It is easier for mayors to award contracts under the minimum-value modality, which the sole criteria for assigning them is the price, and pay them off quickly when they are supply contracts. Looking at the legal category of contracts in Table A10, I verify that donor contracts tend to be more concentrated in supplies compared to non-donors contracts.

5.2.1 Multiplier of contracts for donors

In order to quantify the potential benefits for donors, in this section I measure the amount obtained in contracts compared to the amount donated⁵⁵. I do this in a narrow electoral margin, where municipality characteristics are similar. Since municipality underlying characteristics are similar (see Table 3): municipality income, number of contracts in the municipality, number of donors in the race, it would be expected that overall market conditions for contracting are similar, except the favorable conditions for contractors who were donors. In particular it would be expected that rewards for donors are higher when the donor funded politician wins:

$$\text{Multiplier} = \text{Total Value of Contracts Received} / \text{Total Value of Donations Made} \quad (7)$$

⁵⁴For example, consultancies, accountants, drivers.

⁵⁵To calculate the exact return to investment, the cost of executing the contract would be necessary

$$MultiplierScaled = \frac{(TotalValueofContractsReceived - TotalValueofDonationsMade)}{TotalValueofDonationsMade} \quad (8)$$

Donors receive a total value in contracts that is, on average, *10.67 times* the value they donated (see Table A11). This value is very large, and Table A11 reveals that this result is driven by several outliers in the distribution of the multiplier, which implies that for several donors there can be a huge return. Table 13 shows that when a donor-funded candidate is elected, donors obtain on average 13.76 times the value they donated. Surprisingly, even when the non-donor politician is elected, donors on average still obtain a positive multiplier of 6.57, which is driven by high-value contracts for the few donors (see Table 13) who obtain large construction contracts despite donating to the candidate who lost by a narrow margin. In order to compare the multiplier for donors when a donor is elected vs. when a non-donor is elected, I employ a t-test. In addition I report the p-values of comparison of medians using the Fisher exact test to account for outliers. The evidence shows that there is a difference of 7.1 in the multiplier, and this difference is statistically significant, which shows that there are higher benefits to donors when the donor-funded politician wins. Most interestingly, average donor contributions in the race when a donor funded politician barely wins or loses are similar, 9 M COP and 7.8 M COP respectively, but the benefits for donors diverge substantially if the donor funded politician wins.

5.2.2 Price premium for donors

A key part of this paper is determining whether there was a price reward for donor contracts compared to non-donors contracts for similar contracts. I compare contracts for donors to non-donor regular contracts within a narrow electoral margin of donor-funded incumbencies. This is to capture how the same type of mayor can treat contractors differentially.

Using the detailed purpose of the contract, and after stripping the purpose of the contract of irrelevant words (stop-words) and comparing identical-purpose contracts (similarity score=1), I found 31 identical contract purposes between donors and non-donors. The price premium for the donor contracts was 1.7 local wages⁵⁶ higher. In order to increase the sample size, I compared contracts with a similarity score of 0.9 or above, and obtained 81 similar contracts that have a difference of 2.3 local wages (See Table 14). A more systematic comparison of the price inflation across different similarity scores can be found in Figure 4. Independent of the score, the price premium for donors is between 3M to 2M COP, which is around 2.2 to 1.9 average local monthly wages. That contracts awarded to donors have a systematically higher pay for a broad range of similar contracts

⁵⁶That is the total value of the contract divided by the local wage.

indicates that there is indeed a premium for being a donor. This evidence is indicative because the price differences could be explained by the fact that the quality of goods provided by donors systematically are higher. However, given that donors contract in more sectors of the economy according to results in Table 12, it less likely that the quality of the good provided is higher, given the lack of economic specialization.

5.3 Case Study

A more detailed case study can further the understanding how contracts are provided for donors and the price premium.

The case of municipality “Village town”, in the department of Meta.⁵⁷

The 2011 election was one of the closest in the history of “Village town”, Meta. The candidate “Juan” of the traditional political party was elected mayor by a narrow electoral margin. According to the National Registry office, “Village town” had less than 25,000 registered voters in 2011, and therefore each candidate could legally raise a maximum of 58M COP. The traditional party candidate was donor funded and was able to raise approximately 31M COP, while the runner-up was self-funded (his wife gave him 2M COP).

Systematic use of minimum value modality contracts: “Donor A” was one of the donors for the winner electoral campaign, with 1.2 M COP. During “Juan’s” incumbency period he obtained 122 public contracts for over 950 M COP, 791 times the value he donated. 117 of the contracts awarded were given via minimum value contracts, representing 880M COP of the total contracts received. Looking at the contracting process for “Donor A” unique patterns can be found. Most of the 117 contracts were awarded for “Donor A” as the *unique* bidder and the bid was open for one day, which is the minimum required by law. This suggests that any person or firm interested in providing the good or service had to present its proposal and comply with all the Colombian regulations for being a contractor within 24 hours. Most likely, the donor knew the bid was coming up. Another interesting aspect is that the contracts assigned for “Donor A” covered a broad range of goods and services, indicating lack of specialization. “Donor A” was a regular contractor for “support” to the municipality meetings, and was also hired for IT and software maintenance for the Mayor’s office. “Donor A” also was one of the main suppliers of the municipality, he received contracts for providing food and lodging for the municipality’s events, he was also hired for providing sound equipment, stationery, clothes and even propellers, among other things. Due to the lack of specialization it would be difficult to argue that the quality of products provided by “Donor A” were higher to justify a price premium. However this evidence is just suggestive, since I don’t have an assessment on quality.

Same purpose of the contract, higher price for donor: In November 2014, the

⁵⁷Name of the municipality and names of the people involved have been changed for security reasons.

municipality of “Village Town”, headed by “Juan”, signed a contract with “Donor B”, who had donated 1.5M COP to the campaign. The project involved technical support for monitoring infrastructure projects in the municipality. The Mayor’s office justified contracting this service directly by noting the relatively high degree of specialization that this project required, as it was necessary to hire a contractor with knowledge of civil engineering or architecture. This implied that a public tender or competitive process was not opened. The contract had a duration of *six months* and a total value of 24M COP. The contractor received monthly payments of 4M COP upon completion of its monthly duties. The project was executed on a regular basis, with no additions or abnormal activities. By the end of the contract, the contractor had received 100% of the contract amount, which was *16 times* the value of his donation. In April 2015 the Municipality of “Village Town” signed a contract with “Non-donor B”, a non-donor contractor. Both the main objective and all the specific tasks of the contract were exactly the same as those specified in “Donor B”contract. However, the duration of the contract was only 4 months. Furthermore, even though the technical specificities of both contractors were identical, “Non-donor B” received monthly payments of 3.2M COP, for a total of 12.9M COP. Thus for the same tasks, the value of the non-donor contract was 53% of the total value of the donor contract.

Both qualitative and quantitative evidence in these sections suggests that donors of the donor funded politician, have a higher multiplier, have contracts under the minimum value modality that could limit competition and screening, and could potentially enjoy a price premium for the same purpose of the contract. The latter could indeed affect public good provision, because with limited budgets, municipalities end up paying more for the similar type of good provided. This could provide a reason why campaign limits should to be enacted. But are they effective?

5.4 Effects of campaign limits

At 25,000 registered voters, campaign limits jumps from 58M COP to 110M COP, and the law establishes that individual donors can donate up to 10% of the total limit. Figure A-5 illustrates that under 25,000, the average donor contribution per municipality is consistently under the 5.8M COP limit which show that limits are indeed binding. There is a small standard deviation and average donations tend to be close to the limit. Above the cut-off where the individual contribution limit is 11 M COP, the average contribution increases and has a larger standard deviation. Running the RDD using the arbitrary campaign limits, I find that when there are looser campaign limits (moving from 5.8 M COP to 11 M COP per individual contribution), the average donor contribution increases to 3.8M COP (see Table 16), and candidates reduce self-funding of their own campaigns, however this latter difference is not statistically significant. Most interestingly, according to evidence in Table 16 there isn’t a higher total campaign income product of looser campaign limits, rather a substitution effects from using less own income to finance campaign

to using donors. As a result, the participation of donor income as a percent of total campaign income increases by 27% (or analogously more likely to elect a donor-funded politician). Moreover looking at results in Table 17 show that the total number of donors remain constant. So there are similar numbers of donors, increasing average size of the donation, and as a result increasing their participation in the candidate campaign portfolio. A potential concern with using the campaign limits as an instrument for proportion of donors funds, is that there are other variables jumping discontinuously at the cut-off. However as discussed in the exclusion restriction section (see section 4.4 - exclusion restriction), testing for over 17 covariates, most do not jump discontinuously. There are two predetermined covariates that do, and therefore as additional robustness check they will be included as controls.

Estimating the fuzzy RDD, the results in Table 18 indicate that allowing looser campaign limits leads to 27–32% more donor income as percent of total income, which in turn leads to 49–52 more contracts for donors, and this result is robust to including unbalanced controls. As an additional robustness check instead of instrumenting the proportion of donor funds, I instrument a dummy whether the politician was donor-funded, and the result holds: (columns 3 and 4), looser campaign limits lead to an increase of 31–32 more contracts to donors.

Interestingly changing the dependent variable from the number of contracts for donors to the probability that donors receive contracts, in Table A13 there is a positive coefficient but not significant. Looking back at Table 16, the number of donors remain constant but rather the average contribution increases, therefore the effect of campaign limits is on the intensive margin, rather than increasing the probability for donors.

6 Discussion

Decisions by voters on the type of politician elected does matter. If donor funded politicians are elected, it increases the probability of donors receiving contracts. Electing donor funded politicians can lead to more investigations or sanctions towards the Mayor, and these sanctions are likely to be related to improper contracting.

Giving donors to contracts can be costly because they result in procurement processes that are less transparent, and can limit competition by other bidders; in particular the minimum value modality used allows for short calls and there is no advertisement online for the procurement bid and the sole criteria for assigning the contract is the price. This allows for easier assignment of contracts to donors. These practices can be costly because donor contracts receive a price premium compared to non-donor contracts for a similar type of contract. However, the size of distortion for donors does not affect the overall budget deficit or investment level in the municipality, rather within the municipality to who and how contracts are assigned. My study represents a lower bounds estimate of money in politics, because there are other forms that money participates in politics, such as lobby or bribes that could lead to further benefits for donors. It is important to note

that the price differential observed could be attributed to a difference in quality of the good provided. I provide suggestive qualitative and quantitative evidence that indicates that this is not necessarily the case given the lack of expertise and specialization of donors in an economic sector, in which it is difficult to argue they provide higher quality goods. However, more systematic evidence can be collected.

This paper shows that contract assignment does not necessarily strictly depend on the economic capacity of companies/individuals; but rather can depend on political contributions made during the campaign. In particular, the size of the payoff for donors depends on the electoral victory of the candidates supported. This result is important because economic returns of a company could depend on an event unrelated to the actual economic capacity of companies, rather the capacity to donate to the winner politician. Finally, the paper shows that looser campaign limits leads an increase in the participation of donor funds as a % of total income, and therefore conferring more contracts to donors. This paper provides the first –to the best of my knowledge– causal evaluation of how campaign limits are effective in reducing the size of the reward for donors.

Given the evidence collected in this paper I can ask again: *Is democratic government inevitable something of a sham?* ([Arrow, 1978](#), pg.479) Evidence by this paper suggests that voter political selection of donor funded politicians make a difference. However, this could depend on the proportion of informed voters of the sources of financing of candidates ([Coate, 2004](#)). More advertising about the transparency system of campaign sources could increase the proportion of informed voters. Moreover, this paper finds that institutional rules under democracies, such as campaign limits, can reduce the influence of money in contract assignment. So decisions and rules within democracies, can limit the influence of money and make the system less of a sham.

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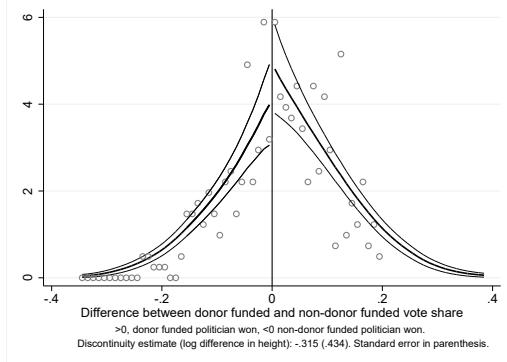
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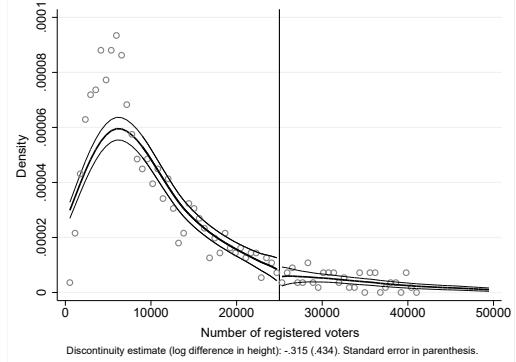
7 Figures

Figure 1: McCrary tests

Winning margin for donor-funded politician:

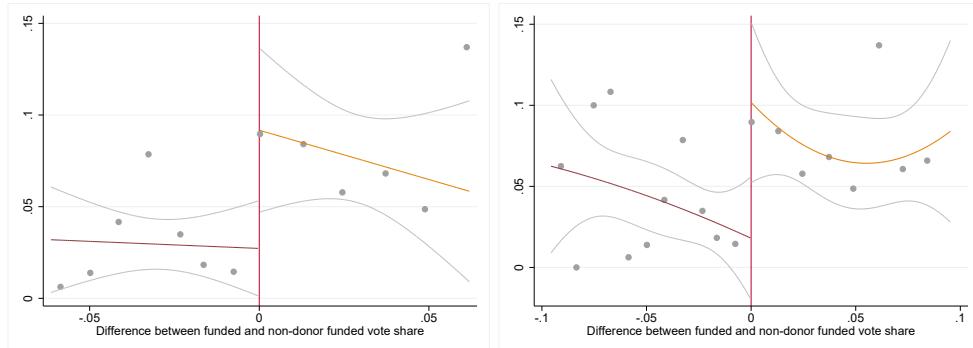


Municipality registered voters:



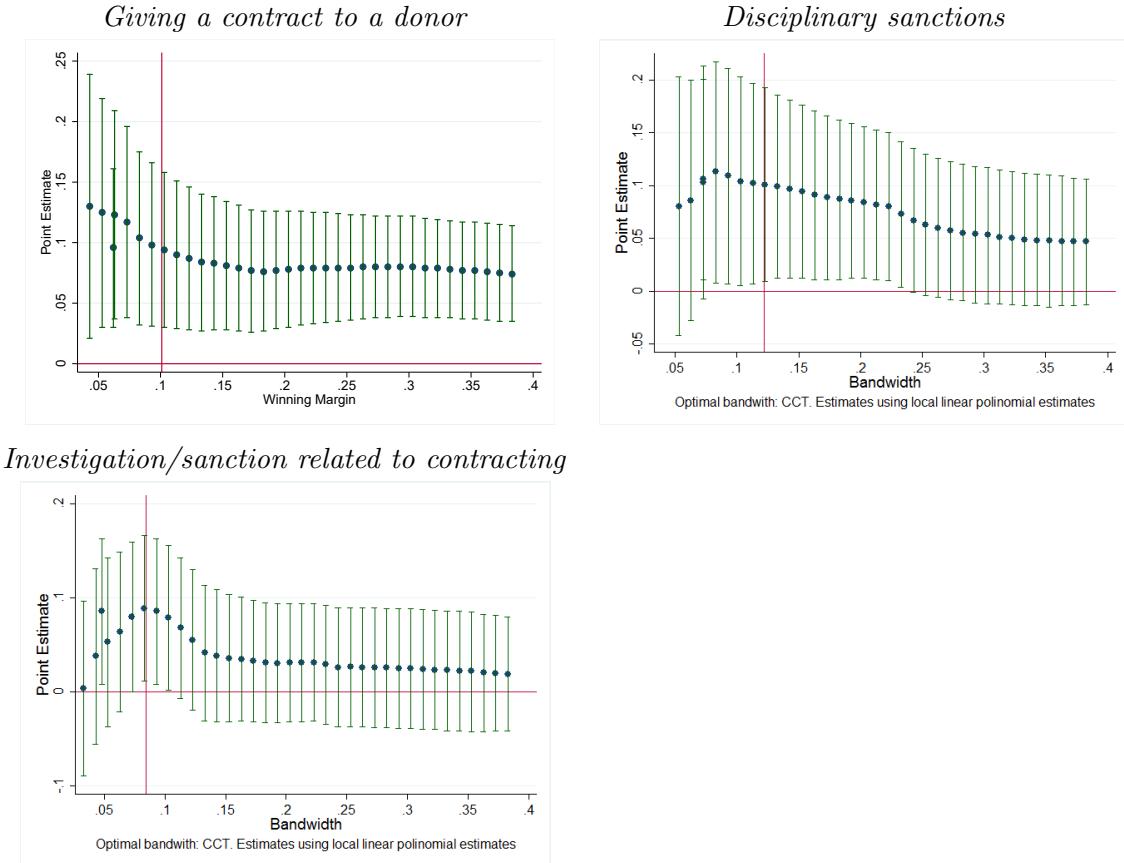
Note: Left: Distribution of winning margins for donor-funded politicians. Right: Distribution of registered voters.

Figure 2: Effect of electing a donor funded on probability of donors receiving contracts



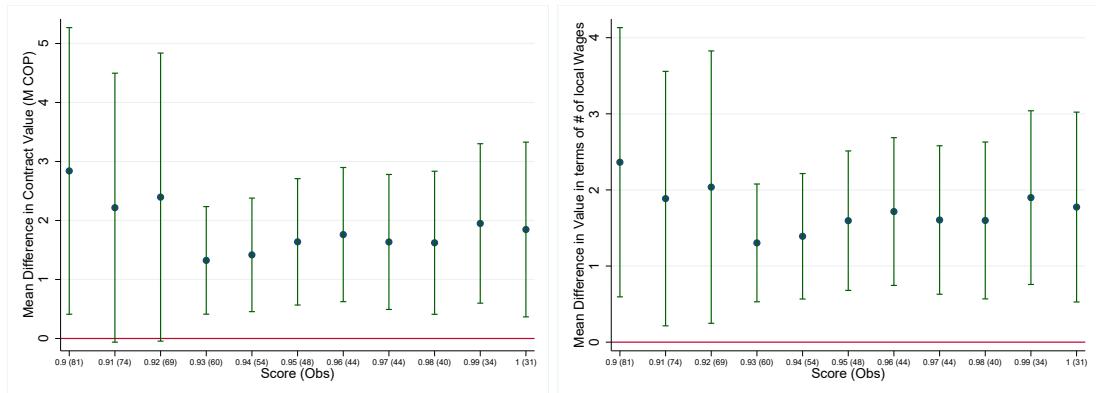
Note: Observations within Calonico et al. (2014) bandwidth displayed. Left: linear fit. Right: quadratic fit. Bin selection method: mimicking variance, evenly spaced using spacings estimators.

Figure 3: Different bandwidth sizes. Effect of electing a donor funded politician on:



Note: Red line denotes the bandwidth used for the bias correction used.

Figure 4: Price difference between donor funded contracts and regular contracts



Left: Value of contract (M COP). Right: Value of contracts in terms of # of local wages.

8 Tables

8.1 Effects of electing a donor funded politician

Table 1: Sources of campaign income across candidate types, % of Total Income

	Non Donor-Funded	Donor-Funded	Mean Difference
Self and family	0.948	0.561	0.387***
Only self funding	0.734	0.382	0.352***
Donor funded campaign	0.000	0.399	-0.399***
Credits obtained financial institutions	0.009	0.007	0.002
Party public events fund-raisers	0.013	0.021	-0.008
State funding	0.006	0.000	0.006
Direct party funding	0.023	0.011	0.011
Total income of campaign (M COP)	32.087	40.765	-8.677***

Note: The number of observations are 164 and 244 for non-donor and donor-funded group respectively. *** p<0.01, ** p<0.05, * p<0.1

Table 2: Differences in individual characteristics between donor funded politicians and non-donor funded

Dependent variable	Mean (1)	Std. Dev. (2)	Donor fund. won (3)	Std. Error. (4)	Obs (5)	Bandwidth (6)	P-value (7)
<i>Individual covariates</i>							
Women	0.105	0.307	0.051	0.109	241	0.083	0.640
Age	44.398	9.318	-4.353	3.218	209	0.075	0.176
Black	0.050	0.219	-0.037	0.126	165	0.051	0.766
Asian	0.122	0.328	0.042	0.118	203	0.074	0.721
Leftist party	0.034	0.182	0.007	0.091	206	0.069	0.936
Rightwing	0.221	0.415	0.281**	0.127	178	0.054	0.028
Previously sanctioned	0.100	0.301	-0.048	0.105	196	0.062	0.643
Illegal Registration of ID.	0.007	0.086	0.004	0.012	204	0.067	0.727
Has political experience	0.449	0.498	0.352**	0.164	168	0.050	0.031
Has electoral experience	0.355	0.479	0.132	0.151	191	0.061	0.380

Notes: Columns 1 and 2 report the basic descriptive statistics of each variable. Column 3 reports RDD point estimates of the effect of a donor funded victory in mayor elections on each variable, using Calonico et al. (2014) optimal bandwidths (reported in column 6), bias correction, and robust standard errors (column 4). All use linear local polynomials and triangular kernels. Column 5 reports the number of observations including in each estimation.

Table 3: Smooth campaign and municipality covariates across the donor funded victory cut-off

Dependent variable	Mean (1)	Std. Dev. (2)	Donor fund won (3)	Std. Error. (4)	Obs (5)	Bandwidth (6)	P-value (7)
<i>Campaign covariates</i>							
Total number of donors (winner + runner-up)	7.642	5.714	1.651	1.816	196	0.062	0.364
Total value donations (winner + runner-up)	67.565	57.108	35.337	27.952	188	0.059	0.206
Total number of contracts in municipality	801.040	687.561	24.935	211.029	197	0.064	0.906
<i>Covariates potentially related to contracting</i>							
Disposable income (mw)	6788.060	10678.524	-790.892	2941.446	229	0.081	0.788
Municipal category	5.870	0.552	0.033	0.180	212	0.071	0.852
Mayor wages	6.265	1.141	-0.067	0.360	212	0.071	0.851
Council size	10.779	2.287	1.138*	0.680	254	0.091	0.094
Total population	20091.512	21779.559	9041.764	6872.333	220	0.074	0.188
Income from royalties	0.080	0.167	0.037	0.058	279	0.105	0.530
Education establishments	280.079	158.969	-86.886	53.183	188	0.059	0.102

Notes: Columns 1 and 2 report the basic descriptive statistics of each variable. Column 3 reports RDD point estimates of the effect of a donor-funded victory in mayor elections on each variable, using Calonico et al. (2014)'s optimal bandwidths (reported in column 6), bias correction, and robust standard errors (column 4). All estimates use linear local polynomials and triangular kernels. Column 5 reports the number of observations including in each estimation.

Table 4: Donors assignment of contracts in donor funded vs non-donor funded incumbencies:

	Donor funded elected			Non-donor funded elected		
	Donor got contract			Donor got contract		
	No	Yes	Total	No	Yes	Total
Donors of winner	1,251	166	1,417	132	0	132
Donors of winner (%)	88.29	11.71	100	100	0	100
Donors of runner-up	351	4	355	891	38	929
Donors of runner-up (%)	98.87	1.13	100	95.91	4.09	100
Total donors (#)	1,602	170	1,772	1,023	38	1,061
Total donors(%)	90.41	9.59	100	96.42	3.58	100

Note: The base sample is 408 municipalities where the race was contested between donor funded politicians and non-donor funded politicians. There are 132 “donors” of the winner when a non donor funded politician wins. These are contributions by the candidates or their families. Similarly, 355 “donors” of the runner-up when the donor funded is elected, are contributions by the candidates or their families. 4 of these runner-up candidates/families got a contract. Note that donors probability of receiving a contract is positive, both when the donor funded politician wins (9.59%) or the non-donor wins (3.58%).

Table 5: Effect of electing a donor funded politician on probability of donor receiving a contract

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Loc.</i>	<i>Linear Pol-1</i>		<i>Loc.</i>	<i>Linear Pol-2</i>	
Donor Funded Elected <i>(Conventional Estimate)</i>	0.086** (0.033)	0.077** (0.033)	0.061* (0.031)	0.097** (0.041)	0.083* (0.040)	0.076* (0.040)
Donor Funded Elected <i>(Robust Estimate)</i>	0.096** (0.040)	0.085** (0.040)	0.067* (0.037)	0.104** (0.046)	0.089* (0.045)	0.082* (0.045)
Council Size		✓	✓		✓	✓
Individual Characteristics			✓			✓
Observations	196	198	200	266	274	254
Mean	0.059	0.059	0.059	0.059	0.059	0.059
Effect Mean(%)	145.76	130.51	103.39	164.41	140.68	128.81
Bandwidth	0.062	0.064	0.065	0.096	0.100	0.092
(Local) polynomial order	1	1	1	2	2	2

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust estimate includes robust standard errors and the optimal bandwidth by Calonico et. al (2014)

Individual characteristics: rightwing and political experience

Table 6: Effect of electing rightwing politician on probability of donor receiving a contract

	(1)	(2)	(3)
	Contract Given to Donor	Donor Fund. Elec	% of Donor Income
Right-Wing Elected <i>(Conventional Estimate)</i>	0.074 (0.053)	0.166 (0.130)	0.016 (0.082)
Right-Wing Elected <i>(Robust Estimate)</i>	0.092 (0.063)	0.183 (0.155)	0.002 (0.097)
Observations	262	278	258
Mean	0.049	0.418	0.321
Effect Mean(%)	151.02	39.71	4.98
Bandwidth	0.079	0.089	0.077
(Local) polynomial order	1	1	1

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust estimate includes robust standard errors and the optimal bandwidth by Calonico et. al (2014).

Table 7: Effect of electing a politically experienced politician on probability of donor receiving a contract

	(1) Contract Given to Donor	(2) Donor Fund. Elec	(3) % of Donor Income
Politically Experienced Elected <i>(Conventional Estimate)</i>	-0.035 (0.040)	0.124 (0.131)	0.102 (0.076)
Politically Experienced Elected <i>(Robust Estimate)</i>	-0.046 (0.047)	0.131 (0.157)	0.092 (0.092)
Observations	247	261	267
Mean	0.049	0.418	0.321
Effect Mean(%)	-71.43	29.67	31.78
Bandwidth	0.072	0.075	0.077
(Local) polynomial order	1	1	1

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust estimate includes robust standard errors and the optimal bandwidth by Calonico et. al (2014).

Table 8: Effect of electing a donor funded politician on fiscal policy variables

	(1) Total Y (M COP)	(2) Total Exp (M COP)	(3) Land Taxes (%Y)	(4) Industry (%Y)	(5) Funct. expen. (%Y)	(6) Investment (%Y)	(7) Deficit (%Y)
Donor Funded Elected <i>(Conventional Estimate)</i>	8619.958 (6795.505)	8125.027 (7012.234)	1.000 (1.156)	1.026 (1.060)	-1.369 (1.849)	-3.839 (3.627)	0.022 (2.734)
Donor Funded Elected <i>(Robust Estimate)</i>	7441.626 (8088.467)	6944.837 (8420.937)	1.170 (1.381)	1.631 (1.100)	-1.483 (2.312)	-5.041 (4.227)	-0.195 (3.272)
Observations	218	238	200	179	204	224	206
Mean	49629.582	50061.531	3.877	3.416	12.591	89.986	-11.384
Effect Mean(%)	17.37	16.23	25.79	30.04	-10.87	-4.27	-0.19
Bandwidth	0.074	0.082	0.066	0.055	0.068	0.075	0.069
(Local) polynomial order	1	1	1	1	1	1	1

*** p<0.01, ** p<0.05, * p<0.1

Note: Conventional estimate includes robust standard errors and the optimal bandwidth by Calonico et. al (2014).

Table 9: Effect of electing a donor funded politician on mayor being sanctioned

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Loc. Linear Pol-1</i>			<i>Loc. Linear Pol-2</i>		
Donor Funded Elected <i>(Conventional Estimate)</i>	0.091* (0.049)	0.090* (0.050)	0.096* (0.051)	0.114* (0.063)	0.110 (0.063)	0.112* (0.063)
Donor Funded Elected <i>(Robust Estimate)</i>	0.106* (0.058)	0.105* (0.059)	0.110* (0.060)	0.116* (0.070)	0.112 (0.070)	0.116* (0.070)
Council Size		✓	✓		✓	✓
Individual Characteristics			✓			✓
Observations	215	212	200	248	248	246
Mean	0.025	0.025	0.025	0.025	0.025	0.025
Effect Mean(%)	364.00	360.00	384.00	456.00	440.00	448.00
Bandwidth	0.073	0.071	0.066	0.086	0.086	0.085
(Local) polynomial order	1	1	1	2	2	2

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust estimate includes robust standard errors and the optimal bandwidth by Calonico et. al (2014)

Individual characteristics: rightwing and political experience

Table 10: Effect of electing a donor funded politician on investigations or sanctions related to contracting

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Loc. Linear Pol-1</i>			<i>Loc. Linear Pol-2</i>		
Donor Funded Elected <i>(Conventional Estimate)</i>	0.066* (0.041)	0.057 (0.041)	0.038 (0.041)	0.061 (0.053)	0.048 (0.053)	0.034 (0.053)
Donor Funded Elected <i>(Robust Estimate)</i>	0.086* (0.047)	0.077 (0.048)	0.056 (0.049)	0.056 (0.060)	0.044 (0.061)	0.032 (0.060)
Council Size		✓	✓		✓	✓
Individual Characteristics			✓			✓
Observations	156	167	169	187	189	191
Mean	0.061	0.061	0.061	0.061	0.061	0.061
Effect Mean(%)	108.20	93.44	62.30	100.00	78.69	55.74
Bandwidth	0.048	0.049	0.051	0.058	0.059	0.060
(Local) polynomial order	1	1	1	2	2	2

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust estimate includes robust standard errors and the optimal bandwidth by Calonico et. al (2014)

Individual characteristics: rightwing and political experience

8.2 Contract Level results

Table 11: Comparison of contracts by mayor type, and whether the contractors were donors

Contractors were:	Donor-funded Mayor			Non-donor funded Mayor		
	Non-donors	Donors	Mean Difference	Non-Donors	Donors	Mean Difference
Avg. Minimum value contracts	0.477	0.609	-0.132***	0.427	0.488	-0.060
Avg. Assigned via waivers	0.434	0.278	0.155***	0.470	0.390	0.080
Avg. Contract duration (days)	102.734	60.535	42.198***	111.058	119.512	-8.454
Avg. Percent time addition	0.024	0.015	0.008	0.022	0.037	-0.015
Avg. Percent value addition	0.013	0.019	-0.005	0.015	0.033	-0.017
Avg. Contracts that finished (*)	0.398	0.835	-0.436***	0.378	0.427	-0.049
Contract for a company	0.307	0.278	0.029	0.282	0.585	-0.304***
Contract for an individual	0.608	0.696	-0.089***	0.645	0.415	0.230***
Contract finished during incumb.	0.140	0.267	-0.127***	0.136	0.159	-0.023
Year 1 contract signed	0.190	0.202	-0.012	0.173	0.159	0.014
Year 2 contract signed	0.252	0.327	-0.074***	0.241	0.220	0.022
Year 3 contract signed	0.280	0.253	0.028	0.301	0.268	0.032
Year 4 contract signed	0.277	0.218	0.059***	0.285	0.354	-0.068

*** p<0.01, ** p<0.05, * p<0.1

Note: The number of observations are 70661 and 514 for control and treatment group in columns (1) and (2) respectively. For columns (4) and (5), the number of observations are 60850 and 82 for control. Base sample are contracts in municipalities within a narrow electoral margin of 6.2% in (1) Table 5. (*) Finished by September 2016 when this data was gathered.

8.3 Donor Level results

Table 12: Comparison of contractors characteristics by Mayor type, and whether the contractors were donors

Contractors characteristics:	Donor-funded Mayor			Non-donor funded Mayor		
	Non-donors	Donors	Mean Difference	Non-Donors	Donors	Mean Difference
Total # of Municipalities	10.020	1.298	8.721***	10.145	2.647	7.498***
Total # of Contracts	2.723	9.018	-6.294**	2.851	4.824	-1.972
Total # of Min. Value Cont.	1.121	5.298	-4.177*	1.083	2.059	-0.976
Total # of Economic Sectors	1.233	1.930	-0.697***	1.230	1.412	-0.182

*** p<0.01, ** p<0.05, * p<0.1

Note: Base sample are contractors in municipalities within a narrow electoral margin of 6.2% in (1) Table 5. The number of observations are 28239 and 57 for control and treatment group in columns (1) and (2) respectively. For columns (4) and (5), the number of observations are 22934 and 17 for control.

Table 13: Donor multipliers separated by victory of donor funded politician

	Non-Donor Elected	Donor-Elected	Mean Difference	P-value means	P-value-medians
Total donation value(COP M)	7.894	9.030	-1.136	0.073*	0.120
Total contract value (COP M)	6.128	25.888	-19.761	0.045**	0.000***
Multiplier	6.673	13.759	-7.086	0.118	0.000***
Multiplier scaled	5.673	12.759	-7.086	0.118	0.000***

Note: Base sample are contractors in municipalities within a narrow electoral margin of 6.2% in (1) Table 5.. The number of observations are 662 and 847 donors for non-donor elected incumbencies and donor-elected incumbencies respectively. Multiplier is Total contract value/Total donated value. P-values for means comes from a t-test of difference in means. P-values for medias comes from a Fisher exact test. *** p<0.01, ** p<0.05, * p<0.1

8.4 Price premium for donors

Table 14: Price Comparison of similar type of contracts for donors and non-donors

Contract Value			Contract value in local wages				
Donor-Contracts	Regular	Mean Difference	Donor-Contracts	Regular	Mean Difference		
Score=1 (31)	11.691	9.844	1.847**	Score=1	10.222	8.447	1.775**
Score>0.9 (81)	11.448	8.608	2.840*	Score=0.9	9.817	7.453	2.364**

*** p<0.01, ** p<0.05, * p<0.1

Note: T-test paired comparison. Number of observations in parenthesis. Score is the similarity score in contract purpose contents. The score removes stop words, and combines the sequence and edit distance. Score is between 0 and 1, where 1 indicates same contract purpose. Base sample are contracts in municipalities within a narrow electoral margin of 6.2% in (1) Table 5.

8.5 Results on Campaign Limits

Table 15: Smooth predetermined municipality covariates across looser campaign limits cut-off

Dependent variable	Mean (1)	Std. Dev. (2)	Donor fund. won (3)	Std. Error. (4)	Obs (5)	Bandwidth (6)	P-value (7)
<i>Individual covariates</i>							
Women	0.096	0.295	-0.044	0.077	95	5582.719	0.569
Age	44.985	9.698	-2.363	5.804	98	6593.576	0.684
Black	0.047	0.211	-0.106	0.103	76	5227.417	0.304
Indigenous background	0.112	0.315	-0.351**	0.153	105	6848.513	0.022
Leftist party	0.026	0.160	-0.021	0.069	96	5624.339	0.759
Rightwing	0.244	0.430	0.442	0.293	122	6782.446	0.132
Previously sanctioned	0.111	0.315	0.012	0.175	80	4921.141	0.945
Ilegal Registration of ID.	0.008	0.089	0.004	0.003	48	3253.438	0.243
Has political experience	0.455	0.498	-0.245	0.217	143	8069.598	0.259
Has electoral experience	0.366	0.482	-0.302	0.269	100	5938.124	0.262

Notes: Columns 1 and 2 report the basic descriptive statistics of each variable. Column 3 reports RDD point estimates of the effect of looser campaign limits on each variable, using Calonico et al. (2014)'s optimal bandwidths (reported in column 6), bias correction, and robust standard errors (column 4), with linear local polynomials and triangular kernels. Column 5 reports the number of observations including in each estimation.

Table 16: Effect of looser campaign limints on:

	(1) Average own contribution M(COP)	(2) Average donation M(COP)	(3) Total campaign income M(COP)	(4) Donor Income as % of Total	(5) Candidate was donor-funded
Looser Campaign Limits (Conventional Estimate)	-8.471 (7.024)	3.827 (9.902)	-13.779 (1.763)	0.268* (0.134)	0.626*** (0.186)
Looser Campaign Limits (Robust Estimate)	-9.374 (8.693)	3.225 (11.947)	-16.286 (2.388)	0.273* (0.165)	0.688*** (0.211)
Observations	143	55	73	141	80
Mean	19.051	2.640	52.922	0.187	0.455
Effect Mean(Per)	-44.46	144.96	-26.036	143.32	137.58
Bandwidth	8045.595	3499.152	4653.014	7867.652	4930.065
(Local) polynomial order	1	1	1	1	1

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust estimate includes robust standard errors and the optimal bandwidth by Calonico et. al (2014)

Table 17: Effect of looser campaign limits on:

	(1) Total candidates	(3) Total # of donors
Looser Campaign Limits <i>(Conventional Estimate)</i>	1.173 (0.708)	-0.532 (0.372)
Looser Campaign Limits <i>(Robust Estimate)</i>	1.319 (0.849)	-0.484 (0.448)
Observations	125	65
Mean	4.054	1.998
Effect Mean(Per)	28.93	-26.63
Bandwidth	6986.524	4323.794
(Local) polynomial order	1	1

*** p<0.01, ** p<0.05, * p<0.1

Note: *Robust estimate* includes robust standard errors and the optimal bandwidth by Calonico et al. (2014)

Table 18: Effects of looser campaign limits on total number of contracts for donors

	(1)	(2)	(3)	(4)
<i>Panel A: Fuzzy RDD estimates</i>				
Total contracts for donors				
Looser Campaign Limits <i>(Conventional Estimate)</i>	50.367** (24.852)	47.882* (15.619)	31.722** (15.65)	24.086* (14.46)
Looser Campaign Limits <i>(Robust Estimate)</i>	52.64* (30.011)	49.451* (29.96)	32.838* (18.114)	23.807 (16.154)
Controls		✓		✓
<i>Panel B: First stage variables</i>				
Donor income(% of total) Candidate was donor funded				
Looser Campaign Limits <i>(Conventional Estimate)</i>	0.273** (0.157)	0.313** (0.156)	0.428*** (0.156)	0.616*** (0.205)
Looser Campaign Limits <i>(Robust Estimate)</i>	0.277* (0.164)	0.324* (0.191)	0.456** (0.182)	0.655** (0.231)
Controls		✓		✓
Observations	921	843	921	843
Bandwidth	7521.765	7873.963	8461.503	6328.584

Bandwidths are estimated using Calonico et al. (2014)'s method.

Controls included: Royalty income as a % of total municipality income and indigenous background candidates. *** p<0.01, ** p<0.05, * p<0.1

Online Appendix

(Not for publication)

A.1 Figures

Figure A-1: Summary of linking contracts with donors

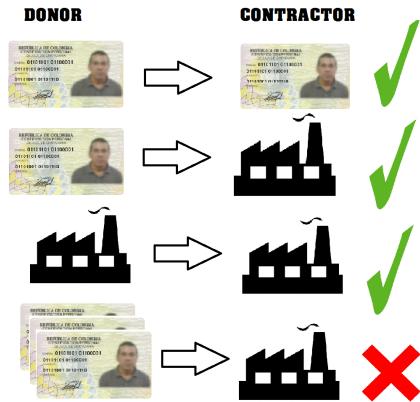
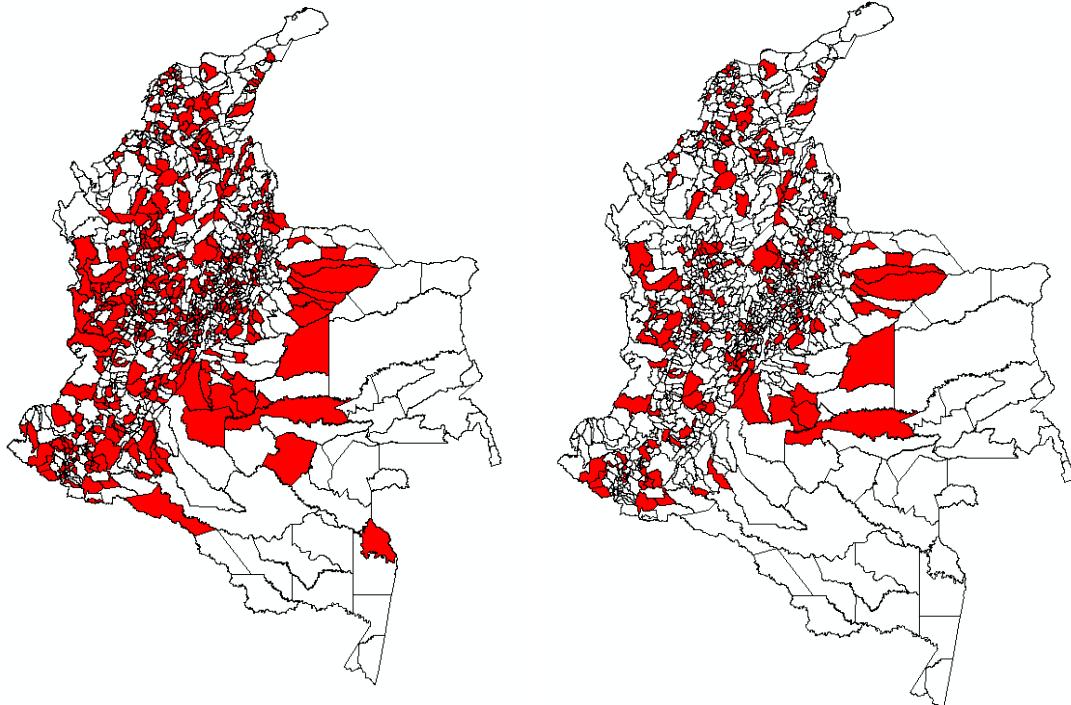


Figure A-2: Colombian municipalities where the donor funded politician placed first or second. 2011 Election



Note: Left full sample(408) Right: In a electoral narrow margin of (6.4%).

Figure A-3: Spatial autocorrelation index (Morans I) for municipalities where the donor funded politician placed first or second in a narrow margin.

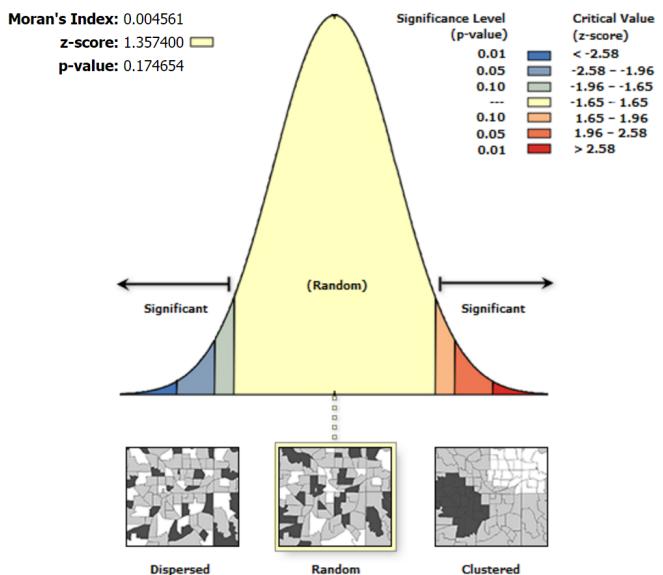
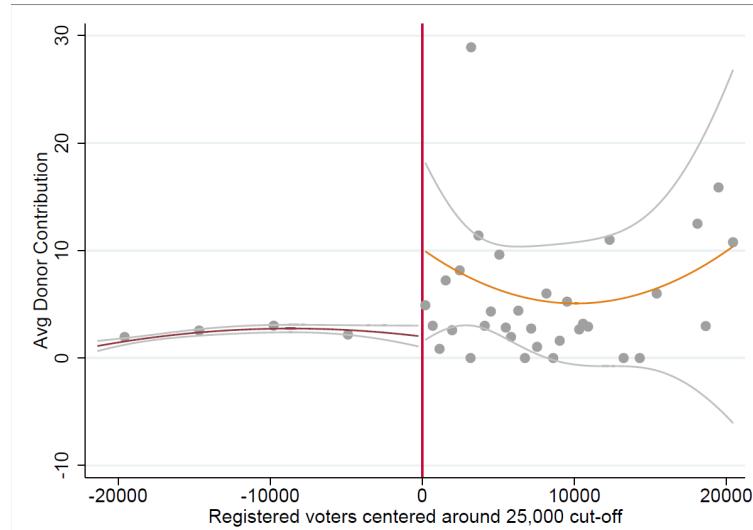


Figure A-4: Example of mayoral election ballot of 2011



Figure A-5: Average donor contribution and campaign limits



A.2 Tables

Table A1: Donations codebook

Revenues	
101	Credits or contributions from the income of the candidates, or direct relatives
102	Contributions, grants and loans, in cash or kind, by private donors
103	Credits obtained in financial institutions to finance the campaign
104	Income originating from public events, or publications by the party or movement
105	State funding
106	Political parties direct financing
Expenditure	
201	Administrative expenses
202	Office expenses and acquisitions
203	Investment in materials and publications
204	Public acts by the candidates
205	Transport and mail service costs
206	Political research and training of party members
207	Judicial accountability and expenses related to campaign accounts
208	Electioneering expenses
209	Financial costs
210	Expenses that exceed the amount set by the National Electoral Council
211	Other expenses

Table A2: Differences in municipality characteristics between campaign sources reporting municipalities and non-reporting

	Non-reporting	Reporting	Mean Difference
Altitude (meter)	889.571	1172.222	-282.650***
Sq km	2023.016	890.410	1132.606***
Distance Deparment capital	106.297	78.315	27.982***
Distance to Bogota	430.782	307.734	123.047***
Literacy rate	85.733	82.726	3.007***
Rurality index (0-1)	0.653	0.561	0.092***
Unsatisfied basic needs	55.949	44.061	11.887***

Note: Reporting is considered when both the winner and runner-up report on campaign financing. The number of observations are 126 for on reporting group and 996 for reporting group. *** p<0.01, ** p<0.05, * p<0.1

Table A3: Differences in municipality characteristics between in sample and out of sample

	Out of sample	In sample	Mean Difference
Altitude (meter)	1166.900	1144.824	22.077
Sq km	931.918	788.174	143.744
Distance Deparment capital	76.697	81.875	-5.178
Distance to Bogota	321.940	314.941	6.999
Literacy rate	83.844	83.978	-0.134
Rurality index (0-1)	0.563	0.566	-0.003
Unsatisfied basic needs	44.670	44.593	0.077

Note: In sample are municipalities where there was a race between a donor funded politician or non-donor funded. The number of observations are 672 and 408 for out of sample and in sample group respectively. *** p<0.01, ** p<0.05, * p<0.1

Table A4: Descriptive Statistics

	N	mean	sd	min	p50	max
<i>Panel A. Main outcomes</i>						
Probability of donors receiving contract	408	0.059	0.121	0.000	0.000	0.667
Total # of contracts for donors	408	2.576	12.256	0.000	0.000	137.000
Mayor sanctioned	408	0.025	0.155	0.000	0.000	1.000
Investigations/sanctions related to contracting	408	0.061	0.240	0.000	0.000	1.000
<i>Panel B - Contract types</i>						
Minimum value contracts	405	0.50	0.212	0.021	0.477	0.964
Waiver contracts	405	0.39	0.221	0.000	0.409	0.937
All directly assigned contracts	405	0.90	0.104	0.132	0.929	0.995
Contract value (COP M)	405	401.17	5577.840	6.108	32.119	110587.734
Contract value addition (COP M)	405	4518.86	60293.782	5.959	33.755	1125156.000
Contract duration (Days)	405	102.01	50.855	12.000	97.812	598.500
Percent time addition	405	0.02	0.033	0.000	0.006	0.232
Percent value addition	405	2.15	42.370	-0.000	0.004	852.696
Avg. contracts that did not finish	405	0.34	0.344	0.000	0.196	0.964
Avg. contracts that finished	405	0.03	0.050	0.000	0.005	0.327
<i>Panel C - Fiscal policy variables during incumbency period</i>						
Total income Y(COP M)	408	20478.85	27711.652	3119.273	13347.738	274440.156
Sources of income						
Land taxes (%Y)	408	3.77	4.646	0.000	1.964	35.093
Industry (%Y)	408	3.13	5.786	0.000	1.260	60.728
Sources of spending						
Funct. expen. (%Y)	408	13.07	4.930	3.955	12.750	33.224
Investment (%Y)	408	86.93	4.930	66.776	87.250	96.045
Deficit (%Y)	408	11.30	10.621	-4.037	8.203	81.869
<i>Panel D - Individual characteristics of incumbent</i>						
Women	408	0.105	0.307	0.000	0.000	1.000
Age	377	44.398	9.318	18.000	44.000	71.000
Black	377	0.050	0.219	0.000	0.000	1.000
Indigenous background	377	0.122	0.328	0.000	0.000	1.000
Left wing	408	0.034	0.182	0.000	0.000	1.000
Right wing	408	0.221	0.415	0.000	0.000	1.000
Sanctioned before holding office	408	0.100	0.301	0.000	0.000	1.000
Registered illegally to vote	408	0.007	0.086	0.000	0.000	1.000
Has political experience	408	0.449	0.498	0.000	0.000	1.000
Has electoral experience	408	0.355	0.479	0.000	0.000	1.000
<i>Panel D - Potential manipulation variables</i>						
Vote buying reports	408	0.301	0.784	0.000	0.000	7.000
Turnout suppression reports	408	0.096	0.430	0.000	0.000	5.000
Total attacks	408	0.400	1.321	0.000	0.000	18.000
Paramilitary attacks	408	0.120	0.617	0.000	0.000	8.000

Note: 408 is the base sample where the donor funded candidates places first or second.

Table A5: Sources of campaign spending across candidate types, % of Total Spending

	Non Donor-Funded	Donor-Funded	Mean Difference
Administrative expenses	0.116	0.125	-0.010
Office expenses and acquisitions	0.037	0.041	-0.004
Investment in materials and publications	0.116	0.097	0.019
Public acts	0.271	0.312	-0.042*
Transport and mail service costs	0.383	0.322	0.061**
Training costs and political research	0.004	0.005	-0.001
Judicial accountability and expenses accounts	0.004	0.004	0.000
Total spending (M COP)	24.750	30.882	-6.132***

Note: The number of observations are 164 and 244 for control and treatment group respectively. ***
p<0.01, ** p<0.05, * p<0.1

Table A6: Smooth municipality covariates across the donor funded victory cut-off

Dependent variable	Mean (1)	Std. Dev. (2)	Donor fund won (3)	Std. Error. (4)	Obs (5)	Bandwidth (6)	P-value (7)
<i>Municipality socio-economic characteristics</i>							
Altitude (meter)	1144.824	1486.769	-340.316	340.187	204	0.068	0.317
Sq km	788.174	1753.898	30.936	368.174	190	0.059	0.933
Distance deparment capital	81.875	54.137	17.519	15.284	192	0.061	0.252
Distance to Bogotá	314.941	186.395	-96.564	113.773	155	0.047	0.396
Literacy rate	83.978	8.422	0.298	3.082	176	0.053	0.923
Rurality index (0-1)	0.566	0.223	-0.134	0.092	173	0.052	0.147
Unsatisfied basic needs	44.593	20.008	8.080	6.312	175	0.053	0.201

Notes: Columns 1 and 2 report the basic descriptive statistics of each variable. Column 3 reports RDD point estimates of the effect of a donor-funded victory in Mayor elections on each variable, using (Calonico et al., 2014)'s optimal bandwidths (reported in column 6), bias correction, and robust standard errors (column 4), with linear local polynomials and triangular kernels. Column 5 reports the number of observations including in each estimation.

Table A7: Smooth manipulation covariates across the donor funded victory cut-off

Dependent variable	Mean (1)	Std. Dev. (2)	Donor fund won (3)	Std. Error. (4)	Obs (5)	Bandwidth (6)	P-value (7)
Vote buying reports	0.301	0.784	-0.085	0.256	187	0.059	0.739
Turnout suppression reports	0.096	0.430	0.147	0.095	212	0.071	0.123
Total attacks	0.400	1.321	0.038	0.254	155	0.047	0.882
Paramilitary attacks	0.120	0.617	-0.030	0.110	129	0.040	0.786

Notes: Columns 1 and 2 report the basic descriptive statistics of each variable. Column 3 reports RDD point estimates of the effect of a donor-funded victory in Mayor elections on each variable, using (Calonico et al., 2014) optimal bandwidths (reported in column 6), bias correction, and robust standard errors (column 4), with linear local polynomials and triangular kernels. Column 5 reports the number of observations including in each estimation.

Table A8: Effect of electing donor-funded politicians on total number of contracts for donors

	(1)	(2)
	<i>Total Contracts</i>	
Donor Funded Elected <i>(Conventional)</i>	5.935*	8.880 (3.482) (7.091)
Donor Funded Elected <i>(Robust)</i>	6.672 (4.435)	10.377 (8.452)
Observations	328	293
Mean	2.576	2.576
Effect Mean(Per)	259.01	402.83
Bandwidth	0.129	0.111
(Local) polynomial order	1	2

*** p<0.01, ** p<0.05, * p<0.1

Note: Includes robust standard errors and the optimal bandwidth by Calonico et al. (2014).

Table A9: Comparison of contracts sector by Mayor type, and whether the contractors were donors.

	Donor Funded Elected			Donor Funded non Elected		
	Non-Donor	Gave Donation	Mean Difference	Non-Donor	Gave Donation	Mean Difference
Agriculture and others	0.011	0.029	-0.018**	0.011	0.000	0.011***
Construction	0.156	0.125	0.031**	0.133	0.122	0.011
Manufacture and Industry	0.010	0.021	-0.011*	0.009	0.037	-0.027
Materials and machinery	0.103	0.161	-0.059***	0.101	0.110	-0.008
Medicine and health	0.020	0.014	0.006	0.039	0.000	0.039***
Environment	0.027	0.019	0.008	0.027	0.000	0.027***
Mining and energy	0.003	0.004	-0.001	0.002	0.000	0.002***
Municipality administrative supplies	0.102	0.160	-0.058***	0.104	0.049	0.055**
Services	0.485	0.447	0.038*	0.501	0.366	0.135**
Transport	0.083	0.019	0.064***	0.073	0.317	-0.244***

*** p<0.01, ** p<0.05, * p<0.1

Note: Base sample are contracts in municipalities within a narrow electoral margin of 6.2% in (1) Table 5. The number of observations are 76902 and 514 for control and treatment group in columns (1) and (2) respectively. For columns (3) and (4), the number of observations are 65389 and 82 for control

Table A10: Comparison of contract types by Mayor type, and whether the contractors were donors.

Contract type:	Donor-funded Mayor			Non-donor funded Mayor		
	Non-donors	Donors	Mean Difference	Non-Donors	Donors	Mean Difference
Lease	0.008	0.008	0.001	0.007	0.061	-0.054**
Consultancy	0.023	0.000	0.023***	0.020	0.000	0.020***
Audit	0.017	0.004	0.013***	0.014	0.000	0.014***
Other Type of Contract	0.041	0.004	0.037***	0.044	0.024	0.020
Service Provision	0.524	0.500	0.024	0.561	0.524	0.037
Supply	0.239	0.387	-0.149***	0.247	0.305	-0.058

*** p<0.01, ** p<0.05, * p<0.1

Note: Base sample are contracts in municipalities within a narrow electoral margin of 6.2% in (1) Table 5. The number of observations are 76902 and 514 for control and treatment group in columns (1) and (2) respectively. For columns (3) and (4), the number of observations are 65389 and 82 for control.

Table A11: Descriptive statistics multiplier to investment and contract value

Variable	mean	sd	min	p50	p75	p95	max
Total donation value(COP M)	8.536	12.149	0.000	5.000	10.000	50.000	180.000
Total contract value (COP M)	17.299	188.914	0.000	0.000	0.000	325.713	4780.517
Multiplier	10.677	86.875	0.000	0.000	0.000	357.533	1410.801
Multiplier scaled	9.677	86.875	-1.000	-1.000	-1.000	356.533	1409.801

Note Multiplier is Total contract value/Total donated value. For multiplier scaled see equation (8)

Table A12: Smooth predetermined municipality covariates across looser campaign limits cut-off

Dependent variable	Mean (1)	Std. Dev. (2)	Donor fund. won (3)	Std. Error. (4)	Obs (5)	Bandwidth (6)	P-value (7)
<i>Municipality covariates</i>							
Disposable Income (mw)	31016.747	410388.781	-643.348	5045.757	76	4849.617	0.899
Municipal category	5.686	1.029	0.213	0.210	57	3649.972	0.310
Mayor wages	6.744	2.634	-0.419	0.420	57	3634.855	0.318
Council size	10.966	2.953	-0.437	0.373	60	3804.922	0.241
Total population	43216.607	267851.336	391.699	2239.875	147	8379.316	0.861
Income from royalties	0.069	0.150	-0.252***	0.080	122	6813.660	0.002
Education establishments	284.661	171.554	61.102	56.348	98	5835.792	0.278

Notes: Columns 1 and 2 report the basic descriptive statistics of each variable. Column 3 reports RDD point estimates of the effect of looser campaign limits on each variable, using (Calonico et al., 2014)'s optimal bandwidths (reported in column 6), bias correction, and robust standard errors (column 4), with linear local polynomials and triangular kernels. Column 5 reports the number of observations including in each estimation.

Table A13: Effects of looser campaign limits on probability of donors obtaining contracts

	(1)	(2)	(3)	(4)
<i>Panel A: Fuzzy RDD estimates</i>				
Probability of donor receiving a contract				
Looser Campaign Limits <i>(Conventional Estimate)</i>	.25455 (.18494)	.1911 (.1899)	.17478 (.13479)	.10707 (.11857)
Looser Campaign Limits <i>(Robust Estimate)</i>	.30088 (.21045)	.22632 (.2175)	.19902 (.15512)	.11957 (.13189)
Controls		✓		✓
<i>Panel B: First stage variables</i>				
	Donor income(% of total)	Candidate was donor funded		
Looser Campaign Limits <i>(Conventional Estimate)</i>	.27889** (.13928)	.3233** (.15351)	.39946*** (.15351)	.56986*** (.19586)
Looser Campaign Limits <i>(Robust Estimate)</i>	.27889* (.15991)	.33195* (.19023)	.42732** (.1804)	.59966*** (.22009)
Controls		✓		✓
Observations	920	842	920	842
Bandwidth	7273.452	7200.763	9274.245	7095.115

Bandwidths are estimated using (Calonico et al., 2014)'s method.

Controls included: Royalty income as a % of total municipality income and indigenous background candidates. *** p<0.01, ** p<0.05, * p<0.1