

Do Campaign Contribution Limits Curb the Influence of Money in Politics?

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Abstract

Over 40% of countries around the world have adopted limits on campaign contributions to curb the influence of money in politics. Yet, we have limited knowledge of whether and how these limits achieve this goal. Using a regression discontinuity design that exploits institutional rules on contribution limits in Colombian municipalities, we show that looser limits increase the number of public contracts assigned to donors to the elected candidate. This is explained by looser limits increasing the influence of top donors over the elected candidate, rather than reducing electoral competition or changing who is elected for office. We further show that looser limits worsen the quality of public contracts given to the winner's donors: these contracts are more likely to run over their stipulated costs. Overall, this paper links looser campaign contribution limits, donor kickbacks, and worse performance of contracts awarded to donors.

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Over 40% of countries around the world have adopted laws limiting campaign contributions as a way of curbing the influence of money in politics (IDEA 2014).¹ On the one hand, limits on political donations might prevent policies that favor moneyed interests. On the other hand, contribution limits can be considered as obstacles to the free expression of preferences and to the flow of information voters receive via campaign spending.² Despite the widespread use of campaign contribution limits and reasonable arguments in favor and against them, empirical assessments of their impacts, as well as an understanding of the underlying mechanisms behind those effects, remain limited.

We study the effect of campaign contribution limits on donor behavior during the 2011 Colombian mayoral elections, the subsequent assignment of public contracts after the elections, and importantly, the performance of those contracts. By tracing the impact of the regulation all the way to government contract performance we provide new evidence on how campaign contribution limits could curb the influence of money in politics, leading to changes in government contract efficiency.

We first establish that a strong bias exists in public spending in favor of donors to the winning candidate. Following Boas, Hidalgo and Richardson (2014), a close elections regression discontinuity (RD) approach shows that the winning candidate's donors receive, on average, three contracts more than donors to the runner up, a threefold increase in the average number of contracts that donors to the top two candidates receive. When we focus on contracts that are assigned under a contractual category that gives mayors more discretion in selecting contract recipients with less oversight, the *minimum value* category,³ we find

¹See also Scarrow (2007) for a review of political finance around the world.

²See U.S. Supreme Court decision *Citizens United vs Federal Elections Commission*, 558 US 310 2010 for a discussion of how contribution limits may inhibit the freedom of speech.

³Contracts under the minimum value category are below 10% of the municipality budget, can be advertised for only one day, and they are automatically assigned to the lowest bidder.

that contracts of the mayor’s donors are 28% larger in value than those assigned to donors to the candidate who barely lost the election.

Next, we ask if campaign limits can curb the benefits received by the winning candidate’s donors. Studying this question empirically is challenging as campaign finance regulations might be influenced by the public perception of corruption and the pressure of private interests on policy makers. Moreover, it is difficult to account for all the historical, cultural, and contextual factors that determine both restrictions on campaign finance and corrupt behavior. We take advantage of the fact that in Colombia, campaign contribution limits in mayoral races vary according to arbitrary thresholds on the number of registered voters. Using a regression discontinuity design, we find that loosening restrictions on the total amount of contributions to campaigns from 58 million to 110 million pesos (approximately 17,000 to 32,000 U.S. dollars) leads to, on average, three more public contracts of all types and two more minimum value contracts to mayor’s donors.

We proceed to explore the theoretical mechanisms behind why mayors’ donors are favored to a larger extent in municipalities with looser limits. The literature has pointed out that campaign contributions can open the door for undue influence and potential conflicts of interest between candidates and donors ([van Biezen 2011](#); [Gilens 2012](#); [Powell 2012](#); [Gokcekus and Sonan 2017](#)). Based on this idea, we argue that less restrictive limits increase the influence of wealthy individuals on the elected official because they are able to contribute a larger share of total campaign revenue. If a campaign is financed in large part by few donors, elected officials would have more pressure to reward these contributions. Although the potential power of campaign finance regulations to curb this influence is often cited as justification for the existence of such regulations ([van Biezen 2011](#); [Gilens 2012](#); [Hummel, Gerring and Burt 2019](#); [Wiltse, La Raja and Apollonio 2019](#)), empirical studies have not explored how campaign finance restrictions alter the composition of campaign revenues (or other proxies for the clout individual donors have over a candidate) nor linked

those changes with observed donors' benefits. Consistent with this argument, we find that donations concentrate among top donors under looser limits. A top donor contributes 9.1 percentage points more of the campaign total revenue under looser limits than what a top donor gives under tighter limits, while non-top donors' donations are unaffected by limit changes.

We also test and rule out three alternative explanations for why the mayors' donors receive more benefits with looser contribution limits. The first is that looser limits make elections less competitive (Stratmann, J and Aparicio-Castillo 2006; Hall 2016; Butcher and Milyo 2020), which reduces the incumbent's incentives to prioritize the general interest over her donors' (Gordon and Huber 2007). We do not find any evidence that electoral competition differs by treatment. The second is that looser limits could attract candidates who are prone to favor private interests (Avis et al. 2017). We show, however, that elected mayors and the pool of candidates in municipalities with higher limits are not systematically different in terms of their previous participation in political campaigns, experience in elected office, or history of sanctions than those in municipalities with lower limits. Finally, we carry out a number of checks that suggest our findings are not driven by more severe under-reporting of donations in municipalities with more limited contributions (La Raja 2014).

The final analysis in the paper considers downstream impacts of looser campaign limits on the quality of government functioning as measured by the performance of public contracts awarded to donors. This sheds light on the efficiency loss versus information loss debate: on the one hand, favoring certain donors for reasons other than merit can negatively affect contract execution. On the other hand, fewer restrictions could improve the flow of information via donations that helps voters elect those candidates who know how to best allocate public resources (Coate 2004). Challenging the latter view, we show that in a municipality with looser limits, it is more likely that donor-managed contracts run over stipulated costs and require time extensions with such cost overruns being 214% larger than

those of contracts managed by a mayor’s donor under more restrictive limits.

The Colombian case is particularly well suited to study how campaign finance regulations affect donors’ influence over elected officials. In addition to the exogenous variation in limits created by institutional rules that help us address identification challenges, a national ID number allows us to link public contracts to individual donors (Ruiz 2017). This enables us to circumvent some challenges faced by roll-call based analyses that are common in the literature. For example, while in our case it is clear who the recipient of a contract is, legislative changes affect a large group of beneficiaries, making it difficult to establish whether a legislator’s support for such changes was aimed at benefiting her donors. Moreover, donors’ influence might manifest at early but less observable stages of the legislative process (Powell 2012; Powell and Grimmer 2016); and voting on bills on industrial policy, regulation, or taxation at the federal level is more ideologically charged than most municipality government purchases of goods and services (Oliver 2012). This makes it more difficult to gather evidence of quid pro quo exchanges in roll-call analyses as donors and candidates could share policy preferences (Fox and Rothenberg 2011).

Our focus on private interests’ influence on local governments in a developing democracy contributes more generally to a literature that, for the most part, has focused on national legislation in industrialized settings (Samuels 2001; Anzia 2019). The Colombian mayoral race context differs significantly from the well-studied federal legislative elections setting in the U.S. As we will show below, donors to Colombian mayoral campaigns are typically local business owners and service providers seeking to gain preferential treatment in public procurement assignment rather than corporations wanting to influence legislation (Bonica 2014), or individuals contributing small amounts to express their political views (Ansolabehere, De Figueiredo and Snyder 2003). The party system is also highly fragmented and party identification is low. Given that our theoretical insights do not depend on Colombian idiosyncrasies, we believe our findings are relevant to other contexts, but we also

contribute to this literature by expanding the study of these issues to geographical settings where existing evidence is particularly thin.

Our paper contributes to a large, mostly U.S.-focused, literature that estimates effects of contributions on elected officials' behavior. In a review of these studies, [Ansolabehere, De Figueiredo and Snyder \(2003\)](#) find that this work is unable to clearly establish *quid pro quo* exchanges between politicians and donors.⁴ More recent work that addresses some of the methodological challenges in this literature has shown that donors target legislators who are most useful to them in a manner consistent with access-seeking ([Powell 2012](#); [Fouirnaies and Hall 2014](#); [Barber 2016a](#); [Kalla and Broockman 2016](#); [Powell and Grimmer 2016](#); [Li 2018](#)) and to indirectly influence legislative procedure ([Fouirnaies and Hall 2018](#)). Particularly close to our work, [Boas, Hidalgo and Richardson \(2014\)](#), find substantial favoritism towards donors to federal deputies in the assignment of government contracts in Brazil. Although our findings are consistent with elected officials rewarding their donors, our main contribution to this literature is to provide evidence of campaign contribution limits curbing the donors' influence over elected officials that is directly observed in the data.

The literature on campaign finance regulation has shown how restrictions on campaign contributions and spending impact electoral results and competitiveness ([Stratmann, J and Aparicio-Castillo 2006](#); [Hall 2016](#); [Fouirnaies 2018](#)), polarization ([Barber 2016b](#)), party systems ([Potter and Tavits 2015](#)), political efficacy ([Primo and Milyo 2006](#)), future career choices of legislators ([Weschle 2019](#)), interest groups' electioneering activities ([Hogan 2005](#)), challenges to incumbents ([Hamm and Hogan 2008](#)), and incumbent reelection ([La Raja and](#)

⁴[Stratmann \(2005\)](#) finds evidence of the influence of money on legislators' behavior in a meta-analysis using the same papers, but given methodological challenges faced by the surveyed work, the study does not draw definitive conclusions. Using better research designs, [Fowler, Garro and Spenkuch \(2020\)](#) do not find evidence of *quid pro quos* in U.S. Senate races.

Schaffner 2014). Few papers, however, have explored the relationship between campaign finance regulations and corruption. Baltrunaite (2019) investigates how donors are advantaged in the bidding process for public contracts and estimates the effect of a ban on corporate contributions on contract assignments in Lithuania. Fazekas and Cingolani (2017) and Hummel, Gerring and Burt (2019) focus on how campaign finance regulation and campaign state funding are linked to measures of corruption in comparative cross-country analyses. Our RD design and the ability to link donors to contracts allows us to estimate the causal effect of campaign finance regulation on the actual biases favoring donors in contract assignment. Importantly, unlike previous work, we propose and find evidence of a theoretical mechanism that accounts for the greater benefits accruing to donors in municipalities with less stringent campaign finance restrictions, we assess alternative explanations, and evaluate the effects of such restrictions on the quality of donor-managed contracts.

Colombian Electoral Context

Mayors in Colombia are powerful figures in their municipalities with discretion over an average of 26% of all local spending.⁵ Because they are in charge of executing the municipality budget, they have plenty of opportunities to repay donors. Most public goods and services in a municipality are provided through third parties who contract with the municipal government. There are three types of contracts: open-bid contracts, contracts with non-bid process and waivers, and minimum value contracts that cover those under 10% of the municipality budget. The first category presents the most difficulty for a mayor who wants to reward a donor. This is because with open-bid contracts there is a call for proposals that is advertised online for five to ten working days, and a committee needs to evaluate the submissions. In the second category, the mayor must provide official justification for the waivers and there is

⁵For the period 2004–2007.

only a limited set of economic activities to which the category applies. In contrast, contracts given under the minimum value category only need to be advertised for one day and are automatically given to the lowest bidder, which precludes a proposal evaluation by a separate committee.

Mayors have incentives not to renege on agreements made with their donors. Although mayors cannot be reelected in consecutive terms, most continue their career in politics.⁶ Of all mayors in 1988, for example, 62% participated in other elections after their term ended. One reason to favor current donors, therefore, is to maintain a flow of resources in future campaigns. Mayors can also ask for a slice of the contracts, called a *mordida* (a bite). That is, the recipient of the contract can give back a fraction of the value of the contract to the politician as payment. A famous example among Colombians is former mayor of Bogotá, Samuel Moreno, who was sentenced to 18 years in prison for receiving 2,790 million pesos—14 times his annual salary—from a recipient of a contract assigned by the local government.

Donors in mayoral races in Colombia generally contribute to only one candidate and are relatively wealthy. In our data, only 138 out of 6,658 donors contribute to more than one candidate and 75.33% of all donors give a contribution that is larger than the average monthly wage in the municipality. This differs from the U.S. case where campaigns rely more on multiple small donations from ordinary citizens ([Ansolabehere, De Figueiredo and Snyder 2003](#)). The fact that the donors do not contribute to mayoral races outside the municipality, and give large donations, is consistent with the general perception that donors to mayoral campaigns are local business owners who could benefit from public contracts.

We also see that of the few donors donating to different candidates, 76.1% are contributing to candidates from different parties, and of those who continue to donate at the next mayoral election (2015), only 23.7% support candidates of the same party. These patterns are in line with the low levels of party identification, a reflection of a weak party system

⁶There is no limit on the number of times someone can be mayor of a municipality.

born from permissive electoral institutions (Shugart, Moreno and Fajardo 2007; Pachón and Shugart 2010).⁷ The fact that local politics tend to be less ideological than national politics given the nature of local concerns and institutional constraints on local governments (see e.g., Oliver 2012), that Colombian parties lack ideological coherence (Botero and Alvira 2012; Botero, Losada and Wills-Otero 2016), and more generally, the presence of a weak party system, suggest that partisanship and ideology are not as strong drivers of contributions as they are in the well-studied U.S. federal legislative setting.

Anecdotal evidence suggests that investing in a mayoral campaign can be highly profitable. Take the case of the mayor of Amalfi.⁸ One donor contributed 3,000 dollars to the mayor’s campaign, equivalent to 22% of the campaign’s revenue. Later, during the mayor’s term, the donor signed 86 contracts with the municipality worth more than half a million dollars. Of these contracts, only five were awarded via competitive tender. Such stories find support in the data. The average donor in the sample receives contracts that are 45 times larger than their contribution. Even if we focus on minimum value contracts, their value is, on average, eight times larger than the donor’s contribution. Below, we systematically examine whether mayors’ donors are favored in contract assignment.

Colombian law establishes limits for total campaign contributions that are set equal to a limit on campaign expenses. The National Electoral Commission sets the campaign limits on the basis of the number of registered voters in the municipality.⁹ These limits jump discontinuously at arbitrary cutoffs of registered voters. For example, at 25,000 registered voters, the campaign contribution limit increases from 58 to 110 million COP (approximately 15,500 to 29,000 U.S. dollars).¹⁰ In addition, individual donors cannot give more than 10% of

⁷Only 25% of Colombians identified with a political party in 2011 (LAPOP 2011).

⁸For more on this case, see Bristow (2018).

⁹See National Electoral Commission Norm 78 of 2011.

¹⁰Subsequently, at 50,000 registered voters the limit jumps to 330 million COP (88,200 U.S. dollars); at 100,000 registered voters the limit jumps to 659 million COP (176,000 U.S.

the total campaign contribution limit.¹¹ Limits are announced months before the candidate registration date and violation of these limits can be punished with removal from office, loss of state funding, and dissolution of the political movement. The voter registration thresholds that determine the contribution limits do not impact other policies, which allows us to avoid the estimation of a compound treatment (Grembi et al. 2018).

Data

We use electoral data compiled by Pachón and Sánchez (2014), gathered from the Colombian national electoral authority, the *Registraduría Nacional del Estado Civil*. These data contain the results for the 2011 mayoral elections for all municipalities. In auxiliary analyses, we also use data from the 2015 mayoral elections.

Data on campaign funding and contracting were compiled by Ruiz (2017) and were taken originally from the National Electoral Commission and *Datos Abiertos*, an online portal that was created to increase transparency in public procurement. Electronic campaign finance reporting has been mandatory by law since 2009 for every candidate who runs for office.¹² The National Electoral Commission fines candidates or parties that do not comply with the reporting requirements. As a result, compliance is fairly high: out of 4,460 mayoral candidates in 2011, 89% reported campaign finance information. In these data, we observe each donation to a given candidate. The contracting data contains the universe of public procurement data including information on the contractor (and their unique national ID), the contractual category, the contract’s economic sector, its value, purpose, and length. We

dollars); at 250,000 the limit jumps to 745 million COP (199,000 U.S. dollars); at 500,000 the limit jumps to 1,318 million COP (352,000 U.S. dollars). For the capital city of Bogotá, the limit is 1,646 million COP (440,000 U.S. dollars).

¹¹See Article 23 of Law 1475 of 2011.

¹²Norm 1094 of 2009.

also know whether the contract was completed, and/or overran in terms of costs.

Following Ruiz (2017), we match the unique ID of each donor to the ID of the contractors in the same municipality in which the candidate ran, which creates a *direct link* between the donor and a beneficiary from government resources. In Colombia, two types of legal entities can contract with the state: individuals and companies. When an individual gives a donation and his/her company receives a contract, we can link them uniquely since the same number is used for the person and their company. The only links that cannot be made are 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, which is identified with a different owner’s ID, receives the contract. Contracts assigned to multiple-owner companies, however, represent only 9.9% of all contracts and are mostly concentrated in large cities that are not included in the sample because they are not close to the threshold used in the RD design.

In terms of politicians’ characteristics, we have access to the entire history of disciplinary sanctions for those who held elected office and whether candidates had illegally registered to vote in the past.¹³ The latter can serve as a proxy for non-elected politicians’ proclivity for malfeasance. We also have data on gender, age, and race.¹⁴

¹³Sanctions are given for violating contracting regulations, running for office without satisfying legal requirements, and less serious offences like not replying to a formal information request, among others. Illegal voting registration covers impersonating a dead person’s vote, registering to vote in a municipality where one does not reside, or voting while underage.

¹⁴These data are obtained from politicians’ pictures on the ballots and a facial detection algorithm. The self-reported gender differs from the predicted gender in less than 3% of cases.

Estimation Strategy

Our analyses rely on several regression discontinuity (RD) designs. There are a number of estimation choices that come with RD models and we adopt the practices advocated in the most recent literature (see e.g., [Gelman and Imbens 2019](#); [Cattaneo, Idrobo and Titiunik 2020](#)). First, we estimate treatment effects by approximating the average control and treatment outcomes at the cutoff using linear polynomials with observations that are near the cutoff and take the difference of these approximations.¹⁵ Second, we follow the common practice of using a triangular kernel which gives more weight to observations near the cutoff.¹⁶ Third, the bandwidth that defines the effective estimation sample in our analysis is the one that minimizes the asymptotic mean squared error (MSE). This accounts for the trade-off between smaller bias and larger variance that shorter bandwidths imply. It is important to note that the MSE will be different for each dependent variable, which implies that, by selecting the bandwidth that minimizes the MSE, the number of observations will change between models.¹⁷ Fourth, we report optimal MSE point estimates, robust confidence intervals, and p-values that account for clustering at the municipality level following [Calonico, Cattaneo and Titiunik \(2014\)](#). This approach avoids the pitfalls of conventional inference that ignore polynomial approximation bias leading to incorrect asymptotic coverage. Finally, we present all our results graphically in [Appendix E](#).

¹⁵Local linear models also avoid inference problems characteristic of high order polynomials ([Gelman and Imbens 2019](#)). We reach similar conclusions with local constant and quadratic specifications (see [Appendix I](#), [Tables I3](#) through [I6](#)).

¹⁶The local linear approach with triangular kernels produces smaller bias at boundary points than local constant regressions ([Fan and Gijbels 1996](#)).

¹⁷In [Appendix F](#), [Figures F1](#) through [F4](#), we assess sensitivity to the choice of non-MSE optimal bandwidths for all our results.

The Returns from Donating to a Winning Candidate

We begin our analysis by documenting that donors who contribute to winning candidates get more contracts. The empirical exercise compares contracts received by donors to the winning candidate to contracts received by the donors to the runner-up. A difficulty when interpreting these comparisons is that winning candidates might differ from losing candidates in ways that make them more likely to assign contracts to their donors. For example, candidates with underlying preferences for malfeasance may be more likely to win through electoral manipulation and to reward their donors. Moreover, donors who are competitive in tendering processes because of their business acumen might also be the best at identifying the most qualified candidate.

To circumvent these problems, we follow [Boas, Hidalgo and Richardson \(2014\)](#) and use a close election regression discontinuity design that compares the contracts received by donors to a candidate who barely wins with donors to the candidate who barely loses. The unit of analysis is a municipality-candidate and the running variable is the difference in vote shares between the winner and the runner-up, which implies a victory cutoff of zero.

In order to interpret our estimates as causal effects, we require that candidates who barely win the election are similar in all characteristics that would affect contract assignment to those who barely lose and that donors to bare winner and loser candidates do not differ in characteristics that affect their chances of receiving a public contract. Table [A1](#) in Appendix [A](#) shows that mayors and runner-up candidates in close elections are similar in their electoral experience, whether they have been elected to office in the past, ideology, campaign size, and notably, in levels of prior malfeasance. Because of this, it is difficult to say that differences in valence can explain why winners would treat their donors differently. Reinforcing this point, we also do not find significant differences in the number of donors or the weight of donations in their campaign revenues. If we were to find such differences, they could be

driven by differences in characteristics between bare winners and losers observed by donors but unobserved by researchers.

Importantly, in Table A2 in Appendix A, we also verify that donors to the bare winners and losers are similar in a number of observable characteristics. We find no evidence of discontinuities at a zero margin of victory in whether the donor is registered in the chamber of commerce or is registered as a company. donors to the bare winner are not significantly more likely to be producers of goods (as opposed to service providers or merchants), and, when they are companies, they have similar ages to those donating to the bare loser.

Table 1: Effect of donating to a winner on contract assignment

Outcome:	# Contracts (1)	ln(Value All) (2)	# Min. Value Contracts (3)	ln(Value Min. Value) (4)
Electoral victory	3.045	0.239	1.803	0.281
Robust p-value	0.028	0.368	0.081	0.063
CI 95%	[0.356,6.231]	[-0.245,0.663]	[-0.249,4.268]	[-0.015,0.583]
Observations	1,982	1,982	1,982	1,982
Bandwidth obs.	1,514	1,182	1,420	1,256
Mean	0.931	0.346	0.462	0.182
Effect mean(%)	327.07	69.08	390.26	154.40
Bandwidth	0.13	0.09	0.12	0.1

Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values with clustering at the municipality level are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth. Each observation is a candidate-municipality.

The first model in Table 1 shows that the donors to a winning candidate receive, on average, three more contracts from the elected mayor than those received by the donors to the runner-up. This effect is statistically significant and represents a threefold increase in the average number of contracts that donors to the top two candidates receive. The second column shows the effect on the total value of the contracts received by donors. Although the coefficient on electoral victory is positive, it is not significant. If we focus on minimum value

contracts in columns 3 and 4—the type of contracts that offer more room for the mayor to reward donors and less oversight—we see that the effects of donating to the winner on the number of those contracts and their values are also positive, and in the case of the value of the contracts, it is more precisely estimated with an associated increase of 28%.

Effects of Looser Contribution Limits

The fact that donors to the mayor receive three times as many contracts as those they would have received if they had donated to the runner-up is consistent with a biased allocation of public procurement contracts, which is also in line with the greater economic benefits accruing to donors to election winners found in different contexts (see e.g., [Stratmann 2005](#); [Boas, Hidalgo and Richardson 2014](#)). We now evaluate whether campaign limits are effective in reducing what donors receive from the mayor.

A challenge one faces when studying the effects of campaign contribution restrictions on the benefits received by donors is that some municipality characteristics can be linked to both public contract assignment and campaign regulations. For example, more economically developed municipalities can attract the attention of watchdog anti-corruption agencies and the press, which could deter mayors from rewarding their donors. At the same time, these municipalities could have fewer restrictions on campaign contributions as they tend to be larger. Without exploiting the fact that regulations are completely determined by the arbitrary threshold of registered voters, a simple selection-on-observables approach estimating the impact of looser limits on donors’ benefits could underestimate the true effect.¹⁸

¹⁸OLS regressions that control for municipality and mayor characteristics (candidate’s illegal registration of ID, previous sanctions, previously elected, political experience, candidate’s ideology, municipality discretionary revenue, municipality category, mayor’s wage, and council size) but not registered voters give smaller estimates than those presented be-

To address these challenges, we employ a quasi-experimental regression discontinuity design that uses campaign limits set by Colombian institutions. Our main explanatory variable takes the value of 1 if the municipality is at or over the 25,000 registered voters threshold and 0 if it is not. Therefore, moving from control to treatment implies that the municipality has *looser* campaign contribution limits.¹⁹ We explore the effect of looser limits on three sets of outcomes: benefits via contracts that individual donors receive, measures of donors’ influence in a given mayoral campaign, and indicators of how contracts received by donors perform. The unit of analysis for the first and third groups of outcomes is the donor and that of the second is the mayor or mayoral campaign. We note that 94% of municipalities where elections are close (with a margin of victory of 10 percentage points or less) have less than 50,000 registered voters and are therefore those for which the contribution limits we examine apply. Moreover, two-thirds of the municipalities that are within 5,000 registered voters of the 25,000 registered voters cutoff also have close elections.²⁰

The main identification assumption is that the change in policy towards looser limits is the only change affecting contract assignment and donations that occurs at the threshold of 25,000 registered voters. If there is no manipulation of registered voters around the campaign limit cutoff and if there are no determinants of the outcomes that vary discontinuously at the cutoff other than the limits, the RD design allows us to estimate the causal effect of looser campaign limits in municipalities with 25,000 registered voters.

low. Controlling for registered voters amounts to estimating a parametric RD, which requires much stronger assumptions than our approach (Cattaneo, Idrobo and Titiunik 2020).

¹⁹Analysis of the effects of interest at other cutoffs is made difficult by the fact that there are only 14 municipalities within 5,000 registered voters of the other four cutoffs and that the magnitude of the treatment changes at each of them.

²⁰The 5,000 registered voters and the 10 percentage points are close to the optimal bandwidth found with the RD analysis.

One concern with this research design is that public officials or donors can artificially try to inflate the number of registered voters to allow some donors to give larger contributions in municipalities below the 25,000 threshold. To test if this concern is important in practice, we carry out the discontinuity in density test proposed by [Cattaneo, Jansson and Ma \(2019\)](#) and find no evidence of a higher concentration of municipalities with numbers of registered voters right above the cutoff (see Appendix [C](#)).

In addition, we check that predetermined characteristics of municipalities such as population, discretionary revenue, total number of contracts assigned by the mayors, and mayor’s wages, are smooth around the 25,000 voters threshold. If we were to find significant discontinuous jumps in these characteristics at the cutoff, it would be difficult to interpret our estimates as local causal effects. Reassuringly, as Appendix [B](#), Table [B1](#) shows, we find no significant effect of looser limits on these characteristics.

Result: Looser Limits Increase Benefits to Donors via Public Contracts

Table [2](#) shows that a donor contributing to the mayor’s campaign in a municipality with higher limits receives three more contracts of all types (Model 1) and two more of minimum value contracts (Model 3) than one who donated to the mayor in a municipality with more restrictive limits. These are large effects considering that the average number of contracts received by mayors’ donors are 0.28 of all types and 0.21 of minimum value. As shown by Model 4, there is also some evidence that the value of the minimum value contracts received by a donor under looser limits is larger than that of a donor in a municipality with lower limits, with an increase close to 64% in value but the coefficient is significant at the 10%

Table 2: Effect of looser campaign contribution limits on contracts assigned to donors to the mayor

Outcome:	# Contracts (1)	ln(Value All) (2)	# Min. Value Contracts (3)	ln(Value Min. Value) (4)
Looser contribution limit	3.091	0.819	2.030	0.638
Robust p-value	0.012	0.234	0.023	0.061
CI 95%	[0.735,5.994]	[-0.513,2.098]	[0.306,4.200]	[-0.030,1.364]
Observations	2,049	2,049	2,049	2,049
Bandwidth obs.	457	366	366	341
Mean	0.280	0.205	0.210	0.101
Effect mean(%)	1,103.93	399.51	966.67	631.68
Bandwidth	6,980	5,312	5,292	5,190

Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values with clustering at the municipality level are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth for each dependent variable. Each observation is a donor.

level.²¹

Mechanism: Looser Limits Increase the Influence of Top Donors

We have seen that donors to winners of mayoral elections receive more contracts where contribution limits are higher. We now study why this is the case. The influence of money in elections has been identified as a key driver of corruption ([Gokcekus and Sonan 2017](#)) and policies that are biased towards the preferences of the rich ([van Biezen 2011](#); [Gilens 2012](#); [Powell 2012](#)). Because of this, campaign finance regulations appear to be an attractive tool to diffuse interests groups' influence ([van Biezen 2011](#); [Gilens 2012](#); [Hummel, Gerring and Burt 2019](#); [Wiltse, La Raja and Apollonio 2019](#)). We now lay out the mechanism by which regulation alters donors' influence over elected officials and test its main implications.

We argue that wealthier donors have more influence over elected officials in higher

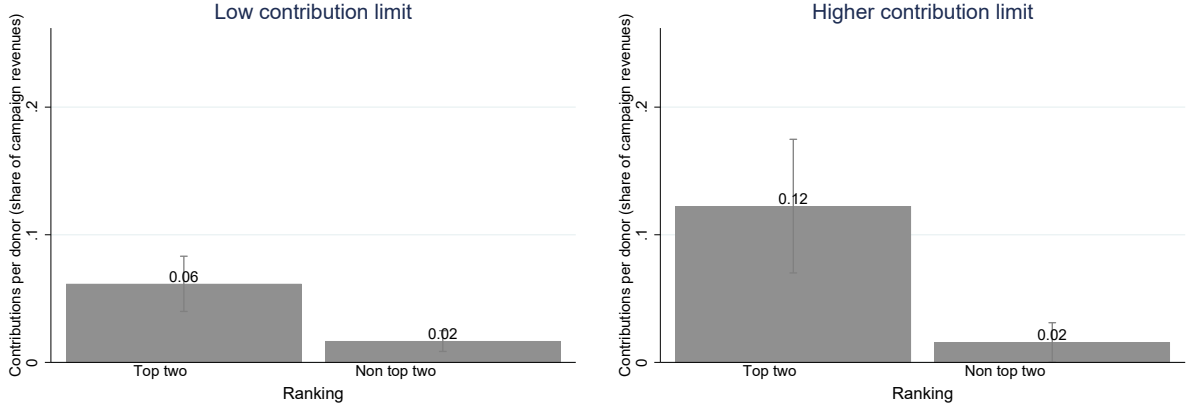
²¹As shown below, we find significant coefficients (substantively and statistically) in these models run on an extended sample that includes the 2015-2019 incumbency period.

limit municipalities because they can contribute at a level that cannot be matched by other, more cash-constrained, donors. By raising their contributions, the wealthier donors increase their chances of getting a reward from an elected candidate. This is because a candidate who receives a larger fraction of her campaign revenue from few donors has more pressure to reciprocate. Moreover, when a candidate who wants to reciprocate faces a limited budget, she is forced to prioritize the assignment of rewards to the more generous donors. While in municipalities with low limits wealthier donors and less wealthy ones donate at more similar levels, with higher limits wealthier donors compete to obtain contracts by giving more and increasing the weight of their contributions in the winner’s campaign revenue. Donors who just want to express their preferences with a small donation are not expected to alter their donation levels with looser limits as much as those who see donations as an investment.

Since we are interested in changes in donors’ influence over mayors induced by changes in contribution limits, the unit of analysis in the results that follow is the mayoral campaign. Model 1 in Table 3 shows that a municipality with higher limits has an average contribution to the mayor’s campaign that is 113.4% larger than one with tighter contribution restrictions. This is consistent with the regulation affecting donors’ behavior. The fact that there are larger donations with looser limits, however, does not imply that the individual influence of some of the donors over the mayor is increased, as all donors could raise their contributions in such a way that the relative share of each individual’s contribution to the campaign revenue is not significantly altered.

If higher contribution limits increase the influence of the most generous donors, we should see a larger difference between what the top and non-top donors contribute to the mayor’s campaign in municipalities with looser limits. Figure 1 shows that this is the case. In municipalities with looser limits, the average individual contribution among the top two donors as a fraction of total campaign resources is 10 percentage points larger than the average contribution of a non-top donor. In low contribution limit municipalities, on the

Figure 1: Contributions per donor around campaign contributions limits cutoff



Total contributions per donor are averaged across donors in each group (top-two or non-top two) and then across municipalities. 95% confidence intervals.

other hand, this difference is less than half at 4 percentage points.²² Here and in the analysis that follows, we have selected the top two donors as the reference group as they contribute to the campaigns significantly more than the rest (see Figure D1 in Appendix D).²³

Table 3 presents consistent evidence using the RD estimation framework. Models 2 and 3 show that the effect of a higher contribution limit on the average contributions of a top donor (as a fraction of campaign revenue) is 9.1 percentage points, while the effect of higher limits on the contribution of a non-top donor is very close to zero and statistically insignificant.

To further examine our proposed theoretical mechanism, we estimate heterogeneous effects of higher limits on contracts received by top and non-top donors. If higher limits increase the influence of top donors over the mayors, the effects of higher limits on the contracts they receive should be stronger for them than for less generous donors. Importantly,

²²For these figures, we have taken municipalities that are within 5,000 registered voters of the 25,000 registered voters cutoff.

²³Similar patterns emerge when we define top donors as the top three or just the most generous donor (see Table D1 in Appendix D).

Table 3: Effect of looser campaign contribution limits on campaign revenues (top and non-top donors)

Outcome:	ln(Avg. Donation)	Share Top	Share Non-top
	(1)	(2)	(3)
Looser contribution limit	1.134	0.091	0.001
Robust p-value	0.002	0.016	0.911
CI 95%	[0.416,1.918]	[0.019,0.176]	[-0.019,0.017]
Observations	999	999	999
Bandwidth obs.	78	239	65
Mean	0.716	0.068	0.009
Effect mean(%)	158.38	133.82	11.11
Bandwidth	4,570	10,880	3,943

Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth for each dependent variable. Each observation is a mayoral campaign.

there could be self-selection into being a top donor with top donors having characteristics that make them more likely to receive contracts, and because of this, the interaction term between a top donor indicator and the higher limit treatment might be endogenous at the cutoff. To account for this source of bias, we take a selection-on-observables approach proposed by [Gerardino, Litschig and Pomeranz \(2017\)](#) in which we use propensity scores to give more weight in the RD estimation to observations where top donors and non-top donors are similar in terms of observed characteristics.²⁴ Given the two-step nature of this methodology, we compute standard errors via bootstrap. We find that the effect of higher limits on the value of all contracts and the number and value of minimum value contracts for top donors is

²⁴We use the following characteristics: the type of economic activities in which they engage, whether they are business owners, and the age of their business operations, which are available for donors registered with the chamber of commerce.

positive and significantly larger than the effect of higher limits on these variables for non-top donors' contracts (see Table D2 in Appendix D).

There are alternative explanations for the increased rewards to donors under less restrictive limits that rely on changes in the nature of the electoral competition (Stratmann, J and Aparicio-Castillo 2006; Hall 2016; Fourinaies 2018; Butcher and Milyo 2020). The possibility of raising more money might attract different types of candidates who could be more willing to reward their donors. It could also be that higher limits amplify the advantages of incumbents, decreasing electoral competition, and diminishing accountability pressures on the elected candidate (Gordon and Huber 2007). To assess whether these explanations account for the observed patterns, we estimate the effects of higher limits on characteristics of the pool of candidates, the winning candidate, and measures of electoral competition.

We find that elected mayors in higher limit municipalities are no different than those in lower limit municipalities in terms of age, gender, ideology, experience in elected office, history of participation in elections, or record of sanctions than those in lower limit municipalities. The one difference we find is that they are less likely to have an indigenous background. This evidence is inconsistent with lower quality candidates being elected with more donor money (see Table D3 in Appendix D).

Additionally, we find that electoral competition in treatment and control municipalities is similar (see Table D4 in Appendix D). We see this pattern in terms of the margin of victory, the number of candidates, the demographic composition of the candidates, the share of candidates that participated in previous elections, and those who have previously been elected. The effect of higher limits on the vote share of the winner is, if anything, negative. This is inconsistent with the idea that larger donors' rewards are the result of reduced electoral competition. It could also be argued that given that higher limits appear to decrease the winner's vote share, candidates in a more competitive environment might have

to compete for the wealthier donors by offering them greater future rewards. We examine this by studying the impact of higher limits on the concentration of large donors among candidates, finding no significant differences on either side of the cutoff.²⁵

Result: The Quality of Contracts Worsens and Other Implications

So far we have established that looser campaign limits concentrate donor power among top donors while also increasing the kickbacks donors receive in terms of government contracts. In this section we explore other implications of looser restrictions of campaign contributions.

We first address the question of whether there are negative consequences in terms of the performance of public contracts received by donors. It could be the case that looser limits increase contract efficiency, as greater campaign spending allows voters to select the candidates who seeks to achieve the desired outcome of most voters (Coate 2004). The results in Table 4 do not support this idea. Model 1 shows that the probability that a donor of the mayor has a contract with costs running over stipulated amounts increases by 14 percentage points in looser limit municipalities. Moreover, the increase in value of such cost overruns is 214% (Model 4). Model 2 shows evidence consistent with an increase of 8 percentage points in the likelihood contracts managed by a mayor’s donor require extensions, but the effect is not as precisely estimated.²⁶ We do not find significant effects of looser limits on the probability of having cost overruns, extensions, the value of cost overruns, or additional days past the deadline for contract completion when we focus on minimum value contracts. These results

²⁵This concentration is captured by the Herfindhal of big donors computed with the shares of large donors per candidate. Big donors are those whose donations are above the median in the municipality, but results are robust to using the 75th percentile as the cutoff.

²⁶In fact, unlike municipalities with looser restrictions, in those with tight limits that are within the optimal bandwidth, there are no donors whose contracts had cost overruns or required extensions (see Figures E4, E5, and discussion in Appendix E).

suggest that, although it is easier for mayors to give contracts to donors via minimum value contracts, the lower quality of execution linked to cost overruns and extensions appears to be concentrated in large contracts. This is intuitive, as it is more difficult to justify extensions or cost overruns with smaller, less complex contracts.

We also explore whether looser limits represent net benefits for the donors themselves. While we have shown that donors are benefited via more numerous and larger contracts with looser limits, we have also shown that the price they pay for such benefits (their donations) increases. Table G1 in Appendix G presents the effect of looser limits on the ratio of the value of all contracts that a donor receives to the value of her contribution—the *profitability ratio*. We find that looser limits increase the profitability ratio of minimum value contracts by 79.2%. The point estimate is even larger (implying a 145% increase) when we count all types of contracts.²⁷

An alternative channel by which changes in campaign contribution limits can affect the public is through their impact on electoral malpractice. If candidates are restricted on the resources they can receive and spend legally, does this push them towards engaging in illegal activities to win elections? This question is relevant in contexts where electoral manipulation and violence are common. In Colombia in particular, right-wing paramilitaries tried to influence the outcomes of local and national elections around this period (Acemoglu, Robinson and Santos 2013). We show that there are no significant differences in vote buying and turnout suppression reports, armed group attacks, nor paramilitaries' attacks on either side of the cutoff. We also find no strong evidence in favor of more campaign contributions financing vote buying (see Table G3 in Appendix G).

²⁷Table G2 in Appendix G shows that the effects are driven by the top donors in the sample for which we have information on donor characteristics.

Table 4: Effect of looser campaign contribution limits on quality of contracts

Outcome:	All contracts			Minimum value contracts				
	Cost Overruns (1)	Extension (2)	ln(Added Days) (3)	ln(Val. Cost Overruns) (4)	Cost Overruns (5)	Extension (6)	ln(Added Days) (7)	ln(Val. Cost Overruns) (8)
Looser contribution limit	0.141	0.079	0.206	2.141	0.079	0.008	0.040	1.136
Robust p-value	0.028	0.083	0.163	0.042	0.072	0.945	0.637	0.109
CI 95%	[0.016,0.285]	[-0.011,0.173]	[-0.092,0.544]	[0.082,4.316]	[-0.007,0.167]	[-0.068,0.073]	[-0.112,0.183]	[-0.244,2.446]
Observations	2,049	2,049	2,049	2,049	2,049	2,049	2,049	2,049
Bandwidth Obs.	291	282	341	301	291	385	250	301
Mean	0.008	0.007	0.017	0.123	0.004	0.003	0.007	0.059
Effect mean(%)	1,762.50	1,128.57	1,211.76	1,740.65	1,975.00	266.67	571.43	1,925.42
Bandwidth	4,686	4,526	5,172	4,803	4,603	6,236	3,917	4,822

Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values with clustering at the municipality level are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth for each dependent variable. Each observation is a donor.

Robustness: Under-reporting and Extended Sample

A concern with some of our findings is that campaigns might under-report their contributions in municipalities with more restrictive regulations (La Raja 2014). This would bias our results in favor of finding a positive effect of looser limits on donations and overstate the additional benefits going to donors in municipalities with looser limits. We first note that online campaign finance reporting became mandatory in 2009 and that the first elections to fully implement the measure were those of 2011. The reporting system was designed by Transparency International and the electoral commission to increase transparency in campaign reporting and fines for violators of the law were introduced. This could have increased expectations of higher scrutiny in the documents presented by campaigns in 2011.

Nevertheless, we conduct a number of checks to assess whether measurement error is affecting our estimates. We first examine the patterns of lack of reporting by candidates in the donation data. If candidates not only underreport contributions but also decide not to report at all when there are lower limits, we should see more campaign finance reporting to the right of the cutoff. Appendix H, Table H1 shows a negative and insignificant coefficient on looser limits with reporting as the outcome. To account for the fact that no winning campaign has missing reports, we additionally estimate a model where we restrict the sample to include only campaigns of second place candidates in close elections which, as we saw above, are similar to those of the election winner.²⁸ This test does not change our conclusions.

We also estimate the effect of looser limits on the number of donors to the winning candidate. If we observe a significant increase in the number of donors in higher limit municipalities, this could be partially explained by campaigns not reporting some of their donors in lower limit municipalities.²⁹ The results of this test, however, show that there are

²⁸We take municipalities for which the margin of victory was less than 10 percentage points.

²⁹This is a stringent test given that looser limits could actually increase the true number

no significant differences in the number of donors linked to changes in contribution limits (see Table D3 in Appendix D). This finding is also inconsistent with the idea that top donors circumvent the limits by donating through third parties whose individual donations are below the legal limits when restricted, which would imply a negative coefficient in this model.

In a third test to examine potential underreporting, we estimate the effect of looser limits on the share of all contracts (given to donors and non-donors) in economic sectors and under contractual categories that increase the mayor’s discretion in recipient selection and that are typically used to reward donors. In our data, 32% of the contracts given to donors are linked to purchases of construction machinery and office supplies, indicating a preference of mayors to reward donors with this type of contract. In addition, when they are given under the minimum value category, mayors have more discretion to chose the contracts’ recipient. Given the above, if off-the-book donations are affecting our findings and there is no real difference in the benefits that donors (reported and not reported) receive with tight and looser limits, we should not see an effect of looser limits on the share of the types of contracts that mayors prefer to repay their contributors. This is because similar municipalities would demand the same amounts and the same types of contracts. Moreover, looser limits would just induce a different allocation of the same number of contracts from non-reported donors to reported donors. Table H2 in Appendix H shows, however, that the share of total minimum value contracts associated with office supplies and purchases of construction materials tends to be higher in looser limit municipalities.

We also address concerns related to our ability to make correct inferences in some models where the number of effective observations is small. As a separate robustness test, we checked that the patterns in contributions and contracting we observe around the 2011 election hold in an extended sample that also includes the 2015 elections for which the rule that alters contribution limits at 25,000 registered voters also applies. Although not all

of donors and not just the reporting.

variables used in the 2011 analysis are available in 2015, such as characteristics of donors or quality of contract execution for the 2015 incumbency period, we are able to replicate all results for which data are available.³⁰ With the larger sample, we confirm that donors to the election winner receive more contracts than donors to the runner-up, that the number of contracts assigned to the mayor’s donors increase in municipalities with looser limits, and that the influence of top donors is also larger in these same municipalities (Table J3 through J5 in Appendix J). Some differences compared to the previous results are worth noting: we find significant, positive, and large effects of donating to the election winner and of looser contribution limits on the value of donors’ contracts. Even though we now find that candidates running in municipalities with looser limits are less experienced, the experience of the elected mayor—who assigns the contracts—is not different under looser limits (Tables J6 and J7 in Appendix J). Moreover, inconsistent with the idea that “bad types” of candidates are drawn to municipalities with looser limits, we find that elected candidates in such municipalities are, if anything, *less* likely to have previous sanctions for violating the code of conduct of public officials.³¹

Conclusions

We examine whether campaign contribution limits curb quid pro quo benefits for donors through the assignment of public contracts in Colombia. We document a bias in public procurement assignment in favor of mayors’ donors. We also find that these donors receive a greater number of public contracts in municipalities with looser campaign contribution limits.

³⁰The incumbency period starts in 2015 and ends in 2019, which means information on whether the contracts overran in costs or required extensions for those contracts signed towards the end of the period is unavailable.

³¹More details about the analysis and findings are in Appendix J.

The evidence suggests that larger benefits given to donors in higher limit municipalities are explained by top donors having a greater influence over the mayor and not by candidate selection effects nor changes in the competitiveness of the election. The contracts given to donors in higher limit municipalities also tend to perform worse, running large cost overruns. Overall, in the absence of tight contribution limits, society pays more for the completion of public projects, while top campaign donors are more than compensated by receiving public contracts.

Although we have focused our empirical analysis on Colombian mayoral elections, our main theoretical mechanism could still be relevant for other settings. As restrictions on campaign finance are loosened, the risk of a campaign being financed (and captured) by a few individuals could increase. It is important to examine whether in other contexts where campaigns rely more on small donations from more voters expressing their preferences, rich individuals still achieve the level of influence over elected officials under looser campaign finance restrictions that we see in Colombia. A separate consideration when studying the effects of campaign finance in other contexts is the fact that candidates can reward donations through legislation and regulations (over which the municipality mayors we study have less power). A particular challenge when studying donors' benefits measured in that way is that donors choose an ideologically close candidate, who will favor her preferred legislation or regulation. In our case, giving local contracts to specific donors suffers less from this concern.

This paper has concentrated on one potential benefit of reducing the influence of donors over elected officials: reducing the biased allocation of public resources. More work should also be done on whether campaign contribution limits impose costs on society by altering the flow of information available to voters during the campaign. The fact that we do not find strong evidence of higher quality candidates running and being elected in municipalities with looser contribution limits, however, is inconsistent with important informational

gains via additional campaign spending.³² Equally important to formulate recommendations on appropriate campaign finance regulations is to acknowledge that corrupt agents adapt to regulatory changes and that the positive effects of regulations we see might change over time (La Raja 2014). As long as governments offer contracts that generate higher profits than those of the private sector—as is common with local government contracts—private interests will have strong incentives to continue to influence elected officials after the elections. More research is needed to study how campaign finance reforms alter other channels of influence in a post-election setting like lobbying.

³²Campaign misinformation financed with donations could also potentially distort public choices towards less ideal candidates.

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Online Appendix: Do Campaign Contribution Limits
Curb the Influence of Money in Politics? (not intended
for publication)

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A Electoral Victory and Predetermined Covariates

We verify that treatment and control units' characteristics are continuous around the victory cutoff. To do so, we estimate the effects of electoral victory on these characteristics using the same RD design and estimation choices described in the paper. Table A1 presents the estimated effects on the characteristics of the candidates, and Table A2 on donors' characteristics.

Table A1: Candidates characteristics around the electoral victory cutoff

	Mean (1)	Std. Dev. (2)	Victory (3)	CI 95% (4)	Obs. (5)	Band. Obs. (6)	Bandwidth (7)	p-value (8)
<i>Panel A: Individual characteristics</i>								
Women	0.116	0.320	0.018	[-0.067,0.133]	1982	944	0.07	0.519
Age	45.226	9.712	0.354	[-2.642,3.423]	1828	1009	0.08	0.801
Black	0.044	0.206	0.019	[-0.037,0.082]	1828	959	0.07	0.457
Indigenous	0.109	0.311	-0.028	[-0.136,0.058]	1828	1000	0.08	0.432
Leftist party	0.025	0.156	-0.017	[-0.078,0.034]	1982	1192	0.09	0.438
Rightwing	0.244	0.429	-0.067	[-0.198,0.039]	1982	1050	0.07	0.188
Sanctioned	0.123	0.328	-0.064	[-0.172,0.021]	1982	1116	0.08	0.124
Illegal Registration of ID.	0.005	0.073	0.000	[-0.029,0.023]	1982	1120	0.08	0.815
Electoral experience	0.450	0.498	0.063	[-0.062,0.187]	1980	1354	0.11	0.325
Held office before	0.364	0.481	0.001	[-0.140,0.120]	1980	1192	0.09	0.885
<i>Panel B: Funding covariates</i>								
Donors	4.151	6.725	0.761	[-0.955,2.578]	1982	1170	0.09	0.368
Campaign revenue	46.655	99.311	13.187	[-10.142,38.904]	1982	1180	0.09	0.250
Donations/Revenue	0.176	0.272	-0.027	[-0.102,0.040]	1982	1422	0.12	0.389

Columns 1 and 2 report descriptive statistics. Column 3 reports local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth (reported in column 7). Columns 4 and 8 report 95% robust confidence intervals and robust p-values computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Columns 5 and 6 report total observations and observations in optimal MSE bandwidth.

Table A2: Donors' characteristics around the electoral victory cutoff

	Mean (1)	Std. Dev. (2)	Victory (3)	CI 95% (4)	Obs. (5)	Band. Obs. (6)	Bandwidth (7)	p-value (8)
Ch. Commerce	0.420	0.494	0.038	[-0.126,0.194]	4877	1883	0.06	0.678
Company	0.262	0.440	-0.071	[-0.353,0.195]	1918	859	0.06	0.572
Producer	0.098	0.297	-0.050	[-0.174,0.055]	1872	647	0.05	0.308
Company age	165.205	171.317	-25.772	[-81.904,12.831]	1532	467	0.04	0.153

Columns 1 and 2 report descriptive statistics. Column 3 reports local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth (reported in column 7). Columns 4 and 8 report 95% robust confidence intervals and robust p-values computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Columns 5 and 6 report total observations and observations in optimal MSE bandwidth. Ch. Commerce denotes registered in the Chamber of Commerce. Company age is in months. Producer takes a value of 1 if donor is a producer and 0 if it is a service provider.

B Looser Campaign Contribution Limits and Predetermined Covariates

The tables in this section present evidence that municipalities and donors contributing in campaigns on opposite sides of the 25,000 registered voters threshold are similar on a number of observables. This amounts to estimating the effects of looser campaign contribution limits on predetermined covariates. Table B1 presents results of municipality characteristics and Table B2 those of donors' characteristics.

Table B1: Municipality characteristics around campaign contribution limits cutoff

	Mean (1)	Std. Dev. (2)	Looser limits (3)	CI 95% (4)	Obs. (5)	Band. Obs. (6)	Bandwidth (7)	p-value (8)
Discretionary revenue	29192.948	395422.226	592.716	[-9.0e+03, 8867.212]	970	76	4518.17	0.986
Municipal category	5.706	0.999	0.111	[-0.196, 0.479]	999	61	3528.11	0.412
Mayor wages	6.696	2.553	-0.222	[-0.955, 0.396]	999	61	3524.19	0.417
Council size	10.961	2.912	-0.354	[-1.134, 0.286]	999	62	3563.62	0.241
Population	41910.156	258170.413	-448.213	[-4.5e+03, 4011.520]	999	171	8786.33	0.907
Schools	283.765	170.396	60.152	[-34.789, 176.287]	999	103	5767.05	0.189
Contracts	1057.740	2689.075	-87.087	[-880.423, 686.785]	992	106	5989.31	0.809

Columns 1 and 2 report descriptive statistics. Column 3 reports local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth (reported in column 7). Columns 4 and 8 report 95% robust confidence intervals and robust p-values computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Columns 5 and 6 report total observations and observations in optimal MSE bandwidth. Discretionary income scaled in # of minimum monthly wages. Schools denotes all educational establishments.

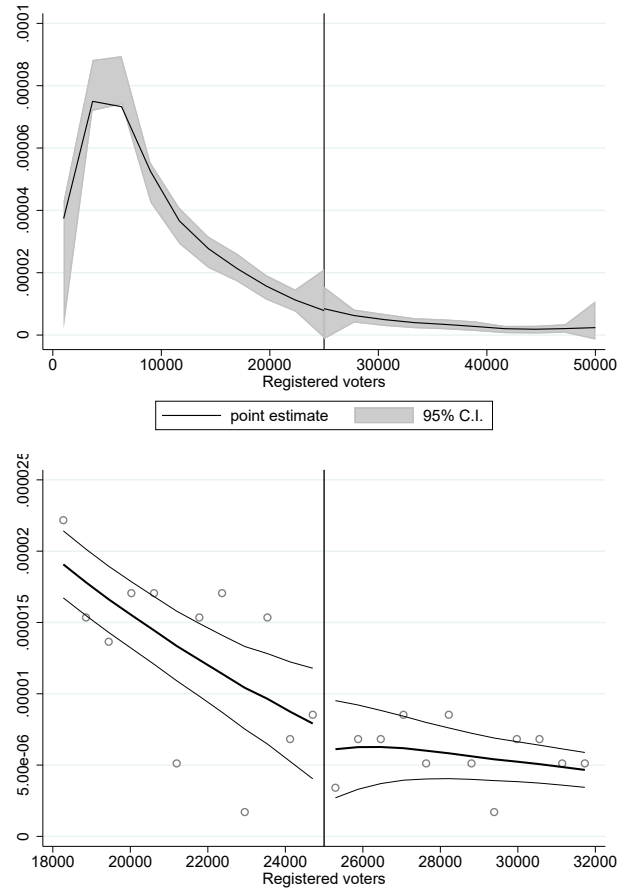
Table B2: Donors' characteristics around campaign contribution limits cutoff

	Mean (1)	Std. Dev. (2)	Looser limits (3)	CI 95% (4)	Obs. (5)	Band. Obs. (6)	Bandwidth (7)	p-value (8)
Ch. of commerce	0.410	0.492	0.050	[-0.185,0.239]	2049	223	3281	0.800
Company	0.247	0.431	0.031	[-0.025,0.165]	697	84	4076	0.146
Producer	0.099	0.298	0.011	[-0.206,0.198]	673	140	7129	0.967
Company age	163.145	175.529	-88.378	[-271.014,63.515]	514	102	7162	0.224

Columns 1 and 2 report descriptive statistics. Column 3 reports local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth (reported in column 7). Columns 4 and 8 report 95% robust confidence intervals and robust p-values computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Columns 5 and 6 report total observations and observations in optimal MSE bandwidth. Ch. Commerce denotes registered in the Chamber of Commerce. Company age is in months. Producer takes a value of 1 if donor is a producer and 0 if it is a service provider.

C Sorting Tests

Figure C1: Sorting Tests



The top figure shows the density of the running variable. The test of no discontinuity at the cutoff ([Cattaneo, Jansson and Ma 2019](#)) gives a statistic of -0.128 and a p-value of 0.98). The bottom figure presents the density graph in a narrower band around the cutoff. Dots represent averages of multiple observations.

D Mechanisms

Table D1: Effect of looser campaign contribution limits on composition of campaign revenues (robustness: top and non-top donors)

Outcome:	Share Top 3 (1)	Share Non-top 3 (2)	Share Top 1 (3)	Share Non-top 1 (4)
Looser contribution limit	0.089	-0.001	0.102	0.032
Robust p-value	0.014	0.651	0.038	0.328
CI 95%	[0.020,0.173]	[-0.011,0.007]	[0.006,0.211]	[-0.035,0.105]
Observations	999	999	999	999
Bandwidth obs.	253	66	233	266
Mean	0.063	0.005	0.077	0.022
Effect mean(%)	141.27	-20.00	132.47	145.45
Bandwidth	11,292	4,036	10,651	11,831

Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth.

Table D2: Effect of looser campaign contribution limits on donors' benefits (top vs. non-top)

Outcome:	# Contracts (1)	ln(Value All) (2)	# Min. Value Contracts (3)	ln(Value Min. Value) (4)
Non-top	3.469 (4.322)	-2.183 (1.677)	-4.218 (3.915)	-1.603 (1.487)
Top	5.887 (4.508)	3.572* (1.841)	10.939* (5.768)	4.281 (2.286)
Difference	2.418 (6.237)	5.755** (2.439)	15.157** (6.833)	5.884** (2.697)
Observations	493	493	493	493
Bandwidth obs.	89	69	69	66
Bandwidth	6,980	5,312	5,292	5,190

Bandwidth is set at optimal MSE bandwidth reported in Table 2 of the paper. Observations in each subgroup (top and non-top) are weighted by the inverse of their conditional probabilities to belong to that subgroup. Variables included in the propensity score are: registered as company with the chamber of commerce, producer as main activity, and age of the company (in months). Clustered bootstrap standard errors at the municipality level with 500 replications in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table D3: Mayors' characteristics across campaign contribution limits cutoff

	Mean (1)	Std. Dev. (2)	Looser limits (3)	CI 95% (4)	Obs. (5)	Band. Obs. (6)	Bandwidth (7)	p-value (8)
<i>Panel A: Individual covariates</i>								
Women	0.098	0.298	-0.010	[-0.129,0.134]	999	81	4869	0.969
Age	44.863	9.740	-3.693	[-15.066,7.071]	927	99	6448	0.479
Black	0.046	0.210	-0.090	[-0.320,0.089]	927	67	4782	0.268
Indigenous	0.112	0.315	-0.317	[-0.653,-0.069]	927	114	7070	0.015
Leftist party	0.028	0.165	-0.023	[-0.134,0.088]	999	92	5268	0.681
Rightwing	0.240	0.427	0.261	[-0.238,0.957]	999	106	5943	0.239
Sanctioned	0.116	0.320	0.047	[-0.326,0.344]	999	79	4759	0.958
Illegal Registration of ID.	0.007	0.086	0.000	[-0.002,0.009]	999	50	3171	0.202
Political experience	0.458	0.498	-0.159	[-0.644,0.201]	999	153	7854	0.304
Held office before	0.369	0.483	-0.125	[-0.684,0.274]	999	131	6939	0.401
<i>Panel B: Funding covariates</i>								
Donors	4.760	7.502	1.874	[-5.484,9.211]	999	161	8574	0.619
Campaign revenue	52.042	106.797	-12.553	[-38.128,8.059]	999	77	4557	0.202
Donations/Revenue	0.187	0.273	0.211	[-0.089,0.504]	999	170	8764	0.170

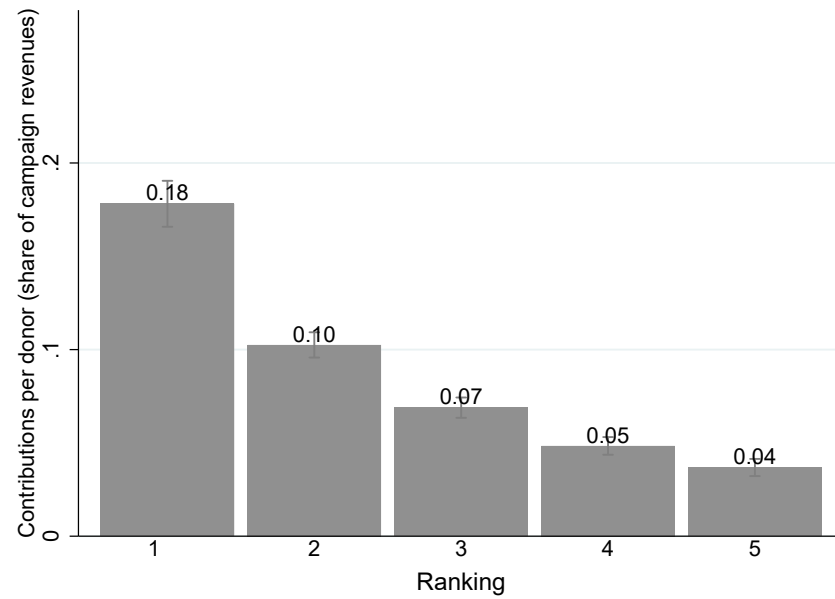
Columns 1 and 2 report descriptive statistics. Column 3 reports local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth (reported in column 7). Columns 4 and 8 report 95% robust confidence intervals and robust p-values computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Columns 5 and 6 report total observations and observations in optimal MSE bandwidth.

Table D4: Election characteristics across campaign contribution limits cutoff

	Mean (1)	Std. Dev. (2)	Looser limits (3)	CI 95% (4)	Obs. (5)	Band. (6)	Obs. (7)	p-value (8)
<i>Panel A: Electoral race covariates</i>								
Candidates	4.030	1.687	0.990	[-0.471,2.748]	1002	153	7896	0.165
Effective N. Candidates	2.739	0.833	0.343	[-0.688,1.281]	933	103	5949	0.555
Vote share winner	0.484	0.106	-0.078	[-0.201,0.017]	933	127	7109	0.100
Vote share margin	14.134	11.863	-5.236	[-17.028,4.677]	932	136	7500	0.265
Herfindahl big donors (p50)	0.605	0.422	0.013	[-0.400,0.394]	1002	159	8282	0.989
Herfindahl big donors (p75)	0.643	0.433	-0.047	[-0.441,0.300]	1002	171	8898	0.708
<i>Panel B: Pool of candidates</i>								
Age	45.544	5.601	0.865	[-3.853,6.616]	999	180	9177	0.605
Women	0.128	0.176	0.077	[-0.132,0.353]	1002	112	6349	0.372
Indigenous	0.099	0.165	-0.149	[-0.329,-0.015]	930	113	6986	0.032
Black	0.044	0.143	-0.016	[-0.068,0.023]	930	59	4108	0.336
Sanctioned	0.116	0.174	0.000	[-0.112,0.107]	1002	143	7468	0.959
Illegal Registration of ID.	0.005	0.037	0.000	[-0.002,0.005]	1002	65	3923	0.415
Political experience	0.254	0.237	-0.147	[-0.400,0.040]	1002	130	6909	0.109
Held office before	0.206	0.223	-0.058	[-0.310,0.136]	1002	147	7596	0.445

Columns 1 and 2 report descriptive statistics. Column 3 reports local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth (reported in column 7). Columns 4 and 8 report 95% robust confidence intervals and robust p-values computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Columns 5 and 6 report total observations and observations in optimal MSE bandwidth. Age in panel B denotes average age of candidates. All other dependent variables in Panel B are in shares of total candidates.

Figure D1: Contributions per donor as share of campaign revenues (by rank)



Total donations per donor are averaged across donors in each rank and then across municipalities. Ranking 1 denotes the most generous donor while ranking 5 denotes the fifth most generous donor. Confidence intervals at 95% level.

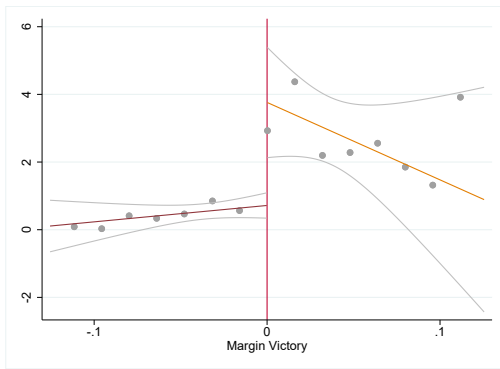
E Estimates Figures (RD Plots)

The following figures present a graphical representation of all the reported estimates with 95% confidence intervals. Each point in the figure represents a bin that averages multiple observations. The linear fit also uses triangular kernels as in our regression tables. Finally, we use the optimal MSE bandwidth for Figures E1 through E4.

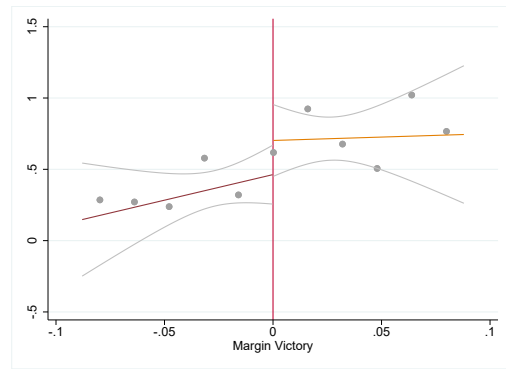
It is worth noting the patterns described by Figures E4 and E5. As we can see in Figure E4, there is strong support for our hypothesis. While to the left of the cutoff there are no donors whose contracts required extensions or ran cost overruns within the optimal bandwidth, to the right (where limits are higher), there are. Further examination confirms that there is more variation to the left of the cutoff as we increase the bandwidth to cover all values of the running variable, as shown in Figure E5. The figures also show, however, that these differences are more prevalent when we examine all contracts. Minimum value contracts do not have much variation in indicators of quality. This is potentially explained by the fact that it is more difficult to justify extensions or additional costs with these less complex contracts.

Figure E1: Effect of donating to a winner on contract assignment

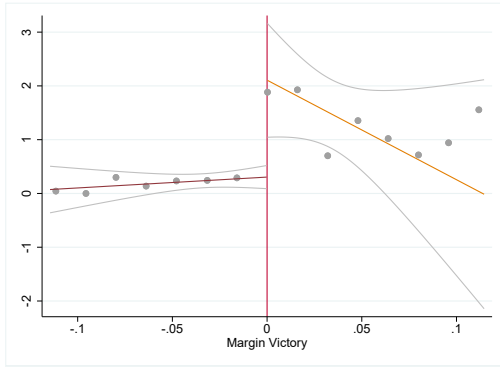
(a) Number of contracts



(b) Logged value of all contracts



(c) Number of Minimum Value contracts



(d) Logged value of Minimum Value contracts

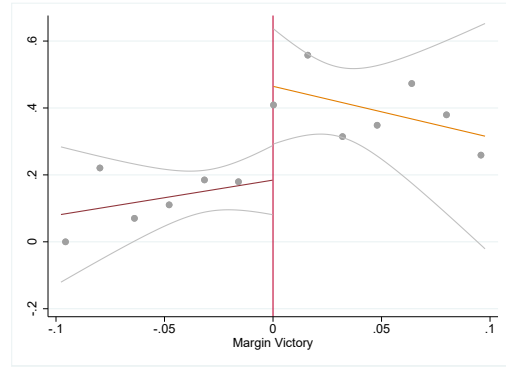
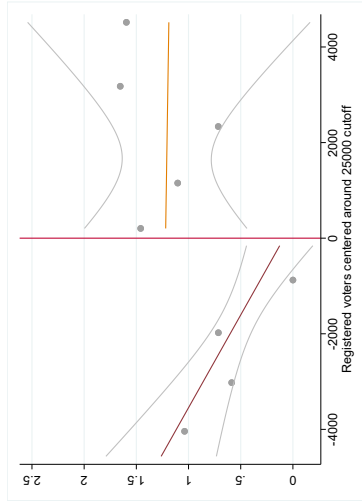


Figure E2: Effect of looser campaign contribution limits on contracts assigned to donors to the mayor

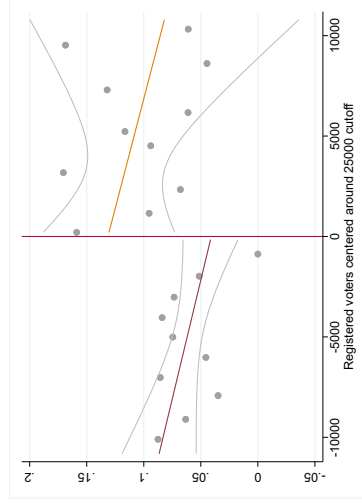


Figure E3: Effect of looser campaign contribution limits on campaign revenues (top and non-top donors)

(a) Logged average donation



(b) Top donor's contribution (share of campaign revenue)



(c) Non-top Donor's contribution (share of campaign revenue)

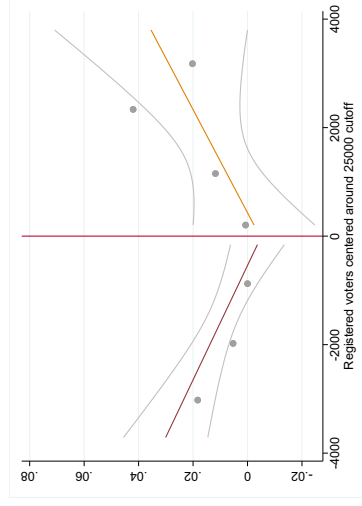


Figure E4: Effect of loser campaign contribution limits on quality of contracts

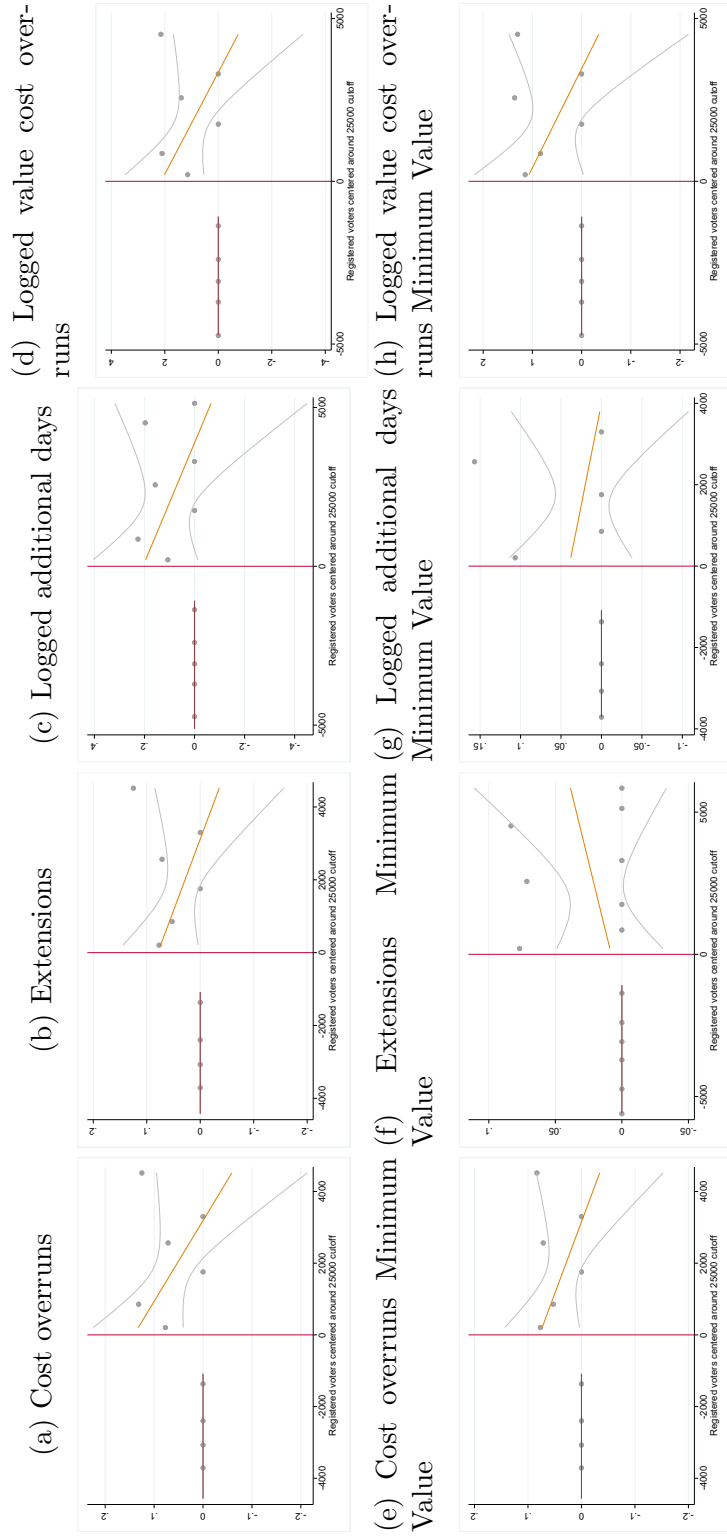
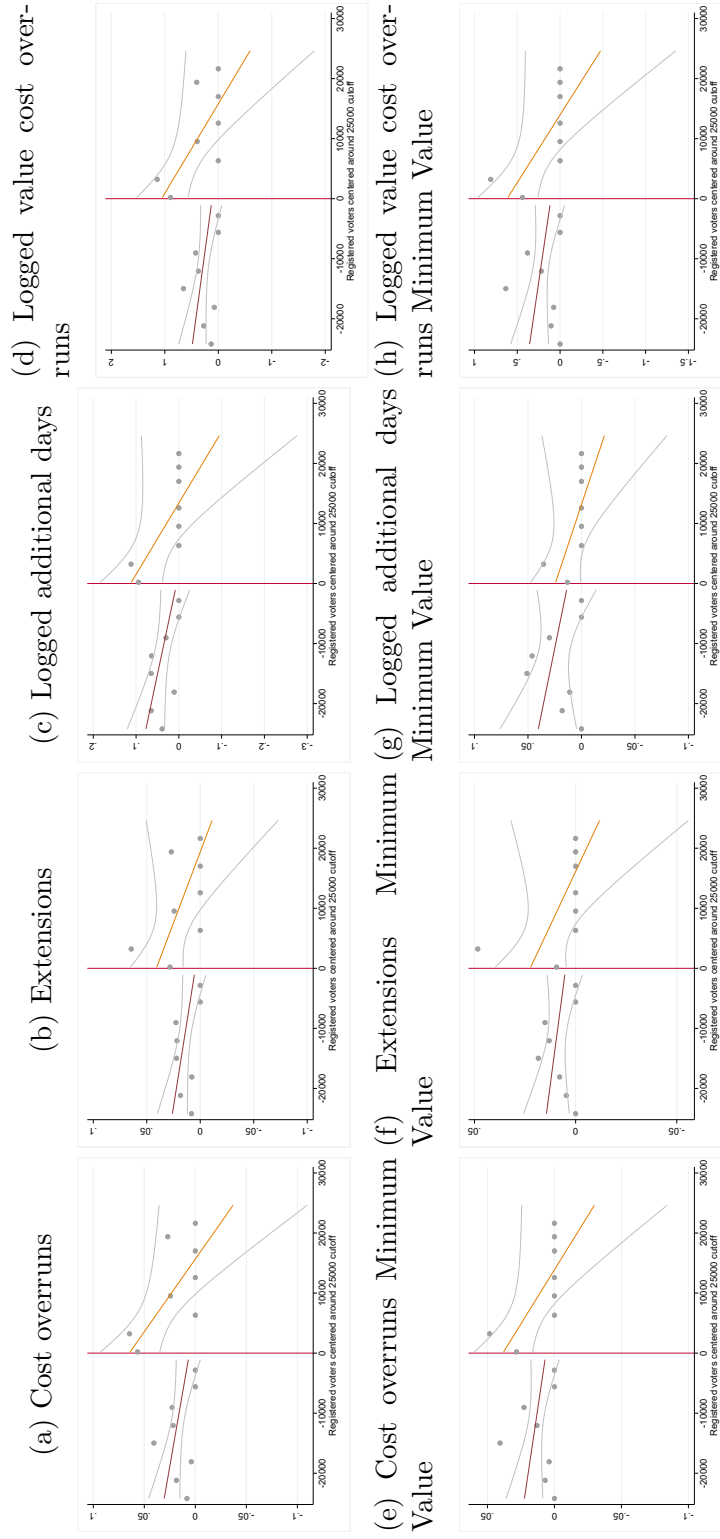


Figure E5: Effect of looser campaign contribution limits on quality of contracts (full range of forcing variable)



F Bandwidth Sensitivity Figures

The following figures present estimates of the effects of interest on all dependent variables reported in the paper at different bandwidths. A red vertical line denotes the bandwidth that minimizes the MSE used to compute the point estimates reported in the paper tables. Following [Cattaneo, Idrobo and Titiunik \(2020\)](#), we report point estimates with no bias approximation correction and robust 95% confidence intervals. As a result, the point estimate might not be at the center of the interval. That would be the case only when the estimated approximation bias is zero. We fix the x -axis to allow comparisons across outcomes for a fixed bandwidth.

Figure F1: Effect of donating to a winner on contract assignment

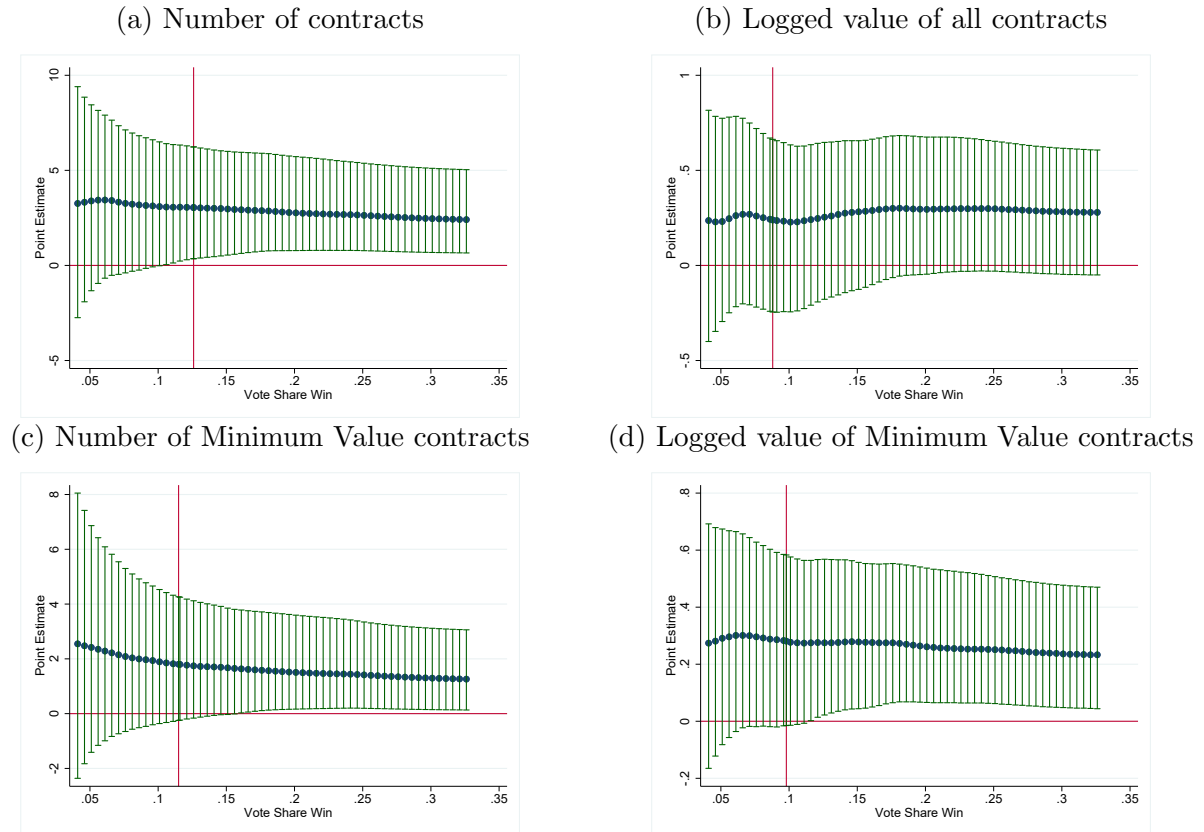


Figure F2: Effect of looser campaign contribution limits on contracts assigned to donors to the mayor

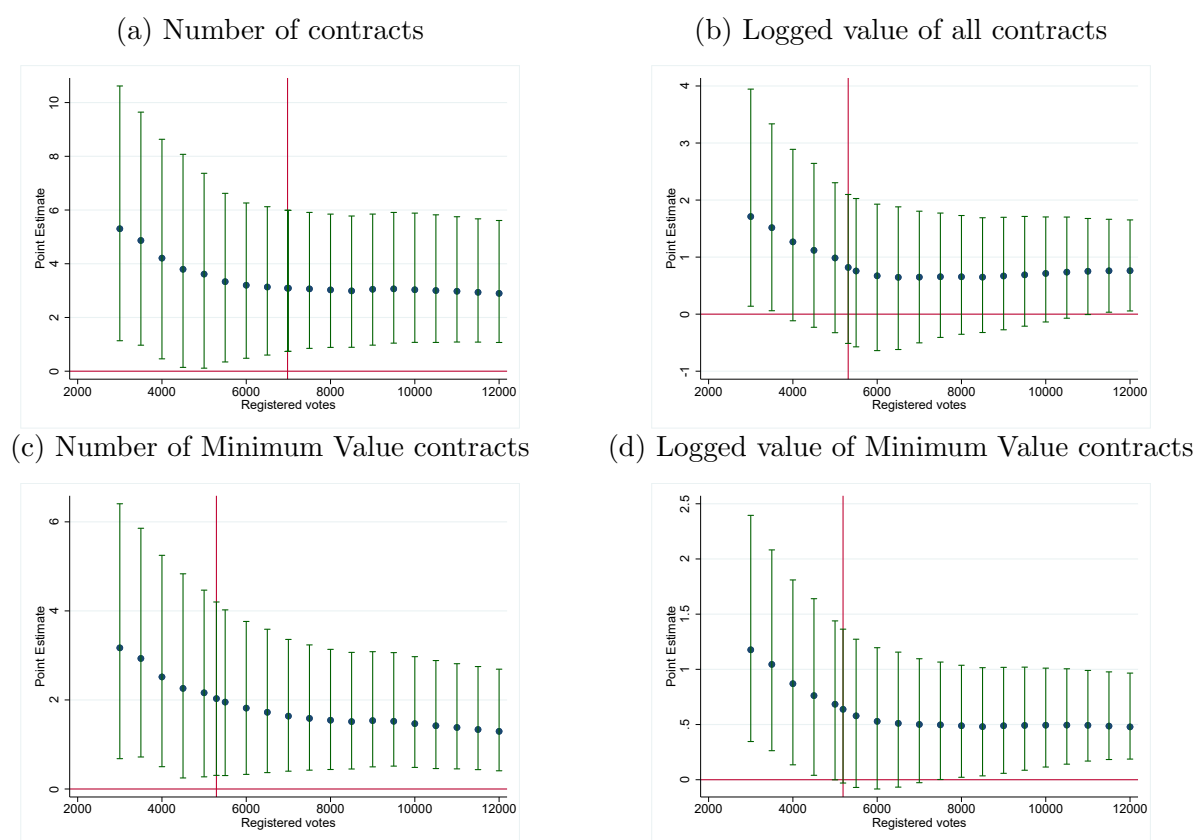
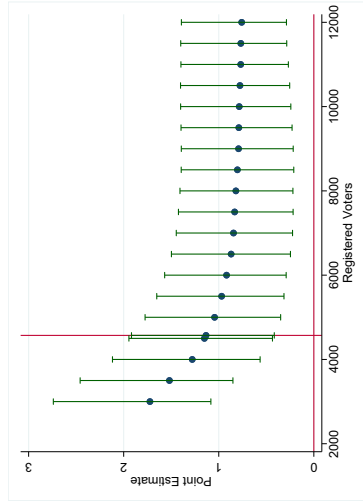
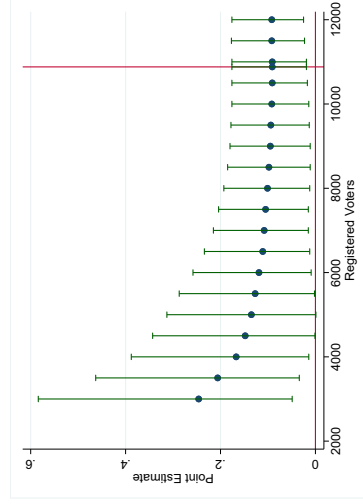


Figure F3: Effect of looser campaign contribution limits on campaign revenues (top and non-top donors)

(a) Logged average donation



(b) Top donor's contribution (share of campaign revenue)



(c) Non-top Donor's contribution (share of campaign revenue)

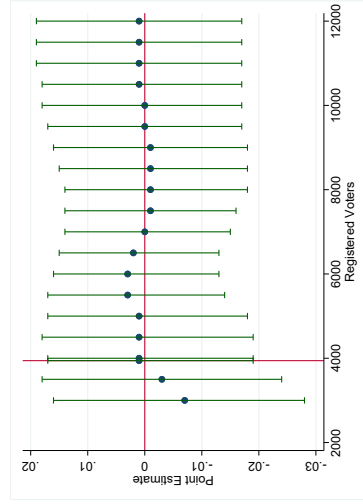
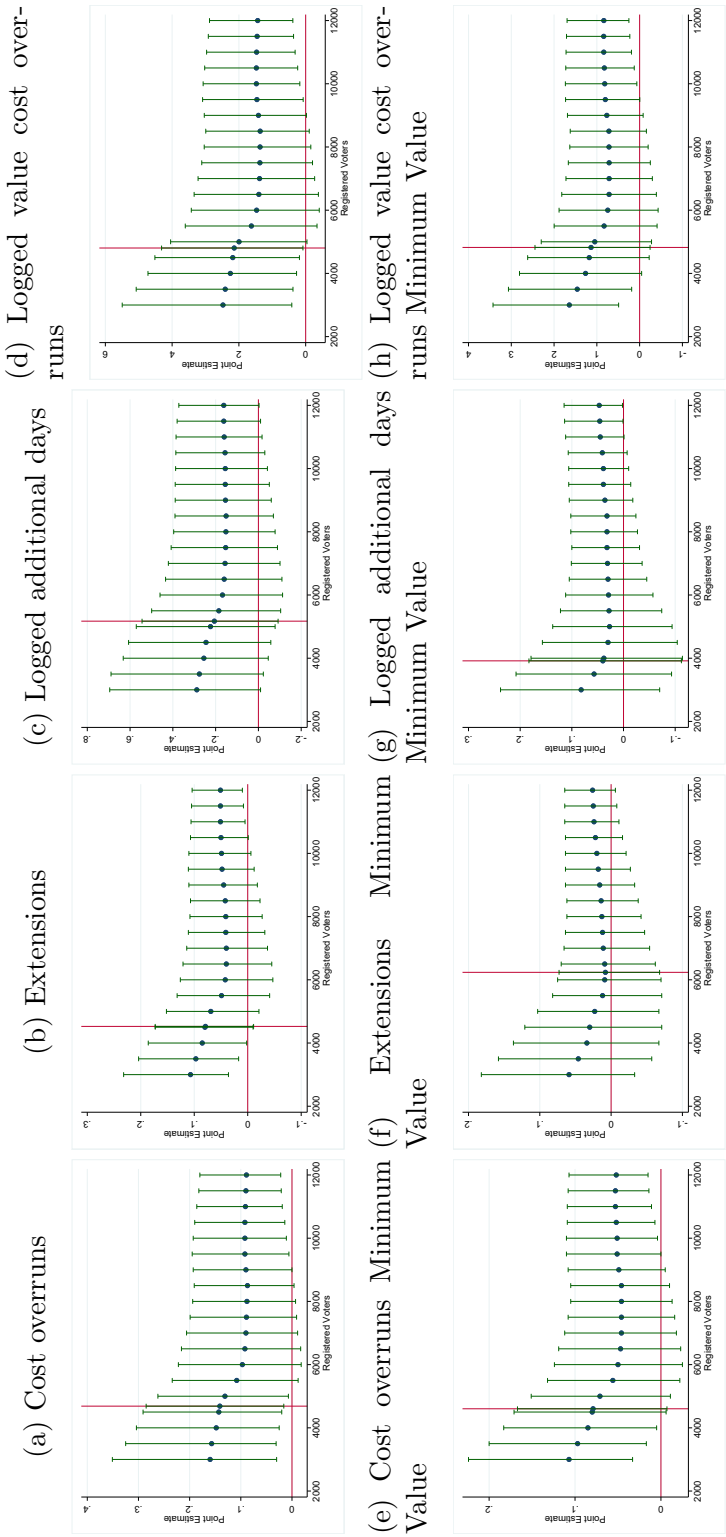


Figure F4: Effect of looser campaign contribution limits on quality of contracts



G Other Implications

Table G1: Effect of looser contribution limits on profitability of contracts

Outcome:	ln(Profitability All) (1)	ln(Profitability Min. Val) (2)
Looser contribution limit	1.457	0.792
Robust p-value	0.068	0.023
CI 95%	[-0.113,3.216]	[0.117,1.588]
Observations	2,043	2,043
Bandwidth obs.	301	278
Mean	0.299	0.116
Effect mean(%)	487.29	682.76
Bandwidth	4,861	4,499

Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth. Profitability denotes the ratio of value of all contracts given to a donor over the value of the donation.

Table G2: Effect of looser limits on profitability of contracts (top vs. non-top)

Outcome:	ln(Profitability All) (1)	ln(Profitability Min. Val) (2)
Non-top	-2.318 (1.378)	-1.852 (1.071)
Top 2	3.834* (1.924)	2.563 (1.467)
Difference	6.153*** (2.211)	4.415*** (1.695)
Observations	493	493
Bandwidth obs.	57	50
Bandwidth	4,861	4,499

Bandwidth is set at optimal MSE bandwidth. Observations in each subgroup (top and non-top) are weighted by the inverse of their conditional probabilities to belong to that subgroup. Variables included in the propensity score are: registered as company with the chamber of commerce, producer as main activity, and age of the company (in months). Clustered bootstrap standard errors at the municipality level with 500 replications in parentheses. Profitability denotes the ratio of value of all contracts given to a donor over the value of the donation. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table G3: Effect of looser campaign contribution limits on electoral manipulation

	Mean (1)	Std. Dev. (2)	Looser limits (3)	CI 95% (4)	Obs. (5)	Band. Obs. (6)	Bandwidth (7)	p-value (8)
Vote buying	0.359	1.252	-0.410	[-1.389,0.371]	999	65	3915	0.257
Turnout suppression	0.170	1.542	-0.010	[-0.528,0.535]	999	109	6277	0.989
Attacks	0.616	2.814	0.554	[-1.134,2.385]	998	159	8352	0.486
Paramilitary attacks	0.194	1.535	-0.281	[-0.836,0.210]	998	89	5160	0.241

Columns 1 and 2 report descriptive statistics. Column 3 reports local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth (reported in column 7). Columns 4 and 8 report 95% robust confidence intervals and robust p-values computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Columns 5 and 6 report total observations and observations in optimal MSE bandwidth.

H Measurement Error

Table H1: Effect of looser contribution limits on campaign finance reporting of runner-up candidate

Outcome:	Campaign reports finance info	
	(1)	(2)
Looser contribution limit	-0.118	0.072
Robust p-value	0.202	0.472
CI 95%	[.112,0.081]	[-0.138,0.299]
Sample	All	Close elections
Observations	1013	663
Bandwidth obs.	178	133
Mean	0.961	0.961
Effect mean(%)	-12.28	7.49
Bandwidth	9,029	10,092

Outcome is 1 if runner-up candidate reports campaign finance information and 0 otherwise. Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth.

Table H2: Effect of looser contribution limits on share of contracts typically received by donors

Outcome:	Min. Val. Materials	Min Val. Supplies
	(1)	(2)
Looser contribution limit	0.041	0.059
Robust p-value	0.065	0.072
CI 95%	[-0.003,0.103]	[-0.007,0.152]
Observations	992	995
Bandwidth obs.	77	75
Mean	0.096	0.166
Effect mean(%)	42.71	35.54
Bandwidth	4,564	4,451

Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth.

I Polynomial Order

In the paper, we follow the most recent literature by presenting local linear estimates combined with triangular kernels which have been shown to reduce bias relative to local constant models (Fan and Gijbels 1996) and avoid multiple inference problems of higher order polynomials (Gelman and Imbens 2019). In this section, we present results estimated using local constant and quadratic (local) specifications, while maintaining the triangular kernel, optimal MSE bandwidth, and robust inference methods as proposed by Calonico, Cattaneo and Titiunik (2014). Substantive conclusions presented in the paper are similar to the ones derived from these models and, in some cases, they even show stronger effects of looser limits.

Table I3 shows that donors to the election winner are rewarded with more contracts with the local constant specification and the quadratic one, although in the latter, the estimate is more noisy. In Table I4 we see that using local constant and quadratic specifications, we can conclude the benefits to donors to the mayor increase not only in the number of contracts, but also in the number of minimum value contracts, and their size. Both alternative specifications also show strong support for our proposed theoretical mechanism indicating that looser limits cause an increase in the weight top donors' individual contributions have in overall campaign revenue but no change in what non-top donors contribute to the campaign (Table I5). Finally, once again we see a positive effect of looser limits on the cost overruns of contracts given to donors to the mayor. The local constant results also suggest significant effects on the probability of cost overruns in minimum value contracts and the quadratic specification results on the probability of contracts managed by donors requiring extensions, as shown in Table I6.

Table I3: Effect of donating to a winner on contract assignment

	(1)	(2)	(3)	(4)
Outcome:	# Contracts		ln(Value All)	
Election victory	2.865	3.293	0.300	0.242
Robust p-value	0.026	0.106	0.235	0.333
CI 95%	[0.371,5.928]	[-0.697,7.240]	[-0.158,0.643]	[-0.261,0.769]
Observations	1,982	1,982	1,982	1,982
Bandwidth obs.	751	1691	639	1420
Mean	0.931	0.931	0.346	0.346
Effect mean(%)	307.73	353.71	86.71	69.94
Bandwidth	0.05	0.15	0.04	0.11
Polynomial order	0	2	0	2

Outcome:	# Min. Value Contracts		ln(Value Min. Value)	
Election victory	1.637	2.155	0.283	0.283
Robust p-value	0.057	0.158	0.032	0.119
CI 95%	[-0.057,3.810]	[-0.912,5.619]	[0.025,0.558]	[-0.069,0.605]
Observations	1,982	1,982	1,982	1,982
Bandwidth obs.	711	1603	671	1595
Mean	0.462	0.462	0.182	0.182
Effect mean(%)	354.33	466.45	155.49	155.49
Bandwidth	0.05	0.14	0.05	0.14
Polynomial order	0	2	0	2

Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values with clustering at the municipality level are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth. Each observation is a municipality-candidate.

Table I4: Effect of looser campaign contribution limits on contracts assigned to donors

	(1)	(2)	(3)	(4)
Outcomes:	# Contracts		ln(Value All)	
Looser contribution limit	2.235	3.352	0.539	0.986
Robust p-value	0.010	0.034	0.049	0.228
CI 95%	[0.705,5.316]	[0.261,6.436]	[0.004,1.394]	[-0.625,2.629]
Observations	2,049	2,049	2,049	2,049
Bandwidth obs.	234	779	370	500
Mean	0.280	0.280	0.205	0.205
Effect mean(%)	798.21	1197.14	262.93	480.98
Bandwidth	3,420	10,976	5,479	8144
Polynomial order	0	2	0	2

Outcomes:	# Min. Value Contracts		ln(Value Min. Value)	
Looser contribution limit	1.328	2.650	0.356	1.045
Robust p-value	0.010	0.036	0.013	0.030
CI 95%	[0.438,3.157]	[0.182,5.401]	[0.098,0.845]	[0.109,2.187]
Observations	2,049	2,049	2,049	2,049
Bandwidth obs.	198	490	249	402
Mean	0.210	0.210	0.101	0.101
Effect mean(%)	632.38	1261.90	352.48	1034.65
Bandwidth	2,584	7,721	3,727	6,340
Polynomial order	0	2	0	2

Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values with clustering at the municipality level are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth. Each observation is a donor.

Table I5: Effect of looser campaign contribution limits on campaign revenues (top and non-top)

	(1)	(2)	(3)	(4)
Outcome:	ln(Avg. Donation)		Share Top	
Looser contribution limit	0.730	1.501	0.077	0.159
Robust p-value	0.003	0.001	0.003	0.053
CI 95%	[0.285,1.337]	[0.677,2.509]	[0.031,0.153]	[-0.002,0.376]
Observations	999	999	999	999
Bandwidth obs.	64	117	79	155
Mean	0.716	0.716	0.068	0.068
Effect mean(%)	101.96	209.64	113.24	233.82
Bandwidth	3,765	6,488	4,798	8,119
(Local) polynomial order	0	2	0	2
Outcome:	Share Non-top			
Looser contribution limit	0.002	0.001		
Robust p-value	0.930	0.959		
CI 95%	[-0.016,0.017]	[-0.024,0.023]		
Observations	999	999		
Bandwidth obs.	79	159		
Mean	0.009	0.009		
Effect mean(%)	22.22	11.11		
Bandwidth	4,781	8,362		
Polynomial order	0	2		

Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values with clustering at the municipality level are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth. Each observation is a municipality.

Table I6: Effect of looser campaign contribution limits on quality of contracts

	(1)	(2)	(3)	(4)
<i>Panel A: All contracts</i>				
Outcome:	Cost Overruns		Extension	
Looser contribution limit	0.066	0.154	0.039	0.107
Robust p-value	0.026	0.033	0.070	0.032
CI 95%	[0.010,0.161]	[0.014,0.327]	[-0.004,0.097]	[0.010,0.225]
Observations	2,049	2,049	2,049	2,049
Bandwidth Obs.	269	452	232	419
Mean	0.008	0.008	0.007	0.007
Effect mean(%)	825.00	1925.00	557.14	1528.57
Bandwidth	4,270	6,886	3,404	6,418
(Local) polynomial order	0	2	0	2
Outcome:	ln(Added Days)		ln(Val. Cost Overruns)	
Looser contribution limit	0.115	0.189	1.031	2.249
Robust p-value	0.101	0.251	0.021	0.054
CI 95%	[-0.030,0.335]	[-0.137,0.526]	[0.201,2.446]	[-0.044,4.885]
Observations	2,049	2,049	2,049	2,049
Bandwidth obs.	261	610	291	460
Mean	0.017	0.017	0.123	0.123
Effect mean(%)	676.47	1111.76	838.21	1828.46
Bandwidth	4,134	9,567	4,653	7,078
Polynomial order	0	2	0	2
<i>Panel B: Minimum value contracts</i>				
Outcome:	Cost Overruns		Extension	
Looser contribution limit	0.039	0.113	0.018	0.024
Robust p-value	0.042	0.021	0.567	0.663
CI 95%	[0.002,0.097]	[0.019,0.234]	[-0.034,0.063]	[-0.087,0.136]
Observations	2,049	2,049	2,049	2,049
Bandwidth Obs.	244	385	198	470
Mean	0.004	0.004	0.003	0.003
Effect mean(%)	975.00	2825.00	600.00	800.00
Bandwidth	3,560	6,104	2,721	7,521
(Local) polynomial order	0	2	0	2
Outcome:	ln(Added days)		ln(Val. Cost overruns)	
Looser contribution limit	0.023	0.032	0.611	1.450
Robust p-value	0.154	0.722	0.053	0.053
CI 95%	[-0.014,0.086]	[-0.120,0.173]	[-0.011,1.537]	[-0.021,3.261]
Observations	2,049	2,049	2,049	2,049
Bandwidth obs.	239	505	232	422
Mean	0.007	0.007	0.059	0.059
Effect mean(%)	328.57	457.14	1035.59	2457.63
Bandwidth	3,511	8,476	3,402	6,520
Polynomial order	0	2	0	2

Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values with clustering at the municipality level are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth. Each observation is a donor.

J Extended Sample (2011 and 2015 Elections)

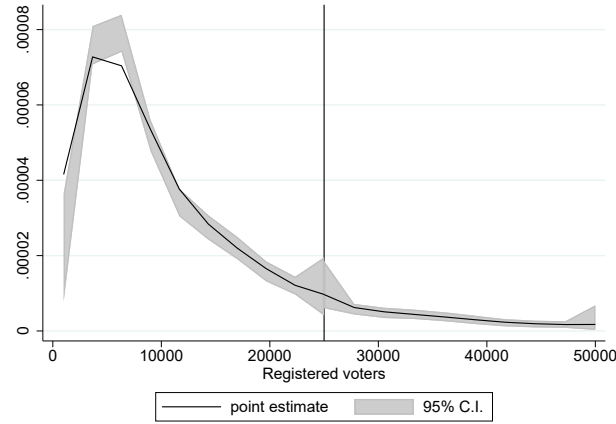
This section presents the results that use an extended sample with elections of 2011 and 2015, as well as contract information from the incumbency periods 2011–2015 and 2015–2019. We also report falsification tests of discontinuities in predetermined covariates and a test of sorting in the running variable at the cutoff to assess the validity of standard RD assumptions in the expanded dataset. Towards the end of the section, we present models that evaluate whether changes in electoral competition can account for the findings with this extended sample.

All results adopt the estimation choices described in the paper regarding kernel, bandwidth selection, confidence intervals, and robust p-value computations. Although we focus on the local linear regression results for interpretation, we have also included local constant and quadratic results. Since we are using two separate election periods, we control for the 2015 election period dummy. This allows us to make comparisons across municipalities with different regulations in the same time period and the estimand is a weighted average of the local average treatment effects of both periods.

Table J1 presents the effects of electoral victory on candidate’s characteristics for the winner and runner-up candidates. Consistent with the original findings, we see no discontinuous jumps at a zero margin of victory in previous participation in elections, having been elected to public office, ideology, nor proxies for malfeasance.

Table J2 shows that there are no significant effects of looser limits on predetermined municipality characteristics like municipality revenues, municipality category (that determines transfers from the central government), population, educational establishments, nor total number of contracts signed by the mayor. Figure J1, shows no indication of a discontinuity in the number of municipalities at the cutoff. This suggests there is no manipulation of the running variable.

Figure J1: Sorting Tests (2011 and 2015)



The figure shows the density of the running variable. The test of no discontinuity at the cutoff ([Cattaneo, Jansson and Ma 2019](#)) gives a statistic of 0.1 and a p-value of 0.92).

Table J1: Mayor's characteristics around the electoral victory cutoff (2011 and 2015)

	Mean (1)	Std. Dev. (2)	Victory (3)	CI 95% (4)	Obs. (5)	Band. Obs. (6)	Bandwidth (7)	p-value (8)
<i>Panel A: Individual characteristics</i>								
Women	0.122	0.327	0.017	[-0.048,0.094]	4112	2129	0.07	0.529
Age	45.293	10.390	1.413	[-0.738,4.082]	3906	1760	0.06	0.174
Leftist party	0.021	0.143	0.001	[-0.031,0.034]	3517	2406	0.11	0.938
Rightwing	0.170	0.375	-0.050	[-0.135,0.016]	3647	1932	0.07	0.125
Sanctioned	0.121	0.326	-0.012	[-0.074,0.046]	4112	2851	0.11	0.655
Illegal Registration of ID.	0.189	0.392	0.014	[-0.054,0.085]	4112	2764	0.10	0.660
Has political experience	0.414	0.493	0.048	[-0.052,0.146]	4107	2297	0.08	0.353
Held office before	0.339	0.473	0.030	[-0.051,0.118]	4107	2735	0.10	0.437
<i>Panel B: Funding covariates</i>								
Donors	3.847	6.476	0.456	[-0.933,1.865]	4112	2427	0.08	0.514
Campaign Revenues	55.817	128.969	10.412	[-13.556,34.753]	4112	2099	0.07	0.390
Donations/Revenues	0.168	0.272	-0.013	[-0.067,0.032]	4112	2708	0.10	0.494

Columns 1 and 2 report descriptive statistics. Column 3 reports local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth (reported in column 7). Columns 4 and 8 report 95% robust confidence intervals and robust p-values computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Columns 5 and 6 report total observations and observations in optimal MSE bandwidth

Table J2: Municipality characteristics around campaign contribution limits cutoff (2011 and 2015)

	Mean (1)	Std. Dev. (2)	Looser limits (3)	CI 95% (4)	Obs. (5)	Band. Obs. (6)	Bandwidth (7)	p-value (8)
Revenues	46098.200	363897.921	-1.0e+04	[-2.4e+04,2900.725]	1998	338	8406	0.126
Municipal category	5.681	1.044	0.227	[-0.087,0.542]	2007	217	5750	0.156
Population	42718.380	262530.694	2639.673	[-828.791,6108.137]	2011	257	6603	0.136
Schools	52.871	112.932	6.073	[-14.778,26.924]	2011	280	7063	0.568
Contracts	672.957	2004.887	-114.097	[-464.306,236.111]	1989	197	5375	0.523

Columns 1 and 2 report descriptive statistics. Column 3 reports local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth (reported in column 7). Columns 4 and 8 report 95% robust confidence intervals and robust p-values computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Columns 5 and 6 report total observations and observations in optimal MSE bandwidth.

The results of Tables J3, J4, and J5 support the general conclusions of the paper. Moreover, while originally we found that benefits accrued to the donors to the mayor were given via more contracts, we now also see clear evidence of large increases in the total value of those contracts. Regarding the magnitude of the effects, while in terms of the number of contracts donors receive, the effect is smaller than that found with the 2011 sample (but still substantively large), the effects on the sizes of contracts are larger and precisely estimated. A donor of the mayor in a municipality with looser limits receives an amount in contracts that is three times as large as one who donated to the mayor in a municipality with more restrictive donations. Table J5 again shows a significant increase of 6 percentage points in the weight of a top donor's contribution in the campaign revenue caused by looser contribution limits.

Table J3: Effect of donating to a winner on contract assignment (2011 and 2015)

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	# Contracts			ln(Value All)		
Election victory	2.476	2.509	2.689	2.396	2.147	1.979
Robust p-value	0.002	0.002	0.009	0.000	0.000	0.002
CI 95%	[0.997,4.342]	[1.004,4.372]	[0.677,4.761]	[1.509,3.024]	[1.078,2.974]	[0.715,3.187]
Observations	4,112	4,112	4,112	4,112	4,112	4112
Bandwidth obs.	1,332	3,001	3,669	1,534	2,449	2,568
Mean	1.408	1.408	1.408	1.960	1.960	1.960
Effect mean(%)	175.85	178.20	190.98	122.24	109.54	100.97
Bandwidth	0.04	0.12	0.17	0.05	0.09	0.09
Polynomial order	0	1	2	0	1	2
Outcome:	# Min. Value Contracts			ln(Value Min. Value)		
Election victory	1.153	1.241	1.378	0.977	0.959	0.997
Robust p-value	0.009	0.011	0.069	0.002	0.002	0.019
CI 95%	[0.325,2.221]	[0.309,2.373]	[-0.109,2.975]	[0.369,1.604]	[0.340,1.568]	[0.172,1.889]
Observations	4,112	4,112	4,112	4,112	4,112	4112
Bandwidth obs.	1,566	3,268	3,485	1,208	2,855	2,746
Mean	0.592	0.592	0.592	0.690	0.690	0.690
Effect mean(%)	194.76	209.63	232.77	141.59	138.99	144.49
Bandwidth	0.05	0.13	0.15	0.04	0.11	0.10
Polynomial order	0	1	2	0	1	2

Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values with clustering at the municipality level are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth. Each observation is a municipality-candidate.

Table J4: Effect of looser campaign contribution limits on contracts assigned to donors (2011 and 2015)

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	# Contracts			ln(Value All)		
Looser contribution limit	1.807	2.390	2.567	0.980	3.271	3.666
Robust p-value	0.001	0.005	0.025	0.082	0.000	0.006
CI 95%	[0.933,3.804]	[0.767,4.352]	[0.327,4.887]	[,2.328]	[1.728,5.942]	[1.149,6.915]
Observations	3,605	3,605	3,605	3,605	3,605	3,605
Bandwidth obs.	339	589	789	325	310	473
Mean	0.272	0.272	0.272	0.683	0.683	0.683
Effect mean(%)	664.34	878.68	943.75	143.48	478.92	536.75
Bandwidth	3,395	6,021	7,545	3,285	3,186	4,898
Polynomial order	0	1	2	0	1	2
Outcome:	# Min. Value Contracts			ln(Value Min. Value)		
Looser contribution limit	1.244	1.698	1.726	0.814	2.925	3.348
Robust p-value	0.001	0.005	0.025	0.019	0.001	0.006
CI 95%	[0.747,2.449]	[0.672,3.071]	[0.297,3.363]	[0.157,1.721]	[1.410,5.320]	[1.045,6.230]
Observations	3,605	3,605	3,605	3,605	3,605	3,605
Bandwidth obs.	200	373	540	283	302	499
Mean	0.150	0.150	0.150	0.212	0.212	0.212
Effect mean(%)	829.33	1132.00	1150.67	383.96	1379.72	1579.25
Bandwidth	2,145	3,866	5,350	2,966	3,075	5,005
Polynomial order	0	1	2	0	1	2

Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values with clustering at the municipality level are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth. Each observation is a donor.

Table J5: Effect of looser campaign contribution limits on campaign revenues (top and non-top donors- 2011 and 2015)

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	ln(Avg. Donation)			Share Top		
Looser contribution limit	0.785	0.863	0.882	0.056	0.062	0.064
Robust p-value	0.000	0.013	0.014	0.005	0.044	0.083
CI 95%	[0.413,1.302]	[0.184,1.580]	[0.182,1.591]	[0.018,0.103]	[0.002,0.126]	[-0.009,0.142]
Observations	1,997	1,997	1,997	1,997	1,997	1,997
Bandwidth obs.	143	192	439	160	250	455
Mean	0.775	0.775	0.775	0.065	0.065	0.065
Effect mean(%)	101.29	111.35	113.81	86.15	95.38	98.46
Bandwidth	3,929	5,221	9,939	4,463	6,504	10,191
Polynomial order	0	1	2	0	1	2
Outcome:	Share Non-top					
Looser contribution limit	0.009	0.007	0.002			
Robust p-value	0.294	0.554	0.886			
CI 95%	[-0.008,0.028]	[-0.014,0.027]	[-0.019,0.022]			
Observations	1,997	1,,997	1,997			
Bandwidth obs.	123	181	250			
Mean	0.010	0.010	0.010			
Effect mean(%)	90.00	70.00	20.00			
Bandwidth	3,351	4,962	6,503			
(Local) polynomial order	0	1	2			

Local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth. 95% robust confidence intervals and robust p-values with clustering at the municipality level are computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Bandwidth obs. denotes number of observations in the optimal MSE bandwidth. Each observation is a municipality.

Table J6: Mayor's characteristics around contribution limits cutoff (2011 and 2015)

	Mean (1)	Std. Dev. (2)	Looser limits (3)	CI 95% (4)	Obs. (5)	Band. Obs. (6)	Bandwidth (7)	p-value (8)
<i>Panel A: Individual covariates</i>								
Women	0.111	0.314	0.004	[-0.221,0.256]	1997	238	6214	0.887
Age	44.871	10.460	-4.701	[-13.257,2.163]	1902	180	5368	0.158
Leftist party	0.022	0.148	0.014	[-0.099,0.120]	1753	173	5410	0.848
Rightwing	0.165	0.372	-0.077	[-0.344,0.213]	1814	253	6954	0.645
Sanctioned	0.112	0.315	-0.136	[-0.356,0.016]	1997	172	4765	0.073
Illegal Registration of ID.	0.201	0.401	0.027	[-0.216,0.335]	1997	220	5808	0.671
Political experience	0.410	0.492	-0.032	[-0.356,0.229]	1996	329	8163	0.670
Held office before	0.334	0.472	-0.039	[-0.412,0.231]	1996	211	5630	0.581
<i>Panel B: Funding covariates</i>								
Donors	4.364	6.814	2.302	[-0.867,5.678]	1997	311	7662	0.150
Campaign revenue	62.662	133.553	11.518	[-12.048,34.501]	1997	259	6645	0.344
Donations/Revenue	0.178	0.271	0.156	[-0.023,0.324]	1997	238	6188	0.088

Columns 1 and 2 report descriptive statistics. Column 3 reports local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth (reported in column 7). Columns 4 and 8 report 95% robust confidence intervals and robust p-values computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Columns 5 and 6 report total observations and observations in optimal MSE bandwidth.

Table J7: Election characteristics across campaign contribution limits cutoff (2011 and 2015)

	Mean (1)	Std. Dev. (2)	Looser limits (3)	CI 95% (4)	Obs. (5)	Band. Obs. (6)	Bandwidth (7)	p-value (8)
<i>Panel A: Electoral race covariates</i>								
Candidates	4.033	1.698	0.187	[-0.725, 1.363]	2000	330	8212	0.549
Effective N. Candidates	2.746	0.831	-0.239	[-0.885, 0.316]	1931	245	6537	0.353
Vote share winner	0.484	0.108	0.049	[-0.017, 0.137]	1931	243	6440	0.125
Vote share margin	6.917	10.820	-1.748	[-6.394, 2.410]	1926	281	7276	0.375
Herfindahl big donors (p50)	0.580	0.431	-0.012	[-0.244, 0.183]	2000	302	7542	0.778
Herfindahl big donors (p75)	0.615	0.444	0.056	[-0.171, 0.257]	2000	286	7269	0.693
<i>Panel B: Pool of candidates</i>								
Age	45.815	6.038	0.965	[-2.769, 5.058]	1996	274	6997	0.567
Women	0.135	0.183	0.049	[-0.080, 0.207]	2000	268	6779	0.385
Sanctioned	0.115	0.172	-0.037	[-0.119, 0.041]	2000	432	9789	0.335
Illegal reg of ID.	0.188	0.273	-0.068	[-0.199, 0.037]	2000	355	8675	0.177
Has political experience	0.322	0.268	-0.164	[-0.340, -0.041]	1999	248	6444	0.013
Held office before	0.248	0.244	-0.109	[-0.253, -0.004]	1999	314	7723	0.043

Columns 1 and 2 report descriptive statistics. Column 3 reports local linear estimates of average treatment effects at cutoff estimated with triangular kernel weights and optimal MSE bandwidth (reported in column 7). Columns 4 and 8 report 95% robust confidence intervals and robust p-values computed following [Calonico, Cattaneo and Titiunik \(2014\)](#). Columns 5 and 6 report total observations and observations in optimal MSE bandwidth.

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