

*The London School of Economics and Political Science*

**Essays on Violence, Money in Politics, and  
Electoral System in Colombia**

Nelson Alejandro Ruiz Guarín  
All rights reserved

PhD thesis submitted to the Department of International Development of the London School of Economics for the degree of Doctor of Philosophy

*London, October 2nd 2017*

# Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

The copyright of this thesis rests with the author. Quotation from it is permitted, provided that full acknowledgement is made. This thesis may not be reproduced without my prior written consent.

I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party. I declare that my thesis consists of 65649 words.

## **Statement of conjoint work**

I confirm that Chapter 1 was jointly co-authored with Leopoldo Fergusson, Juan Vargas and Pablo Querubin, I contributed to 50% of this work.

I confirm that Chapter 3 was jointly co-authored with Dominik Hangartner (secondary supervisor), and Janne Tukiainen, I contributed to 50% of this work.

## **Statement of inclusion of previous work**

I can confirm that chapter 1 (Real Winners Curse), very early draft was presented as single authored coursework for MY557 - Causal Inference for Observational Studies. This course was part of the PhD coursework.

## **Statement of use of third party for editorial help.**

I can confirm that chapter 1 and 2 were edited by Kelley Friel, academic editor.

# Acknowledgments

*“A revolution may well put an end to autocratic despotism (...) or power-seeking oppression, but it will never produce a true reform in ways of thinking”*

Immanuel Kant 1784 in “What is Enlightenment”.

I had always wondered why there are societies which can guarantee welfare to their members and others which seem to be destined to poverty, and if anything can be done about it. This led me to pursue education in economics and political science. After some years working at an international development organization, I decided to pursue an academic career in search for answers and intellectual independence. I was convinced that through the power of public reason, as argued by Kant, society could improve. This PhD is a product of my constant search for answers, my belief in the use of public reason, my belief in the importance of evidence, and a worry for the issues my home country, Colombia, faces.

Doing a PhD in four years and partly self-funded was a daunting task. I believe an individual’s achievements are a product of the people who influence him. First I am grateful to the tireless effort of my parents who worked hard most of their lives so I could have a great upbringing and the best education possible to pursue my dreams. I am grateful to my family with who I have happy memories with. A PhD wouldn’t be possible without academic support. I am grateful to my supervisors Jean-Paul, Dominik, and Sandra. Jean-Paul brought me into the PhD, and I am grateful for his support through countless references and his good humor. Dominik believed in me as a potential academic, and brought me on-board as his supervisee. He was supportive during the PhD process both academically and personally, and his financial support was crucial in times of financial need. To Sandra’s friendliness and her advice about the academic job market. I am grateful for the innumerable seminars that LSE staff organizes, where I was able to meet numerous academics that kept me inspired. I am also grateful to the discussions with other LSE PhD students and professors.

Outside LSE I’m thankful to Pablo Querubín, for his constant support through my PhD, helping set up my visit to the US and his availability to discuss papers and ideas. I learned a lot from working with Pablo, not only academically but on the importance of humbleness, listening, and helping students. I am thankful to the other students I met while visiting NYU and Harvard, for the motivating intellectual exchange. My time spent in the US taking courses and presenting my work at a multitude of venues was very helpful.

I am also grateful to the countless friends I encountered through the different life stages: my undergraduate in Economics friends at University of Los Andes in Colombia, who are some of the smartest, hard working and most inspiring people I’ve ever met. To this day we still keep in contact to discuss academia or life. To my childhood friends, Jairo, Javier and Leo, who have always been there. To the friends I made

while working in Washington DC, and the innumerable parties we had. Lilitana for our countless hours discussing a variety of topics on the phone. The friends that I made while studying the MSc and PhD at LSE, and friends at Goodenough College with who we had countless dinners right before closing time. I am gratefully to Aimee for the happy memories. I am especially thankful to Andrés Corredor Fonseca, whose friendship was very influential shaping the person who I am today. This thesis is dedicated to the his memory.

# *Essays on Conflict, Money in Politics, and Electoral System in Colombia*

## **Abstract**

This thesis focuses on the political economy of development, in particular on the causes and consequences of political selection of leaders. It analyses how different electoral systems can determine the type of politician that runs in elections, and the effects of electing different politician types.

The first paper studies the consequences of electoral victory of newcomer political parties in a context of newly-introduced local elections and weak institutions. Using a regression discontinuity approach, we find that narrow electoral victories of previously excluded left-wing parties to local executive office in Colombia result in an almost one-standard-deviation increase in violent attacks by right-wing paramilitaries against municipalities that elected-left wing parties. Violence can surge as a de facto reaction of traditional political and economic elites when there is a victory of radically different groups and in the absence of monopoly on violence. This paper shows that the introduction of elections does not necessarily lead to less violence, but can lead to more violence when the electoral winner are newcomers with radically different ideas.

The second paper focuses on the consequences of electing a politician funded by donors, and how such politicians may benefit his/her donors disproportionately. Using a novel dataset that uniquely identifies campaign donors and recipients of public contracts during a mayor's incumbency period in Colombia, I find that barely electing a politician who received donations more than doubles the probability of donors receiving contracts from a mean of 5.9% to 15.5%. Moreover, electing a donor funded politician does not lead to lower local government deficit, nor increasing investment, but it does increase the probability of having a disciplinary sanctions against them, and investigations related to public procurement. I then study the role that campaign contribution limits can play in undermining the role of money in politics. Results indicate that campaign limits lead to less participation of donor funding in campaigns, and as a result reduces the number of contracts that the mayor will reward to donors. This paper shows evidence of the consequences of electing a donor funded politician, and how campaign limits can reduce the influence of campaign contributions.

The third paper studies how open or closed lists in proportional representation systems can affect electoral performance and political selection. In open lists, a candidate's internal party ranking is determined by voter preferences, in contrast to closed

lists, in which ranking is predetermined by party preferences. Colombia provides a unique electoral system where parties can field open or closed lists in municipal elections, producing a mixed-list type of electoral system. Using qualitative interviews and constructing a new dataset on politician characteristics and campaign investment, we find that open lists (vs closed lists) produce better electoral returns to the party, induce higher campaign efforts by candidates, and select higher-quality candidates—that are less likely to have registered illegally to vote, and have more political experience. This paper shows that voter discretion – through open lists – can have a dramatic effect on party performance and on the type of politicians ranked in elections.

# Contents

<b>1</b>	<b>Real Winner’s Curse</b>	<b>13</b>
1.1	Introduction . . . . .	14
1.2	Context: Local elections in Colombia’s political landscape . . . . .	18
1.3	Empirical strategy and data . . . . .	21
1.3.1	Data . . . . .	21
1.3.2	RD Design . . . . .	24
1.4	Main results and robustness . . . . .	26
1.4.1	Baseline results . . . . .	26
1.4.2	Robustness to party coding . . . . .	27
1.4.3	Ruling Out Pre-Existing Trends . . . . .	27
1.4.4	Were Left-Wing Victories Anticipated? . . . . .	28
1.5	Alternative interpretations . . . . .	29
1.6	Mechanisms . . . . .	32
1.7	The consequences of violent paramilitary responses . . . . .	33
1.8	Evidence from case studies . . . . .	35
1.9	Discussion . . . . .	38
1.10	Tables . . . . .	40
1.11	Data appendix: description of coding protocol and variables . . . . .	51
1.11.1	Coding left-wing and right-wing parties . . . . .	51
1.11.2	Coding the ideological stance of victims . . . . .	54
1.11.3	Additional variables . . . . .	55
1.12	Additional results and robustness tests . . . . .	61
1.12.1	Balance on covariates . . . . .	61
1.12.2	Robustness to party coding . . . . .	61
1.12.3	Dropping Recurring Municipalities . . . . .	65
1.12.4	Alternative Interpretations: right wing parties, new parties and the UP . . . . .	66
1.12.5	Incumbency disadvantage . . . . .	70
<b>2</b>	<b>The Power of Money. The consequences of electing a donor funded politician</b>	<b>71</b>
2.1	Introduction. . . . .	72
2.2	Related literature . . . . .	74
2.2.1	Donors and candidates . . . . .	76
2.3	Context . . . . .	77
2.3.1	Colombian institutions and electoral context . . . . .	77
2.4	Empirical strategy and data . . . . .	79
2.4.1	Data . . . . .	79

2.4.2	RD Design . . . . .	83
2.4.3	Text analysis and contract price comparison . . . . .	85
2.4.4	Fuzzy RDD Set-up . . . . .	87
2.5	Results . . . . .	88
2.5.1	Effects of electing a donor-funded politician . . . . .	88
2.5.2	Mechanism for benefiting donors: underlying contract characteristics . . . . .	91
2.5.3	Case Study . . . . .	95
2.5.4	Effects of campaign limits . . . . .	96
2.6	Discussion . . . . .	97
2.7	Tables and figures . . . . .	99
2.7.1	Figures . . . . .	99
2.7.2	Tables . . . . .	106
2.7.3	Effects of electing a donor funded politician . . . . .	107
2.7.4	Contract Level results . . . . .	114
2.7.5	Donor Level results . . . . .	115
2.7.6	Price premium for donors . . . . .	115
2.7.7	Results on Campaign Limits . . . . .	116
2.7.8	Online Appendix . . . . .	119

**3 Open or closed? How list type affects electoral success, candidate selection and campaign effort 121**

3.1	Introduction . . . . .	122
3.2	Election Reforms in Colombia . . . . .	124
3.3	Data, and Sample . . . . .	126
3.4	What is the effect of open-/closed list on parties' electoral success? . . . . .	128
3.5	Why Do Parties Fare Better With Open-list? . . . . .	130
3.6	Why Do Some Parties Sometimes Choose Closed-List? . . . . .	133
3.7	Conclusions . . . . .	134
3.8	Tables . . . . .	136
3.9	Figures . . . . .	141
3.10	Appendix A: Interviews . . . . .	143
3.10.1	Interviews . . . . .	143
3.10.2	Questionnaire . . . . .	143
3.10.3	Who decides on list type? . . . . .	145
3.10.4	Advantages of open and closed lists? . . . . .	145
3.11	Appendix C: Additional results and variable descriptions . . . . .	149
3.12	Appendix D: Determinants of list choice . . . . .	156
3.13	Appendix E: Additional robustness checks . . . . .	158

**References 160**



# List of Tables

1.1	Effect of electing a left-wing mayor on paramilitary attacks . . . . .	40
1.2	Effect of electing a left-wing mayor on other forms of violence, corruption and government performance measures . . . . .	41
1.3	Effect of electing a right-wing mayor on violence . . . . .	42
1.4	Effect of electing a left-wing mayor on paramilitary attacks Heteroge- neous effects by timing of AUC demobilization . . . . .	43
1.5	Effect of electing a left-wing mayor on paramilitary attacks Heteroge- neous effects by year of violence after the election . . . . .	44
1.6	Incumbency advantage in Colombia . . . . .	45
1.7	Effect of electing a left-wing mayor on land registry updates . . . . .	46
1.8	Descriptive Statistics of main variables (Sample: Electoral races in which left-wing parties won or came second: 1997- 2014) . . . . .	57
1.9	Variables and sources . . . . .	58
1.10	Effect of electing a left-wing mayor on municipal characteristics . . . . .	62
1.11	Effect of electing a left-wing mayor on paramilitary attacks (Alternative samples resulting from different codings of party ideology) . . . . .	64
1.12	Descriptive statistics of the main variables Sample: Electoral races in which right-wing parties are winners or runners-up: 1997 - 2014 . . . . .	66
1.13	Effect of electing a mayor from a new (non-left) party on paramilitary attacks . . . . .	68
1.14	Effect of electing a left-wing mayor on paramilitary attacks (Differential effect of UP) . . . . .	69
1.15	Incumbency advantage in Colombia using alternative approach (not con- ditioning on past incumbency) . . . . .	70
2.1	Donations Codebook . . . . .	106
2.2	Descriptive Statistics . . . . .	107
2.3	Sources of campaign income across candidate types, % of Total Income	108
2.4	Sources of campaign spending across candidate types, % of Total Spending	108
2.5	Differences in individual characteristics between donor funded politicians and non-donor funded . . . . .	108
2.6	Donors assignment of contracts in donor funded vs non-donor funded incumbencies: . . . . .	109
2.7	Effect of electing a donor funded politician on Contract Given to any donor . . . . .	109
2.8	Effect of electing donor-funded politicians on total number of contracts for donors . . . . .	110
2.9	Smooth manipulation covariates across the donor funded victory cut-off	110

2.10	Smooth municipality covariates across the donor funded victory cut-off	110
2.11	Smooth campaign and municipality covariates across the donor funded victory cut-off . . . . .	111
2.12	Effect of electing rightwing politician on probability of giving contracts to donors . . . . .	111
2.13	Effect of electing a politician with political experience on probability of giving contracts to donors . . . . .	112
2.14	Effect of electing a donor funded politician on fiscal policy variables . .	112
2.15	Effect of electing a donor funded politician on mayor being sanctioned	113
2.16	Effect of electing a donor funded politician on investigation or sanctions related to contracting . . . . .	113
2.17	Comparison of contracts by Mayor type, and whether the contractors were donors. . . . .	114
2.18	Comparison of contract types by Mayor type, and whether the contractors were donors. . . . .	114
2.19	Comparison of contracts sector by Mayor type, and whether the contractors were donors. . . . .	114
2.20	Comparison of contractors by Mayor type, and whether the contractors were donors. . . . .	115
2.21	Descriptive statistics multiplier to investment and contract value . . .	115
2.22	Donor average returns conditional on electoral victory of donor funded politicians . . . . .	115
2.23	Price Comparison of similar type of contracts for donors and non-donors	115
2.24	Effect of looser campaign limits on: . . . . .	116
2.25	Effect of looser campaign limits on: . . . . .	116
2.26	Smooth predetermined municipality covariates across looser campaign limits cut-off . . . . .	117
2.27	Smooth predetermined municipality covariates across looser campaign limits cut-off . . . . .	117
2.28	Effects of looser campaign limits on total number of contracts for donors	118
2.29	Effects of looser campaign limits on probability of donors obtaining contracts . . . . .	119
2.30	Differences in municipality characteristics between campaign sources reporting municipalities and non-reporting . . . . .	119
2.31	Differences in municipality characteristics between in sample and out of sample . . . . .	120
3.1	Share of municipalities (of total municipalities) with number of closed lists . . . . .	136
3.2	Share of municipalities with %of closed lists. . . . .	136
3.3	Descriptive Statistics and Differences in Means Across List Type . . . .	137

3.4	Effect of Open lists on electoral outcomes . . . . .	138
3.5	Effects of Open List on Campaign Investment . . . . .	138
3.6	Effects of list type on political experience. . . . .	139
3.7	Effects of list type on engagement in constituency. . . . .	139
3.8	Effects of list type on election fraud. . . . .	140
3.9	Transition Matrices . . . . .	140
3.10	Timeline for list type decisions . . . . .	149
3.11	Variables and sources . . . . .	149
3.12	Descriptive Statistics and Differences in Means Across List Type . . . .	153
3.13	Effects of use of closed lists on municipality electoral outcomes. . . . .	154
3.14	Presence and list type choice across municipalities, 10 largest Colombian parties in local council elections 2003-2015 . . . . .	154
3.15	Effects of candidate placement on campaign investment within closed lists. . . . .	155
3.16	Effects of candidate placement on campaign investment within open lists. . . . .	155
3.17	Determinants of choosing closed list. Colombian parties 2003-2015. . .	157
3.18	Determinants of choosing closed list based on previous electoral results of parties. Colombian parties 2003-2015. . . . .	157
3.19	Distribution of switchers across parties % of Total . . . . .	158
3.20	<i>Electoral results omitting one party at a time</i> . . . . .	158
3.21	Electoral results controlling for party experience . . . . .	159

# List of Figures

1.1	Brief historical timeline . . . . .	47
1.2	Election years and available data . . . . .	48
1.3	McCrary test: Sorting around the winning threshold for the left . . . . .	48
1.4	Effect of electing a left-leaning mayor on paramilitary attacks . . . . .	49
1.5	Effect of electing a left-leaning mayor on measures of violence . . . . .	50
1.6	Violence data: examples of attacks . . . . .	56
1.7	Effect of electing a left-wing mayor on violence (dropping recurring municipalities) . . . . .	65
1.8	Effect of electing a right-leaning mayor on violence Robustness to bandwidth selection . . . . .	67
1.9	McCrary test: Sorting around the winning threshold for the right . . . . .	68
2.1	Example of a contract in the Dataset . . . . .	99
2.2	Summary of linking contracts with donors . . . . .	99
2.3	Colombian municipalities where the donor funded politician placed first or second. 2011 Election . . . . .	100
2.4	Colombian municipalities where the donor funded politician placed first or second in a electoral narrow margin of (6.4%). 2011 Election . . . . .	100
2.5	Spatial autocorrelation index (Morans I) for municipalities where the donor funded politician placed first or second in a narrow margin. . . . .	101
2.6	Example of mayoral election ballot of 2011 . . . . .	101
2.7	Distribution of winning margins for donor-funded politicians and McCrary test . . . . .	102
2.8	Distribution of registered voters and McCrary test . . . . .	102
2.9	Effect of electing a donor funded on probability of donors receiving contracts . . . . .	103
2.10	Robustness check: Different bandwidth sizes. Effect of electing a donor funded politician and giving a contract to a donor. . . . .	103
2.11	Robustness check: Different bandwidth sizes. Effect of electing a donor funded politician on disciplinary sanctions . . . . .	104
2.12	Robustness check: Different bandwidth sizes. Effect of electing a donor funded politician on investigation/sanction related to contracting . . . . .	104
2.13	Price difference between donor funded contracts and regular contracts . . . . .	105
2.14	Average Donor contribution and campaign limits . . . . .	105
3.1	Example of a mixed ballot in Colombia. . . . .	141
3.2	Map of council candidates interviewed. Colombia local elections 2015. . . . .	142

# Chapter 1

## Real Winner's Curse<sup>1</sup>

LEOPOLDO FERGUSSON<sup>2</sup>    PABLO QUERUBIN<sup>3</sup>

NELSON A. RUIZ<sup>4</sup>    JUAN F. VARGAS<sup>5</sup>

### Abstract

We study the unintended consequences of political inclusion in a context of weak institutions. Using a regression discontinuity approach, we show that the narrow election of previously excluded left-wing parties to local executive office in Colombia results in an almost one-standard-deviation increase in violent attacks by right-wing paramilitaries, more than tripling the sample mean. We interpret this surge in violence as a *de facto* reaction of traditional political and economic elites, who seek to offset the increase in outsiders' *de jure* political power. Consistent with this interpretation, we find that other types of violence are unaffected, and that levels of violence are not influenced by the victory of right-wing parties in close elections. Moreover, we show that the surge in paramilitary violence is concentrated in the year of the next election, which gives left-wing parties a large incumbency *disadvantage* in Colombia.

---

<sup>1</sup>For their helpful comments, we thank Juan Carlos Angulo, Tim Besley, Catherine Boone, Laura Bronner, Adriana Camacho, Ernesto Dal-Bó, Emilio Depetris, Guadalupe Dorna, Juan Dubra, Marcela Eslava, Claudio Ferraz, Jean-Paul Faguet, Xavier Freixas, Sebastián Galiani, Francisco Gallego, Jenny Guardado, Frances Hagopian, Dominik Hangartner, Daniel Hidalgo, Marc Hofstetter, Oskar Nupia, Rafael Santos, José Tessada, Santiago Tobón, Hernán Vallejo, Diana Weinholt, and participants at the Brown “Violence: Processes, Responses, & Alternatives” Workshop, CEDE-Universidad de los Andes Weekly Seminar, Universidad del Rosario, Universidad Católica de Chile, Forum Ridge-Lacea Political Economy, Harvard-DRCLAS Tuesday Seminar Series, Harvard Political Economy Graduate Workshop, LSE’s Political Science Work in Progress Seminar, LSE Political Economy and Public Policy Workshop, LSE’s Research Seminar in International Development, MIT’s Latin American Working Group, MPSA Annual Meetings 2016, NEWEPS 2016, the NYU Graduate Political Economy Seminar, and the 6<sup>th</sup> Annual Conference of the Al Capone network. Juan Carlos Angulo, Juliana Aragón, Carmen Delgado, Francisco Eslava, Diego Martín, and Juan Camilo Mejía provided superb research assistance. Fergusson gratefully acknowledges Harvard’s David Rockefeller Center for Latin American Studies; this research was partly conducted during his stay as a Santo Domingo Visiting Scholar. Ruiz-Guarin gratefully acknowledges UNU-WIDER for its support.

<sup>2</sup>Facultad de Economía, Universidad de los Andes. Calle 19A No. 1-37 Este Bloque W, Bogotá, Colombia. Tel: +57 1 339-4949 Ext 2439, Email: [lfergusson@uniandes.edu.co](mailto:lfergusson@uniandes.edu.co)

<sup>3</sup>The Wilf Family Department of Politics, New York University. 19 W 4th Street, Room 208, New York, NY 10012. Tel: +1 212 992 6525, Email: [pablo.querubin@nyu.edu](mailto:pablo.querubin@nyu.edu)

<sup>4</sup>International Development Department, London School of Economics and Political Science. 6-8th Floors, Connaught House, Houghton Street, London WC2A 2AE. Tel: +44 (020) 7955 6565, Email: [n.a.ruiz-guarin@lse.ac.uk](mailto:n.a.ruiz-guarin@lse.ac.uk)

<sup>5</sup>CAF-Development Bank of Latin America and Facultad de Economía, Universidad del Rosario. Casa Pedro Fermín, Calle 12C No. 4-59, Bogotá, Colombia 110321. Tel: +57 1 297-0200, Email: [jvargas@caf.com](mailto:jvargas@caf.com) & [juan.vargas@urosario.edu.co](mailto:juan.vargas@urosario.edu.co)

## 1.1 Introduction

In many countries, despite the presence of nominally democratic institutions, some political groups remain largely excluded from formal political power. *De facto* barriers include fraud (Schedler, 2002; Lehoucq, 2003), clientelism (Anderson, Francois, and Kotwal, 2015; Larreguy, 2013), uneven access to economic resources (Baland and Robinson, 2008), violence (Acemoglu, Robinson, and Santos, 2013), and other constraints on political participation (Naidu, 2012). Yet in spite of these barriers, traditionally excluded groups may succeed in winning elections and entering the political system. What happens when these outsiders gain formal political power?

One possibility is that giving excluded groups a voice and a stake in the political process strengthens democracy and promotes political stability. However, another likely implication is that, faced with electoral defeat by outsiders, powerful political elites who have previously enjoyed a monopoly over access to power will feel that their interests are threatened. Where *de jure* institutions such as elections fail to favor the more powerful groups in society, these groups may strengthen their emphasis on *de facto* means to avoid policy changes and prevent other groups from gaining formal power (Acemoglu and Robinson, 2008). Moreover, in weakly institutionalized environments in which political power is concentrated in a few hands, this may help explain the relatively mild or null effects of democratic reforms on economic policies and other political and economic outcomes (Mulligan, Gil, and Sala-i Martin, 2004). As long as the underlying distribution of power remains unchanged, traditional political elites may prevent these reforms from having the intended effect.

This paper examines elite responses to previously excluded (left-leaning) parties gaining local representation in Colombia by winning mayoral elections. We focus on the most direct form of *de facto* power: violence. We assess whether the victory of left-leaning parties in mayoral elections (1) generates (or exacerbates existing) violence and (2) if so, whether this prevents non-traditional parties from attaining political power in the future.

Colombia is an ideal setting in which to study this question. Following a legacy of power-sharing agreements between the Liberal and Conservative parties (which are described in more detail in Section 1.2), Colombia introduced local elections in the late 1980s to open up the political system and broaden access to power to formerly excluded groups. These reforms included the introduction of single plurality rule elections to select municipal mayors. Previously, these were appointed by departmental governors, themselves selected by the president who was historically a member of one of the two traditional parties. A new constitution enacted in 1991 further weakened the dominance of traditional parties. While the left remained a political minority, some of its candidates were elected to local offices like mayoral posts and municipal councils, which represented an important change in the local political arena. These new

political actors began advocating different policy preferences than those of traditional parties, including a stronger emphasis on redistribution, communal property rights, land reform, and vindication of peasant rights.

To study the effect of left-wing victories on violence, we use a regression discontinuity design (RDD) based on close elections and compare municipalities in which the left narrowly won versus narrowly lost the mayoral race. Our results show that a narrow left-wing victory leads to up to 6.8 additional yearly attacks per 100,000 inhabitants by right-wing paramilitary groups during the subsequent government term. This effect is large: it is equivalent to almost one standard deviation and over three times the sample mean. Importantly, we do not find a significant surge in violence when other (non-left-wing) parties win by a small margin. Furthermore, we show that left-wing parties suffer from an incumbency *disadvantage* that is almost six times larger than that experienced by other parties in Colombia (which has been documented by [Klašnja and Titiunik \(2017\)](#)).

Several additional findings support our interpretation that paramilitary attacks following left-wing victories form part of a deliberate strategy by local elites to offset (via *de facto* methods) the political power gained by the left through institutional means. For instance, consistent with the idea that traditional elites incite violence to prevent left-wing groups from increasing their representation in local government, we show that the increase in violence is concentrated around the time of the subsequent local election. Moreover, we find that this effect becomes much weaker after 2006, when paramilitary groups signed a peace deal with the government and demobilized. While splinter criminal bands continued to engage in violent acts against left-wing activists after 2006, violence has been less politically motivated since then.

Ruling out some alternative interpretations of our results, we find no comparable increase in paramilitary (or any type of) violence in the period *before* narrow victories by left-wing candidates. Similarly, we find no changes in violence perpetrated by groups other than right-wing paramilitaries after narrow victories by the left. Thus, our results do not seem to reflect pre-existing trends in violence or an increase in overall violence in constituencies where the left wins. Nor does increased violence appear to be a reaction to corruption or poorer performance by leftist mayors while in office. We do not find that left-leaning parties are involved in more corruption investigations or convictions than other parties, or that their administrations exhibit worse governance indicators.

Our results are consistent with anecdotal and case study evidence (which we present in detail in [Section 1.8](#)) that left-wing political activists have often been the target of paramilitary groups following left-wing victories. We show that these patterns of violence against the general population and party activists in areas where the left wins local elections are systematic and do not represent isolated incidents.

The paper is related to several strands of literature. Our purported mechanism of informal control provides evidence in line with [Acemoglu and Robinson's \(2008\)](#)

idea that, when operating in weak institutional settings, elites may react to a loss in *de jure* power by investing in *de facto* methods to avoid substantial changes in equilibrium institutions and policies. They argue that following the enfranchisement of freed slaves after the US Civil War, southern elites responded with the enactment of literacy tests and poll taxes that *de facto* disenfranchised the black population. [Naidu \(2012\)](#) shows that these strategies were successful in hurting schooling outcomes and electoral participation of black citizens. [Bruce and Rocha \(2014\)](#) show that after democratization in Brazil in the 1980s, turnout patterns were consistent with illiterate voter manipulation by elites aligned with the former dictatorship. Consistent with this, [Fujiwara \(2015a\)](#) finds that the introduction of electronic voting in Brazil enfranchised poor illiterate voters, translating into a better electoral performance for leftist parties. Other studies show how elites can use their control over economic resources such as land to create patron-client relations and manipulate voter behavior. Examples include [Baland and Robinson \(2008\)](#) for Chile and [Anderson et al. \(2015\)](#) for India. Finally [Bandiera and Levy \(2011\)](#) provide suggestive evidence of the potential relevance of *de facto* power for the political equilibrium by showing that in Indonesia, policy is tilted towards the elites in areas where the poor population is more ethnically diverse and therefore has a harder time organizing against the elites' potential influence.

Yet, few papers have studied what is perhaps the most obvious (and potentially damaging) form of *de facto* power: outright political violence. The Post-Bellum U.S. South provides another example as Southern elites also responded with lynchings to the enfranchisement of freed slaves. [Naidu \(2012\)](#) finds that lynchings and other *de facto* methods of disenfranchisement such as literacy tests operated as complements rather than substitutes during this period. More recently, for the Colombian context [Fergusson, Vargas, and Vela \(2013\)](#) study the use of violence in the form of electoral coercion by paramilitaries following media scandals affecting their preferred candidates. Here, we instead study violent reactions to the election of formerly excluded groups that threaten the interests of traditional elites. [Acemoglu et al. \(2013\)](#) and [Dube and Naidu \(2015\)](#) also study the role of paramilitaries during elections in Colombia, though their focus is on incentives not to consolidate the state's monopoly of violence and on the influence of military assistance on illegal armed group's violence, respectively.

The reaction by elites that we document constitutes a response to democratization reforms – i.e., the introduction of local elections to broaden access to formal political power. While we study the effect of formerly excluded groups gaining access to power rather than the introduction of elections *per se*, our results also relate to the literature on elections and violence. Elections are said to provide an “antidote to international war and civil strife” (Bill Clinton, 1994, in [Snyder \(2000\)](#)).<sup>6</sup> Indeed some papers find

---

<sup>6</sup>Scholars have emphasized several mechanisms via which elections may lead to a reduction in violence: the preferences of the opposition receive attention as part of the political debate, which reduces their incentives for violent opposition ([Regan and Henderson, 2002](#)); avoid social unrest by allowing formal channels of dissent ([Davenport, 2007](#)); and elites can credibly commit to future redistribution when policy concessions are insufficient to persuade excluded groups not to revolt ([Acemoglu and](#)



evidence that political representation of formerly excluded groups decreases violence. For the Indian case, [Bhalotra, Clots-Figueras, and Iyer \(2012\)](#) find that an increase in the share of Muslim politicians in state assemblies results in a decline in the incidence of Hindu-Muslim riots. Similarly, [Chandra and Garcia-Ponce \(2016\)](#) find that the success of ethnic subaltern-led parties in India deters armed violence. Other studies find that the introduction of elections more generally lead to a decrease in violence ([Davenport, 1997](#); [Fergusson and Vargas, 2013](#)).

However, by creating winners and losers, elections may increase incentives for violent behaviors that could otherwise be avoided, for example, by power-sharing agreements. As [Chacón, Robinson, and Torvik \(2011\)](#) put it, the key issue is the conditions under which losers will peacefully relinquish power. Consistent with our argument that elections may generate violence in weakly institutionalized settings, [Collier and Rohner \(2008\)](#) find that the introduction of democracy leads to an increase in political violence in poor (but not in rich) countries. [Eifert, Miguel, and Posner \(2010\)](#) also show that political competition may exacerbate (ethnic) identities, which represent another source of conflict. Despite the conflicting theoretical effects, several authors suggest that elections and “democratic transitions” nurture violence ([Huntington, 1991](#); [Horowitz, 1993](#); [Sahin and Linz, 1995](#); [Flores and Nooruddin, 2012](#); [Casper and Taylor, 1996](#); [Snyder, 2000](#)). Our paper shows that the identity and preferences of election winners is critical for understanding when elections may exacerbate violence.

Finally, our study is also closely associated with the literature on “subnational authoritarianism” (e.g. [Gibson, 2005, 2014](#); [Giraudy, 2010](#); [Sidel, 2014](#)). The patterns of violence that we document are consistent with elites using “boundary control” strategies to maintain control over their local authoritarian enclaves following the national democratization reforms in Colombia in the late 1980s ([Gibson, 2014](#)).

While our empirical evidence focuses on the case of Colombia, our argument and empirical findings are relevant for a wider sample of countries. Increases in violence after previously excluded groups are newly elected to office have been observed at the national level across the world: in Egypt, when the Muslim Brotherhood came to power and enacted very different policies – including redrafting the constitution – this triggered increased violence and a coup. Similarly, when Haiti transitioned from dictatorship to democracy in 1990, Jean Bertrand Aristide, a priest representing a new group in politics, won the election. Aristide proposed several reforms, such as a military under civilian control and much more redistribution. These policies generated violent reactions from the old elite, which culminated in a violent military coup in 1991 ([Collins Jr and Cole, 1996](#); [Naidu, Robinson, and Young, 2015](#)). While these examples suggest that political inclusion has a potentially destabilizing effect when groups with very different policy preferences have access to power, it is hard to determine whether the political inclusion of a formerly excluded group was what caused the increase in

---

[Robinson, 2006](#)).

violence. Our study allows us to address this causal question more systematically.

Finally, our paper also relates to the literature on the incumbency (dis)advantage. While [Gelman and Huang \(2008\)](#) claim that “incumbency advantage is one of the most widely studied features in American legislative elections” (p. 437), an incumbency curse or *dis*advantage has been documented in other settings, mostly in developing countries, which are often characterized by weak parties and politicians’ incentives to use local office opportunistically ([Roberts, 2008](#); [Uppal, 2009](#); [Klašnja and Titiunik, 2017](#); [Klašnja, 2015](#)). Our findings point to the *de facto* reaction of elites as a complementary explanation for the incumbency disadvantage of some parties in weakly institutionalized democracies.

The remainder of the paper is organized as follows. Section 1.2 discusses the general context and describes the history of local elections in Colombia. Section 1.3 presents our data and empirical strategy. In Section 1.4 we present our main result: the election of a left-leaning mayor in Colombia leads to increased violence by right-wing paramilitary groups. We also report some basic robustness checks. In Section 1.5 we address and rule out alternative interpretations of our results. Section 1.6 provides evidence to support our preferred interpretation of the reasons behind the increase in violence following left-wing victories. In Section 1.7 we document the consequences of the surge in violence after the electoral success of left-wing parties. In Section 1.8 we discuss some anecdotal evidence, and in Section 1.9 we conclude and discuss the implications of our findings and contribution.

## 1.2 Context: Local elections in Colombia’s political landscape

Figure 1.1 provides a brief outline of Colombia’s recent political history. Colombian politics were dominated by the Liberal and Conservative parties from independence until the late 20<sup>th</sup> century ([Bushnell, 1993](#)). Inter-party violence was widespread, and reached its height between 1948 and 1953 in a period known as *La Violencia*. In order to pacify the country, both parties agreed to the *Frente Nacional* (National Front) deal, which included alternating the presidency every four years between 1958 and 1974, and ensuring parity in party representation in all government bodies.

The National Front blurred the ideological line dividing the two main parties and consolidated a highly clientelistic system of political exchange. There were relatively few differences in the socio-economic origins of supporters of both parties, which were ultimately seen as agents of different factions of economic elites ([Leal-Buitrago and Davila, 1990](#); [Dávila, 1992, 1999](#)). Indeed, the National Front openly excluded other political movements from national and local political processes. Among the excluded groups, peasants, workers and others ideologically aligned with the left stood out, and

some of their most important demands, in particular land reform, were attempted but always failed under an elite-friendly National Front (Safford and Palacios, 2002, Chapter 14).

Bipartisan dominance persisted after the National Front formally ended in 1974, and only collapsed in the late 1980s and early 1990s with the adoption of the 1986 electoral reforms and the enactment of the 1991 constitution.

The absence of political opportunities for outsiders, combined with the lack of state presence in the Colombian periphery and the survival of Liberal rural guerrillas from *La Violencia*, led to the formation of left-leaning guerrilla movements in the early 1960s (Bushnell, 1993), the most powerful of which was the Armed Revolutionary Forces of Colombia (*Fuerzas Armadas Revolucionarias de Colombia* – FARC), which is currently in the process of disarmament and reintegration following the signing of a peace agreement with the government.<sup>7</sup>

In the late 1970s, to finance their activities the FARC and other guerrilla movements began kidnapping and extorting wealthy individuals, particularly landowners. This precipitated the creation of paramilitary self-defense militias, which in many cases operated with at least the implicit complacency of the national army, local politicians, and the local elite (Dudley, 2004; Gutierrez-Sanin and Baron, 2005; Duncan, 2007; Gutierrez-Sanin, 2008; Acemoglu et al., 2013).

By the early 1980s the Colombian state's legitimacy was at stake: there were few political options for third parties, violence in rural areas, and repression of left-leaning supporters by the government of Julio Cesar Turbay, from 1978 to 1982 (Bushnell, 1993; Centro Nacional de Memoria Histórica, 2013). This situation motivated the government of Belisario Betancur (1982-1986) to negotiate with insurgents. As part of the peace talks, and to signal a credible opening of the country's democratic system, the electoral system was reformed to allow the direct election of local mayors by simple plurality rule (Maldonado, 2001). This reform sought precisely to give voice to excluded groups, especially the traditionally excluded left. It became effective with the first local elections in 1988. The 1991 constitution further consolidated the opening of the political system and increased resources and devolved responsibilities to local governments.<sup>8</sup>

The reforms allowed left-leaning groups that had been historically excluded – such as groups of peasants, union workers, and other political outsiders – to participate in local elections. As part of the peace negotiations with the government, the FARC created its own political party, the *Union Patriótica* (UP), thus combining “all forms of

---

<sup>7</sup>Other guerrillas include the still-active National Liberation Army (*Ejército de Liberación Nacional* – ELN), and the *Movimiento 19 de Abril* or M-19. The latter demobilized shortly before the 1991 constitution and participated as a political party in the Constitutional Assembly.

<sup>8</sup>The 1991 constitution allowed citizens to collect signatures to either run independently without the support of any party, or to create a new party. In addition, public financing (proportional to the number of votes) and access to television was granted to all political parties. These reforms facilitated the creation of third parties and made politics more competitive.

struggle” – ballots and guns. Initially the UP openly supported and received support from the FARC, and some FARC members participated in politics. This generated widespread criticism from different sectors of Colombian society and forced the UP to distance itself from the FARC, which reacted in turn by kidnapping several top UP politicians (Dudley, 2004). By the early 1990s most of the UP hardliners in favor of armed struggle had left the party and most of its remaining members openly criticized the FARC, but many outsiders conflated the FARC and the UP, which led to the assassination of UP supporters: two presidential candidates, eight congressmen, 13 deputies, 70 councilmen, 11 mayors, and thousands of militants were killed (Centro Nacional de Memoria Histórica, 2013, pg. 142).<sup>9</sup> Referring to violence against the UP, Leal-Buitrago and Davila (1990) note that “facing any political movement representing a challenge to the *status quo*, the long-standing state weakness induced informal and illegal mechanisms to defend the system” (p. 85), which resonates with our results and interpretation. These illegal and informal mechanisms represent *de facto* elite reactions in their most extreme form: violence against left-leaning parties that had recently begun to compete for local office.

The Colombian context we study is therefore characterized by three main features: (1) the declining importance of traditional parties, which had been largely stripped of their ideological differences and legitimacy with the signing of the National Front agreement, with a resulting heavy reliance on clientelism, (2) (left-leaning) political groups gaining access to the local political arena for the first time, and (3) the presence of both left- and right-wing violence in various parts of the country.

The distinction between these two types of violence is important and helps explain the focus and findings of our investigation. Left-wing guerrillas are clearly anti-establishment, question the legitimacy of Colombia’s democracy, and have not mingled systematically with institutional parties. Given Colombia’s history of political exclusion and the power-sharing pact between traditional parties, the entry of the excluded left, with its radically different policy preferences, makes it hard to bargain a policy compromise. This creates incentives for a *de facto* reaction by traditional insiders. Right-wing paramilitaries, instead, colluded with the establishment, especially the army and local land-owning elites. Moreover, in 1997 they joined forces under an umbrella organization called *Autodefensas Unidas de Colombia* (AUC) with clear political connections and goals. Its leaders signed a secret pact in 2001 in which politicians (including state governors and members of Congress) called for an explicit role for the AUC in electoral politics (Acemoglu et al., 2013). Their objectives were to strengthen the agrarian model of large landholdings and to use violence and intimidation to protect regional elites from social and political opposition (Centro Nacional de

---

<sup>9</sup>Steele (2011) studies the Urabá region in northwest Colombia and shows that residents of urban neighborhoods that voted for the UP in local elections were selectively targeted by paramilitary groups and thus more likely to flee after the elections than residents of similar neighborhoods where the UP was less successful electorally.

*Memoria Histórica*, 2013, pg. 170).<sup>10</sup> The right thus had a comparative advantage in exercising *de facto* power with institutional acquiescence in ways that the left did not. In contrast, the election of traditional politicians from non-leftist parties with similar policy preferences constitutes no threat to local elites, and thus violence perpetrated by insiders is unlikely. Finally, left-wing sympathizers do not systematically respond violently when non-left challengers are elected mayor because they do not enjoy such close links with the local political establishment and security forces. Indeed, as we document below, we find no comparable systematic increase in violence (from left-wing guerrillas or other groups) when the right wins local elections by a narrow margin.

## 1.3 Empirical strategy and data

### 1.3.1 Data

#### Violence

Our violence dataset was originally compiled by Restrepo, Vargas, and Spagat (2003), and was updated through 2014 by Universidad del Rosario. This dataset codes violent events recorded in the *Noche y Niebla* reports from the non-governmental organization (NGO) Centro de Investigación y Educación Popular (CINEP) of the Company of Jesus in Colombia, which provides a detailed description of the violent event, date, the municipality in which it occurred, the identity of the perpetrator, and the count of victims involved in the incident. *Noche y Niebla* sources include (Restrepo et al., 2003, p. 404): “1. Press articles from more than 20 daily newspapers of both national and regional coverage. 2. Reports gathered directly by members of human rights NGOs and other organizations on the ground such as local public ombudsmen and, particularly, the clergy.” Notably, since the Catholic Church is present in even the most remote areas of Colombia, we have extensive coverage of violent events across the entire country.<sup>11</sup> Violent events are coded for the period 1988 to 2014 as either an uncontested one-sided attack (e.g., shootings against the population, assaults on police stations, or an ambush on a military patrol) or a clash (in which two or more groups exchange fire).

This dataset allows us to identify the three main perpetrators of violence in the Colombian conflict: the government (armed forces), the paramilitaries, and the guerrillas. As explained in Section 1.2, we conjecture that paramilitaries are the main perpetrators of violence against left-wing politicians or their supporters. Therefore our

---

<sup>10</sup>Most of the AUC demobilized in 2005 and 2006, following peace talks that started in 2003 under President Alvaro Uribe. However, remnant paramilitary groups persist to date.

<sup>11</sup>Figure 1.6 in the Appendix shows two examples of events in our violence dataset. Both are paramilitary attacks in the municipality of Viotá, in Cundinamarca. One local councillor was “disappeared” in the first case, and in the second a thirteen year old faced the same fate, this time with the army’s acquiescence.

main variable of interest is the number of attacks perpetrated by paramilitary groups during the mayor’s term following a narrow victory or defeat by the left. In order to take into account the size of municipalities, we measure the number of attacks per 100,000 inhabitants. We also compute similar measures of violence perpetrated by the guerrillas and government to help rule out some alternative interpretations of our results.

## Electoral results and party classification

We use the electoral data compiled by Pachón and Sánchez (2014), which is gathered from the Colombian national electoral authority, the *Registraduría Nacional del Estado Civil*. Figure 1.2 describes the timing of local elections since their introduction and the availability of electoral data for our analysis. Local elections take place in October, and the term starts in January of the following year. For all elections between 1988 and 1994, there is no detailed information on the vote count of losers; only the total votes cast for the election winners are available. Thus, our analysis covers elections between 1997-2011.

Mayors who were elected in 1997 and 2000 (and who began their terms in 1998 and 2001, respectively) had three-year terms. However, starting in 2003, the terms were extended to four years, so the remaining election years of our sample are 2003, 2007, and 2011, with associated terms starting, respectively, in 2004, 2008, and 2012. Violence data, while starting early enough, are available only until 2014. Given the difference in term lengths across the sample, as well as the lack of violence data for 2015, for our main results we focus on the effect of left-wing victories on violence during the years available for the government term.<sup>12</sup>

A central part of our empirical exercise involves identifying and coding left-leaning parties (we also need to identify and code right-wing parties for key robustness exercises reported in Section 1.5). This is a challenging task, since there are 9,216 candidates who were either winners or runners-up in the 4,608 mayoral races during our period (we drop unopposed races from the analysis). We classified the ideology of 178 different parties, and of 212 independent candidates who did not run on behalf of any party.<sup>13</sup>

The coding of parties as left-wing, right-wing, or neither followed a three-step sequential procedure that is explained in greater detail in Appendix Section 1.11.1. Here we provide a brief summary. First, following Beck, Clarke, Groff, Keefer, and Walsh (2012), we check party names, mottos, and slogans for words that identify the party as clearly left-leaning or right-leaning (e.g., “communist”/“socialist” or “conservative”/“Christian,” respectively).<sup>14</sup> For example, the Communist Party of Colombia

---

<sup>12</sup>The results using average violence during the first three years produce virtually identical results, and are available upon request.

<sup>13</sup>There was a large increase in the number of parties after the enactment of the 1991 constitution; recent reforms have sought to create incentives for the maintenance of fewer (but stronger) parties (Rodríguez Raga and Botero, 2006).

<sup>14</sup>The Colombian Conservative Party is an exception for the reasons discussed in Section 1.2.



was classified as leftist using this criterion. Second, since only a handful of parties can be classified directly using this method, following [Budge, Bara, Volkens, and Klingemann \(2001\)](#) we also search the party statutes (when available) for policy stances that are clearly left- or right-leaning. In particular, we code a party as left-wing if the party statutes include at least three of the following five leftist policy positions: (1) pro-peasant, (2) advocates greater market regulation, (3) thinks that workers should be defended against exploitation, (4) advocates state-owned or communal property rights, and (5) anti-imperialist. In turn, we code a party as right-leaning if its statutes include at least three of the following five right-wing policy positions: (1) economic growth is emphasized over redistribution, (2) advocates free market, orthodox policies, and privatization, (3) believes that family and religion are the moral pillars of society, (4) appeals to patriotism and nationalism, and accepts the suspension of some freedoms in order to guarantee security, and (5) prioritizes law and order. Parties that do not include at least three of the policy stances from either list in their statutes are classified as neither left- nor right-wing.<sup>15</sup> Third, for parties for which official statutes are not available, we look at the government plan that candidates submit to the electoral authority before elections and, when available, search them for the same policy stances as in the second criterion.<sup>16</sup>

Not all parties analyzed are included in our estimation sample, as some of them compete in races with wide winning margins or compete in races without a left-wing or right-wing party as a winner or runner-up. In particular, our baseline estimation sample of races involving a left-wing candidate includes 51 parties of which 14 are left-wing, 3 are right-wing, and 34 are neither. Once we focus on the sample of races with a left-wing candidate and with a win margin within the optimal bandwidth of [Calonico et al. \(2014\)](#), we end up with a sample of 43 parties (13 left-wing, 2 right-wing and 28 neither). It is worth noting, however, that all the left-wing parties that either win or

---

<sup>15</sup>For independent candidates who do not run on behalf of a party, we first check if they were supported by a coalition of parties and assign the ideology of the coalition to them, provided that the ideology matches across all parties in the coalition. Second, if there is no supporting coalition or if the ideologies of the coalition parties do not match, we turn to the third step and search their government plan. See Appendix Section 1.11.1 for details.

<sup>16</sup>Overall, we could find information on the ideological stance of 112 (62%) of the 178 parties in the sample (and of 70 of the 212 candidates who ran independently). In the baseline analysis, parties/candidates that cannot be classified in steps 1 to 3 of the coding procedure are assumed to be neither left- nor right-wing, a reasonable assumption since i) Out of the 112 parties for which we could find information, a large majority (87, or 78% of those analyzed) are neither (14 are left-wing and 11 are right-wing) and ii) Parties in the extremes of the ideological spectrum typically have clearer signals (in their names and/or programs) of ideological stance and thus absence of explicit reference to their left/right stance more likely means they are neither. Moreover, the fraction of parties for which information on their ideological stance is available is larger in the baseline sample of races with at least one left-wing candidate (68% or 35 out of 51 parties), and is even larger for races within the optimal bandwidth of [Calonico, Cattaneo, and Titiunik \(2014\)](#) (70% or 30 out of 43 parties). However, we also verify that our results are robust to dropping parties classified as “neither” due to lack of information (and the associated races in which they compete) from the sample. We also explore the robustness of our results to including a fourth classification step where parties that are factions of, or splinter movements from, other parties (that in turn are classifiable in steps 1 to 3) are assigned the ideology of the parent party (see Table 1.11).

come second during our sample period do so in at least one close electoral race. This is important, because it implies that our analysis includes the entire set of left-wing parties that successfully contested mayoral elections in Colombia between 1997 and 2011.

### Additional variables

For some of our robustness and mechanism tests we use additional data sources. We use data collected by [Martinez \(2017a\)](#) on local government performance and the extent to which mayors and other local officials were involved in corruption. These will allow us to test the extent to which municipalities under left-wing parties were targeted because of their poorer (or better) governance and corruption. We also use data on the update of the municipal land registry, a policy under the control of the mayor that facilitates the increase of property taxes, a policy often favored by left-wing politicians. This will allow us to test the extent to which left-wing politicians pursue policies that threaten the interests of local elites. We also collected data on a broad range of predetermined municipal characteristics to assess the validity of our identification strategy.<sup>17</sup>

Finally, throughout our analysis we drop cities with a population greater than 300,000. Institutions and state presence are much stronger in large cities and thus guerrilla and paramilitary attacks are more rare.<sup>18</sup> In [Table 1.8](#) we present descriptive statistics for our main variables of interest.

### 1.3.2 RD Design

The electoral victory of a left-wing candidate is plausibly correlated with a wide range of municipal-level socio-economic characteristics. Thus, a naive comparison of violent attacks across municipalities with and without newly elected left-wing mayors may be confounded by the effect of other local characteristics. In order to address this problem, we use an RD approach based on close elections.

We exploit the fact that a mayor’s partisan affiliation changes discontinuously at the threshold between a left-wing party’s victory or loss.<sup>19</sup> Our empirical analysis is based on regressions of the form:<sup>20</sup>

$$y_{it} = \alpha + \beta_1 L_{it} + \beta_2 f(X_{it}) + \beta_3 L_{it} \times f(X_{it}) + \varepsilon_{it}. \quad (1.1)$$

---

<sup>17</sup>A detailed description of all the variables and their sources is available in [Appendix Section 1.11.3](#) and [Table 1.9](#), including those used for robustness, falsification tests, and testing the underlying mechanisms.

<sup>18</sup>Of the 12 Colombian cities of this size, only four held elections in which a left-wing party won or came second during our sample period. Only in Bogotá (in 2003 and 2011) and Bucaramanga (in 2011) was the winning margin within our optimal bandwidth. Our main results remain unchanged when we include these two cities (which had a total of three races that fit the criteria).

<sup>19</sup>Mayors cannot run for re-election in Colombia, thus our discussion focuses on the potential re-election of (left-wing) parties.

<sup>20</sup>See [Dell \(2015\)](#) for a similar application of RD based on close elections in Mexico.



where  $y_{it}$  is the outcome variable and  $L_{it}$  is a dummy for whether a left-wing party won the race.  $f(X_{it})$  is a polynomial in our forcing variable, the left wing party margin of victory,<sup>21</sup> and  $\varepsilon_{it}$  is an idiosyncratic error term. Throughout our empirical analysis we focus on the sample of races in which the left-wing candidate either wins or comes second.

Estimation of our coefficient of interest,  $\beta_1$ , can be done both parametrically and non-parametrically. The choice of bandwidth involves a trade-off between efficiency and bias. To deal with this issue, in our baseline estimates we use the optimal bandwidth, bias correction, and robust standard errors proposed by Calonico et al. (2014). These estimates are a refinement of the non-parametric local polynomial estimators usually employed. For both our parametric and non-parametric estimates we verify the robustness to the choice of bandwidth and order of the polynomial (Lee and Lemieux, 2010a). Following Gelman and Imbens (2014a), we do so only for linear and quadratic polynomials.

Our empirical approach relies on the underlying assumption that other covariates, besides our treatment variable, vary smoothly at the threshold. Thus, any discontinuous increase in violence is only attributable to the partisan affiliation of the electoral mayor. To test this, in Appendix Section 1.12.1 (Table 1.10) we report estimates of  $\beta_1$  based on regression (1.1) for different municipal characteristics measured at baseline (i.e. measured prior to the close race). Reassuringly, we find no statistically significant differences at the threshold between treatment and control municipalities for most of these variables. The only exception is the number of years since the land registry was last updated, which is higher by about 4 years and significant at the 95% level in municipalities in which the left won, an issue we return to below.

We also rule out manipulation of electoral results, which would violate our identification assumption. If the results are manipulated, then any subsequent violence could be triggered by suspicions of fraud, rather than the political stance of the winner. Testing for sorting around the threshold is a useful way of examining potential manipulation (Lee and Lemieux, 2010a). We thus follow McCrary (2008a) and check the distribution of our forcing variable around the winning threshold. A discontinuous jump in either direction would indicate that the left is systematically more or less likely to win close races. Figure 1.3 shows the results of this test, and reports the statistic of the null hypothesis of no jump in the distribution. Reassuringly, there is no jump in the density at the threshold.<sup>22</sup>

---

<sup>21</sup> $X_{it}$  is the vote share of the left-leaning candidate minus the vote share of the non-left candidate. The vote share is computed as a fraction of the total number of votes obtained by the top two candidates in the race. Thus, our treatment variable  $L_{it} = 1$  if  $X_{it} > 0$  and  $L_{it} = 0$  if  $X_{it} < 0$ .

<sup>22</sup>The estimate is 0.09 with a standard error of 0.24.

## 1.4 Main results and robustness

### 1.4.1 Baseline results

In Table 1.1 we report our main result: electing a left-wing mayor leads to a substantial and statistically significant increase in subsequent paramilitary violence. Panel A reports the non-parametric estimates following Calonico et al. (2014) and Panel B the parametric estimates of the treatment effect.<sup>23</sup> Columns 1 and 5 include no controls; Columns 2 and 6 control for time-invariant geographic characteristics of the municipalities (such as altitude, average historical rainfall, distance to Bogotá, and region-specific dummies); Columns 3 and 7 include pre-determined socio-economic and political controls (such as the vote share of left- and right-wing presidential candidates in 1994, rurality, literacy rates, presence of coca crops, and historic violence); and Columns 4 and 8 include all the controls simultaneously. While in principle the inclusion of these covariates should not have a major impact on the coefficients, doing so may help improve the precision of the estimates (Lee and Lemieux, 2010a).

The non-parametric estimates are positive and statistically significant across all specifications. The parametric estimates are smaller and not significant under a linear polynomial specification, but are statistically significant (and of similar magnitude) to the non-parametric estimates under the quadratic polynomial. However, the parametric estimates should be interpreted cautiously since they do not include the bias correction and the robust standard errors suggested by Calonico et al. (2014).<sup>24</sup>

Focusing on the non-parametric estimates, the election of a left-wing mayor leads to an additional 4.4 to 6.8 attacks per 100,000 inhabitants per year during his or her term. This increase is quantitatively important. It is equivalent to 2.2 to 3.4 times the sample mean and 63–97% of a standard deviation. Despite our very small sample by the standards of typical RD analyses, the coefficients are statistically significant at standard confidence levels. Moreover, our results do not depend on our choice of bandwidth, and are robust to considering less-competitive elections. Panel A of Figure 1.5 shows the estimated coefficient and the 95% confidence interval using a wide range of bandwidths. The effect of a left-wing electoral victory on paramilitary attacks remains

---

<sup>23</sup>In Panel A, we implement Calonico et al. (2014)'s bias correction and robust standard errors, as well as their optimal bandwidths for local polynomials of orders one (Columns 1 to 4) and two (Columns 5 to 8). Optimal bandwidths range from 4.8% to 11.9% depending on the controls included. Estimates in Panel B fit linear and quadratic polynomials (in Columns 1–4 and 5–8, respectively) and restrict the sample to that defined by the optimal bandwidth computed for the non-parametric case without including controls.

<sup>24</sup>As can be seen in Table 1.8, the distribution of our different violence measures is right-skewed. However, our coefficients remain stable and statistically significant if we drop municipalities with violence values in the top 3% of the regression sample (which given the distribution, implies dropping the top 20% of observations with positive paramilitary attack values and is thus a demanding test). Our results are also qualitatively similar if we measure our outcome variable as a dummy for whether at least one attack took place, though the estimates are noisier. Point estimates in this case suggest that a left-wing victory increases the probability of a paramilitary attack by 25 percentage points.

positive and statistically significant for bandwidths as small as 0.07 and as large as 0.2.<sup>25</sup> For bandwidths of 0.05 or smaller, the point estimates become small and noisy, and the sample sizes become prohibitively small. For bandwidths larger than 0.2 the coefficients remain positive and stabilize at around 40% of a standard deviation, even if no longer statistically significant at conventional levels.<sup>26</sup>

Figure 1.4 illustrates these (non-parametric) findings. Observations within Calonico et al. (2014)’s bandwidths for polynomials of orders one and two are displayed in the left- and right-hand side panels, respectively. Each point represents the average of our paramilitary attacks variable within bins of equal size, selected so that there are 10 bins at each side of the cutoff. Linear and quadratic fits (based on the raw, unbinned data with no controls) are depicted together with the bin averages. A jump in the number of attacks across the threshold is evident in both figures.

### 1.4.2 Robustness to party coding

Even after following a very strict three-step procedure to code the ideology of political parties, some parties were left unclassified. As described above, these parties were coded as neither left- nor right-wing in our baseline analysis. In Appendix Section 1.12.2 we show that our estimates remain similar if we drop these unclassified parties (Panel A of Table 1.11), or if we code the ideology for some of them as the same as their parent party (Panels B and C of Table 1.11).

### 1.4.3 Ruling Out Pre-Existing Trends

An important robustness check is to show that a left-wing victory is not correlated with pre-election trends in paramilitary (or other forms of) violence. We study this in Panels D to I of Figure 1.5, where we plot RD estimates (for several bandwidths) of the effect of a left-wing electoral victory on pre-election violence. Panels D, E and F focus on violence during the mayor’s term prior to the election. In turn, Panels G, H and I focus on violence in the *year* prior to the election, since this may be when armed groups are likely to try use violence to shape electoral outcomes. All the point estimates are statistically insignificant across the six panels, for both small and relatively larger bandwidths. The only exception is paramilitary attacks in Panel D when focusing on a very short range of bandwidths just above 0.1. Even in this case, however, the point estimates are just over half of our benchmark effect of close

---

<sup>25</sup>In order to compare the size of the effects across multiple outcomes, Figure 1.5 reports the effects on standardized outcomes.

<sup>26</sup>As a validation test, we re-estimate the treatment effect at different “placebo” cutoffs other than the threshold at which treatment occurs (in this case 0). This practice is especially useful when there are other cutoffs of the forcing variable that may capture changes that are erroneously attributed to the treatment of interest. While this is unlikely in close election settings, for completeness we estimate the effect of left-wing electoral victories on violence for different cutoffs in the range of -0.14 to 0.14. Estimates at alternative cutoffs (not reported) are unstable, imprecisely measured, and not statistically different from zero.

left-wing victories on subsequent paramilitary attacks, and just marginally significant. For other bandwidths, and for the case of paramilitary violence in the year prior to the election (Panel G), the point estimates are very close to zero. This is also the case for preceding guerrilla attacks.<sup>27</sup> Overall, Figure 1.5 provides compelling evidence that previous violent dynamics are unlikely to explain our main findings.

#### 1.4.4 Were Left-Wing Victories Anticipated?

An intriguing question raised by our findings is why do elites not intervene strategically *before* races expected to be close in order to prevent a left-wing victory. The McCrary test in Figure 1.3 suggests that elites did not attempt or did not succeed in manipulating the outcome of close races. Similarly, if voters anticipate the consequences of electing a left-wing party they may change their electoral behavior and we would not observe close contests between the left and other parties. In the absence of any local history of left-wing victories in local elections (which is very likely, given the exclusion of the left from politics throughout most of the 20<sup>th</sup> century (see Section 1.2)), it seems plausible that elites and voters failed to anticipate, respectively, the outcome (or competitiveness) of the election and the consequences of a left-wing victory.<sup>28</sup> As noted by Benoit and Dubra (2013) individuals often fail to anticipate events that have never occurred.

On the other hand, for elections in municipalities in which the left has previously won, both voters and elites should be able to anticipate the possibility of this electoral outcome and in this case we may be concerned that the occurrence of another close race between the left and other non-left parties is correlated with implemented policies or the reaction by the elites in the past. For example, we may only observe repeated close races between the left and other parties in places in which the left was not a threat to the elites, or places in which elites failed to deter the left from seeking incumbency again. As a robustness check, we address this by dropping recurring municipalities from the sample (i.e., those that show up more than once in our sample because they have more than one close election with left-wing participation). Reassuringly, this yields results that are similar and if anything larger in magnitude, in both close and non-close races (see Appendix Figure 1.7, Panel A).<sup>29</sup>

---

<sup>27</sup>This is not to say that the guerrillas do not increase their attacks during election years (in fact, they historically have), but our findings suggest that this is uncorrelated with the outcome of close elections.

<sup>28</sup>Also, polling prior to elections in Colombia is very rare outside large cities, and often there are many candidates competing. Thus, it is usually very hard to predict who will win local elections.

<sup>29</sup>At the optimal bandwidth of Calonico et. al (2014) the non-parametric (parametric) estimates are 5.8 (5.1) and 6.1 (5.8) additional paramilitary attacks, on average, for local polynomials of orders one and two, respectively. Moreover, Figure 1.7 (Panels B, C and D) confirms that for this alternative sample, there are no statistically significant differences in pre-electoral violence (as measured by paramilitary, guerrilla, or government attacks).

## 1.5 Alternative interpretations

So far we have focused on single-sided attacks by the paramilitary, arguing that such attacks best exemplify the type of *de facto* response that traditional elites might exert when facing increased *de jure* contestation by left-wing outsiders with different political preferences. However, there are other potential interpretations of our results.

We start by examining the impact of a narrow left-wing electoral victory on other types of violence. It is important to rule out, for instance, the possibility that paramilitary attacks might have risen in response to either increasing or decreasing guerrilla attacks. If the armed and democratic left are strategic complements (substitutes), then we would expect a spike (decrease) in guerrilla violence following a left wing victory. In turn, because of their counterinsurgent nature, paramilitaries are likely to react to these dynamics with violence, either by contesting an empowered armed left or by filling the power vacuum left by a guerrilla retreat. Likewise, and through similarly complex mechanisms related to the complementarity/substitutability of violence across armed groups, the surge in paramilitary violence may be partly driven by a change in the incidence of attacks by government forces following a left-wing victory. Finally, another alternative is that left-wing mayors are simply unable to curb (any type of) violence, perhaps because they do not prioritize security and law and order (see Appendix Section 1.11.1).

We reject these hypotheses by showing that neither guerrilla nor government attacks change differentially in municipalities in which a left-wing candidate narrowly wins versus comes second. This is reported in Columns 1 and 2 of Panel A of Table 1.2: not only are the point estimates not statistically significant, but the magnitude of the coefficients for both guerrilla and government attacks is rather small (0.7 and 1.6 additional attacks per 100,000 inhabitants, respectively, which is much smaller than our baseline effect for paramilitary violence). For completeness, we also look at two-sided armed confrontations (clashes) between different groups, and confirm that no other type of violence increases as a result of a left-wing candidate being elected mayor. This is reported in Columns 3 to 5 of Panel A, Table 1.2 for close races (as defined by the optimal bandwidth of Calonico et al. (2014)).<sup>30</sup>

Another hypothesis is that left-wing parties and politicians are targeted not because they advocate policies that are contrary to the interests of traditional elites, but because their governments are corrupt or perceived as inept. The contrary is also a plausible: the left may be more honest and competent than previous local administrations, and hence may be targeted for changing the way in which municipalities are traditionally run. While measuring corruption is challenging, in Panel B of Table 1.2 we test whether

---

<sup>30</sup>While the coefficient for paramilitary violence is over three times the mean and almost a full standard deviation, the estimated effect on guerrilla violence is about a fifth of the mean and less than a tenth of a standard deviation. That said, the estimates for guerrilla attacks and clashes between the government and the guerrillas are larger and noisy and should be interpreted cautiously.

in places where the left won, the mayor (Columns 1 to 3) or other top municipal officials at the rank of secretary (Columns 4 to 6) are more likely to be investigated for misconduct by *Procuraduría General de la Nación*, the government Watchdog Agency (Columns 1 and 4), found guilty (Columns 2 and 5), or removed from their post (Columns 3 and 6). We find no evidence that left-wing mayors or their secretaries are more corrupt than municipal executive officials from other parties. The point estimates are statistically insignificant (especially in the case of mayors) and small in magnitude compared to the average in the sample (Table 1.8). Furthermore, in Panel C we look at the three indices of government performance described in Section 1.3.1 (Columns 1 to 3), as well as municipal capital and current fiscal expenditure, to check whether left-wing mayors spend more than non-left-wing incumbents (Columns 4 and 5). We find no evidence that left-wing mayors perform worse than those from other parties.<sup>31</sup> In short, the evidence does not corroborate the hypothesis that the violent reaction we observe is driven by higher (or lower) corruption levels or the poorer (better) governance of left-wing mayors.

Another potential interpretation of our results is that, due to the weak legitimacy of the democratic system in Colombia, a violent reaction would have taken place after a narrow victory of other parties as well. For example, increased violence may follow the election of a candidate from any party on the extremes of the ideological spectrum. The most natural comparison is assessing the impact of narrow electoral victories of right-wing parties on levels of violence. Panels A and B of Table 1.3 report the estimated impact on different types of violence of narrow victories by right-wing versus non-right-wing parties in mayoral elections in Colombia during our sample period. There is no significant effect on either total attacks (aggregated across all groups), or on attacks perpetrated by the paramilitary or guerrilla groups. The effect on attacks carried out by government forces is *negative* and significant, and the point estimate suggests that, after narrow victories of right-wing parties, government attacks drop by 0.5 per 100,000 inhabitants during the mayor’s term in office. However, this is a comparably small effect, equivalent to less than 40% of a standard deviation, and is significant only at the optimal bandwidth or relatively larger (greater than about 0.1) ones.<sup>32</sup> Instead, the null effects for other types of violence are robust to varying the estimation bandwidth across a large range of values (Figure 1.8, Panels A, B and C).<sup>33</sup> In addition, the magnitude of the coefficients is small compared to our baseline estimates for paramilitary violence after left-wing parties win in a close election. The point estimate for paramilitary attacks in Table 1.3 is 0.18, which is equivalent to 30%

---

<sup>31</sup>These estimates, especially those reported in Columns 2 and 3, are based on a smaller subset of years due to data availability.

<sup>32</sup>Panel A of Table 1.3 reports non-parametric estimates and Panel B reports parametric estimates. All estimates are based on local linear polynomials within the optimal bandwidth and include bias correction and robust standard errors. The results for the second-order polynomials are similar in magnitude and also not significant.

<sup>33</sup>Moreover, Appendix Figure 1.9 shows that there is no significant evidence of manipulation of the running variable in close elections in which right-wing parties are either the winners or the runners-up.



of the sample mean and 5% of a standard deviation, as reported for this sample in Table 1.12. In summary, and in line with our expectations given the nature of Colombia’s political history, the right is not a political outsider, and thus its victories are less threatening to existing interest groups with the capacity to react via *de facto* means.

Another possibility is that our estimates simply reflect the effect of the electoral victories of *new* parties. As discussed in Section 1.2, the 1991 constitution facilitated the creation of new political movements across the entire ideological spectrum, many which (leftist or not) have been electorally successful in some places. Thus, the violent response of paramilitaries may reflect a more general reaction to the threat of new political actors to traditional elites’ grip on power, and not necessarily a reaction to left-wing ideology. To address this possibility we first follow Galindo-Silva (2015) and code as a *new party* any party in a given municipality that (1) is not one of the two traditional parties (Conservative and Liberal) and (2) has never won an election in that municipality. We then estimate the effect of a narrow electoral victory of a new party on paramilitary attacks. Importantly, we drop from our estimation sample all left-wing parties and thus isolate the effect of new parties that were not associated with a left-wing ideology. The effect of narrowly electing a mayor from a non-left new party on paramilitary attacks is reported in Table 1.13. The estimates are very small (about a tenth or less of the baseline effects of Table 1.1) and statistically insignificant (with the exception of the parametric estimates fitting a linear polynomial). This implies that our results *are* related to the ideological stance of left-wing parties, and are not explained by the fact that left-wing parties were simply new to the local political arena. In the Colombian context, only left-wing parties seem to have been particularly threatening to the interests of local elites.

One remaining question is whether our estimates reflect a widespread phenomenon associated with all left-wing parties, or are simply driven by the persecution of the UP, the party formerly associated with the FARC (see Section 1.2).<sup>34</sup> The persecution of the UP is partly the phenomenon that we are documenting in this paper, but we want to show that *de facto* response of elites to the *de jure* accumulation of power is a more widespread and systematic phenomenon that holds for any left-wing party, and not only the party with past connections to communist guerrillas. To address this possibility, we revisit the baseline empirical exercise of Columns 1 and 5 of Panel A of Table 1.1 but add as controls a dummy for whether the left-wing party in the close electoral race is the UP and the interaction of this dummy with an indicator of whether the left-wing party won. The results are reported in Appendix Table 1.14. The point estimates become somewhat smaller but remain statistically significant. This suggests that our baseline estimates are not simply driven by the UP, and that paramilitary violence also followed

---

<sup>34</sup>Even as recently as this year, a UP leader who returned to Colombia from exile in 2015 was the victim of a violent attack. See “Defensoría pide esclarecer con urgencia ataque contra líder de Unión Patriótica,” *El Espectador*, May 7, 2016. Available at <http://www.elespectador.com/noticias/nacional/bolivar/defensoria-pide-esclarecer-urgencia-ataque-contra-lider-articulo-631172> (last accessed May 16, 2016).

the election of other left-wing parties. The interaction term between the UP and the victory dummy is positive, as expected, suggesting that violence in places where the UP narrowly won was much larger. However, the coefficient is not statistically significant, probably due to power limitations (the UP contested eight elections during our sample period, and won half of them).

## 1.6 Mechanisms

In this section we present additional evidence that supports our preferred interpretation. We start by testing what happens to our overall effect after 2006, when the paramilitaries (which by then had joined forces under the AUC umbrella organization) demobilized after signing a peace agreement with the Uribe government.<sup>35</sup> Table 1.4 interacts the dummy of a left-wing victory with a time indicator that captures all local elections that took place after 2006 (i.e., in 2007 and 2011). The estimated interaction coefficient is negative and statistically significant. Interestingly, we cannot reject the null hypothesis that the effect of a left-wing victory in elections after 2006 is equal to zero, which suggests that the increase in violence following the narrow election of left-wing candidates has noticeably decreased after the demobilization of the AUC.<sup>36</sup> Moreover, this suggests that our baseline estimates in Table 1.1 for the full 1997-2014 period are a lower bound, since they incorporate election years for which the effect on paramilitary violence is very limited due to the demobilization of the AUC.

The timing of the observed increase in paramilitary attacks following left-wing victories also has implications for the validity of our interpretation. We argue that in order to avoid the consolidation of political power in the hands of left-wing parties, paramilitaries are likely to concentrate their violent reaction as the subsequent elections approach, thus preventing the left from winning again.<sup>37</sup> Known paramilitary tactics include “terrorizing voters to vote in particular ways, ... to stay away from the polls so they could stuff ballots, voting instead of citizens by confiscating their identify cards, terrorizing politicians so that they would not run against their preferred candidates, and manipulating subsequent vote totals electronically” (Acemoglu et al., 2013). Table 1.5 presents estimates of the effect of electing left-wing candidates as mayor on paramilitary attacks during each year of his or her term in office.<sup>38</sup> The results indicate that the increase in paramilitary violence is driven by increased attacks in the year of the subsequent election. The coefficient for the first year is positive (4.8), while the coefficient for the second year is negative (though relatively small in magnitude,

---

<sup>35</sup>While splinter paramilitary groups persisted after this time, they were mainly guided by economic rather than political motivations.

<sup>36</sup>We must nonetheless interpret this result cautiously, since a simple time dummy may also capture other changes that took place after 2006 in Colombia in addition to the demobilization of paramilitaries. For example, it may indicate an overall improvement in institutions and state capacity in the last decade, or changes in the electoral law that may have shifted the incentives of political parties.

<sup>37</sup>This incumbency (dis)advantage is discussed further in Section 1.7.

<sup>38</sup>Recall from Section 1.3.1 that mayoral terms are either 3 or 4 years.



-1.2). However, the coefficients for the third year (10.9) and the year of the subsequent election (18.5) are not only positive but also substantially larger than the baseline estimates. These estimates are noisy, and only the one for the third year is significant at conventional levels, which is a consequence of our small sample. But the point estimates suggest a pattern in which violence tends to spike right after the left-wing candidate is elected and, more significantly, approaching the year of the subsequent election. The next section examines whether this paramilitary strategy of increasing violence in the last year of a mayor’s term is effective.

## 1.7 The consequences of violent paramilitary responses

We now look at the performance of left-wing parties in the subsequent election – i.e., the one after the close race in which they narrowly won or lost – and establish whether they suffer from an incumbency disadvantage, at least relative to other political parties. There are several challenges in estimating incumbency advantage or disadvantage: incumbency status is usually correlated with other party characteristics that explain both why the party was successful in getting elected in the first place and its performance in the next election. Moreover, as discussed in Section 1.3.1, the large number of local parties in Colombia, many of which are short-lived and disorganized, makes it harder to identify the electoral effects of incumbency.

To assess subsequent electoral performance we follow [Klašnja and Titiunik \(2017\)](#), who use a close-elections-based RD approach very similar to the one we use in this paper. For each electoral period  $t$  they focus on incumbent parties (those elected in period  $t - 1$ ) and estimate the effect of the (arguably random) arrival in office on future electoral success. Our main measure of future success is a dummy variable for whether incumbent parties run in and win the next election (in period  $t + 1$ ). For close races, a dummy indicating whether the period  $t$  incumbent wins in  $t + 1$  compares the subsequent electoral success of the incumbent party in municipalities in which it was a close winner versus a close loser.

We report the estimates from this exercise in Table 1.6. Columns 1 to 4 of Panel A estimate the average degree of incumbency advantage in Colombia.<sup>39</sup> The estimates for the election winner dummy are negative and very similar to those reported by [Klašnja and Titiunik \(2017\)](#) for Colombia.<sup>40</sup> This suggests that political parties in Colombian local elections experience an incumbency disadvantage. However, in Columns 5 and 6

<sup>39</sup>Columns 1 and 2 follow the non-parametric approach using polynomials of orders 1 and 2, respectively, while in Columns 3 and 4 we report estimates from a parametric approach.

<sup>40</sup>For this analysis we use a somewhat larger sample than the one used in the rest of the empirical exercises, because the 1994 electoral results (which, as explained in Section 1.3.1, are not available for election losers) allow us to identify incumbent parties that participated in the 1997 elections, the first of our sample period. While we want our sample to be as large as possible, the incumbency disadvantage estimates are not sensitive to this change.

we extend the exercise of [Klašnja and Titiunik \(2017\)](#) and interact the winner dummy with an indicator for whether the party is left-wing. The interaction term is negative, statistically significant, and large. The point estimate suggests that left-wing parties in Colombian local elections experience an incumbency disadvantage that is five to six times larger than that of other (non-left-wing) parties.<sup>41</sup> We argue that this may be (at least partly) explained by the attacks targeted at left-wing incumbent parties right before their potential re-election.<sup>42</sup>

The exercise presented in Panel A may hide an important consequence of the attacks aimed at preventing left-wing parties from remaining in power. After being subjected to violent intimidation, incumbent parties may simply decide not to run in the next election. We explore this alternative definition of incumbency disadvantage in Panel B of [Table 1.6](#). The dependent variable is no longer whether the incumbent party that competed in the election in period  $t$  runs and wins in  $t + 1$ , but simply whether the incumbent party runs at all. In contrast to the results presented in Panel A, we find no statistically significant average effects for non-left parties who win the election at  $t$ . However, resonating with the results of Panel A, we find that the interaction term of the winning party with an indicator for left-wing parties is negative and significant (and larger in absolute terms than Panel A’s interaction coefficients). This implies that left-wing incumbent parties are less likely than non-left-wing incumbents to put forward a candidate in the next election.<sup>43</sup>

Another objective of local elites’ *de facto* responses to the election of left-wing mayors is to prevent these outsiders from implementing elite-threatening policies. [Table 1.5](#) hints that this is likely the case, as the attacks are concentrated during the first and final years of the mayor’s term in office. While the higher intensity of attacks at the end of the period is intended to shape the results of the subsequent election, the increase in violence at the beginning of the term is likely designed to intimidate the incumbent into maintaining the status quo in terms of policies. We present anecdotal evidence that this is likely the case in [Section 1.8](#). Here we focus on the policy that is the most threatening

---

<sup>41</sup> To test whether paramilitary violence following close left-wing municipal victories affects voters’ support of left-wing parties in subsequent *national* elections, we estimate the effect of a narrow left-wing victory in mayoral elections on the municipal vote share of left-wing parties in the next presidential and congressional (Senate and House) elections. The results (not shown) suggest that such violence does not affect support for the left in national elections.

<sup>42</sup> Admittedly, this conclusion is based on very few observations. There are just four instances in which left-wing incumbent parties (in  $t - 1$ ) won the election in  $t$  and contested a new mayoral election in  $t + 1$ . Of these, they lost three and won one. Since even fewer right-wing incumbent parties (unsuccessfully) contest new elections, for these instances the interaction term is perfectly collinear with the “right-wing party” dummy, which makes it impossible to replicate [Table 1.6](#) for the right.

<sup>43</sup> While the evidence in [Table 1.6](#) is obtained using [Klašnja and Titiunik’s \(forthcoming\)](#) approach to studying incumbency advantage *conditional on being an incumbent* (that is, on having been elected to office in  $t - 1$ ), an alternative approach is to estimate the success in period  $t + 1$  of all parties that won elections in  $t$ , regardless of their incumbency status. We estimate this alternative specification and report the results in [Table 1.15](#). In contrast to the results from [Table 1.6](#), when departing from the approach of [Klašnja and Titiunik \(2017\)](#), we find no incumbency disadvantage for the left. Our argument that right-wing paramilitary violence following left-wing electoral victories generates an incumbency disadvantage for the left should thus be interpreted with caution.

to local elites, most of whom are landowners: land registry updates. Municipal mayors have the constitutional authority to update local land registries in order to keep the value of land up to date for the purpose of calculating property and land taxes. This is the most important source of revenue for most Colombian municipalities, and one of the few taxes collected at the local level (Vargas and Villaveces, 2016).<sup>44</sup>

We gathered data on land registry updates and estimated the effect of a narrow left-wing victory on the probability that the registry will be updated at least once during the new mayor's term. The results are reported in Table 1.7. Columns 1 and 2 focus on the non-parametric estimates and Columns 3 to 6 on the parametric ones. Odd (even) columns fit a linear (quadratic) local polynomial. The results are not significant in any specification, which suggests that, even if left-wing candidates are in principle much more likely to adopt redistributive policies, they are unable to do so while in office. This is not surprising since in equilibrium the violent intimidation we have documented succeeds in preventing the implementation of these policies. But it is the threat of more redistributive policies (off the equilibrium path) what motivates political violence in the first place. Recall from Table 1.10 that municipalities in which a left-wing party narrowly won present a larger lag since the last registry update. Columns 5 and 6 further control for this covariate, but we still find no effect of a left-wing electoral victory on the probability of updating the land registry.

In addition to the regression evidence shown so far, a look at some qualitative studies can help complement our regressions by further understanding the underlying causal mechanisms (Franzese, 2007; Mahoney and Villegas, 2007). The next section discusses some revealing examples about the nature of the paramilitary attacks used in our analysis.

## 1.8 Evidence from case studies

Our interpretation of the econometric results is in line with abundant anecdotal evidence on the nature of paramilitary violence. The *Centro Nacional de Memoria Histórica* (2013, pg. 50), an autonomous group commissioned by the government to compile the history of victims of violence in Colombia, notes that from 1988 to 1992 following the introduction of local elections, “big massacres were true expeditions to punish social mobilization and reject the political success of the left, in particular the *Unión Patriótica* and the *Frente Popular*.” This source cites some of the most emblematic cases of massacres of left-wing militants and the general population in areas where the left scored important electoral victories. Perhaps the best-known massacre took place in Segovia, in the department of Antioquia, on November 11, 1988 (*Centro Nacional de Memoria Histórica*, 2014). This attack killed 46 people in retaliation for

---

<sup>44</sup>The others are sales taxes, fuel taxes, and temporal taxes on specific activities. The non-local municipal sources of revenue are transfers from the central government and royalties obtained from the exploitation of natural resources.

the election of a UP mayor. Before the mayor's election, leaflets were distributed with the following message:

“We back the big *caudillo* in this region, César Pérez García (...) We will not accept Communist mayors or municipal councils made up of idiotic peasants or vulgar workers like those who make up the *Unión Patriótica*. They don't have the intelligence to handle these positions and manage these municipalities that have always been ours. Now we will get them back NO MATTER WHAT IT COSTS! ... You wait ... We will hit you with a mortal blow” (Dudley, 2004, pg. 121, upper case in the original).

In 2000, the UP candidate, Adelia Benavides, was narrowly elected mayor in the Liberal Party stronghold of Viotá. In 2003, in the last year of Benavides' term, Viotá experienced almost 20 paramilitary attacks. While paramilitary leaders justified this violence as a counter-insurgency strategy,<sup>45</sup> the attacks were likely triggered by the new mayor's aggressive property tax plans, which threatened to substantially increase the tax burden of local landowners.<sup>46</sup> Viotá has not elected a leftist mayor again since experiencing these unprecedented levels of violence against civilians. In 2003 the left placed third with only 11% of the votes, and in 2011 it received 3.5% of the votes. In 2007 there was no leftist candidate in the mayoral race.

In another example, Carlos Zambrano, the leftist party Polo Democrático's candidate, was narrowly elected mayor of the municipality of Baranoa in 2003. Zambrano planned to increase taxes on the relatively wealthy in order to subsidize the utility bills of the poor, which made him unpopular with the local elite. He also forced local utility providers to cut their tariffs.<sup>47</sup> In 2004 paramilitary groups started killing local Polo Democrático leaders who worked closely with Zambrano in this initiative.<sup>48</sup> One of the victims was Elías Durán, a board member of the Civic Committee for the Defense of Utilities, created by Zambrano.<sup>49</sup> Another victim was a community leader who led a civic initiative in 2003 for citizens to stop paying their water bill because of the high rates.<sup>50</sup> In 2005 Zambrano was forced by paramilitary leader “Jorge 40” to flee the

---

<sup>45</sup>Paramilitary leader Martín Llanos once argued that the Viotá campaign was launched to “help the displaced landowners of the guerrillas return to their lands.” However, our dataset indicates that post-2000 paramilitary violence in Viotá was entirely targeted against civilians; there were no clashes with guerrillas.

<sup>46</sup>By 2003, Benavides had plans to increase property tax revenues by up to 70% after her Liberal predecessor had allowed property tax revenues to drop to less than 30% of total tax revenues by the end of the term in 2000.

<sup>47</sup>“Baranoa busca acuerdo con consecionario,” *El Heraldo*, November 1, 2010. Available at <http://www.elheraldo.co/local/baranoa-busca-acuerdo-con-consecionario-15703> (last accessed March 2, 2016).

<sup>48</sup>“Amenazas a 63 Alcaldes,” *El Tiempo*, June 10, 2004. Available at <http://www.eltiempo.com/archivo/documento/MAM-1533589> (last accessed March 2, 2016).

<sup>49</sup><http://www.nocheyniebla.org/files/u1/29/pdf/13Mayo2004.pdf> (last accessed November 21, 2016).

<sup>50</sup><http://www.nocheyniebla.org/files/u1/31/pdf/05casos31.pdf>, (last accessed November 21, 2016).

municipality.<sup>51</sup>

In 2000 Oscar Quintero, representing the indigenous leftist political movement *Autoridades Indígenas de Colombia*, was narrowly elected mayor in the municipality of Corinto. Starting in 2001, paramilitary groups killed local indigenous political leaders who they labeled “guerrilla supporters.” In two roadblocks in 2001 and 2002, paramilitaries killed a leader of the “Indigenous Civic Guard” and three members of Corinto’s *Cabildo* (the semi-autonomous indigenous government).<sup>52</sup>

On November 27, 2000, shortly before the end of his tenure as mayor of Unguía, Rigoberto Castro was killed by paramilitary forces. Castro had won the 1997 election by a small margin. Castro’s friends reported that his request to the local police chief for additional protection after receiving threats by armed men was ignored. In 2015 the State Council found that the National Police was at fault for not protecting Castro, and awarded his family US\$ 400,000.<sup>53</sup>

These are just a few examples of a much wider and deliberate campaign by paramilitary groups to target left-wing politicians. In some cases the available information makes it clear that, once in office, the victims intended to adopt policies that hampered the interests of powerful local elites, which may have triggered the violence. In order to study how systematic this pattern was, we reviewed the descriptions in our conflict dataset (see Section 1.3.1) of *every single* paramilitary attack that occurred during the term of a left-wing mayor elected by a narrow margin – or in the years following the narrow *defeat* of a left-wing candidate. Our dataset is comprised of reports in national and local newspapers and other media sources; we particularly focused on information about whether the victim was involved in local politics as well as his or her political affiliation (see Appendix Section 1.11.2 for details on the coding protocol).

Two patterns emerge from this exercise, which are consistent with our hypothesis. First, based on the reports we are able to establish that 3.5% of the victims of paramilitary attacks in municipalities in which a left-wing mayor was narrowly elected were left-wing activists, compared with 0.8% where the left barely lost the election.<sup>54</sup> That is, the incidence of left-wing victimization in paramilitary attacks is almost four and half times higher in places where the left won by a small margin than in places where it lost. Second, 86% of the leftist activists killed by paramilitary groups in municipalities in which the left won were actually involved in local politics (some were the elected

---

<sup>51</sup>Ibid.

<sup>52</sup>“Colombia: masacres en la zona rural de Corinto, Cauca; y en zonas rurales de El Santuario, Cocorná, La Pintada, y San Carlos, Antioquia. Durante estos hechos fueron asesinadas cerca de 25 personas,” *Organización Mundial Contra la Tortura*, November 22, 2001. Available at <http://www.omct.org/es/urgent-campaigns/urgent-interventions/colombia/2001/11/d16132/> (last accessed March 2, 2016).

<sup>53</sup>“La condena a la Nación por el homicidio de un alcalde por parte de ‘paras,’” *El Espectador*, March 28, 2015. Available at <http://www.elespectador.com/noticias/judicial/condena-nacion-el-homicidio-de-un-alcalde-parte-de-para-articulo-551888> (last accessed March 2, 2016).

<sup>54</sup>Notice that we often cannot determine the political orientation of the victims. Thus, rather than focusing on the number of left-wing victims in each type of municipality, we emphasize the *difference* between municipalities where the left won and lost.

mayors), while the figure for places where the left barely lost is 75%.

## 1.9 Discussion

In the late 1980s and early 1990s Colombia undertook a number of democratizing reforms, notably the introduction of mayoral elections. The opening up of the political system marked the entry of traditionally excluded groups, particularly left-leaning parties. But these reforms, and the overall shift towards a more inclusive set of institutions, threatened the traditional balance of power in authoritarian enclaves where economic and political elites held a significant amount of both institutionalized and illegal (violent) power, a feature that is typical of countries with an uneven distribution of functioning institutions.<sup>55</sup>

We show that left-wing party victories in mayoral elections in Colombia triggered a surge in attacks by right-wing paramilitaries. As predicted in Acemoglu and Robinson (2008)’s theory about the persistence of power and elites in weakly-institutionalized environments, we interpret this increase in violence as a *de facto* reaction of local political and economic elites to counteract the increase in the *de jure* power of traditional outsiders. We rule out several alternative hypotheses and provide evidence of mechanisms that support this interpretation.

Our findings, however, raise some important questions. For example, why do political elites agree to open up the political system in the first place? If they are powerful enough to respond to the electoral success of outsiders with violence, shouldn’t they be able to prevent reforms that threaten their local monopoly of power? We posit that this is due to two main reasons. First, democratization is often conceded by national elites, not by the local elites, following pressure from local interest groups and the international community. Hence, these reforms are often imposed exogenously on local elites, and thus their only alternative is to respond with strategies of boundary control that, given the low state capacity in weakly institutionalized societies are able to coexist with democratizing national reforms.<sup>56</sup> The second reason has to do with uncertainty about the outcome of future elections. Traditional political groups may overestimate the electoral success (or underestimate the appeal of outsiders), thus gambling their chances of losing power when the reform is adopted.

One implication of our findings is that several dimensions of institutions must effectively function together in order for democracy to prosper. Open elections that are

---

<sup>55</sup>This feature has been the focus of the “subnational authoritarianism” strand of political science literature, which emphasizes the coexistence of national-level democratization and local authoritarianism (see Gibson (2014)).

<sup>56</sup>This was certainly the case in the Colombian context studied in this paper. The reform that introduced local elections in 1986 was promoted by the central government (as an outgrowth of its peace negotiation with the rebels), not by the local elites. In turn, the 1991 constitution (which complemented the 1986 reform) was promoted by a student movement calling for a Constitutional Assembly. The resulting coexistence of local elites’ control with democratizing national reforms in a context of low state capacity is described in Robinson (2013) and Robinson (2016).



not accompanied by a state monopoly over violence, or by checks and balances against the disproportional accumulation of political power in the hands of a few individuals, may have unintended negative consequences. The absence of strong and functioning institutions across all dimensions is likely to lead to *see-saw* effects in elites' use of different forms of power. When democratizing reforms strengthen political institutions, elites may simply switch their investments away from the formal or *de jure* exercise of political power, and towards other more violent means to preserve their influence and power.

Our findings are thus relevant for other countries in which the political system is opened up in a context of weak institutions and informal means of local authoritarian control over the territory. This has been the case in many developed countries, and is the case today in several developing countries with nominally democratic regimes. [Fox \(1994\)](#) discusses democratization in Latin America and the attempts to eliminate local authoritarian enclaves. Examples include the uneven nature of state democratization in Mexico, where the PRI has held onto power via violent means: “This pattern was most notable in Michoacan, the only state where the PRD had a serious chance of winning a governorship. (...) Political violence against the opposition went unpunished” (p.112). [Gibson \(2014\)](#), when referring to Santiago del Estero in Argentina, also notes that “where institutional control and clientelism failed to neutralize opponents, outright oppression filled the void.” In the Philippines, after the restoration of democracy in 1946, a new left-wing party representing the organized peasantry (the Democratic Alliance, DA), participated in legislative elections despite being violently repressed by the private armies of landlords, and won legislative races in six congressional districts. However, an elite-controlled Congress illegally refused to allow the DA to take its seats ([Franco, 2001](#)). Even in the US South, where authoritarian enclaves could devise strategies of control through “perfectly legal” means, “the mixes of boundary-control strategies –violent and nonviolent, legal and illegal – shifted with features of the national territorial regime” ([Gibson, 2014](#), p.73).

## 1.10 Tables

**Table 1.1: Effect of electing a left-wing mayor on paramilitary attacks**

Dependent variable: <i>Average yearly paramilitary attacks per 100,000 inhabitants during term in office</i>								
	Linear polynomials				Quadratic polynomials			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: <i>Non-parametric estimates</i>								
Left-wing mayor elected	4.351** (2.200)	5.258** (2.247)	6.366*** (2.401)	6.757*** (2.555)	5.750** (2.385)	5.321** (2.348)	6.121** (2.471)	6.300** (2.594)
Observations	157	121	106	100	186	136	156	143
Bandwidth	0.0930	0.0620	0.0520	0.0480	0.119	0.0770	0.0930	0.0810
(Local) polynomial order	1	1	1	1	2	2	2	2
Panel B: <i>Parametric estimates</i>								
Left-wing mayor elected	3.654* (2.035)	3.545 (2.168)	3.461 (2.126)	3.526 (2.217)	5.092** (2.453)	5.049** (2.509)	5.603** (2.645)	5.664** (2.825)
Observations	157	157	156	156	157	157	156	156
R-squared	0.031	0.068	0.198	0.229	0.036	0.074	0.210	0.241
Bandwidth	0.0930	0.0930	0.0930	0.0930	0.0930	0.0930	0.0930	0.0930
(Local) polynomial order	1	1	1	1	2	2	2	2

**Notes:** Standard errors in parentheses. \* is significant at 10%, \*\* 5%, and \*\*\* 1% level. [Calonico et al. \(2014\)](#) optimal bandwidth and triangular kernel weights in all columns. In Columns 1 to 4 and in Columns 5 to 8 of Panel A, the (unknown) polynomial is approximated with local linear and quadratic polynomials respectively. All regressions in Panel A include the bias correction and robust standard errors of [Calonico et al. \(2014\)](#). Panel B reports parametric OLS estimates that vary the polynomial degree consistently with Panel A. Columns 1 and 5 include no controls. All the other columns include pre-determined controls: columns 2 and 6 include geographic controls (altitude, average historical rainfall, distance (in km) to Bogotá and to the closest market place), and region dummies (Caribbean, Eastern, Andean and Pacific); Columns 3 and 7 include socio-economic controls (vote share for left and right presidential candidates in 1994 elections, rurality index, total population, literacy index in 1993, presence of coca plantations in 1994 and historic incidence of political violence during *La Violencia* civil war in the mid 20<sup>th</sup> century); and Columns 4 and 8 include all the controls simultaneously.



**Table 1.2: Effect of electing a left-wing mayor on other forms of violence, corruption and government performance measures**

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. <i>Dependent variable: Average yearly attacks or clashes per 100,000 during term in office, by group</i>						
	Attacks by		Clashes between			
	guerrillas	government	guerrilla & paramilitary	guerrilla & government	paramilitary & government	
Left-wing mayor elected	0.731 (1.886)	1.602 (1.544)	0.228 (0.229)	1.776 (1.437)	0.281 (0.186)	
Observations	135	177	148	142	129	
Bandwidth	0.0761	0.112	0.0850	0.0787	0.0704	
Panel B. <i>Dependent variable: disciplinary prosecutions</i>						
	Mayor is			Top official is		
	investigated	guilty	impeached	investigated	guilty	impeached
Left-wing mayor elected	0.168 (0.225)	0.173 (0.166)	0.0890 (0.141)	0.0468 (0.103)	-0.0675 (0.0505)	-0.000592 (0.0340)
Observations	99	72	73	123	78	66
Bandwidth	0.0861	0.0580	0.0592	0.121	0.0648	0.0519
Panel C. <i>Dependent variable: local government performance</i>						
	Index of		Capital	Current		
	fiscal performance	legal rules compliance	admin. capacity	expenditure	expenditure	
Left-wing mayor elected	-7.663 (4.947)	7.869 (9.592)	-11.19 (8.909)	0.210 (0.401)	-0.108 (0.365)	
Observations	90	62	41	174	182	
Bandwidth	0.0799	0.0871	0.0519	0.114	0.118	

**Notes:** Standard errors in parentheses. \* is significant at 10%, \*\* 5%, and \*\*\* 1% level. Calonico et al. (2014) optimal bandwidth, bias correction, robust standard errors, triangular kernel weights and linear local polynomials in all panels and columns.

**Table 1.3: Effect of electing a right-wing mayor on violence**

Dependent variable: <i>Average yearly attacks per 100,000 during term in office by:</i>				
	All groups	Paramilitary	Guerrilla	Government
	(1)	(2)	(3)	(4)
Panel A. <i>Non-parametric estimates</i>				
Right-wing mayor elected	0.440 (1.124)	0.175 (0.612)	0.0440 (0.143)	-0.543** (0.274)
Observations	386	380	269	437
R-squared				
Bandwidth	0.0657	0.0644	0.0443	0.0754
Panel B. <i>Parametric estimates</i>				
Right mayor	0.274 (0.864)	0.186 (0.472)	0.0198 (0.118)	-0.508** (0.229)
Observations	386	378	268	436
R-squared	0.001	0.003	0.013	0.014
Bandwidth	0.0660	0.0640	0.0440	0.0750

**Notes:** Standard errors in parentheses. \* is significant at 10%, \*\* 5%, and \*\*\* 1% level. [Calonico et al. \(2014\)](#) optimal bandwidth, bias correction, robust standard errors, triangular kernel weights and linear local polynomials in all panels and columns.

**Table 1.4: Effect of electing a left-wing mayor on paramilitary attacks**  
**Heterogeneous effects by timing of AUC demobilization**

Dependent variable: <i>Average yearly paramilitary attacks per 100,000 during term in office</i>		
	(1)	(2)
A Left-wing mayor elected	5.659** (2.343)	7.332** (2.942)
Post AUC demobilization	2.337 (1.792)	2.341 (1.796)
B Post AUC demobilization × Left-wing mayor elected	-5.345** (2.304)	-5.429** (2.336)
Constant	-0.435 (0.483)	-1.083 (0.872)
Observations	157	157
R-squared	0.075	0.081
Bandwidth	0.0930	0.0930
A + B	.314	1.903
<i>H<sub>0</sub>: A + B = 0</i>		
F-statistic	.02	.86
P-value	.88	.36
(Local) polynomial order	1	2

**Notes:** Standard errors in parentheses. \* is significant at 10%, \*\* 5%, and \*\*\* 1% level. Parametric estimates. Triangular kernel weights, bias correction and optimal bandwidth of [Calonico et al. \(2014\)](#) in all columns.

**Table 1.5: Effect of electing a left-wing mayor on paramilitary attacks**  
**Heterogeneous effects by year of violence after the election**

Dependent variable: <i>Average yearly paramilitary attacks per 100,000 inhabitants in year... of term in office</i>				
	Year 1	Year 2	Year 3	Next election
	(1)	(2)	(3)	(4)
Left-wing mayor elected	4.783 (3.375)	-1.203 (1.410)	10.90* (6.355)	18.48 (11.56)
Observations	148	149	150	100
R-squared				
Bandwidth	0.0842	0.0860	0.0881	0.0677

**Notes:** Standard errors in parentheses. \* is significant at 10%, \*\* 5%, and \*\*\* 1% level. Calonico et al. (2014) optimal bandwidth, bias correction, robust standard errors, triangular kernel weights and linear local polynomials in all panels and columns.

**Table 1.6: Incumbency advantage in Colombia**

	Non-parametric estimates				Parametric estimates	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent variable: <i>Indicator of whether party elected in t runs and wins in t + 1</i>						
Winner party in t	-0.190*** (0.0725)	-0.228** (0.0981)	-0.176*** (0.0599)	-0.212** (0.0833)	-0.174*** (0.0600)	-0.212** (0.0834)
Left-wing party					0.572*** (0.0249)	0.575*** (0.0309)
Winner party in t × Left-wing party					-0.918*** (0.0491)	-0.926*** (0.0447)
Observations	995	1052	991	1053	991	1053
R-squared			0.019	0.019	0.022	0.021
Bandwidth	0.127	0.141	0.127	0.141	0.127	0.141
(Local) polynomial order	1	2	1	2	1	2
Panel B. Dependent variable: <i>Indicator of whether party elected in t runs in t + 1</i>						
Winner party in t	0.0747 (0.0805)	0.0683 (0.0975)	0.0550 (0.0675)	0.0693 (0.0835)	0.0571 (0.0675)	0.0711 (0.0835)
Left-wing party					-0.122*** (0.0289)	-0.127*** (0.0298)
Winner party in t × Left-wing party					-0.966*** (0.0462)	-0.947*** (0.0486)
Observations	1046	1239	1045	1240	1045	1240
R-squared			0.004	0.007	0.011	0.013
Bandwidth	0.138	0.202	0.138	0.202	0.138	0.202
(Local) polynomial order	1	2	1	2	1	2
(Local) polynomial order	1	2	1	2	1	2

**Notes:** Standard errors in parentheses. \* is significant at 10%, \*\* 5%, and \*\*\* 1% level. Calonico et al. (2014) optimal bandwidth in all columns. Triangular kernel weights used in all columns. In Columns (1) and (2) the (unknown) polynomial is approximated with local linear and quadratic polynomials, respectively, and include the bias correction and robust standard errors of Calonico et al. (2014). Columns (3) - (6) are parametric estimates with polynomials of the forcing variable not displayed. (3) & (5) Linear estimates with varying slopes. (4) & (6) Quadratic estimates with varying slopes.

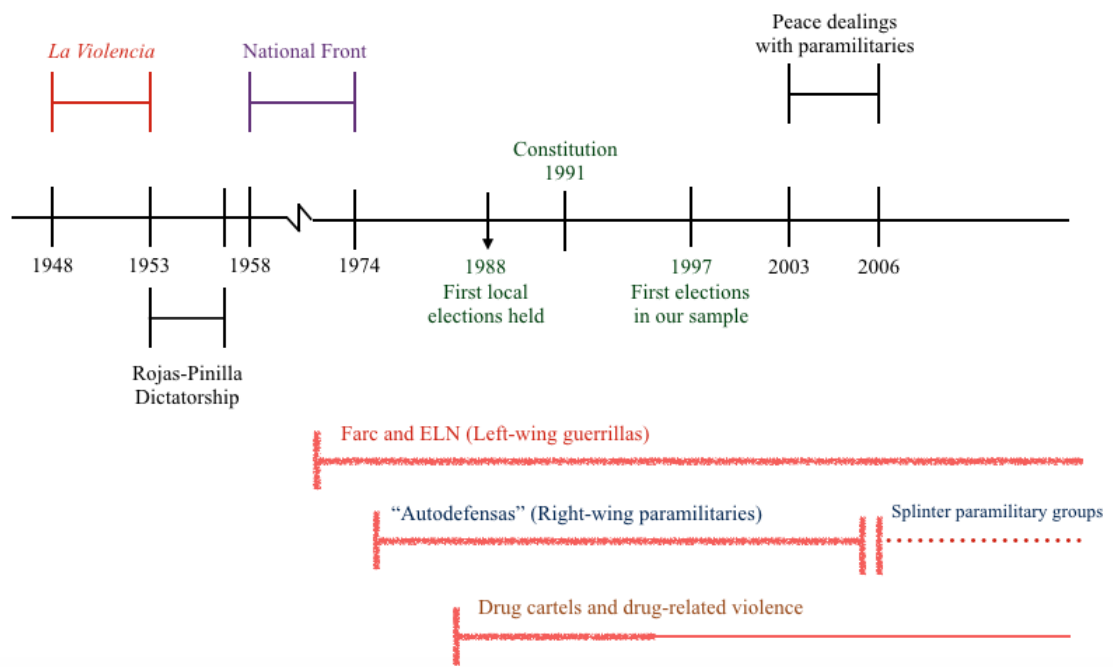
**Table 1.7: Effect of electing a left-wing mayor on land registry updates**

Dep. variable: <i>Indicator of whether registry was updated during term in office</i>	Non-parametric estimates		Parametric estimates			
	(1)	(2)	(3)	(4)	(5)	(6)
	Left-wing mayor elected	0.173 (0.177)	0.140 (0.201)	0.157 (0.141)	0.0622 (0.201)	0.116 (0.148)
Years since cadastral update					0.0119* (0.00693)	0.0108 (0.00688)
Observations	148	194	148	148	148	148
R-squared			0.015	0.030	0.035	0.046
Bandwidth	0.0851	0.127	0.0850	0.0850	0.0850	0.0850
(Local) polynomial order	1	2	1	2	1	2

**Notes:** Standard errors in parentheses. \* is significant at 10%, \*\* at 5%, and \*\*\* at 1% level. Calonico et al. (2014) optimal bandwidth in all columns. Triangular kernel weights used in all columns. In Columns (1) and (2) the (unknown) polynomial is approximated with local linear and quadratic polynomials, respectively, and we report the bias correction and robust standard errors of Calonico et al. (2014).

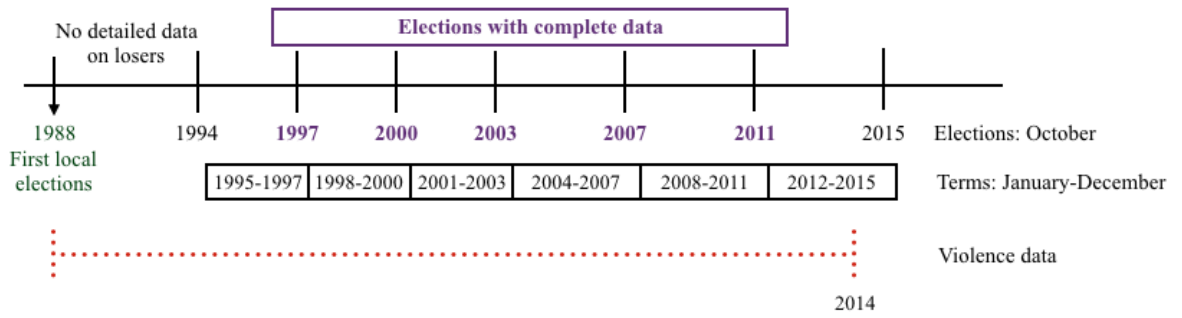
# Figures

Figure 1.1: Brief historical timeline



Source: Authors' own.

Figure 1.2: Election years and available data



Source: Authors' own.

Figure 1.3: McCrary test: Sorting around the winning threshold for the left

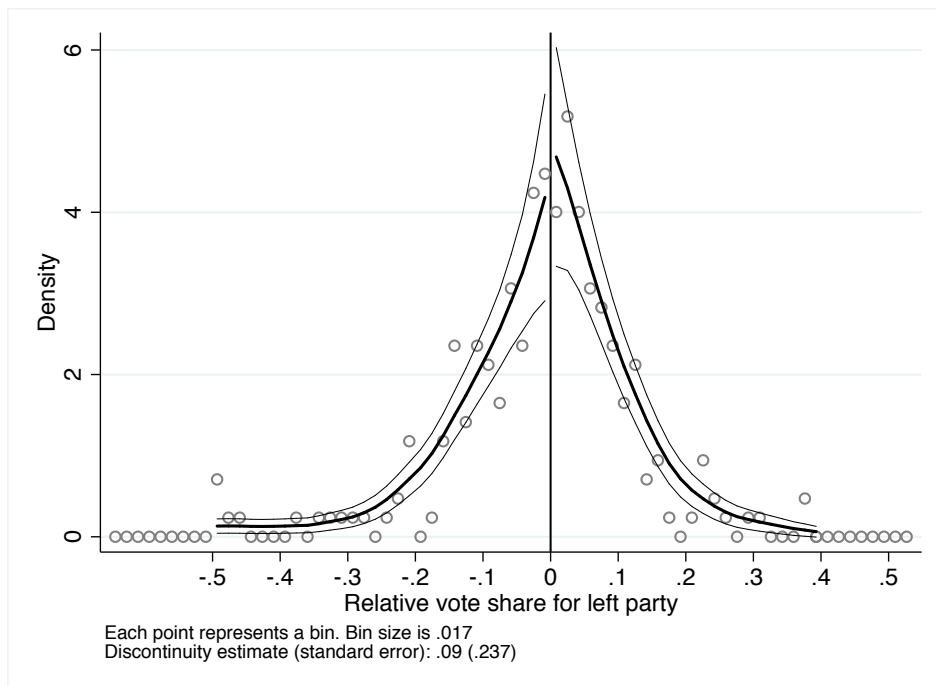
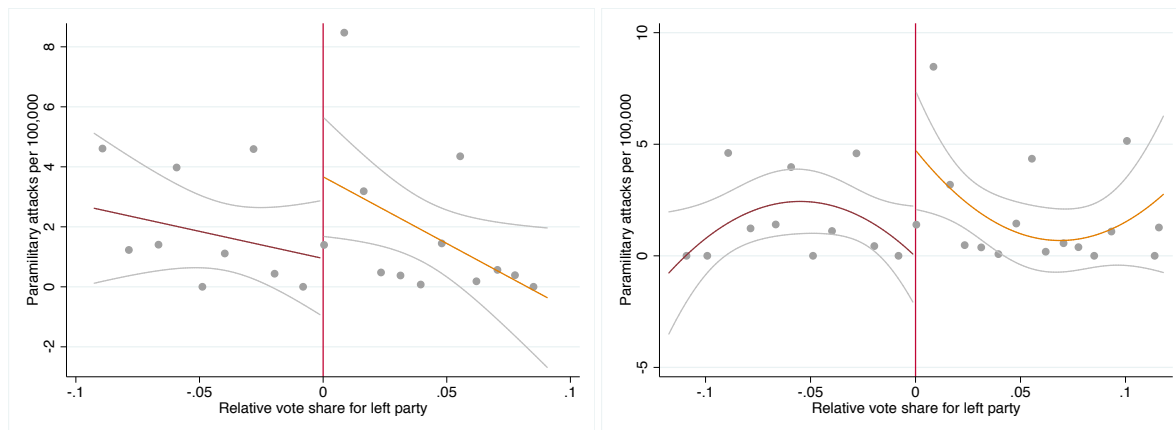




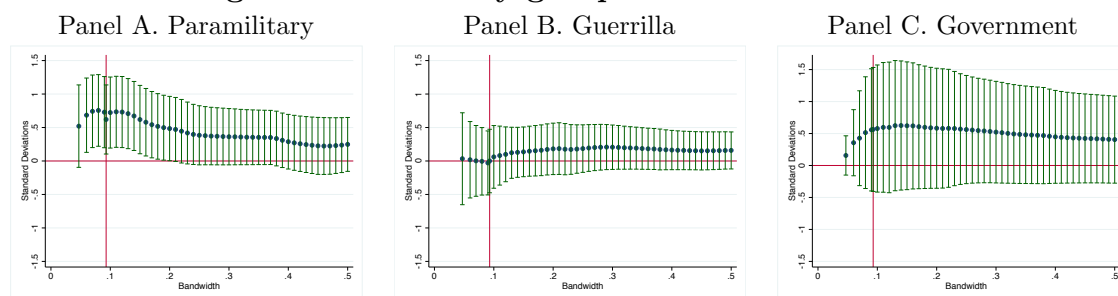
Figure 1.4: Effect of electing a left-leaning mayor on paramilitary attacks



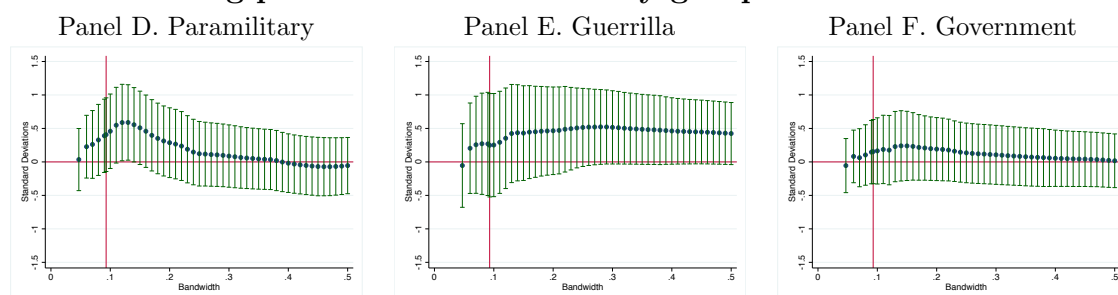
**Notes:** Observations within [Calonico et al. \(2014\)](#)'s bandwidth displayed. Left: linear fit. Right: quadratic fit. 10 bins of equal size at each side of the cutoff.

Figure 1.5: Effect of electing a left-leaning mayor on measures of violence

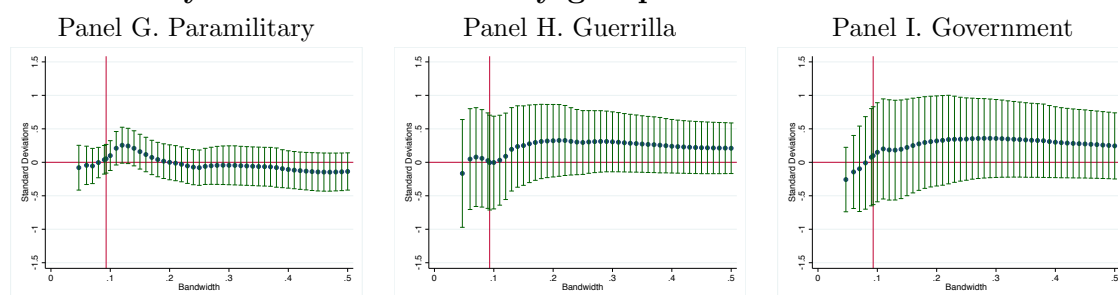
**Attacks during term in office by group**



**Attacks during previous term in office by group**



**Attacks in year before elections by group**



Notes: The solid line marks the Calonico et al. (2014) optimal bandwidth. Non-parametric estimates with bias correction, robust standard errors, triangular kernels, and linear local polynomials (Calonico et al., 2014).

# Appendix

## 1.11 Data appendix: description of coding protocol and variables

### 1.11.1 Coding left-wing and right-wing parties

In this appendix we explain the classification of parties into left-wing, right-wing, or neither left- nor right-wing. We apply the following procedure to the 505 parties that either won or came second in mayoral elections during our sample period.<sup>57</sup>

1. As in [Beck et al. \(2012\)](#), we first code parties as left-leaning if they self-define, based on their name, motto, or slogan as “communist,” “socialist,” “social democratic,” or simply “left-wing.” The parallel terms for right-leaning parties are “conservative,” “Christian democratic,” or “right-wing.”<sup>58</sup> Most parties, however, cannot be classified based on this criterion, in which case we move to step 2.<sup>59</sup>
2. When available, we look at the party statutes and, following [Budge et al. \(2001\)](#), code the party as left-wing if at least three of the following five policy stances are present in the document:
  - (a) pro-peasant or social re-vindication in nature,
  - (b) more market regulation,
  - (c) defense of workers’ rights against exploitation,
  - (d) defense of state-owned or communal property rights,
  - (e) anti-imperialism.

Also following [Budge et al. \(2001\)](#), we code the party as right-wing if at least three of the following five policy stances are mentioned in its statutes:

- (a) emphasis on economic growth/development over inequality and redistribution,
- (b) endorsement of free-market, orthodox policies, a limited role for the state, and the promotion of private enterprises,
- (c) family and religion as crucial moral pillars of society,
- (d) appeal to patriotism and/or nationalism and the suspension of some freedoms in order to protect the state against subversion,

---

<sup>57</sup>It is worth noting that 78 of these parties (15% of the 505) simply represent individual politicians who ran under their own name, even if they are often endorsed by a coalition of parties. In this case the classification procedure is slightly different than for actual parties, as explained in the text.

<sup>58</sup>An exception is the Colombian Conservative Party, which in spite of its right-wing origins in the 19<sup>th</sup> century has been a centrist party since the start of the National Front in 1958 (see Section 1.2). This is confirmed by the party’s policy stance, which is also the criterion used to classify both the Conservative and Liberal parties as neither left- nor right-wing (see criterion 2 below).

<sup>59</sup>Using this criterion, we identified eight left-leaning parties and no right-wing parties. Note that this criterion only allows us to classify left- and right-wing parties, but cannot be used to identify those in the “neither” category; the subsequent criteria allow us to do so.

- (e) priority of law and order and a military approach to preserve the state's monopoly of violence.

Parties that, according to their statutes, are neither left- nor right-wing are classified as neither.<sup>60</sup> If the party statutes are not available, we apply the next criterion.

3. We look at the government plan that the party drafts for each municipality/election and, as in step 2, we identify the policy stance associated with a left- or right-wing ideology.<sup>61</sup> Parties that, according to their government plan, are neither left- nor right-wing are classified as neither.<sup>62</sup>
4. In some robustness specifications (see Table 1.11), we make further classification attempts. Some short-lived parties for which formal statutes or government plans (steps 2 and 3, respectively) are not readily available are factions of, or splinter movements from, other (well-established and thus readily classifiable) parties, or simply old parties that changed their name. In these cases we assign the ideology of the predecessor party. Parties that, according to their predecessor party, are neither left- nor right-wing are classified as so.<sup>63</sup>

We do not include splinter parties or factions in our baseline estimates, since this category relies on the classification of other parties, and is thus indirect and probably more prone to measurement error. For these estimates we prefer to use a conservative classification procedure. However, as shown in Table 1.11, the results are substantively unchanged if we include parties classified in this way.

The procedure for the 78 candidates that run under their own name is somewhat different:

- 1A. Because we are interested in classifying the ideology of parties, rather than individual politicians, we first determine whether these candidates in effect represent a coalition of parties with a known ideological stance (using the 4-step procedure described above). This information is available from the National Registry Bureau. If this is the case, and the ideology of the parties forming the coalition coincides (as either left-wing, right-wing, or neither), then the same ideology is assigned to the candidate. However, if the candidate does not represent a coalition, or if he/she does but the ideology of the parties forming the coalition does not match, then we apply the next criterion.<sup>64</sup>
- 2A. Same as step 3 above.<sup>65</sup>

---

<sup>60</sup>Using this criterion, we identified seven left-leaning parties, six right-wing parties, and 15 parties that are neither left- nor right-wing.

<sup>61</sup>Since all candidates running for municipal executive office are required to submit their government plan prior to the election, in principle these plans are also available for runners-up.

<sup>62</sup>Using this criterion we identified no left-leaning parties, seven right-wing parties, and classified 141 parties as neither.

<sup>63</sup>Using this criterion, we identified nine left-leaning parties, 18 right-wing parties, and classified 105 parties as neither.

<sup>64</sup>Using this criterion, we identified no left- or right-leaning parties, and classified 36 parties as neither.

<sup>65</sup>Using this criterion, we classified no left-leaning parties, three right-wing parties, and 24 parties as neither.

3A. Same as step 4 above.<sup>66</sup>

The resulting classification can be found online at: [https://docs.google.com/spreadsheets/d/1WP2sPWB15p3bbfJuYWqLiZTeDmwvNmCRA-5Kgdf\\_BiM/pubhtml](https://docs.google.com/spreadsheets/d/1WP2sPWB15p3bbfJuYWqLiZTeDmwvNmCRA-5Kgdf_BiM/pubhtml)

---

<sup>66</sup>Using this criterion we identified no left-leaning parties, one right-wing party, and classified four parties as neither.

### 1.11.2 Coding the ideological stance of victims

In this appendix we explain the classification of the ideological stance and involvement in politics of civilian victims in paramilitary attacks following a mayoral election in which a left-wing party narrowly won or came second. We focus on the sample of such close elections that took place during our period of study and identify all the paramilitary attacks that occurred during the mayor's term in office. To code the political ideology of the civilian victims of each attack, we follow a three-step procedure:

1. We search the main national and local newspapers for detailed information about the attacks.<sup>67</sup> If there is no information about the event, or if it is reported but the available information cannot be used to classify the resulting victim(s) as left-wing activists or not (for example because of the victim's affiliation with a union or a left-wing political party), then we turn to the next criterion.
2. We search the websites of human rights NGOs known for monitoring political violence in Colombia for detailed information about the attack.<sup>68</sup> If there is no information about the event, or if the event is reported but the available information cannot be used to classify the resulting victim(s) as left-wing activists or not, then we turn to the next criterion.
3. CINEP's *Noche y Niebla* magazine includes narratives with specifics on all the events included in our violence data.<sup>69</sup> Within these narratives we look for hints that can be used to classify the resulting victim(s) as left-wing militants or not.

Victims who cannot be classified as either left-wing or non-left-wing after applying the three criteria are coded as having an "unknown" ideology.

The results from applying this protocol are used to compute the figures reported in Section 1.8.

---

<sup>67</sup>The newspapers include *El Tiempo*, *El Espectador*, *El Colombiano*, *El Herald*, *El Nuevo Siglo*, *El País*, and *Vanguardia*.

<sup>68</sup>These include the World Organization Against Torture, the International Labor Organization (ILO), *Verdad Abierta*, *Asociación Colombiana de Juristas*, and *Asociación de Cabildos de Indígenas del Norte del Cauca*.

<sup>69</sup>Recall from Section 1.3.1 that CINEP is the main source of this dataset.

### 1.11.3 Additional variables

The main source for variables used in balance tests is the municipal panel maintained and hosted by the Center For Economic Development Studies (CEDE) at Universidad de los Andes (Acevedo and Bornacelly, 2014). Specifically, we check balance across a number of geographic and socio-economic variables, described in Table 1.9.

In addition, to explore whether the policies adopted by left-wing mayors differ from those adopted by mayors representing other parties, we look at land registry updates, which are available from the national land registry agency.<sup>70</sup> Since land is mainly taxed based on assessed values recorded in the registry, updates to this registry are a policy tool that can be used to increase taxes on landowners.

Furthermore, in order to rule out the possibility that post-electoral violence following left-wing victories is driven by poorer performance by left-wing mayors relative to incumbents from other parties, such as a weaker/ stronger fiscal management of the municipal treasury, we look at the governance indices developed by *Departamento Nacional de Planeación* (DNP, the National Planning Department). Specifically we use the DNP's "index of fiscal performance," "index of legal requirements," and "index of administrative capacity." The first index summarizes the performance of municipal governments based on the size of the deficit and the proportion of municipal income that is spent on operational costs versus invested, as well as the proportion of income that originates from national government transfers versus municipal tax revenue. The second index assesses the compliance of the municipal administration with national rules on how to spend the central government transfers (targeted specifically at items related to improving the municipality health and education indicators). The third index measures the municipal administration's capacity to rule effectively, based on the turnover of top officials, the share of top officials that holds a professional degree, the share of top officials with access to computers, the administration's access to specialized software that helps automate processes, and the use of protocols for internal administrative controls.

Moreover, to make sure that post-electoral violence is not driven by the potential differential engagement of elected left-wing mayors in corrupt practices, we build on recent work by Martinez (2017a), who uses information from *Procuraduría General de la Nación* (Colombia's Watchdog Agency), to code disciplinary prosecutions of the municipal mayor and his/her top officials, as a proxy for misbehavior.<sup>71</sup> Specifically, the author codes whether the official was investigated, found guilty, or impeached (which entails removal from office and a temporal ban from public service).<sup>72</sup>

---

<sup>70</sup>Data for the department of Antioquia come from the department's land registry agency, which is independent from the national agency.

<sup>71</sup>Unfortunately, the performance and corruption data are only available for a shorter period, which reduces the sample we can use to test for differences on these variables. Table 1.8 specifies the sample years for which these data are available.

<sup>72</sup>Not all officials who are found guilty are impeached, as the sanction depends on the severity of the misconduct. Some guilty officials are fined.

Figure 1.6: Violence data: examples of attacks

## Mayo 01/2003

DEPARTAMENTO: CUNDINAMARCA  
MUNICIPIO: VIOTA

*Paramilitares desaparecieron a Miriam, concejal de Viotá por un movimiento cívico, en momentos en que se dirigía al municipio de Apulo, por la vía Recovecos-Puente Bala. Según la denuncia: "Iba a cumplir una citación que le habían hecho los grupos paramilitares, acompañada por el señor Ernesto García quien la conduciría al lugar de la cita. En este lugar es donde insistentemente ha manifestado la población que los paramilitares patrullan y hacen retenes permanentes".*

Presuntos Responsables: PARAMILITARES

VIOLACIONES A LOS DERECHOS HUMANOS

Desaparición por Persecución Política  
MIRIAM CLAVIJO FLOREZ

## Marzo 29/2003

DEPARTAMENTO: CUNDINAMARCA  
MUNICIPIO: VIOTA

*Una menor de 13 años de edad, fue desaparecida con la aquiescencia de tropas del Ejército Nacional por miembros de un grupo paramilitar que se movilizaban en tres vehículos a la altura de la vereda Mोगambo. El hecho se presentó hacia las 3:00 de la madrugada.*

Presuntos Responsables: EJERCITO Y PARAMILITARES

VIOLACIONES A LOS DERECHOS HUMANOS

Desaparición por Persecución Política  
JANETH ZAMBRANO CASAS



**Table 1.8: Descriptive Statistics of main variables**  
**(Sample: Electoral races in which left-wing parties won or came second: 1997- 2014)**

Variable	Mean	Standard Deviation	Minimum	Median	Maximum
<i>Panel A. Average yearly attacks per 100,000 inhabitants during government period</i>					
Paramilitary	1.980	7.015	0.000	0.000	75.750
Guerrilla	3.820	7.948	0.000	0.065	89.908
Government	0.663	2.561	0.000	0.000	35.224
<i>Panel B. Average yearly clashes per 100,000 inhabitants during government period</i>					
Guerrilla-Paramilitary	0.169	0.930	0.000	0.000	7.251
Guerrilla-Government	2.247	4.912	0.000	0.000	51.322
Paramilitary-Government	0.074	0.691	0.000	0.000	10.093
<i>Panel C. Mean occurrence of land cadaster updates during government period</i>					
Land cadaster update	0.233	0.424	0.000	0.000	1.000
<i>Panel D. Mean occurrence of corruption episodes during government period</i>					
<i>Mayor is...</i>					
Investigated	0.204	0.404	0.000	0.000	1.000
Found Guilty	0.121	0.327	0.000	0.000	1.000
Impeached	0.089	0.286	0.000	0.000	1.000
<i>Top local official is...</i>					
Investigated	0.064	0.245	0.000	0.000	1.000
Found Guilty	0.038	0.192	0.000	0.000	1.000
Impeached	0.025	0.158	0.000	0.000	1.000
<i>Panel E. Average value of government performance indices during government period</i>					
Fiscal performance	61.687	7.950	39.210	60.793	87.715
Legal rules compliance	73.278	15.581	17.020	75.562	98.170
Administrative capacity	73.604	15.929	28.090	79.112	97.620
<i>Panel F. Forcing variable:</i>					
$\frac{Votes\ left - Votes\ non-left}{Votes\ top\ 2}$	-0.012	0.133	-0.500	-0.000	0.382
$ \frac{Votes\ left - Votes\ non-left}{Votes\ top\ 2} $	0.094	0.095	0.000	0.067	0.500
<i>Panel G. G. Forcing variable within bandwidth:</i>					
$\frac{Votes\ left - Votes\ non-left}{Votes\ top\ 2}$	0.004	0.047	-0.093	0.007	0.091
$ \frac{Votes\ left - Votes\ non-left}{Votes\ top\ 2} $	0.040	0.026	0.000	0.034	0.093

**Notes:** Number of observations: 254 in Panels A-C and F; 157 in panel D (only available since 2000); and 157 in panel G. In panel E there are 152 observations for fiscal performance (only available since 2000), and 94 observations for the indices of legal rules compliance and administrative capacity (only available for 2007 and 2011). In Panels A-F the sample includes all mayoral elections where a left-wing party is either the winner or the runner-up and the corresponding variable is available. The sample in Panel G is restricted to Calonico et. al (2014)'s optimal bandwidth (corresponding to the estimate of the effect of left-wing electoral victories on paramilitary attacks, with first-degree local polynomials and no controls.

**Table 1.9: Variables and sources**

Variable	Source	Description
<b>Panel A. Dependent variables: Violence</b>		
Total Attacks by all groups	Total number of attacks, by all groups, in the municipality during the first 3 years of the term in office (per 100,000 inhabitants). Attacks are defined according to (Restrepo et al., 2003): a violent event in which there is no direct, armed combat between two groups.	(Restrepo et al., 2003) updated until 2014 by Universidad del Rosario.
Total attacks by the paramilitary	Same as above but the groups identified in the attacks are the paramilitary.	(Restrepo et al., 2003) updated until 2014 by Universidad del Rosario.
Total attacks by the guerrilla	Same as above but the groups identified in the attacks are the guerrillas.	(Restrepo et al., 2003) updated until 2014 by Universidad del Rosario.
Total attacks by the government	Same as above but the groups identified in the attacks is the government.	(Restrepo et al., 2003) updated until 2014 by Universidad del Rosario.
<b>Panel B. Dependent variables: Land registry, Corruption &amp; Performance</b>		
Land registry up-date	Dummy = 1 if the land registry was updated during the first 3 years of the mayor's term in office.	Agustin Codazzi Geographic Institute (Colombia's National Geographic Institute) and Antioquia Land Registry Agency. (Agency for the Antioquia department).
Mayor investigated, guilty, or impeached	Dummy variables indicating whether the mayor was investigated, found guilty, or impeached for corruption by <i>Procuraduría General de la Nación</i> , the government agency that investigates disciplinary faults by public officials.	Martinez (2017a), with data from <i>Procuraduría General de la Nación</i> .
Top official investigated, guilty, or impeached	Dummy variables indicating whether a top local official (at the rank of Secretary) was investigated, found guilty, or impeached for corruption by <i>Procuraduría General de la Nación</i> .	Martinez (2017a), with data from <i>Procuraduría General de la Nación</i> .
Fiscal performance index	Index of fiscal performance based on (+ improves the index, - deteriorates it): size of the municipality's debt (-), % of income from own resources (+), % invested (+), % spent on administrative functioning (-).	Colombia's National Planning Department
Compliance with legal rules index	Index based on whether the municipality complies with legal spending rules, comparing budgeted and executed resources as well as expenditure in each sector compared to what is legally permitted.	Colombia's National Planning Department
Administrative capacity index	Index aggregating: the stability of directives in the municipality, personnel qualifications, the extent to which internal processes follow a clear system, and the existence of internal controls.	Colombia's National Planning Department.
<b>Panel C. Forcing Variable</b>		

Continued on next page

<b>Table 1.9 – Variables and sources, continued from previous page</b>		
Variable	Description	Source
Left party win margin (normalized around 0)	Winning margin (in %) of the left-wing incumbent, normalized around 0. Values above 0 indicate that the left won (below 0 = the left lost).	Electoral results at the municipality level, obtained from the Colombian national registry and compiled by (Pachón and Sánchez, 2014).
<b>Panel D. Other predetermined covariates</b>		
<u>Political covariates</u>		
% of votes for the left-leaning presidential candidates in 1990	% of total votes (in the municipality) for all left-leaning presidential candidates in 1990	Colombia’s National Registry data compiled by (Pachón and Sánchez, 2014).
% of votes for the Conservative Party presidential candidate in 1990	% of total votes (in the municipality) for Rodrigo Lloreda, the Conservative Party presidential candidate in 1990	Colombia’s National Registry data compiled by (Pachón and Sánchez, 2014).
Presence of historic violence (1948–1953)	Dummy = 1 if there was historic violence in the municipality in (1948–1953). This variable is based on the magazine <i>Criminalidad</i> published by the National Police from 1958–1963, which described the municipalities affected by historic partisan violence in each year.	National Police. Data coded by CEDE Universidad de los Andes.
<u>Demographic Covariates</u>		
Initial population	Number of inhabitants in the municipality in 1993	DANE (Colombia’s National Department of Statistics) 1993 National Census.
Literacy Rate	(%) of literate in the municipality	DANE’s 1993 National Census.
<u>Geographic covariates</u>		
Meters above sea level.	Altitude of municipality seat above sea level, in meters.	CEDE, Universidad de los Andes
Index of soil erosion	Based on georeferenced information at the sub-municipality level. Land is classified into seven ordinal categories, and the number of acres in each category is counted to estimate an index. The index is standardized between 0 and 4.5, where high values represent more soil erosion.	Estimates by CEDE Universidad de los Andes, based on Agustin Codazzi Geographic Institute

Continued on next page

<b>Table 1.9 – Variables and sources, continued from previous page</b>		
Variable	Description	Source
Distance to department capital, km	Straight line distance to the capital of the department in which the municipality is located.	Estimates by CEDE Universidad de los Andes, based on Agustin Codazzi Geographic Institute
Distance to main city, km	Straight line distance to the four main Colombian cities (Medellín, Cali, Bogotá, and Barranquilla)	Estimates by CEDE Universidad de los Andes, based on Agustin Codazzi Geographic Institute
Index of rurality	(Rural population / total population) in municipality. Data from 1993.	Estimates by CEDE Universidad de los Andes, based on information provided by DANE
Connection to the country center	Dummy = 1 if the municipality has a road connection the country center	Coded in (Vargas, 2009) from analysis by (Giraldo, Lozada, and Muñoz, 2001).
Index of soil aptitude for agriculture	Land is categorized into seven ordinal categories based on its suitability for agriculture, and the number of acres in each category is counted to estimate an index.	Estimates by CEDE Universidad de los Andes, based on information provided by DANE
Average precipitation, in mm	Mean annual rainfall level in each municipality.	IDEAM, Institute of Hydrology, Meteorology and Environmental Studies

## 1.12 Additional results and robustness tests

### 1.12.1 Balance on covariates

In Table 1.10 we report estimates of the effect of a narrow left-wing victory (Column 3) on a large set of covariates. In Panel A we look at the election year to verify whether left-leaning candidates were disproportionately more likely to win close races earlier or later in the sample period. Panel B examines geographic variables including altitude, rainfall, and distance to main cities and the department capital. Panel C includes socio-economic and political variables, such as having experienced violence during *La Violencia* in the 1940s and 1950s, and socio-economic conditions of municipalities like the share of population living in rural areas, a dummy for the presence of coca plantations, total population, and the literacy rate (all measured prior to our sample period). In Panel D we look at variables related to land inequality and land policy, such as the number of years since the land registry was last updated, measured in the election year prior to when the winner of the close race would have taken office. In Panel E we look at different measures of tax revenue, also measured in the election year prior to when the winner of the close race would have taken office. Finally, Panel F focuses on basic electoral variables such as the average number of parties competing in the race, the average number of candidates, and overall turnout.

We find no statistically significant differences between treatment and control municipalities for most of these variables. The only exception is the number of years since the land registry was last updated, which is about 4 years higher and significant at the 95% level in municipalities in which the left won. However, for the remaining variables the estimated effect of a narrow left-wing victory is both small (typically just a fraction of the mean and standard deviation) and insignificant. Thus, these are precisely estimated coefficients that allow us to reject even small effects. Overall, the results reported in Table 1.10 give us further confidence that our benchmark estimates capture the causal effect of a left-wing electoral victory on paramilitary violence rather than the effect of other municipal characteristics.

### 1.12.2 Robustness to party coding

Parties that could not be classified based on their name or slogan, statutes, or government plan were coded as neither left- nor right-wing in our baseline analysis. This may potentially introduce bias if a sufficient number of such parties is actually either left- or right-wing. One extreme alternative is to drop all unclassified parties from the sample, at the cost of drastically reducing the sample size. Panel A of Table 1.11 reports the robustness of our main results to this alternative sample. The structure of the columns is the same as that of Table 1.1. Note that the sample size drops in all columns relative to that of the baseline regressions. Reassuringly, most of the coefficients remain

**Table 1.10: Effect of electing a left-wing mayor on municipal characteristics**

Dependent variable	Mean	Standard Deviation	Left victory	Std. Error.	Obs	Bandwidth
Panel A. <i>Election year</i>						
Year elected	2002.701	5.447	1.257	2.426	167	.1
Panel B. <i>Geographic characteristics</i>						
Altitude, meters	1752.587	3469.152	69.265	671.765	94	.042
Average precipitation	93.010	18.665	1.505	8.379	152	.09
Distance to department capital, km	81.129	53.511	-1.778	23.432	152	.089
Distance to main city, km	145.999	91.279	6.226	41.881	129	.069
Andean region dummy	0.417	0.494	-.128	.203	142	.08
Pacific region dummy	0.398	0.490	.025	.165	156	.092
Eastern region dummy	0.098	0.298	-.065	.112	126	.066
Caribbean region dummy	0.087	0.282	.143	.105	166	.099
Panel C. <i>Socioeconomic characteristics</i>						
Vote % for left-wing presidential candidates, 1994	0.067	0.070	-.019	.019	111	.056
Vote % for conservative presidential candidates, 1994	0.422	0.209	-.006	.103	129	.069
La Violencia incidence (1948-1953)	0.146	0.353	-.146	.16	142	.079
Rurality index	0.654	0.238	-.035	.091	178	.114
Initial population, 1993	26328.799	33888.293	18839.104	17523.015	148	.083
Coca, 1994	0.075	0.264	.089	.093	120	.062
Literacy rate, 1993	85.452	8.783	-.664	3.646	150	.088
Panel D. <i>Land variables</i>						
Land GINI, based plot sizes	0.707	0.118	-.023	.05	81	.075
Land GINI, based on landowner holdings	0.722	0.103	-.013	.049	67	.06
Number of years since last cadaster update	5.435	5.109	4.157**	1.972	118	.061
Panel E. <i>Tax revenue</i>						
Tax income (per capita)	0.071	0.308	-.191	.156	206	.145
Non tax income (per capita)	0.015	0.029	-.015	.012	199	.136
Tax income from land taxes (per capita)	0.017	0.057	-.046	.035	196	.129
Tax income from commerce and industry (per capita)	0.027	0.188	-.092	.087	216	.163
Panel F. <i>Electoral variables</i>						
Number of candidates in election	3.949	1.968	.107	.647	135	.075
Number of parties in election	3.587	1.968	.455	.668	142	.079
Voter turnout	0.590	0.170	.014	.054	146	.088

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

significant and of similar magnitude (4.1 to 5 additional paramilitary attacks during the term in office).

Another approach is to use alternative criteria to code the ideological stance of parties that could not be classified in steps 1-3. For instance, because many of the 505 parties that participated in local elections during our sample period originated from previously established parties (notably from the two traditional parties), we could assign to them the ideology of their parent party (see criterion 4 of the classification procedure described in Appendix Section 1.11.1). This, however, is subject to some caveats, particularly if the ideology of the faction or splinter movement is different to that of its predecessor (which may have motivated the split). Since it is impossible to know *a priori* whether including this additional party classification step (that we refer to as step 4) represents an improvement over our baseline estimates, we take an agnostic position and investigate the robustness of the baseline results to using the ideology of predecessor parties as an additional classification criterion.

Panels B and C of Table 1.11 report the estimates after using this criterion (step 4) to code the ideology of parties that could not be classified in steps 1-3. As in the baseline results of Table 1.1, Panel B assumes that all the parties left unclassified after steps 1-4 are neither left- nor right-wing. In turn, similar to Panel A of Table 1.11, Panel C drops from the estimation sample all parties left unclassified after steps 1-4. Most of the point estimates (particularly in Panel B) remain statistically significant and of similar magnitude to those reported for our baseline sample (Table 1.1). This is reassuring, and suggests that our specific choices of party ideology classification are not driving our substantive findings.

**Table 1.11: Effect of electing a left-wing mayor on paramilitary attacks (Alternative samples resulting from different codings of party ideology)**

Dependent variable: <i>Average yearly paramilitary attacks per 100,000 inhabitants during term in office</i>								
	Linear polynomials				Quadratic polynomials			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. <i>Dropping all unidentified parties after applying criteria 1 to 3<sup>a</sup></i>								
Left-wing mayor elected	2.807 (1.780)	3.038* (1.692)	4.701** (1.939)	4.473*** (1.704)	4.170** (1.966)	4.134* (2.196)	5.064** (2.030)	4.970** (2.052)
Observations	133	112	97	99	154	108	140	118
Bandwidth	0.0950	0.0770	0.0590	0.0600	0.119	0.0710	0.103	0.0790
(Local) polynomial order	1	1	1	1	2	2	2	2
Panel B. <i>Coding all unidentified parties as being neither left- nor right-wing after applying criteria 1 to 4<sup>a</sup></i>								
Left-wing mayor elected	2.896 (1.899)	4.228** (1.931)	4.525** (2.189)	4.734** (2.337)	4.769** (2.036)	4.351** (1.897)	4.742** (2.236)	4.393* (2.333)
Observations	191	137	120	112	202	152	178	154
Bandwidth	0.118	0.0760	0.0610	0.0540	0.129	0.0860	0.107	0.0900
(Local) polynomial order	1	1	1	1	2	2	2	2
Panel C. <i>Dropping all unidentified parties after applying criteria 1 to 4<sup>a</sup></i>								
Left-wing mayor elected	1.936 (1.536)	2.121 (1.451)	2.840 (1.754)	1.669 (1.536)	3.269** (1.600)	3.131* (1.632)	3.235* (1.852)	2.698 (1.900)
Observations	168	145	119	145	183	138	165	143
Bandwidth	0.107	0.0880	0.0650	0.0900	0.121	0.0800	0.106	0.0860
(Local) polynomial order	1	1	1	1	2	2	2	2

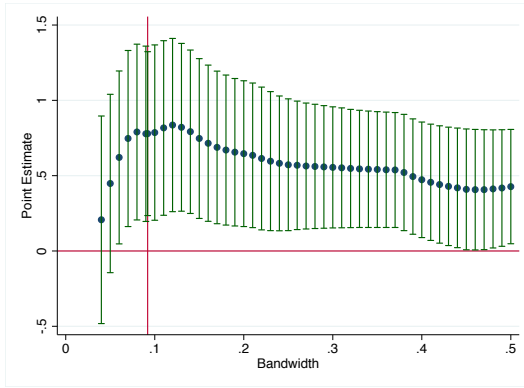
**Notes:** Standard errors in parentheses. \* is significant at 10%, \*\* 5%, and \*\*\* 1% level. Calonico et al. (2014) optimal bandwidth and triangular kernel weights in all columns. In Columns 1 to 4 and in Columns 5 to 8 of Panel A, the (unknown) polynomial is approximated with local linear and quadratic polynomials, respectively. All regressions in Panel A include the bias correction and robust standard errors of Calonico et al. (2014). Panel B reports parametric OLS estimates that vary the polynomial degree consistently with Panel A. Columns 1 and 5 include no controls. All the other columns include pre-determined controls: Columns 2 and 6 include geographic controls (altitude, average historic rainfall, distance (in km) to Bogotá and to the closest marketplace, and region dummies (Caribbean, Eastern, Andean, and Pacific); Columns 3 and 7 include socio-economic controls (vote share for left and right presidential candidates in 1994 elections, rurality index, total population, literacy index in 1993, presence of coca plantations in 1994, and historic incidence of political violence during *La Violencia*). Columns 4 and 8 include all the controls simultaneously.



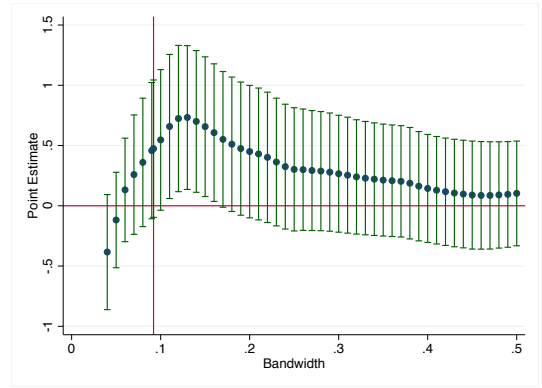
### 1.12.3 Dropping Recurring Municipalities

Figure 1.7: Effect of electing a left-wing mayor on violence (dropping recurring municipalities)

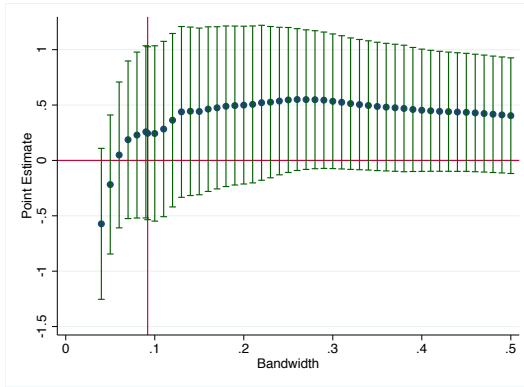
Panel A. Paramilitary attacks during term in office



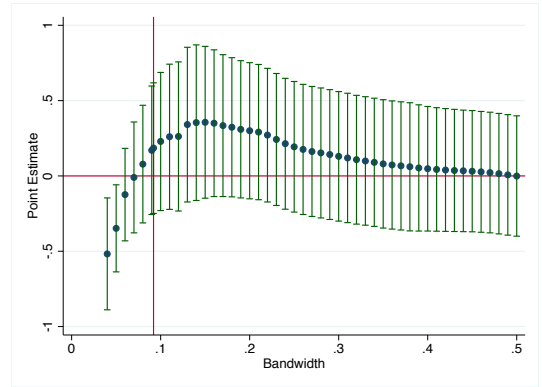
Panel B. Paramilitary attacks in previous term



Panel C. Guerrilla attacks in previous term



Panel D. Government attacks in previous term



Notes: The solid line marks the Calonico et al. (2014) optimal bandwidth. Non-parametric estimates with bias correction, robust standard errors, triangular kernels, and linear local polynomials (Calonico et al., 2014).

### 1.12.4 Alternative Interpretations: right wing parties, new parties and the UP

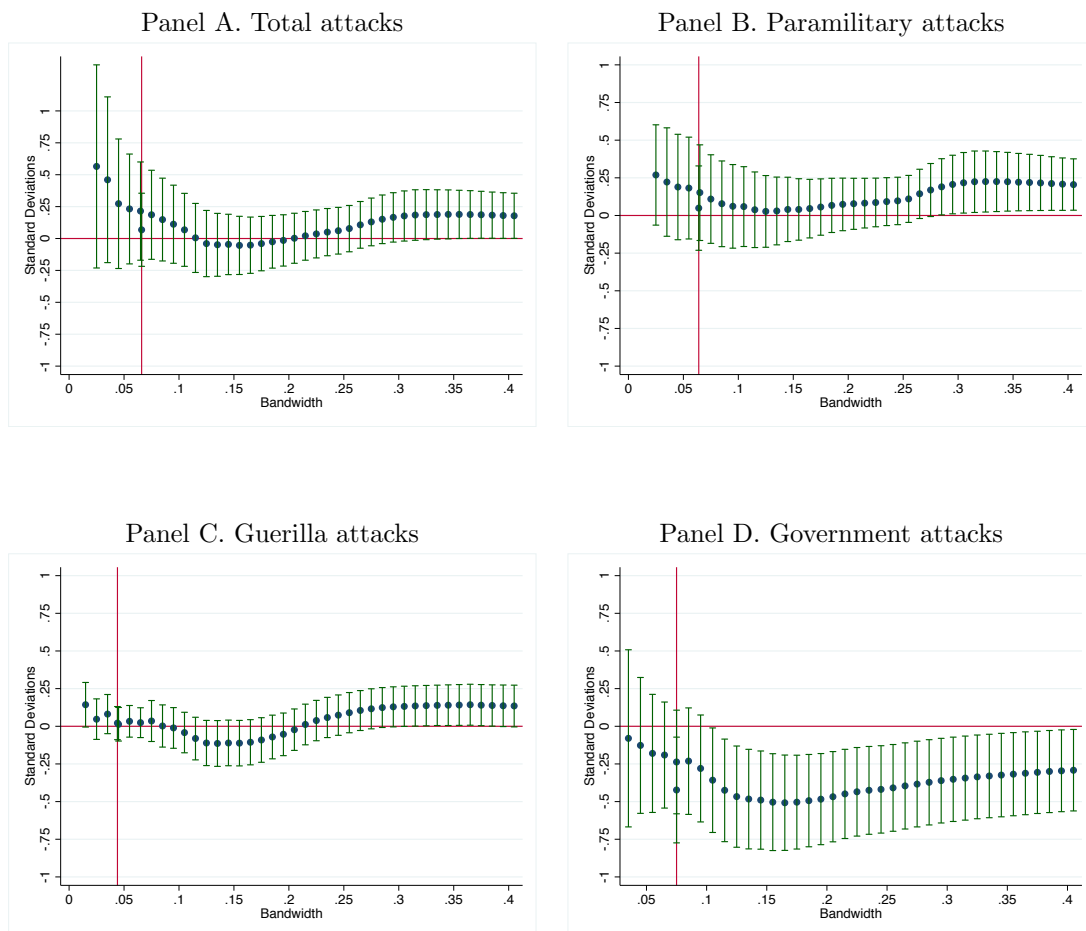
Table 1.12: Descriptive statistics of the main variables  
Sample: Electoral races in which right-wing parties are winners or runners-up: 1997 - 2014

Variable	Mean	Standard Deviation	Minimum	Median	Maximum
Panel A. <i>Average yearly attacks per 100,000 inhabitants during government period</i>					
Total attacks	1.910	6.451	0.000	0.000	93.864
Paramilitary	0.597	3.595	0.000	0.000	76.573
Guerrilla	0.511	2.143	0.000	0.000	30.315
Government	0.211	1.285	0.000	0.000	15.803
Panel B. <i>Forcing Variable</i>					
$\frac{Votes\ right - Votes\ non-right}{Votes\ top\ 2}$	-0.000	0.113	-0.405	0.001	0.372
$ \frac{Votes\ right - Votes\ non-right}{Votes\ top\ 2} $	0.088	0.070	0.000	0.072	0.405
Panel C. <i>Forcing Variable within bandwidths</i>					
$\frac{Votes\ right - Votes\ non-right}{Votes\ top\ 2}$	0.001	0.036	-0.066	0.000	0.066
$ \frac{Votes\ right - Votes\ non-right}{Votes\ top\ 2} $	0.031	0.019	0.000	0.029	0.066

**Notes:** Number of observations: 838 in Panels A and B (for 634 municipalities) and 386 in Panel C. The sample in Panels A and B is the set of mayoral elections where a right-wing candidate was the winner or runner up and the corresponding variable is available. Panel C in addition restricts the sample to the Calonico et. al (2014) optimal bandwidth for our baseline estimates of the effect of right wing victories on total attacks (by all groups) with first-degree local polynomials.

**Figure 1.8: Effect of electing a right-leaning mayor on violence**  
**Robustness to bandwidth selection**

Average yearly attacks (per 100,000 inhabitants) during term in office



**Notes:** Attacks by paramilitary or guerilla groups (per year and per 100,000 inhabitants) during the 3 years preceding each election (90% confidence bands). The solid line marks the Calonico et al. (2014) optimal bandwidth, the dashed line the Imbens and Kalyanaraman (2012) optimal bandwidth. Non-parametric estimates with bias correction, robust standard errors, triangular kernels, and linear local polynomials (Calonico et al., 2014).

Figure 1.9: McCrary test: Sorting around the winning threshold for the right

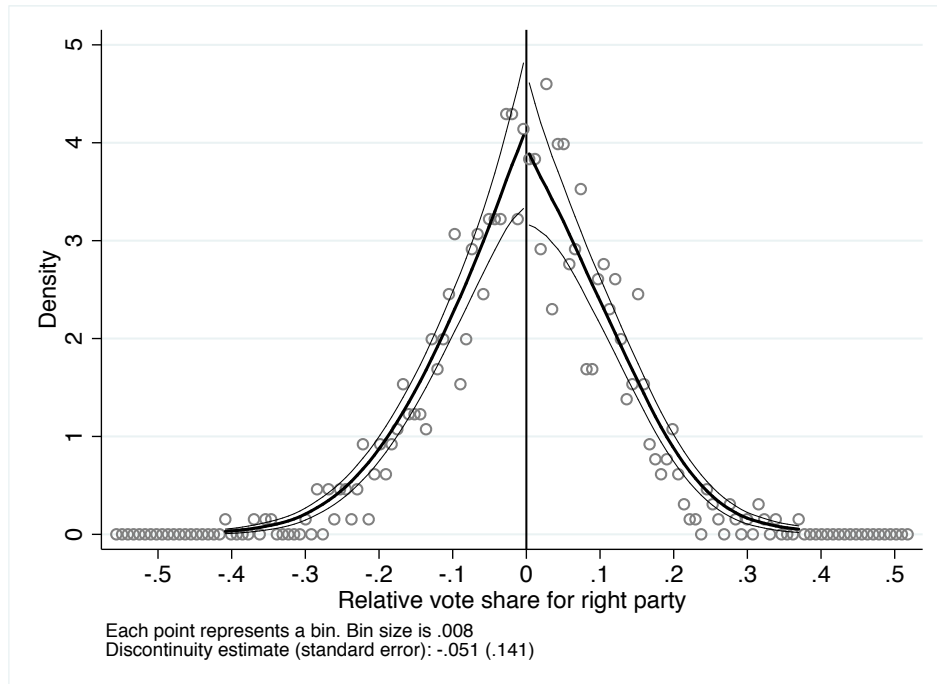


Table 1.13: Effect of electing a mayor from a new (non-left) party on paramilitary attacks

Dep. variable: <i>Average yearly paramilitary attacks per 100,000 inhabitants during term in office</i>				
	Non-parametric estimates		Parametric estimates	
	(1)	(2)	(3)	(4)
(Non left) New party elected	0.475 (0.334)	0.446 (0.424)	0.516* (0.286)	0.300 (0.410)
Observations	1099	1268	1100	1100
Bandwidth	0.0759	0.0941	0.0760	0.0760
(Local) polynomial order	1	2	1	2

**Notes:** Standard errors in parentheses. \* is significant at 10%, \*\* 5%, and \*\*\* 1% level. Calónico et al. (2014) optimal bandwidth in all columns. Triangular kernel weights used in all columns. In Columns (1) and (2) the (unknown) polynomial is approximated with local linear and quadratic polynomials, respectively, and include the bias correction and robust standard errors of Calónico et al. (2014).

**Table 1.14: Effect of electing a left-wing mayor on paramilitary attacks  
(Differential effect of UP)**

Dependent variable: <i>Average yearly paramilitary attacks per 100,000 during term in office</i>		
	(1)	(2)
Left-wing mayor elected	2.660* (1.521)	4.558** (1.938)
Unión Patriótica (UP)	-0.657 (0.916)	-0.736 (1.049)
UP × Left-wing mayor elected	14.51 (11.65)	14.88 (11.60)
Observations	157	157
R-squared	0.145	0.154
Bandwidth	0.0930	0.0930

**Notes:** Standard errors in parentheses. \* is significant at 10%, \*\* 5%, and \*\*\* 1% level. [Calonico et al. \(2014\)](#) optimal bandwidth and triangular kernel weights in all columns. In Columns 1 and 2 the (unknown) polynomial is approximated with local linear and quadratic polynomials respectively. We report bias correction and robust standard errors of [Calonico et al. \(2014\)](#).

## 1.12.5 Incumbency disadvantage

**Table 1.15: Incumbency advantage in Colombia using alternative approach (not conditioning on past incumbency)**

	Non-parametric estimates				Parametric estimates	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent variable: <i>Indicator of whether party elected in t runs and wins in t + 1</i>						
Winner party in t	-0.0391 (0.0269)	-0.0388 (0.0288)	-0.0443* (0.0230)	-0.0423 (0.0261)	-0.0547*** (0.0183)	-0.0450* (0.0262)
Left-wing party					-0.205*** (0.0204)	-0.205*** (0.0204)
Winner party in t × Left-wing party					0.0127 (0.0243)	0.0125 (0.0244)
Observations	5508	7842	5504	7834	7834	7834
R-squared			0.002	0.002	0.009	0.009
Bandwidth	0.0840	0.146	0.0840	0.146	0.146	0.146
(Local) polynomial order	1	2	1	2	2	2
Panel B. Dependent variable: <i>Indicator of whether party elected in t runs in t + 1</i>						
Winner party in t	0.0387 (0.0315)	0.0461 (0.0349)	0.0347 (0.0265)	0.0411 (0.0314)	0.0314 (0.0219)	0.0382 (0.0314)
Left-wing party					-0.335*** (0.0218)	-0.335*** (0.0219)
Winner party in t × Left-wing party					-0.0414 (0.0292)	-0.0412 (0.0293)
Observations	5750	7682	5766	7692	7692	7692
R-squared			0.001	0.002	0.018	0.018
Bandwidth	0.0900	0.141	0.0900	0.141	0.141	0.141
(Local) polynomial order	1	2	1	2	2	2
(Local) polynomial order	1	2	1	2	1	2

**Notes:** Standard errors in parentheses. \* is significant at 10%, \*\* 5%, and \*\*\* 1% level. [Calonico et al. \(2014\)](#) optimal bandwidth in all columns. Triangular kernel weights used in all columns. In columns (1) and (2) the (unknown) polynomial is approximated with local linear and quadratic polynomials, respectively, and include the bias correction and robust standard errors of [Calonico et al. \(2014\)](#). Columns (3) - (6) are parametric estimates with polynomials of the forcing variable not displayed. (3) & (5) Linear estimates with varying slopes. (4) & (6) Quadratic estimates with varying slopes.

# Chapter 2

## The Power of Money. The consequences of electing a donor funded politician<sup>1</sup>

NELSON A. RUIZ<sup>2</sup>

### Abstract

Do privately donor-funded politicians benefit donors disproportionately? This paper examines the impact of electing donor-funded politicians using a novel dataset that uniquely links campaign donors and recipients of public contracts during a mayor's incumbency period in Colombia. Using a regression discontinuity design (RDD) that compares close elections between politicians who received campaign donations and those who *did not*, I find that electing a politician who received donations more than doubles the probability of donors receiving contracts from a mean of 5.9% to 15.5%. The benefits to the donors of the donor-funded politician are significant: on average total contracts received is *13.75 times* the value of their donations. From a social welfare point of view, electing a donor-funded politician does not affect local government deficits and it has no impact on investment, however they are more likely to have disciplinary sanctions against them and investigations related to public procurement. Moreover contracts for donors have a short duration, have an inflated value compared to similar contracts for non-donors, and tend to be given under a minimum value threshold modality, in which there is less screening and lower chances of competitors bidding for contracts. In order to isolate the effect of donor money from other personal characteristics associated with being donor funded, I employ a fuzzy RDD approach that uses arbitrary campaign limits as an instrument for the candidates' proportion of donor funds. Results indicate that looser campaign limits lead to more participation of donor funding in campaigns, and as a result reduce the effect of favoring donors with contracts. This paper shows evidence that political selection of donor-funded candidates can have repercussions in public procurement, and that campaign limits can limit the influence of money in politics.

---

<sup>1</sup>Thanks to Carlos Cardona for great research assistance and Paula Castaneda for helping with the coding of sanctions. I am grateful for their comments to Karun "King" Adusumilli, Oriana Bandiera, Jean-Paul Faguet, Dominik Hangartner, Selina Hofstetter, Ethan Ilzetzki, Jaakko Merilainen, Clement Minaudier, Liliana Olarte, Pablo Querubin, Livia Schubiger, Sandra Sequeira, Abbey Steele, Janne Tukiainen, Miguel Uribe, Juan F. Vargas and Stephane Wolton. Thanks also to the participants at the EPSA 2017 annual conference, IEB Political Economy and Fiscal Federalism Conference, Oxford-Nuffield Graduate Student Workshop, LSE-NYU Annual conference, LSE Comparative Politics Workshop, LSE Political Economy Political Science (PSPE) work in progress, and LSE STICERD work in progress seminar. Finally, thanks to the invaluable effort of Colombian public institutions for making this information public.

<sup>2</sup>International Development Department, London School of Economics and Political Science. 6-8th Floors, Connaught House, Houghton Street, London WC2A 2AE. Tel: +44 (020) 7955 6565, Email: [n.a.ruiz-guarin@lse.ac.uk](mailto:n.a.ruiz-guarin@lse.ac.uk)

## 2.1 Introduction.

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

Recent events have placed the study of money in politics at the forefront of the policy debate. In the 2016 US election, Hillary Clinton was perceived to be too close to donors, which some analysts argue affected her chances of winning.<sup>3</sup> Peru’s ex-president, Alejandro Toledo, was recently found guilty of receiving money from the Brazilian construction giant Odebrecht, and then awarding the company a big contract to build a transoceanic road, that came under public scrutiny for its poor construction and cost overruns. It was initially forecast to cost \$800M USD and ended up costing around \$2,000M. According to Peru’s Ministry of Interior, the president received \$20M dollars, which would imply the contract was *100 times larger* than the money received.<sup>4</sup> Is electing a donor-funded politician detrimental to the economy?

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

What does the evidence say of the potential costs (or benefits) of electing donor-funded politicians? How do donors benefit? Progress in the empirical literature is difficult due to severe data limitations: it is difficult to observe systematically how politicians benefit donors. Furthermore it is difficult to disentangle whether companies benefited from giving money to politicians, or whether their success is due to other potential explanations.<sup>5</sup> Despite the lack of conclusive evidence, countries have enacted

---

<sup>3</sup>BBC News, “Is Wall Street a problem for Hillary Clinton?”, April 14, 2016. Available [here](#).

<sup>4</sup>BBC News, “Juez de Perú ordena el arresto del expresidente Alejandro Toledo por caso Odebrecht,” February 10, 2016. Available [here](#).

<sup>5</sup>It is difficult to assess whether these companies would still have benefited if their funded politicians had *lost* the election. For example, companies could benefit from the economic stability produced by an election, or their economic success may happen to coincide with the timing of an election.



limits to money in politics -in particular- campaign contributions, with the premise that they will reduce the cost of society of money in campaign. But, are they effective?

I make progress on these issues by constructing a novel dataset that links campaign donors and contractors during a mayor's incumbency period in Colombia (2012–2015) using unique national identifiers (IDs). I also make progress by exploiting close races between politicians who received campaign donations by private donors and those who *did not*<sup>6</sup> using a RDD. I also exploit arbitrary campaign limits (which jump discontinuously at arbitrary voter cut-offs)<sup>7</sup> as an instrument for the candidates' proportion of donor funds using a fuzzy RDD. The latter allows me to study the effects of campaign limits and disentangle the effect of campaign donations, from the politician personal characteristics associated with being donor funded. Using detailed contracting data, I study the procurement process for donors and how they could enjoy a price premium for the same type of contract executed. Therefore this paper aims to respond two closely linked questions: **i)** What are the consequences of electing a donor-funded politician? (Consequences in terms of contract assignment, economic policy, corruption, procurement process and contract cost). **ii)** Can campaign contribution limits reduce the influence of money in politics?

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

---

Recent papers that address this concern are: [Boas, Hidalgo, and Richardson \(2014\)](#); [Szakonyi \(2016\)](#). However compared to those papers, this paper focuses on the selection of donor-funded politicians vs. non-donor funded, rather than the effects of general electoral victory.

<sup>6</sup>Since not all politicians are recipients of campaign donations

<sup>7</sup>In particular at 25,000 registered voters cut-off, the campaign limits increase from 58M to 110M COP. 1M COP is equivalent to \$350 USD. And each donor can contribute up to 10% of the maximum campaign total.

less transparent way compared to regular contracts.<sup>8</sup> Compared to regular contractors, I find that donors on average receive more minimum value contracts, contract in less number of municipalities (are more local), and contract in more sectors of the economy, lacking specialization. Finally using text analysis, I match similar contracts for donors and non-donors within donor-funded incumbencies, and I find that donors enjoy a price premium of 2 M COP, which is equivalent to approximately 2 average monthly wages in the municipality.<sup>9</sup>

In the case of evaluating campaign limits, evidence shows that loosening campaign limits from 58M to 110M does not change the underlying characteristics of the winner politician, showing that more money in politics does not change political selection, however it does increase the participation of donor campaign funds by 20%, which in turn leads to an increase in the number of contracts given to donors. This is an important finding, because this is the first paper -to best of my knowledge-, to causally estimate the effect of campaign limits (donations) on benefiting donors, which could be considered the main purpose of campaign limits: limiting the influence of donations on public procurement.<sup>10</sup>

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

## 2.2 Related literature

Money can influence public policy through bribes, lobby, or campaign contributions, and there can be a large prize given that over 15% of the world's GDP is spent on public

---

<sup>8</sup>This is in sharp contrast with a regular bid, where it is required to publish call for applications between 5 to 10 working days, this call has to be in the on-line system an evaluation committee can be used, and the award of the contract has to be justified publicly.

<sup>9</sup>However a potential limitations of just looking at price, is that the price differential could be justified by a quality differential. However the lack of specialization in an economic sector of donors, could imply that it is less likely that quality of goods provided by donors are of a higher quality.

<sup>10</sup>Avis, Ferraz, Finan, and Varjão (2017) estimates the effects of campaign limits on the number of candidates and how competitive the election is, but not if the donor-funded politicians benefit their donors or general corruption.

procurement (Baltrunaite, 2016). According to Witko (2011), there is a broad range of anecdotal evidence regarding how donating money to election campaigns has led to politicians benefiting the interests of donors, which in turn has resulted in corruption cases. However, campaign donations have the potential to play a positive role: they could serve as a mechanism to select the most prepared politicians for office, and those politicians may in turn select the donor companies that could be effective at execution. Furthermore, when a company donates to a politician, this could potentially reduce the risk of adverse selection, because companies and politicians could determine each other's capacities and intentions in advance. Therefore it could be better to give a contract to a known company rather than an unknown company that may have less capacity.

Studying how politicians reward donors is challenging, because it is difficult to measure what the exact benefits is. According to Stratmann (2005) there have been two common approaches in the literature: looking at politicians' roll-call votes and the financial performance of donor companies. Regarding the former, the premise is that politicians will vote in ways that benefit their donors. However, a large survey conducted by Ansolabehere, De Figueiredo, and Snyder (2003) shows that most studies find no effects of campaign contributions on voting.<sup>11</sup> A potential issue with using roll-call votes is that it is difficult to assess whether campaign contributions go to candidates who are ideologically aligned with a company, or if the donations truly changed a politician's position.<sup>12</sup> Another difficulty of using roll-call votes to measure benefits to donors is that voting for legislation in favor of donors does not necessarily produce immediate benefits. There could be a lag before the legislation is implemented and the company realizes any benefits. Therefore it is uncertain when benefits (if any) will materialize, and when they should be measured.

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

---

<sup>11</sup>(Stratmann, 2005) uses an alternative methodology to perform a meta-analysis on studies of campaign contributions and roll-call votes, and shows that campaign contributions affect politicians' voting records.

<sup>12</sup>To address this concern, Stratmann (2002) has used within-politician variations in contributions, and Ansolabehere et al. (2003) has employed an instrumental variable approach.

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

This paper contributes to the literature that shows that elected leaders *do* make a difference (see [Jones and Olken, 2009](#)). Most of the studies focus on the effects of diverse politician types: education of leaders ([Besley, Montalvo, and Reynal-Querol, 2011](#); [Freier and Thomasius, 2012](#)), women ([Ferreira and Gyourko, 2014](#); [Bagues and Campa, 2017](#); [Chattopadhyay and Duflo, 2004](#)), their professional background ([Matter and Stutzer, 2015](#)), and their minority status ([Pande, 2003](#)). This is the first paper to causally estimate the effect of electing a politician is donor-funded.

### 2.2.1 Donors and candidates

Why do individuals or companies donate to politicians? [Ansolabehere et al. \(2003\)](#) argue that campaign contributions can be considered a consumption activity in which individuals mostly gain from participating in the democratic process. Most formal models (for example see [Snyder Jr, 1990](#); [Grossman, 2002](#); [Coate, 2004](#); [Ashworth, 2006](#)) assume campaign contributions are used to influence politicians’ policy stances or obtaining favors.<sup>13</sup> From a politician’s perspective, accepting donations could allow them to further advertise their campaign and qualifications for office (see [Ashworth \(2006\)](#) or [Coate \(2004\)](#)), which could increase their chances of winning the election. If donations can potentially benefit both donors and candidates, why are there candidates that are non-donor funded?

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

---

<sup>13</sup>Which could entail moving to a certain policy stance closer to the donor’s interests, or directly conferring contracts to donors.

financing.

Another potential explanation is that donors only give money to candidates who are ideologically aligned with them. For example, donors may prefer financing a pro-enterprise candidate rather than a leftist candidate who would prefer a higher taxation rate. In my case I find that more right-wing<sup>14</sup> politicians are donor funded. This is in line with US evidence reviewed in (Stratmann, 2005), in which campaign donors fund candidates who share their ideological preferences.

Donor funded candidates –if they win office– could be willing to give contracts to their donors in exchange for support given during elections (Snyder Jr, 1990). For the incumbent, awarding contracts is low cost since it is public money and in the case of Colombia there can be low risk of punishment, given the lack of control offices in local municipalities. Moreover since mayors serve only a single term in Colombia, they do not worry that their actions while in office will harm their chances of re-election. An immediate source of benefits for elected politicians, is that they could ask a % of the conferred contract directly to the donor/contractor as further payback. There is anecdotal evidence that shows that elected politicians do so.<sup>15</sup> For the elected politicians there could be also long term benefits to politicians where donor companies may employ politicians once they leave office. Evidence by Eggers and Hainmueller (2009) for the UK shows that politicians can be hired by companies once they leave office.

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

## 2.3 Context

### 2.3.1 Colombian institutions and electoral context

Colombia recently experienced political and economic decentralization (Bushnell, 1993). In 1986 there was introduction of elections for local mayors in municipalities. This allowed for first-past-the-post elections, in which every party could put one candidate

---

<sup>14</sup>For a classification of ideology see see Fergusson, Querubin, Ruiz, and Vargas (2017))

<sup>15</sup>This is informally known as “the bite”, see discussion here: La Silla Vacía, “Santos, su Ñoño y su Musa”. Available [here](#).

name forward. In 1991, a new constitution allowed increased social spending and decentralization of fiscal resources to the regions. The new constitution made municipalities and departments jointly responsible for the provision of basic public services (Faguet and Sanchez, 2008). It also allowed new political parties to play a greater role through an increased presence at the local level (Hoyos, 2005a). In recent years there has been an increase in the number of parties participating in local elections.<sup>16</sup>

As a result, Mayors are currently responsible for designing the budget and implementing an annual development plan in the municipalities. Although most municipalities receive transfers from the central government that are tied to specific expenses or the central government spends directly in the municipality, mayors have discretion over an average of 20.24% of all local spending<sup>17</sup>. According to (Martinez, 2017b), most discretionary resources come from property tax revenues, which are used for the provision of education, health insurance, water, and sanitation projects as well as the functioning of the municipality. Most public services at the municipality level are provided by contracting third parties. There are three main forms of contracting in Colombia: 1. An open-bid process in which applicants submit their proposals. The call for applicants have to be opened for 5 to 10 working days, and has to be published in an on-line reporting system. A committee can select the winner, and the award of the contract has to be justified publicly. 2. Minimum value modality contracts. This applies when the size of the contract is below 10% of the total municipality budget. It is required to publish the call for applicants only for a day or more, it is not required to publish the call in the national on-line system, the sole criteria for awarding contracts is the lowest bidder, and there is no need of a committee to evaluate proposals. 3. A non-bid process that involves specific waivers that need to be formally justified<sup>18</sup>. In practice, 83% of all contracts during the 2011 mayoral incumbency period were given by the non-bid modality: 53% using waivers and 30% were the minimum value threshold.

In order to limit the influence of money in politics, Colombian law<sup>19</sup> establishes limits for both total campaign contributions and individuals contribution size. The National Electoral Commission sets the campaign limits for each election. These limits jump discontinuously at arbitrary registered voter cut-offs, for example at 25,000 registered voters the campaign limit increases from 58M to 110M COP.<sup>20</sup> In addition, individual donors cannot give more than 10% of the total campaign limit.<sup>21</sup> Limits are

---

<sup>16</sup>An average of 4.4 parties contested the 2011 mayoral elections.

<sup>17</sup>Data from 2012-2015 incumbency period.

<sup>18</sup>The waiver list applies to: 1. The acquisition or supply of goods and services of uniform technical characteristics and common use by entities; 2. Contracting in which the tender process has been declared abandoned; 3. Contracts for the provision of health services; 4. Goods produced by or intended for agricultural purposes, offered on legally constituted product exchanges; 5. The contracting of goods and services required for defense and national security; and 6. Disposal of assets.

<sup>19</sup>Article 28 of Law 130 of 1994.

<sup>20</sup>Subsequently at 50,000 registered voters the limit jumps to 330M COP; at 100,000 registered voters the limit jumps to 659M COP; at 250,000 the limit jumps to 745M COP; at 500,000 the limit jumps to 1,318M COP. For the capital city of Bogotá the limit is 1,646M COP.

<sup>21</sup>According to article 23 of Law 1475 of 2011.



announced before campaigning starts. For the 2011 elections campaigning was allowed from the 30th of July; voting took place on 30th October 2011, and mayors did not take office until 1st January 2012, therefore I measure the outcomes during the 2012–2015 incumbency period.

## 2.4 Empirical strategy and data

### 2.4.1 Data

I use electoral data compiled by Pachón and Sánchez (2014), gathered from the Colombian national electoral authority, the *Registraduría Nacional del Estado Civil*. This data contains the results for mayoral elections for all municipalities in Colombia for 2011. Additionally I gathered data reported by candidates to the the National Electoral Commission, on sources of income and expenditures of political campaigns. This comes from a new campaign reporting system financed by the US Agency for International Development in collaboration with Transparency International. This data is available publicly and its intention is to increase transparency to the public in campaign finance sources. In order to increase compliance, campaign finance reporting is mandatory by law since 2009.<sup>22</sup> Parties have to electronically submit this information within two months after the election<sup>23</sup>, and subsequently the physical evidence of each source of campaign income and spending. The National Electoral Commission fines candidates or parties that do not comply with the reporting requirements.<sup>24</sup> The data reports the donors' unique national ID numbers<sup>25</sup>, which allows me to link candidates with publicly available information on contracting.

Table 2.1 illustrates the types of sources of campaign revenue reported in campaign reporting finance forms. For example, candidates can self-fund their campaigns, obtain personal bank loans, the party can organize fund-raisers, receive state funding, and/or receive donations by companies and/or people. In order to generate the counterfactual, I separate candidates who received campaign donations (code 102 in Table 2.1) from those who *did not*.<sup>26</sup>

There are 1,098 municipalities in Colombia, in order to implement the RDD, I limit the sample to races in which the mayoral winner and runner-up candidates report

---

<sup>22</sup>Resolution 1094 of 2009.

<sup>23</sup>Article 25, Law 1475 of 2011

<sup>24</sup>Compliance is fairly high: out of 4,460 mayoral candidates in 2011, 89% reported campaign information. However, the commission's capacity to fine candidates who do not submit the information was removed in late 2012, which could limit the compliance after for the 2015 electoral period.

<sup>25</sup>In Colombia a unique national ID is assigned when a person turns 18 and is used for a many purposes such as getting a mobile phone line, obtaining health care, or a loan. IDs are also assigned to companies, and their assignment is mandatory to conduct any business. When a person owns a company, the same ID is used for the individual and for the company

<sup>26</sup>As a robustness check I separated code 101, and coded family members as donors, but the results remain unchanged.

their campaign income (966).<sup>27</sup> In order to implement the RDD, I must first ensure that I have enough power – i.e. that enough races are decided between a candidate who had donors and one who did **not**. Out of the 996 municipalities that report information on campaign financing sources, there are 408 such races, while there are 209 races between donor-funded politicians and 379 between non-donor-funded politicians. I concentrate on the first group since I am interested in comparing two different *politician types*. One potential concern is that these close races are clustered in a certain region of the country, but this is not the case. A map of the full sample is available in Figure 2.3, and a sample of those within a winning margin of 6.5%<sup>28</sup> in Figure 2.4. Furthermore, in the sample of a narrow margin I find no spatial autocorrelation. The Moran I index, available in Figure 2.5, indicates that municipalities included in the RDD estimate are randomly distributed. Another potential concern is that municipalities that are contested between donor-funded candidates and non-donor-funded candidates (408) are not representative of the entire country. I check with a broad range of municipality covariates (see Table 2.31), and there are no statistically significant differences between the municipalities included in the sample and those that are excluded, which strengthens the external validity.<sup>29</sup>

Comparing sources of financing across treatment donor-funded politicians and non-donor funded politicians (see Table 2.3), winners without donors finance on average 95% of their campaign using their own resources. When there are donor funds involved, on average they represent 38% of the campaign financing, thus reducing the burden on the candidate and their families. It is important to note that state financing is almost non-existent in both the control and treatment groups, which increases candidates' reliance on donor funds and self-financing. Also parties, on average, contribute only 3% of campaign funds<sup>30</sup>. In addition, having donors increases the amount of disposable income available by campaigns by over 8.7M COP, and are able to finance more public events (see Table 2.4).

In order to uniquely link campaign donors to contracts assigned in a municipality, I obtained detailed data on contracting, which was gathered in order to increase transparency in public procurement.<sup>31</sup> This data reports the entity in charge of contracting, the contractor (and their unique ID), under which modality the contract was made, the broad sector of the economy, the size of the contract, the detailed purpose of the contract, the length of the contract, whether it was completed, and/or overrun in costs. Figure 2.1 presents an example of information contained in the contracting

---

<sup>27</sup>Table 2.30 shows that the places where there is no reporting by the top two candidates, are more remote, are more rural, and have more unmet basic needs. The lack of reporting can be associated with the level of development in the municipality.

<sup>28</sup>For a discussion of bandwidth choice see the RDD set-up section

<sup>29</sup>This result is not surprising, considering that the Morans I spatial autocorrelation index shows that the municipalities in the narrow sample are randomly distributed.

<sup>30</sup>Hangartner, Ruiz, and Tukiainen (2017) interviewed candidates for local councils, and in many instances were told that parties just lent their credentials so candidates could run in the election.

<sup>31</sup>I am grateful to *datos abiertos* online portal, for posting the contracting data.



form. I dropped contracts that were assigned by the national government, and examine only those under the municipality's jurisdiction since mayors are not in charge of the contracts executed by the national government. A summary of the contract data descriptive statistics can be found in Panel B of Table 2.2.

A key aspect of this research is linking the donor and checking whether it is the same person/company getting a contract. In Colombia, two types of legal entities can contract with the State: individuals and companies, both with unique IDs. If an individual donates money to a candidate and then receives a contract, the same unique ID is used; when an individual gives a donation and his/her company receives a contract, I can also link them uniquely since the same number is used for the person and their company. This unique feature of Colombia allows people to be linked with their companies even if they have different names. The only link that cannot be made is between individuals and public companies or companies with multiple owners: It could be the case that one of the owners gives a donation and then the company receives the contract <sup>32</sup> Figure 2.2 illustrates which links were found.

Using the links above, I coded whether *any donor* – for the winner or runner-up candidate in the mayoral election – received a contract. This is to take into account the fact that donors of non-winner candidates also get contracts, since they can bid for contracts. If contracts are given to the most economically competitive contractor, the probability of receiving a contract would be orthogonal to the contractor making a donation and their funded candidate winning the election. However it is not the case: Table 2.6 shows that when a donor funded politician is elected 11.71% donors of the winner politician obtain contracts, while only 1.13% runner-up "donors" obtained a contract. Therefore if I find that electing a donor-funded politician has an effect on awarding contracts for donors, it would mostly be driven by contracts awarded to the donors of donor-funded politicians. In the case when non-donor politician funded is elected, 3.58% of donors receive contracts, driven by contracts given to donors of the runner-up. Interestingly even when donating to the loser politician companies hold a positive probability of obtaining a contract. Why? donor companies of the opposing candidate could still be competitive in winning public procurement contracts, or they could receive contracts by the elected politician to attract support for the party for the following election. However given the lack of reelection and weak party system in Colombia the latter is less plausible.

In order to measure politicians underlying characteristics, I made an extensive effort to obtain candidate-level data. The registry office only holds data on the gender of candidates. I obtained data on disciplinary sanctions from *Procuraduría*, which is the main public watchdog in charge of prosecuting corruption charges. Via a formal requirement, I requested the entire history of disciplinary *sanctions* for all mayoral candidates in the 2011 election and the date the sanction was executed, which allowed me to code

---

<sup>32</sup>Of all local contracts given in the 408 municipality sample, 9.9% were for a multiple owner companies. These companies can donate themselves, and(or), one of the owners can donate.

whether the candidate had been sanctioned before or after holding office. On average, only 2.5% of the mayors in the sample were found guilty of disciplinary sanctions after they entered office. Disciplinary sanctions can happen for a variety of reasons, for example if a mayor does not reply to a formal information request by citizens, running for office without having the legal entitlement to do so, contracting improperly, or any extended violation of the law. In order to code whether the sanctions and investigations were related to contracting, I followed the methodology by (Martinez, 2017b)) and web-scraped from the official Procuraduría website<sup>33</sup> all the public bulletins of investigations or sanctions after the 2011 period. Using the mayor's name, I organized all bulletins associated with a particular mayor. Then using QDA miner and Word-Stat and stemming analysis, I searched all bulletins of investigations or sanctions that contained the root of the word "contract" and coded as sanction/investigation related to contract if the root of the word contract appeared in the news bulletin. I further refined the coding by manually reading the contract-related bulletins and verifying that the investigation/sanction was indeed related to contracting.

In addition to the disciplinary sanctions, I obtained information on candidates voting registration available at *Registraduría del Estado Civil* and I coded if the candidate was registered to vote and if he/she had previously illegally registered to vote.<sup>34</sup> In order to obtain further individual covariates, I requested all 1,098 mayoral election ballots for the 2011 election, which contain politicians' pictures, from the National Registry Office. A sample can be found in Figure 2.6. Using a Python facial detection API algorithm, I obtained an estimate of gender, age, and race from the politicians' pictures. In order to validate these results, I compared the self-reported gender to the gender predicted by the algorithm and there was less than a 3% difference. For the sample, descriptive statistics in panel D Table 2.2 show that less than 11% of mayors were female during the study period, 12% were categorized with indigenous background,<sup>35</sup> and 5% are black. The average politician age is 44. I used the coding used in Fergusson et al. (2017) to measure ideology. In the study sample, 22% of mayors were classified as right-wing, while only 10% were classified as left-wing. I also wanted to code candidates' experience in politics, since it could be the case that donors choose to give money to more experienced candidates. To do so, I exploit the fact that all descendants from the Spanish use two last names<sup>36</sup> to code the number of times the candidate participated in elections and held office. In order to account for the possibility of homonyms in full names, I coded any names that appeared twice as candidates in the same electoral year as missing, since it is only legally possible to be

---

<sup>33</sup><https://www.procuraduria.gov.co/portal/>

<sup>34</sup>That is, the person either used a dead person's ID to vote, changed his or her registration ballot to another municipality in exchange for money, or tried to vote while underage. The most common fault is moving to another municipality to vote.

<sup>35</sup>There algorithm predicted 12% Asians, but there are few Asians in Colombia. A further investigation of the pictures showed that they were actually Colombians with a distinct indigenous background.

<sup>36</sup>One from each parent

candidate for a single mayoral post. Only two candidates had exactly the same name and both last names in the 2011 election.

Finally, to obtain municipality-level predetermined covariates, and potential policies enacted by mayors, I obtained a municipality-level panel (2011–2014) with a broad range of economic, social and institutional covariates, thanks to a pre-existing effort by *Universidad de los Andes*. In order to complete the fiscal data for 2015 (since the mayoral period runs from 2012–2015, and there was a year missing), I requested the fiscal data from the National Department of Planning and created averages during the incumbency period of different fiscal variables. I measure predetermined covariates in 2011, before the Mayor term starts in 2012.

Extended descriptive statistics of the base sample of 408 municipalities are available in Table 2.2. On average, 17.2% of donor are awarded contracts. Of the contracts given in a municipality, on average 40% were given directly with waivers that have to be justified and 50% by minimum value modality, while only 10% are open to competitive bid. On average, municipalities invest 86% of their budget; most of the remaining income is for administrative operations.<sup>37</sup>

## 2.4.2 RD Design

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

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

Since the campaign reporting system has only been in place since 2009, I can only use the 2011–2015 election period. I check the mayors’ funding sources as well as the effects on ( $Y_i$  in eq (3)): contract assignment (explained above), disciplinary sanctions, whether the investigations/sanctions are related to contracting, the types of policies implemented. I also look at contract level data to check the difference contract modality assignment for donors and non-donors during the 2012–2015 incumbency period.

---

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

A potential concern with this analysis is that there is systematic misreporting of donors that leads to misclassification. Candidates with donors are most likely to *under-report* donations, and therefore be misclassified as non-donor-funded politicians, while I would not expect candidates to report donors who did not donate<sup>38</sup>. Therefore I would expect several control units to be classified as treatment. If I was able to classify these candidates correctly, I would expect to find even bigger effects of electing a donor-funded politician because I would observe more potential links between donors and contractors, so the effect I are finding represents a lower bound.

This section of the paper focuses on *close* races in which the donor-funded (DF) candidate wins or is the runner-up.<sup>39</sup> I count the winner and runner-up votes and let:

$X_i$  = be the vote share of the DF politician in municipality  $i$  minus the vote share of non-DF.  $X_i$  = is centered around 0, so  $DF_{it} = 1$  if  $(X_{it} > 0)$ . In particular:

$$DF_i = \begin{cases} DF_i = 1 & \text{if } X_i > 0 \\ DF_i = 0 & \text{if } X_i < 0 \end{cases} \quad (2.1)$$

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

If there is no manipulation of the electoral outcome near the margin (i.e., it is not the case that donor-funded politicians always barely win), and if there are no other predetermined factors that vary discontinuously when donor-funded politicians win, the RDD allows me to estimate the causal effect of electing a donor-funded politician:

$$\alpha = \lim_{x \downarrow c} E[Y_1 | X = c] - \lim_{x \uparrow c} E[Y_0 | X = c], \quad (2.2)$$

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

$$y_i = \alpha + \beta_1 DF_i + \beta_2 f(X_i) + \beta_3 DF_i \times f(X_i) + \varepsilon_i \quad (2.3)$$

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

I estimate  $\alpha$  non-parametrically to make sure it is not the functional form driving the result. As discussed in Fergusson et al. (2017) the choice of bandwidth  $h$  involves a trade-off between efficiency and bias in the non-parametric estimation: very small

---

<sup>38</sup>There is no plausible gain from adding non-existent donors with their unique ID to the reporting form, while doing this could increase scrutiny

<sup>39</sup>Since Colombia has a first-past-the-post system, I am only interested in close races between the winners and runners-up.

bandwidths are more likely to approximate the quasi-experimental assignment of the treatment variable, which ensures that municipality characteristics do not vary discontinuously at the cut-off (Lee and Lemieux, 2010b). Yet they often produce small sample problems and imprecise estimates. To deal with this issue, in my baseline estimate I report the conventional estimates and the optimal bandwidth, bias correction, and robust standard errors proposed by (Calonico et al., 2014). Evidence by Hyytinen, Merillinen, Saarimaa, Toivanen, and Tukiainen (2017) shows that this type of bias correction produces RDD results that are similar to the experimental estimate. Following Gelman and Imbens (2014b), I estimate  $\alpha$  for first- and second-order polynomials to avoid using higher-order polynomials.

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

Moreover it is important to note that the current RDD estimate shows the effect of electing a certain *type* of politician – being a donor-funded politician (a bundle of characteristics) – rather than money received on its own. While my fuzzy RDD estimate aims to isolate the effect of accepting donor funding from individual characteristics, see section 4.4.

### 2.4.3 Text analysis and contract price comparison

The paper aims to determine whether there was a price reward for donor contracts compared to non-donors contracts, for similar contracts. To maintain comparison with the RDD design, I compare contracts within donor funded incumbencies that were elected in a narrow margin. The narrow electoral margin allows the comparison to be made within similar municipalities, since their predetermined covariates are smooth across the cut-off (see Table 2.10). Moreover focusing only on donor-funded incumbencies, helps capture differential conditions for donors and non-donors, that the donor-funded mayor may be giving. Similarly I compare contracts for donors of the runnerup vs.

regular contracts, in non-donor funded incumbencies <sup>40</sup> to check whether there is a punishment in contract terms for donors of the runner-up candidate.

A first approximation involved comparing the value of contracts for the exactly the same purpose contracts and in the same municipality, but the sample was limited. However there could be several contracts that differ by one or two words but have the same purpose. In order to look for these, I used a natural language toolkit in Python, described in Bird, Klein, and Loper (2009). First I removed the stopwords from the description of the purpose of the contract – i.e., words that are not relevant to explaining the contents of the contract (i.e., is, the, by, or, with). A complete list of stopwords used is available upon request. Then I compared the same wording (except stopwords) for the purpose of the contract in the same municipality. This increased the sample size of comparison, but still there could be several contracts that are similar in purpose but slightly differ in words not identified in the stopwords. In order to make this comparison I employed a composite index that contains: 1) similarity in the sequence of wording that describes the purpose of the contract and 2) the Jaro-Winkler edit distance<sup>41</sup> between the two contract purpose descriptions. I used this to construct an index of similarity that ranges between 0 and 1, where 1 is exactly the same wording of the contract except stop-words. The following following table shows the process:

<b>Example of text analysis</b>	
<b>Donor-funded contract purpose</b>	<b>Non-donor-funded contract purpose</b>
Support to the administration in the adequate management, organization, collection and classification of the archives and all its components, of the archive of the administration of the municipality of Betania	Support to administration in the management, organization, adequate collection and classification of the files and all its components of the municipal administration file
<i>Stopwords removed</i>	<i>Stopwords removed</i>
support administration adequate management organization collection classification archives components archive administration municipality betania	support administration management organization adequate collection classification files components municipal administration file
<b>Similarity score</b>	
<i>0.942</i>	

Since it is difficult to establish what is the minimum similarity score to be able to ensure a comparison, I calculated the difference in contract value for a similarity score from 0.9 to 1 in short intervals to ensure the result holds. Another potential concern is that contracts in different municipalities could have a different real value. Economic

---

<sup>40</sup>Note that the only type of donor, in the non-donor funded incumbencies are donors for the runner-up.

<sup>41</sup>For example, between the word *car* and *cart* there is an edit distance of 1.



development, and therefore wages, vary considerably across regions in Colombia.<sup>42</sup> To scale the value of contracts relative to local wages, I calculated the average wage during the contracting period (2012–2105) in each municipality.<sup>43</sup>

#### 2.4.4 Fuzzy RDD Set-up

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

**Second stage:**

$$Y_i = \alpha + \beta_1 \widehat{DFP}_i + \beta_2 f(V_i) + \beta_3 DFP_i \times f(V_i) + \varepsilon_i. \quad (2.4)$$

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

In order to estimate the effect of  $DFP$  I employ a fuzzy RDD, that instruments the proportion of donor funds received with campaign limits that jump discontinuously at arbitrary registered voter thresholds. These cut-offs do not coincide with other public policy cut-offs, since they use the registered voters’ cut-offs instead of population cut-offs. This is important because according to [Grembi, Freier, Eggers, and Nannicini \(2017\)](#), some studies use population cut-offs where several policies vary at the cut-off, which would imply a compound treatment. Such approach makes it difficult to disentangle the effect of one of the policies varying at the cut-off. Using this cut-offs I estimate the first stage:

**First stage:**

$$DFP_i = \beta_1 CampaignLimit_i + \beta_2 f(V_i) + \beta_3 CampaignLimit_i \times f(V_i) + \varepsilon_i. \quad (2.5)$$

Where  $CampaignLimits$  is a dummy variable taking a value of 1 if the municipality is over the 25,000 registered voters’ cut-off that implies 110M COP campaign limit, 0 under the 25,000 registered voters which implies 58M COP campaign limit. I am unable to use other campaign limit cut-offs due to power restrictions, since most of the municipalities are under around the 25,000 registered voters (see [Figure 2.8](#)). Therefore the sample is limited to municipalities with registered voters under 50,000, where campaign limits jump discontinuously again.<sup>44</sup>  $V_i$  is the forcing variable, that

---

<sup>42</sup>During 2012–2015 the average wage in municipalities ranged from 0.5 M COP, (equivalent to \$197 USD) to 1.9 (M COP)

<sup>43</sup>Based on Harvard’s Colombian Atlas of Economic Complexity, which gathered wage data from the Integrated Report of Social Security Contributions, managed by the Colombian Ministry of Health.

<sup>44</sup>At 50,000 registered voters the limit jumps to 330M COP, from 110 M COP

is the number of registered voters around the 25,000 voters' cut-off, from 0 to 50,000.  $V_i =$  is centered around 0, so  $CampaignLimit_i = 1$  if ( $V_i > 0$ ). In particular:

$$CampaignLimit_i = \begin{cases} CampaignLimit_i = 1 & \text{if } V_i > 0 \\ CampaignLimit = 0 & \text{if } V_i < 0 \end{cases} \quad (2.6)$$

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

### **Exclusion restriction**

There could be other potential effects of campaign limits affecting the exclusion restriction. According to the formal model by [Avis et al. \(2017\)](#), fewer candidates may participate in elections where there are campaign limits. Yet [Table 2.25](#) shows that when there are looser campaign limits, there are no differences in the total number of candidates participating in elections or the total number of donors. Another potential concern is that when there are more registered voters, municipalities are bigger, and more covariates could change, like politicians' salaries or the income of the municipality which could in turn affect the probability of giving a contract to a donor. A further concern is that with looser campaign limits there could be a selection effect. For example, donors may prefer certain types of candidates (i.e., more right-wing), or certain types of candidates could be motivated to participate in elections if there is more campaign money to be raised.

In order to empirically check this potential violation of the exclusion restrictions, I check whether any municipality or individual-level covariates jump discontinuously at the cut-off. The results show that across 7 municipality covariates (See [Table 2.26](#)), and 10 individual-level covariates (See [Table 2.27](#)), when there are looser campaign limits there is 0.2% total less income from natural resource royalties in the municipality and a lower proportion of winners from an indigenous background. These differences could be by chance, since I am testing for numerous covariates, or potentially correlated with the outcome. As an additional robustness check in order to disentangle the effect of money from other characteristics, I include the fuzzy RDD specifications both with and without these individual and municipality controls, and the results hold.

## **2.5 Results**

### **2.5.1 Effects of electing a donor-funded politician**

This section compares the effect of electing a donor-funded politician vs. a non-donor-funded politician – i.e., it compares municipalities with different types of elected politi-



cians. Before running this RDD I check if there is manipulation of the electoral outcome. It could be the case that mayors who receive campaign donations systematically barely win (or lose), which would indicate that there is some electoral manipulation. Running the McCrary test (McCrary, 2008b) (see Figure 2.7) to check if there is any discontinuity in the density of vote share around the winning cut-off shows no discontinuity in the distribution at the winning cut-off.<sup>45</sup> As an additional robustness check, I test whether other measures of electoral manipulation jump discontinuously when the donor-funded politician wins. For this I used measures of vote buying and turnout suppression used in Rueda and Ruiz (2017). It could be the case that the additional funding received by donor-funded politicians is used for vote buying in order to win elections. In addition I use the measurements of violence developed by Restrepo et al. (2003), which identify violent attacks by armed groups. Violent groups could be pressuring the population to vote for a certain group, and then ask for contracts once their favored candidate wins. The results in Table 2.9 show that there is no difference in vote-buying reports, turnout suppression reports, or actual violence, which indicates that in a narrow margin, when the donor-funded candidate won, there is no electoral manipulation.

In order to conduct a valid RDD estimate, another potential threat to identification is that there are other covariates that jump discontinuously when a donor-funded candidate wins. Running an RDD estimate with the predetermined covariates as a dependent variable, I show that there is no difference among treatment and control groups in a broad range of covariates (see Tables 2.10 and 2.11). Municipality total income, mayors' salaries, and income from royalties do not vary discontinuously at the cut-off.

Another potential explanation for the outcome is that in races in which the donor-funded politician won, there were more donors, and that is why more contracts were awarded to donors. However results in Table 2.11, show this is not the case: a similar number of donors appear in the races whether the donor-funded candidate wins or loses. The same results holds for the size of the donations, and the number of contracts available in the municipality. Of the 16 covariates I checked to see if they jumped discontinuously at the cut-off, there is only a systematic difference in the size of the local council (see Table 2.11). This could be a potential concern, since giving more contracts to donors could be explained by buying support of the local council via awarding contracts. In order to address this concern, I control for council size to determine whether results change.<sup>46</sup>

What is expected to be different across treatment and control groups are the individual characteristics of politicians, given that receiving donor funding implies potential

---

<sup>45</sup>Similarly, for the arbitrary campaign cut-offs, there is no sorting of registered voters around the 25,000 voters' cut-off (see Figure 2.8).

<sup>46</sup>These unbalanced covariates can be explained by chance given that I tested for discontinuities for 16 covariates and one could be significant.

differences in characteristics. The results on individual characteristics (reported in Table 2.5) show that there is a difference in ideology: 28% of donor funded mayors are right-wing in a narrow electoral margin.<sup>47</sup> This could imply that donors prefer candidates who are ideologically aligned with them; they could prefer more pro-enterprise candidates, who tend to be more right-wing. Another individual covariate that jumps at the cut-off is political experience (having participated in an election before). It could be argued that candidates become more skilled at raising funds the more they participate in elections, or that donors prefer candidates with more experience in elections.

To further investigate whether ideology or experience affects whether contracts are awarded to donors, I ran two separate RDDs in which the treatment is barely electing a right-wing politician or barely electing a politician who has political experience.<sup>48</sup> The results (available in Table 2.12) show that electing a right-wing candidate does not result in awarding more contracts to donors. Moreover, the proportion of donor-funded politicians remains constant, and donor income as a percent of the total do not vary discontinuously. Therefore, given the same level of donor involvement, electing a right-wing candidate by itself does not lead to more contracts given to donors. Table 2.13 shows similar results: electing a candidate with previous political experience does not lead to more contracts given to donors, with similar level of funding at both sides of the cut-off. As stated earlier this result is intuitive given that it is difficult to find a reason why either ideology or political experience would cause politicians to award contracts to donors, except for the fact that donors provided a financial contribution.

Estimating equation (3) in Table 2.7, both with polynomials of order 1 and 2, and using both the conventional RDD estimate and the bias-corrected estimate proposed by Calonico et al. (2014), I find that when a donor-funded politician wins, the probability that he or she will award a contract to a donor increases by 9.5%. This result is robust to including the unbalanced covariate, council size in column (2) and the individual characteristics in column (3). However as discussed above, the later specification is only indicative given the the individual characteristics of donor funded politicians, are covariates that are part of the treatment (post-treatment). Nevertheless, the effect ranges from around 103–164% of the mean, which means that the probability of receiving a contract more than doubles when the donor-funded candidate wins. The results (displayed on Figure 2.9), show that the effect is slightly stronger close to the winning cut-off point, which could suggest that donor-funded politicians are more willing to award contracts when they win by a narrow margin. This could be to buy support for the next election, while a secure win may indicate less need to repay donors.<sup>49</sup> In order

---

<sup>47</sup>This finding is in line with the US evidence reviewed in Stratmann (2005), which shows that campaign donors select candidates with the most experience who best align with their ideological preferences (Snyder, 1992).

<sup>48</sup>Evidence of smooth covariates around the winning cut-off and the McCrary test for these RDDs is available upon request.

<sup>49</sup>However, the farther I go from the cut-off, the fewer balance covariates there are, so this interpretation is only indicative

to check if the result holds for a variety of bandwidths, I estimate the result for bandwidths in small intervals and find that the results also hold across larger bandwidths (see Figure 2.10).

In addition to measuring the outcome the probability of receiving a contract, I measure the number of contracts obtained by donors. The results (presented in Table 2.8) show that donor-funded politicians give 5.9 more contracts to donors if the links are measured directly (i.e., a company/person donated and got the contract). However it is important to note that the result in total number of contracts is not significant using robust estimates nor for higher order polynomials, which indicated that electing a donor funded politicians, increases the probability of obtaining a contract, but does not increase the total number of contracts for donors, which would show the effect is on the extensive margin. The results so far show that electing donor-funded politicians indeed leads to higher probability for campaign donors to obtain a contract. But does this result necessarily entail more corruption and waste?

Table 2.14 shows that electing a donor-funded politician does not lead to having a higher income during the incumbency period, more tax collection, less operational expenditure or more investment in the budget, or a lower deficit. However Table 2.15 shows that the probability of a mayor receiving a disciplinary sanction increases considerably when a donor funded politician wins, from an average of 2.45% to 9–11.6%. This result is robust to a variety of bandwidth sizes (see Figure 2.11) and different functional forms in Table 2.15. In order to check if the sanctions were related to contracting, I use my coding of whether the sanction or investigation was related to contracting as the dependent variable. Due to power restrictions I pool sanctions and investigations related to contracts.<sup>50</sup> The results, shown in Table 2.16, show that electing a donor-funded politician increases sanctions or investigations related to contracting by 8.6%, which is a sizable effect compared to the 6.1% mean. This result is only suggestive because it is not robust to using a higher-order polynomial, or including controls, however it is significant when the biased corrected estimate is used. The result is valid only for bandwidths up to 10% (see Figure 2.12), yet the coefficient remains positive across larger bandwidths. This result is suggestive that the type of corruption taking place in the municipality is related to contracting. But what exactly are the mayors doing to benefit their donors?

## 2.5.2 Mechanism for benefiting donors: underlying contract characteristics

One way of understanding how donor-funded politicians may benefit their donors is to compare the contracts awarded to donors to regular non-donor contracts. When a donor funded politician wins, I expect to find that contractors who were funders will get more beneficial contract terms than those who were not funders. Similarly, when

---

<sup>50</sup>There were only 13 cases of sanctions related to contracting

a non-donor-funded politician wins, they could be less likely to reward the donors of the runner-up, or may provide less favorable contract terms as punishment. Table 2.17 reports the conditions under which the contracts were given.

According to Table 2.17, when the donor-funded politician wins, contracts are awarded to donors using the minimum-value modality 60.9% of the time, and using waiver contracts only 27% of the time. The proportion of minimum-value contracts for non-donors is lower, and this difference is statistically significant. This modality can provide a significant advantage for donors, since contracts under this modality are awarded under a minimum value modality, where it is only required to publish the call for applicants only for a day or more, it is not required to publish the call in the national on-line system, the sole criteria for awarding contracts is the lowest bidder, and there is no need of a committee to evaluate proposals. This is in sharp contrast with a regular bid, where it is required to publish call for applications between 5 to 10 working days, this call has to be in the on-line system an evaluation committee can be used, and the award of the contract has to be justified publicly. And is in contrast to contracts assigned via waivers where it is necessary to legally justify and prove with evidence why the contract was given.

A potential for donors drawback of employing this modality of contracting for donors is that the contract value has to be under a minimum threshold which is 10% of the municipality income. However a way to circumvent this, used by donor-funded politicians is to issue several contracts under minimum value modality. In Table 2.20 I check difference in donor characteristics, vs. regular contractor characteristics. Effectively, on average donors receive over 5 minimum value contracts, while regular contractors obtain 1.23 contracts. Moreover, the donor contracts in more sectors of the economy compared to a regular contractor.

Looking at aggregate evidence of contracts, in Table 2.17, for donors, the average duration is much shorter, 60.5 days compared to 102.7 days for non-donors, and 83.5% of donor contracts finished execution by September 2016<sup>51</sup> while only 40% of contracts for non-donors finished by September 2016. Moreover contracts for donors are signed earlier in the incumbency term, mostly in the second year of incumbency, while regular contracts are given in the last two years of incumbency. Given the short duration and, and earlier assignment it is more likely that contracts finish during a Mayor incumbency term, compared to regular contracts. Moreover donors who are contractors tend to be regular individuals, rather than established companies. Interestingly, if a non-donor Mayor is elected, there is no statistically significant difference in how contracts are assigned to donors compared to non-donors, so there is no punishment to donors of the runner-up, rather donors of the loser candidate face the same contracting conditions as any other contractor in the municipality. Moreover they the donors who are able to obtain a contract despite being a donors of the runner-up tend to be more established

---

<sup>51</sup>Date when the dataset was obtained

companies.

Table 2.19 shows the sector of the economy of contracts for donors and non-donors under different mayor types. When the donor-funded politician wins and donors are awarded contracts, 44% of these contracts are given in the personal services sector<sup>52</sup>. Interestingly, donor-funded contracts have a larger proportion of supplies of materials and machinery, as well as supplies for the municipality. These types of contracts can be quickly executed (and paid off). Surprisingly, non-donors tend to be awarded more construction contracts (15.6%, compared to 12.5% to donors). This discrepancy may be because construction contracts take longer to execute, and therefore they are less likely to finish during an incumbency period. Moreover, they tend to be larger contracts, and are therefore legally required to open up for competitive bids. It is easier for mayors to award contracts under the minimum-value modality, which tend to be smaller contracts and pay them off quickly if they are supply contracts. Looking at the legal type of contract in Table 2.18, I verify that donor contracts tend to be more concentrated in supplies compared to non-donors contracts.

### Multiplier of contracts for donors

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

$$\text{Multiplier} = \text{Total Value of Contracts Received} / \text{Total Value of Donations Made} \quad (2.7)$$

$$\text{Multiplier Scaled} = \frac{(\text{Total Value of Contracts Received} - \text{Total Value of Donations Made})}{\text{Total Value of Donations Made}} \quad (2.8)$$

Donors receive a total value in contracts that is, on average, *10.67 times* the value they donated (see Table 2.21). This value is very large, and Table 2.21 reveals that this result is driven by several outliers in the distribution of the multiplier, which implies that for several donors there can be a huge return. Table 2.22 shows that when a donor-funded candidate is elected, donors obtain 13.76 times the value they

---

<sup>52</sup>For example, consultancies, accountants, drivers.

donated. Surprisingly, even when the non-donor politician is elected, donors still obtain a positive multiplier of 6.57, which is driven by high-value contracts for the few donors (see Table 2.22) who obtain large contracts despite donating to the candidate who lost by a narrow margin. In order to compare the multiplier for donors when a donor is elected vs. when a non-donor is elected, I employ a t-test and a comparison of medians using the Fisher exact test to account for outliers. The evidence shows that there is a difference of 7.1 in the multiplier, and this difference is statistically significant, which shows that there are higher benefits to donors when the donor-funded politician wins. Most interestingly, average donor contributions in the race when a donor funded politician barely wins or loses are similar, 9 M COP and 7.8 M COP respectively, but the benefits for donors diverge substantially if the donor funded politician wins.

### Price premium for donors

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

Using the detailed purpose of the contract, and after stripping the purpose of the contract of irrelevant words (stop-words) and comparing identical-purpose contracts (similarity score=1), I found 31 identical contract purposes between donors and non-donors. The price premium for the donor contracts was 1.7 local wages<sup>53</sup> higher. In order to increase the sample size, I compared contracts with a similarity score of 0.9 or above, and obtained 81 similar contracts that have a difference of 2.3 local wages (See Table 2.23). A more systematic comparison of the price inflation across different similarity scores can be found in Figure 2.13. Independent of the score, the price premium for donors is 3M to 2M COP, which is around 2.2 to 1.9 average local monthly wages. That contracts awarded to donors have a systematically higher pay for a broad range of similar contracts indicates that there is indeed a premium for being a donor. This evidence is indicative because the price differences could be explained by the fact that the quality of goods provided by donors systematically are higher. However, given that donors contract in more sectors of the economy according to results in Table 2.20, it is less likely that the quality of the good provided is higher, given the lack of economic specialization.

---

<sup>53</sup>That is the total value of the contract divided by the local wage

### 2.5.3 Case Study

A more detailed case study<sup>54</sup> can further the understanding how contracts are provided for donors and if there is a price premium.

#### **The case of municipality “Village town”, in the department of Meta.<sup>55</sup>**

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

#### **Systematic use of minimum value modality contracts:**

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

#### **Same purpose of the contract, higher price for donor:**

---

<sup>54</sup>This is useful according to Franzese (2009) and Mahoney and Villegas (2009) who argue that qualitative studies can help understand the underlying causal mechanisms behind two variables in econometrics; this because qualitative studies allow for more detail

<sup>55</sup>Name of the municipality and names of the people involved have been changed for security reasons.



In November 2014, the municipality of “Village Town”, headed by “Juan”, signed a contract with “Donor B”, who had donated 1.5M COP to the campaign. The project involved technical support for monitoring infrastructure projects in the municipality. The Mayor’s office justified contracting this service directly by noting the relatively high degree of specialization that this project required, as it was necessary to hire a contractor with knowledge of civil engineering or architecture. This implied that a public tender or competitive process was not opened. The contract had a duration of *six months* and a total value of 24M COP. The contractor received monthly payments of 4M COP upon completion of its monthly duties. The project was executed on a regular basis, with no additions or abnormal activities. By the end of the contract, the contractor had received 100% of the contract amount, which was *16 times* the value of his donation.

In April 2015 the Municipality of “Village Town” signed a contract with “Non-donor A”, a non-donor contractor. Both the main objective and all the specific tasks of the contract were exactly the same as those specified in “Donor B” contract. However, the duration of the contract was only 4 months. Furthermore, even though the technical specificities of both contractors were identical, “Non-donor A” received monthly payments of 3.2M COP, for a total of 12.9M COP. Thus for the same tasks, the value of the non-donor contract was 53% of the total value of the donor contract.

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

#### **2.5.4 Effects of campaign limits**

At 25,000 registered voters, campaign limits jumps from 58M COP to 110M COP, and the law establishes that individual donors can donate up to 10% of the total limit. Figure 2.14 illustrates that under 25,000, the average donor contribution per municipality is consistently under the 5.8M COP limit which show that limits are indeed binding. There is a small standard deviation and average donations tend to be close to the limit. Above the cut-off where the individual contribution limit is 11 M COP, the average contribution increases and has a bigger standard deviation. Running the RDD using the arbitrary campaign limits, I find that when there are looser campaign limits (moving from 5.8 M to 11 per individual contribution), the average donor contribution increases to 3.8M COP (see Table 2.24), and candidates reduce self-funding of their own campaigns, however this latter difference is not statistically significant. Most interestingly, according to evidence in Table 2.24 there isn’t a higher



total campaign income product of looser campaign limits, rather a substitution effects from using less own income to finance campaign to using donors. As a result, the participation of donor income as a percent of total campaign income increases by 27%. Moreover looking at results in Table 2.25 show that the total number of donors remain constant. So essentially there are similar numbers of donors, increasing average size of the donation, and increasing their participation in the politician campaign portfolio.

As discussed in the exclusion restriction section, a potential concern with using the campaign limits as an instrument for proportion of donors funds, is that there are other variables jumping discontinuously at the cut-off. According to (Avis et al., 2017) there can be fewer candidates, but this is not the case according to results in Table 2.25. Moreover testing across a broad range of municipality characteristics (Table 2.26), and winner candidate characteristics (Table 2.27) there are almost no differences when there are looser campaign limits. The sole difference I find is that there are more mayors with an indigenous background and less income from royalties, in municipalities with looser campaign limits. Therefore I include controls in the fuzzy-RDD as an additional robustness check.

Estimating the fuzzy RDD, the results in Table 2.28 indicate that allowing looser campaign limits leads to 27–32% more donor income as percent of total income, which in turn leads to 49–52 more contracts for donors, and this result is robust to including unbalanced controls. As an additional robustness check instead of instrumenting the proportion of donor funds, I instrument a dummy whether the politician was donor-funded, and the result holds: (columns 3 and 4), looser campaign limits lead to an increase of 31–32 more contracts to donors.

Interestingly changing the dependent variable from the number of contracts for donors to the probability that donors receive contracts, in Table 2.29 there is a positive coefficient but not significant. Looking back at Table 2.24, the number of donors remain constant but rather the average contribution increases, therefore the effect of campaign limits is on the intensive margin, rather than increasing the probability for donors.

## 2.6 Discussion

Political selections of donors funded politician do matter. If donor funded politicians are elected, it increases the probability of donors receiving contracts. Indicative evidence suggests that this can lead to more investigations or sanctions towards the Mayor due to improper contracting. Moreover this paper shows that contract assignment does not necessarily strictly depend on the economic capacity of companies/individuals; but rather depends on political contributions made during the campaign. The size of the payoff for donors depends on the electoral victory of the candidates supported. This result is important because economic returns of a company could depend on an event unrelated to the actual economic capacity of companies. Finally the paper shows that

looser campaign limits leads to more higher level of contributions by donors, and less own funding, increase the participation of donor funds as a % of total income, and therefore conferring more contracts to donors. Campaign limits, reduce the size of the reward for donors.

Most importantly from the social welfare perspective, giving donors to contracts can be costly because they result in procurement processes that are less transparent, and can limit competition by other bidders; in particular the minimum value modality used allows for short calls and there is no advertisement online for the procurement bid. Also, these practices can be costly because donor contracts receive a price premium compared to non-donor contracts of the similar type. However, the size of distortion for donors does not affect the overall budget deficit or investment level in the municipality. Moreover my study represents a lower bounds estimate of money in politics, because there are other forms that money participates in politics, such as lobby or bribes that could lead to benefits for donors.

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

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

## 2.7 Tables and figures

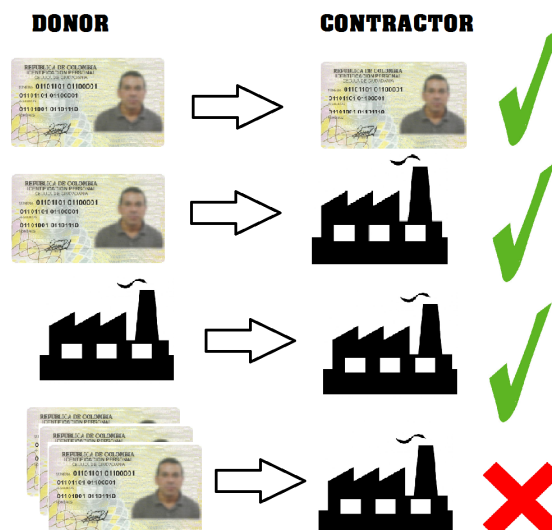
### 2.7.1 Figures

Figure 2.1: Example of a contract in the Dataset

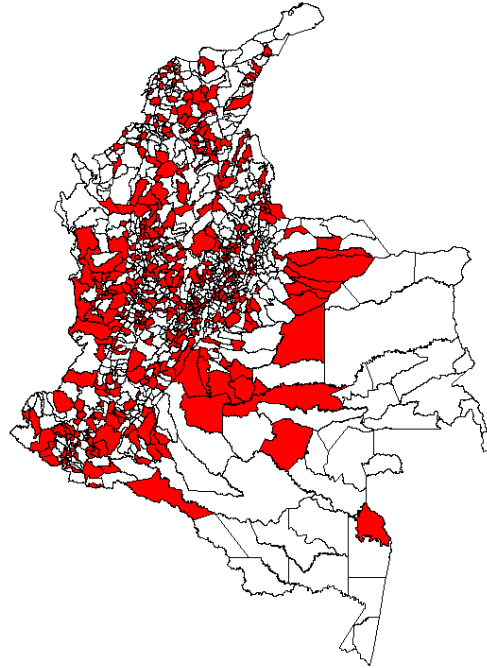
Ubicación Geográfica del Proceso	
Departamento y Municipio de Ejecución	Antioquia : Santa Bárbara
Datos de Contacto del Proceso	
Correo Electrónico	gobierno@santabarbara-antioquia.gov.co
Información de los Contratos Asociados al Proceso	
Número del Contrato	CPS N 174 DE 2015
Estado del Contrato	Celebrado
Objeto del Contrato	PRESTACIÓN DE SERVICIOS DE DIFUSIÓN DE LOS PROGRAMAS, PROYECTOS, INFORMES Y AVISOS ORIGINADOS EN LA ADMINISTRACIÓN MUNICIPAL A TRAVÉS DE MEDIOS RADIALES.
Cuantía Definitiva del Contrato	\$6,600,000.00 Peso Colombiano
Nombre o Razón Social del Contratista	CORPORACIÓN ECOS DE SANTA BARBARA
Identificación del Contratista	Nit de Persona Jurídica No. 830513709-6
Pais y Departamento/Provincia de ubicación del Contratista	Colombia : Antioquia
Dirección Física del Contratista	Calle 130 sur 43-49 Caldas- Ant
Nombre del Representante Legal del Contratista	JOSE SOLEIBE ARBELAEZ
Identificación del Representante Legal	Cédula de Ciudadanía No. 6234005
Valor Contrato Interventoría Externa	\$.00
Fecha de Firma del Contrato	16 de julio de 2015
Fecha de Inicio de Ejecución del Contrato	16 de julio de 2015
Piazo de Ejecución del Contrato	159 Días
Destinación del Gasto	No Aplica

Source: Colombia Compra Eficiente contract reporting system.

Figure 2.2: Summary of linking contracts with donors



**Figure 2.3: Colombian municipalities where the donor funded politician placed first or second. 2011 Election**



**Figure 2.4: Colombian municipalities where the donor funded politician placed first or second in a electoral narrow margin of (6.4%). 2011 Election**

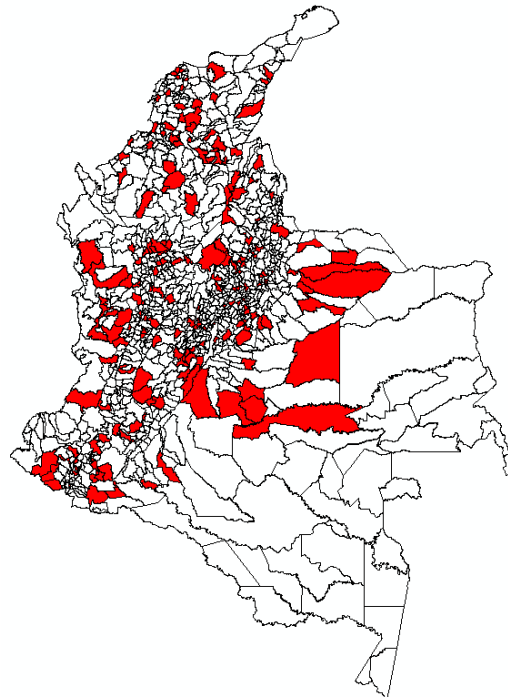


Figure 2.5: Spatial autocorrelation index (Morans I) for municipalities where the donor funded politician placed first or second in a narrow margin.

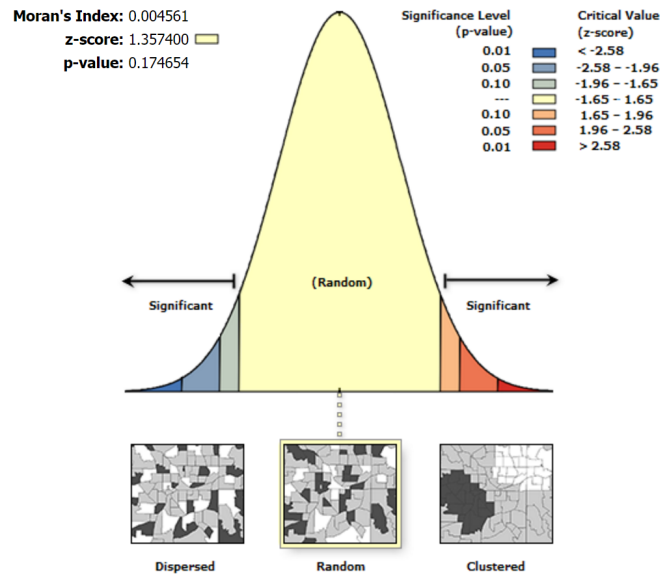


Figure 2.6: Example of mayoral election ballot of 2011



Figure 2.7: Distribution of winning margins for donor-funded politicians and McCrary test

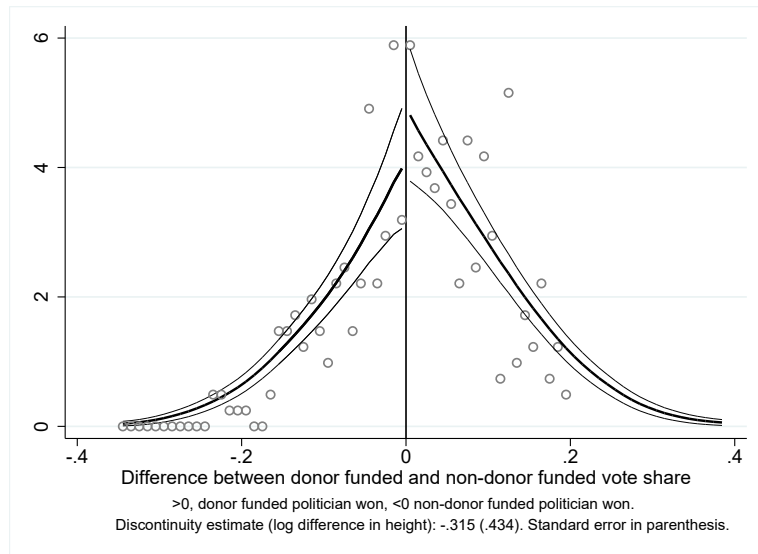
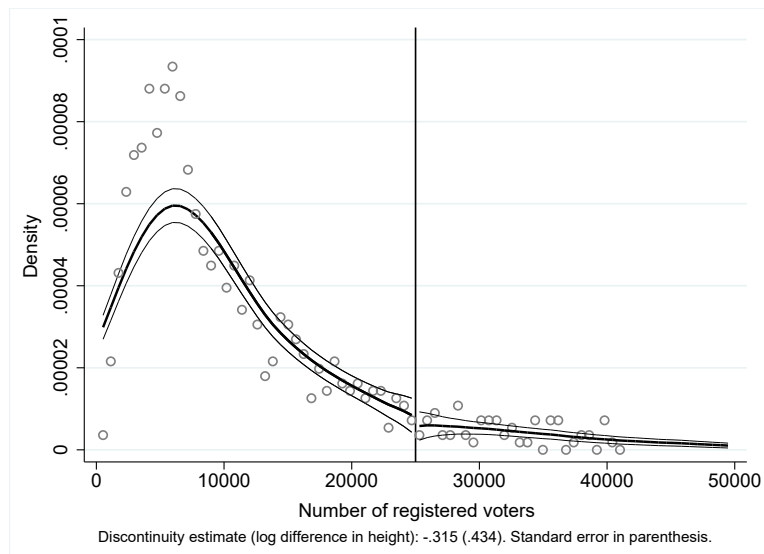
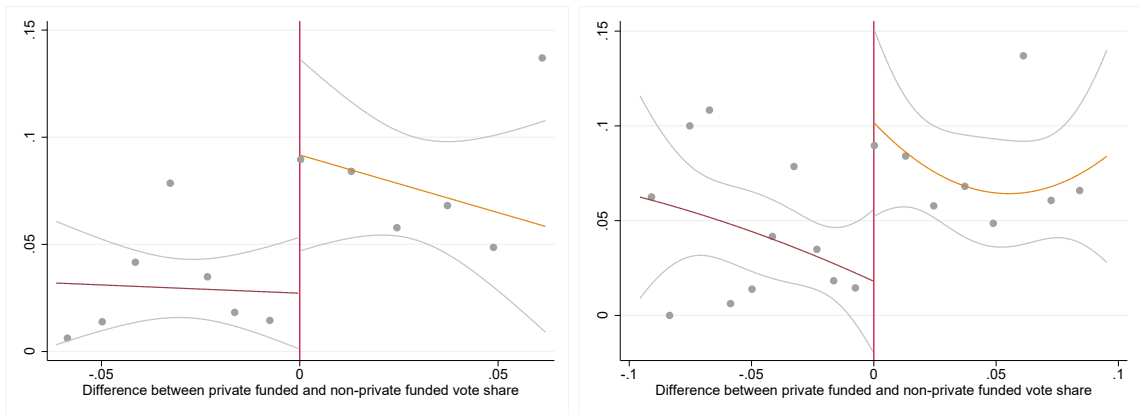


Figure 2.8: Distribution of registered voters and McCrary test



**Figure 2.9: Effect of electing a donor funded on probability of donors receiving contracts**



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

**Figure 2.10: Robustness check: Different bandwidth sizes. Effect of electing a donor funded politician and giving a contract to a donor.**

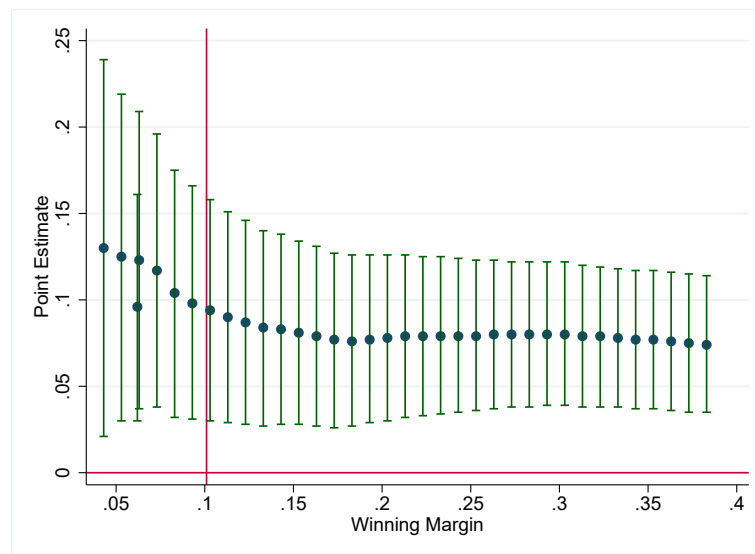


Figure 2.11: Robustness check: Different bandwidth sizes. Effect of electing a donor funded politician on disciplinary sanctions

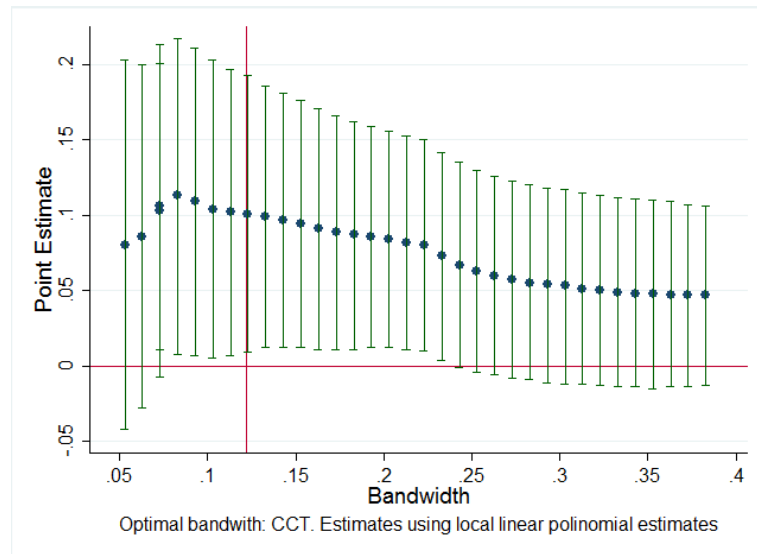
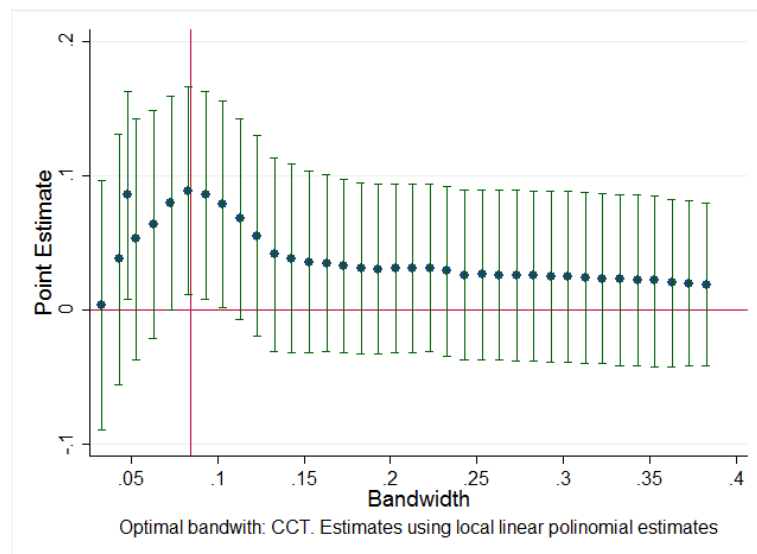
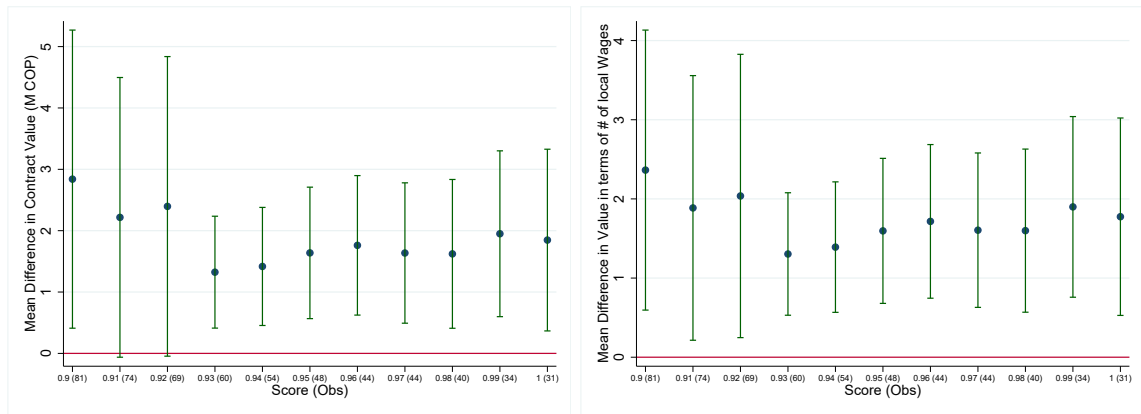


Figure 2.12: Robustness check: Different bandwidth sizes. Effect of electing a donor funded politician on investigation/sanction related to contracting



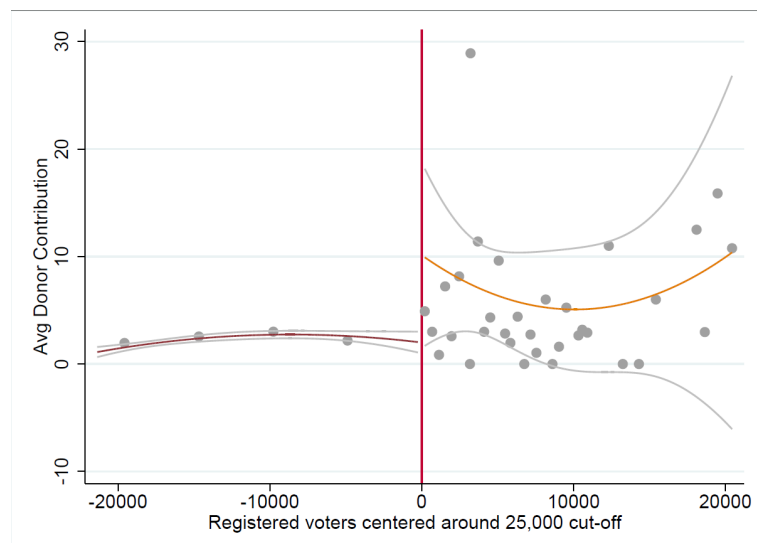


**Figure 2.13: Price difference between donor funded contracts and regular contracts**



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

**Figure 2.14: Average Donor contribution and campaign limits**



## 2.7.2 Tables

**Table 2.1: Donations Codebook**

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

## 2.7.3 Effects of electing a donor funded politician

**Table 2.2: Descriptive Statistics**

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

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

**Table 2.3: Sources of campaign income across candidate types, % of Total Income**

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

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

**Table 2.4: Sources of campaign spending across candidate types, % of Total Spending**

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

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

**Table 2.5: Differences in individual characteristics between donor funded politicians and non-donor funded**

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

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

**Table 2.6: Donors assignment of contracts in donor funded vs non-donor funded incumbencies:**

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

**Note:** The base sample is 408 municipalities where the race was contested between donor funded politicians and non-donor funded politicians. There are 132 “donors” of the winner when a non donor funded politician wins. These are contributions by the candidates themselves. Similarly, 355 “donors” of the runner-up when the donor funded is elected, are contributions by the candidates themselves. 4 of these runner-up candidates got a contract.

**Table 2.7: Effect of electing a donor funded politician on Contract Given to any donor**

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

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

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

Individual characteristics: Rightwing and political experience

**Table 2.8: Effect of electing donor-funded politicians on total number of contracts for donors**

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

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

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

**Table 2.9: Smooth manipulation covariates across the donor funded victory cut-off**

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

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

**Table 2.10: Smooth municipality covariates across the donor funded victory cut-off**

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

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

**Table 2.11: Smooth campaign and municipality covariates across the donor funded victory cut-off**

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

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

**Table 2.12: Effect of electing rightwing politician on probability of giving contracts to donors**

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

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

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

**Table 2.13: Effect of electing a politician with political experience on probability of giving contracts to donors**

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

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

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

**Table 2.14: Effect of electing a donor funded politician on fiscal policy variables**

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Income (Y) (M COP)	Land Taxes (%Y)	Indus. Taxes (%Y)	Funct. expen. (%Y)	Investment (%Y)	Deficit (%Y)
Donor Funded Elected ( <i>Conventional</i> )	8940.041 (6510.759)	0.974 ( 1.152)	0.863 ( 1.046)	-0.369 ( 1.846)	0.369 ( 1.846)	1.051 ( 2.899)
Privately Funded Elected ( <i>Robust</i> )	10006.696 (7857.657)	1.152 ( 1.374)	1.462 ( 1.059)	-0.220 ( 2.302)	0.220 ( 2.302)	1.435 ( 3.405)
Observations	213	200	179	200	200	206
Mean	47102.906	3.889	3.377	13.284	86.716	11.346
Effect Mean(Per)	18.98	25.04	25.56	-2.78	0.43	9.26
Bandwidth	0.072	0.065	0.056	0.066	0.066	0.070
(Local) pol. order	1	1	1	1	1	1

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

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



**Table 2.15: Effect of electing a donor funded politician on mayor being sanctioned**

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

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

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

Individual characteristics: Rightwing and political experience

**Table 2.16: Effect of electing a donor funded politician on investigation or sanctions related to contracting**

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

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

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

Individual characteristics: Rightwing and political experience

## 2.7.4 Contract Level results

**Table 2.17: Comparison of contracts by Mayor type, and whether the contractors were donors.**

<i>Contractors were:</i>	Donor-funded Mayor			Non-donor funded Mayor		
	Non-donors	Donors	Mean Difference	Non-Donors	Donors	Mean Difference
Avg. minimum value contracts	0.477	0.609	-0.132***	0.427	0.488	-0.060
Avg. assigned directly contracts	0.434	0.278	0.155***	0.470	0.390	0.080
Avg. all directly assigned contracts	0.910	0.887	0.023*	0.898	0.878	0.020
Avg. Contract Duration (Days)	102.734	60.535	42.198***	111.058	119.512	-8.454
Avg. Percent time addition	0.024	0.015	0.008	0.022	0.037	-0.015
Avg. Percent value addition	0.013	0.019	-0.005	0.015	0.033	-0.017
Avg. contracts that finished	0.398	0.835	-0.436***	0.378	0.427	-0.049
Contract for a company	0.307	0.278	0.029	0.282	0.585	-0.304***
Contract for an individual	0.608	0.696	-0.089***	0.645	0.415	0.230***
Contract finished during incumb	0.140	0.267	-0.127***	0.136	0.159	-0.023
Year 1 contract signed	0.190	0.202	-0.012	0.173	0.159	0.014
Year 2 contract signed	0.252	0.327	-0.074***	0.241	0.220	0.022
Year 3 contract signed	0.280	0.253	0.028	0.301	0.268	0.032
Year 4 contract signed	0.277	0.218	0.059***	0.285	0.354	-0.068

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

**Note:** The number of observations are 70661 and 514 for control and treatment group in columns (1) and (2) respectively. For columns (3) and (4), the number of observations are 60850 and 82 for control. (\*) Finished by September 2016 when this data was gathered.

**Table 2.18: Comparison of contract types by Mayor type, and whether the contractors were donors.**

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

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

**Note:** The number of observations are 76902 and 514 for control and treatment group in columns (1) and (2) respectively. For columns (3) and (4), the number of observations are 65389 and 82 for control. (\*) Finished by September 2016 when this data was gathered.

**Table 2.19: Comparison of contracts sector by Mayor type, and whether the contractors were donors.**

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

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

**Note:** The number of observations are 76902 and 514 for control and treatment group in columns (1) and (2) respectively. For columns (3) and (4), the number of observations are 65389 and 82 for control

## 2.7.5 Donor Level results

**Table 2.20: Comparison of contractors by Mayor type, and whether the contractors were donors.**

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

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

**Note:** The number of observations are 28239 and 57 for control and treatment group in columns (1) and (2) respectively. For columns (3) and (4), the number of observations are 22934 and 17 for control. (\*) Finished by September 2016 when this data was gathered.

**Table 2.21: Descriptive statistics multiplier to investment and contract value**

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

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

**Table 2.22: Donor average returns conditional on electoral victory of donor funded politicians**

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

**Note:** The number of observations are 662 and 847 donors for non-donor elected incumbencies and donor-elected incumbencies respectively. Multiplier is Total Contract Value/Total donated value. P-values for means comes from a t-test of difference in means. P-values for medias comes from a Fisher exact test. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

## 2.7.6 Price premium for donors

**Table 2.23: Price Comparison of similar type of contracts for donors and non-donors**

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

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

**Note:** T-test paired comparison. Number of observations in parenthesis. Score is the similarity score in contract purpose contents. The score removes stop words, and combines the sequence and edit distance.

## 2.7.7 Results on Campaign Limits

**Table 2.24: Effect of looser campaign limits on:**

	(1) Average own campaign contribution	(2) Average donation	(3) Total Campaign Income	(4) Donor Income as % of total
Looser Campaign Limits (Conventional Estimate)	-8.471 (7.024)	3.827* (1.763)	-13.779 (-9.902)	0.268* (0.134)
Looser Campaign Limits (Robust Estimate)	-9.374 (8.693)	3.225 (2.388)	-16.286 (-11.947)	0.273* (0.165)
Observations	143	55	73	141
Mean	19.051	2.640	52.922	0.187
Effect Mean(Per)	-44.46	144.96	-26.04	143.32
Bandwidth	8045.595	3499.152	4653.014	7867.652
(Local) polynomial order	1	1	1	1

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

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

**Table 2.25: Effect of looser campaign limits on:**

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

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

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

**Table 2.26: Smooth predetermined municipality covariates across loser campaign limits cut-off**

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

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

**Table 2.27: Smooth predetermined municipality covariates across loser campaign limits cut-off**

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

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

**Table 2.28: Effects of looser campaign limits on total number of contracts for donors**

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

Standard errors clustered at the municipality level in parentheses.

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

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

**Table 2.29: Effects of looser campaign limits on probability of donors obtaining contracts**

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

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

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

## 2.7.8 Online Appendix

**Table 2.30: Differences in municipality characteristics between campaign sources reporting municipalities and non-reporting**

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

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

**Table 2.31: Differences in municipality characteristics between in sample and out of sample**

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

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



# Chapter 3

## Open or closed?

### How list type affects electoral success, candidate selection and effort<sup>1</sup>

DOMINIK HANGARTNER<sup>2</sup>

NELSON A. RUIZ<sup>3</sup>

JANNE TUKIAINEN<sup>4</sup>

#### Abstract

What is the effect of open-list (as opposed to closed-list) in proportional representation elections on electoral outcomes? We provide new evidence by studying local elections in Colombia, where parties have unique discretion over fielding open or closed-lists. Using panel data covering all 1,100 Colombian municipalities for the 2003–2015 period, we leverage within-party, within-municipality, and over-time variation to identify the effect of list type. We find that the adoption of open-lists increases parties' vote and seat shares by 88 % and 117%, respectively. Semi-structured interviews with a representative sample of candidates reveal that parties choosing closed lists struggle to attract candidates of high-quality for lower positions with little chances of winning a seat, and to incentivize them to campaign. Consistent with these mechanisms, candidate-level analyses show that candidates running on open lists expend more campaign effort, have more political experience, are more engaged in their constituency, and less likely to have committed election fraud. Taken together, our findings highlight that the decision between open and closed-list systems has important implications for political selection, electoral success, and the quality of representational democracy.

---

<sup>1</sup>We thank Alexander Vega for his outstanding research assistance. We also thank all the municipality council candidates interviewed from the Partido de la Unidad Social, Partido Alianza Verde, Cambio Radical and Polo Democrático Alternativo. We thank Allison Benson, Francisco Cantu, Micael Castanheira, Torun Dewan, Jean-Paul Faguet, Salomo Hirvonen, Rafael Hortala-Vallve, Anthony McGann, Nicolas Sauger, Sandra Sequeira, Juuso Välimäki and Stephane Wolton for comments. We are also grateful for participants at APSA, EPSA, HECER, LSE-PSPE and Paris. The usual disclaimer applies.

<sup>2</sup>Department of Government, London School of Economics and Political Science, Houghton Street, London WC2A 2AE, UK, and Center for Comparative and International Studies, ETH Zurich, 8092 Zurich, Switzerland. Email: [D.Hangartner@lse.ac.uk](mailto:D.Hangartner@lse.ac.uk)

<sup>3</sup>International Development Department, London School of Economics and Political Science. 6-8th Floors, Connaught House, Houghton Street, London WC2A 2AE. Tel: +44 (020) 7955 6565, Email: [n.a.ruiz-guarin@lse.ac.uk](mailto:n.a.ruiz-guarin@lse.ac.uk)

<sup>4</sup>VATT Institute for Economic Research, Arkadiankatu 7, FI-00101, Helsinki, Finland, and Department of Government, London School of Economics and Political Science, Houghton Street, London WC2A 2AE, UK. Email: [janne.tukiainen@vatt.fi](mailto:janne.tukiainen@vatt.fi)

### 3.1 Introduction

Scholars have long argued that electoral rules systematically influence the selection of politicians.<sup>5</sup> Furthermore, a large literature shows that the quality of political selection typically has an effect on policy outcomes, and thus, on the day-to-day life of citizens (e.g., Besley, 2005; Besley et al., 2011; Chattopadhyay and Duflo, 2004; Fujiwara, 2015b). Electoral rules cover a broad range of features of the political system, from the form of (majoritarian or proportional) representation, the usage of primaries, to the type of ballot. Within the context of proportional representation (PR), parties commonly use one of two ballot types: open or closed lists. In open-list systems, voters vote for their preferred candidates and the order in which candidates take seats is determined by the candidate’s vote totals. In contrast, in closed-list systems voters choose a party, and the order in which candidates take seats is determined by their position on the list, that is, by their party. The key difference between open and closed-lists is that the former gives voters influence over the number of seats each party wins and also which candidates from a given list take seats. This induces intra-party competition among candidates (Blumenau, Eggers, Hangartner, and Hix, 2017) and contrasts with closed-list system, in which the party retains much higher levels of control. Given these differences between the two list types, we might expect a sizable literature shedding light on their consequences. Many scholars argue that this intra-party competition under open-list rewards more experienced candidates (Shugart, Valdini, and Suominen, 2005), incentives politicians to deliver particularistic services to their constituencies (Ames, 1995; Ashworth and Bueno de Mesquita, 2006; Carey and Shugart, 1995; Crisp, Escobar-Lemmon, Jones, Jones, and Taylor-Robinson, 2004; Grimmer, Messing, and Westwood, 2012; Hallerberg and Marier, 2004) and to steer away from good government to engage in corrupt practices (Chang, 2005, 2005). However, almost all of this research—the two exceptions that we are aware of are discussed below—studies politicians’ behavior solely under open-lists and is therefore unable to answer any *comparative* question about the effect of open versus closed-list. Hence, we lack empirical answers even on some of the most basic questions.

What is the effect of open-list as opposed to closed list on electoral outcomes such as vote shares? What is the effect of list type on candidate selection and effort, and which list type leads to more preferable selection from the voters’ perspective? As fundamental as these questions are for our understanding of electoral systems, quantifying their impact is empirically challenging. In most PR countries, the constitution prescribes either open or closed list, such that comparative research interested in the effect of electoral system has to resort to cross-country comparisons. However, the em-

---

<sup>5</sup>See e.g., Myerson (1999); Carey and Shugart (1995); Besley (2005). For influential work (see Galasso and Nannicini, 2011), and (Braendle, 2016) for a recent survey. Moreover, among others, (Persson and Tabellini, 2004, 2003, 2002) argue that the type of electoral system influences policy outcomes.

pirical identification of the effect of open and closed lists across countries is typically plagued by unobserved heterogeneity, raising the spectre of serious confounding.

In this paper, we make considerable progress on this front by studying the unique PR system of Colombia, where parties can unilaterally choose whether they adopt open or closed list in local elections. Since the electoral reform of 2003, each party can (and frequently do) decide to field open lists in one municipality, and closed lists in another during the same election cycle. This complete discretion over list type leads to within-party, within-municipality (and within party times municipality), and over-time variation in the adoption of open- and closed-list. We leverage this variation by employing panel data models controlling for election year, and party times municipality fixed effects to identify the effect of list type on political outcomes.

After an extensive effort, we collect novel candidate-level panel data from about 1,100 Colombian municipalities for the 2003-2015 period to analyze the effects of list type on electoral success. We find that parties adopting open lists benefit from dramatically higher vote and seat shares. Building on the insights from semi-structured interviews with a representative sample of candidates, we next turn to question of what explains this effect. We find both qualitative and quantitative support for two main mechanisms. First, open lists induce all candidates, independent of their ranking, to campaign. This is not the case under closed lists where lower-ranked candidates with little chance of winning a seat have few incentives to campaign. Second, the qualitative interviews suggest that open lists are better suited to attract high-quality candidates. Using a broad set of quality indicators covering candidates' experience, constituency engagement, and past election fraud, we find that across all those measures, quality of candidates is, on average, significantly higher on open-lists.

Finally, we also shed light on the puzzle of why would any party choose a closed lists given their detrimental impact. Candidates argue that closed lists allow for party cohesion, and signaling to the voter the cohesion. The empirical evidence shows that mostly new parties adopt closed lists. Moreover, parties tend to switch from closed to open lists over time, but not vice versa, implying that maybe the detrimental effects are learned only by experience.

This paper makes several contributions to the literature. First, we provide some of the first credible empirical evidence of the effect of list type on electoral outcomes in a real-world setting. While we are not the first to study the effect of list type, most of the existing research (e.g., [Bergman, Shugart, and Watt, 2013](#); [Nemoto and Shugart, 2013](#)), is limited by a lack of within-country and over-time variation in list type, thereby raising the concern that unaccounted heterogeneity between countries, rather than list type, may be driving the results.<sup>6</sup> Relying on credible, subnational and

---

<sup>6</sup>There are, however two exceptions. One is ([Sanz, 2015](#)) who investigates the impact of closed versus open list on turnout by exploiting population thresholds determine list type in Spanish local elections. A second is ([Blumenau et al., 2017](#)), as they rely on a survey experiment to study the effect of open versus closed list on (hypothetical) party choice in the U.K.

over-time variation, we believe our estimates to be considerably closer to the causal effects of open versus closed list. Moreover, we provide evidence of the mechanisms behind the effects of list type on voting: Candidate effort and selection.

Second, using the same empirical strategy, we also contribute to the effect of list type on candidate selection. Recently, there has been a renewed interest in the selection of political candidates see [Dal Bó, Finan, Folke, Persson, and Rickne \(2017\)](#) and the fundamental question of democracy’s ability to attract competent leaders. We expand on this literature by providing some of the first credible empirical evidence on how electoral systems in general (for rare exemption see [Beath, Christia, Egorov, and Enikolopov, 2016](#)) for comparison of at-large and district elections) and list choice in PR systems in particular affect political selection .

Third, our study is related to the literature studying the relationship between political selection and endogenous electoral systems. The choice that parties have on whether or not to adopt primaries is essentially about deciding whether to adopt open and closed nomination procedures, and thus, structurally similar to our case. In the case of primaries, the parties also face the trade-off between keeping control and popularity among the voters (e.g [Snyder and Ting \(2011\)](#); [Serra \(2011, 2013\)](#); [Hortala-Vallve and Mueller \(2015\)](#)). These theoretical results are similar to ours in the sense that open procedures such as primaries increase expected candidate quality. Finally, [Achury, Ramírez, and Cantú \(2017\)](#) study Colombian parliamentary elections and focus on explaining the reasons behind the list type choices. In contrast, we focus also on the effects of list choice and use both interviewing and large data on individual characteristics on the candidates.

The rest of the paper proceeds as follows. In the next section, we describe the institutional context and the rules according to which parties decide on list type in municipal elections. In Section 3, we describe the newly collected dataset covering all municipal elections for the 2003-2015 period and present selected descriptive statistics. Section 4 presents the main results on the effect of list type on electoral success. Section 6 combines insights from qualitative interviews and quantitative candidate-level analyses to shed light on the main mechanism. Section 7 addresses the question why any party would choose closed list give their detrimental impact. Finally, section 8 concludes the paper.

## **3.2 Election Reforms in Colombia**

Colombia is home to the third largest population and the fourth largest economy in Latin America. While endowed by rich natural resources, Colombia is characterized by large economic and social heterogeneity across regions, and high levels of inequality([Bushnell, 1993](#)).

For most of the 20th century, Colombia had a strong central government and a

bipartisan system where either the conservative or liberal party holding the presidency. In the last decades of the 20th century several reforms were enacted to counterbalance the strong bipartisanship, in order to politically decentralize the country. One of these reforms, for example, introduced election for local mayors, which were previously appointed by governors who themselves were appointed by the president (Castro, 1986). In 1991, a new constitution was enacted that decentralized fiscal resources to the regions to counterbalance the history of strong fiscal centralization. After 1991, third-parties began to play a stronger role and their presence began to increase at the local level (Hoyos, 2005b).

Colombia is currently structured in 1100 municipalities<sup>7</sup>, which are located within departments. In each local election, politicians are elected for: local administrative juntas, municipality councils, municipality mayors, department assemblies, and governors for each department. Local elections are held one year after the national level elections. Municipalities themselves are represented in a group of 32 department assemblies and by 32 governors. Local administrative juntas are only present in a handful of municipalities, while local councils exist for every municipality, and the size is relative to the population of the municipality. Their role is to approve the annual budget, and projects proposed by the municipal mayors.

In 2003, a major electoral reform took place. According to Shugart, Moreno, and Fajardo (2006), before the reform, parties presented multiple candidate lists per district. Instead of presenting one list per party, in practice most candidates within a party would present their own list to take advantage of the proportional representation formula that benefited small lists. According to Pachón and Shugart (2010), by 2002 it was rare that any candidate list would obtain more than one seat, and this led to high fractionalization of the parties and made them very personalistic (Shugart et al., 2006).

In order to address these issues, the 2003 reform required that each party would present one list. But it was difficult to reach an agreement between open or closed lists (Shugart et al., 2006) and a unique feature was allowed in the electoral system: the type of ballot would be chosen by the parties themselves, parties could opt to choose for closed lists or open lists in each district they ran. In closed lists, ranking of candidates is organized by the party, and votes for the party would be distributed according to the ranking. In open lists, a numbered list of candidates would be given by the party, the voter could opt to vote for any person within the list, and the ranking would be given by the amount of votes each candidate had. In addition to voting for candidates, in open lists, the voter could opt to vote just for the party. These votes would be for the party total but would not alter the ranking of candidates given by the votes for candidates. Most votes in open lists go to specific candidates<sup>8</sup>.

---

<sup>7</sup>The exact number varies in our period of analysis since new municipalities have been created between 2003 and 2015.

<sup>8</sup>Of all open lists for local municipality councils between 2003 and 2015, on average only 9% of the

Since the 2003 reform, parties can choose their preferred list type in both municipal and national elections. At the national level the upper house (senate) is elected in a single district, with 100 seats and plus one district for minority indigenous communities with 2 seats. The lower house (house of representatives) is representative at the department level, and each department elects at least two politicians or more depending on the department population size. In both the upper house and lower house, the party can chose to opt for open or closed lists. At the local level, parties can chose to go for open or closed lists when running for the local juntas (only present in 80 municipalities, and gives representation at the subnational level), and the local councils present for the over a thousand municipalities.<sup>9</sup>

To have a sufficient number of observations and sufficient variation in list type choices, we focus on elections for municipal councils. Moreover they are conducted across the country, and thus, allow for geographically representative qualitative and quantitative analysis. An example of a mixed ballot for a local election in the municipality of Abejorral is available in the Annex Figure 3.1. The upper-right most party is the only one that fielded a closed lists.

Parties have to decide on a list type three months before the election, and a few days before the official start of the campaign is allowed. Table 3.10 in the Appendix details this timeline for the 2015 election cycle.

### 3.3 Data, and Sample

We use the electoral data compiled by Pachón and Sánchez (2014) and gathered by the Colombian national electoral authority (*Registraduría Nacional del Estado Civil*). We updated this data with the 2015 election results. This data identifies results for local elections for all municipalities in Colombia for the period 2003 to 2011<sup>10</sup>. These data record the parties participating the local council election, the candidates, and the votes for each candidate or list. Table 3.1 and show that 3.2 show that most parties field open list, but that the fraction parties and the share of municipalites using closed lists stays roughly constant over our study period 2003–2015.

In order to understand the effects of list choice on the type of candidates and their behavior, we construct a novel dataset that includes information on the amount of campaigning and a broad range of candidate characteristics before they ran in the elections.<sup>11</sup> To measure the candidates' political experience, we trace the political careers of the candidates and code—at the moment of the election—the number of times the candidate has run for the local council, any other office, and if they have

---

votes of open lists go to the party and not specific candidates

<sup>9</sup>In addition, parties can also chose the list type in the 32 department assemblies

<sup>10</sup>However this data, for local councils had missing information in 2003 for all municipalities that had at least one closed list. We downloaded all the original electoral ballots and coded the missing names

<sup>11</sup>This is to make sure the characteristics observed are not a consequence of the election result.



ever served in office. To measure other relevant indicators of candidate quality, we obtained information on their voting registration available at *Registraduría del Estado Civil*. Based on this information, we can code if the candidate is registered to vote and if so, where, to measure whether the candidate is registered in the municipality she is seeking to represent. Moreover, we have information on whether the candidates were reported to have been registered to vote illegally in the past, and we code if they had been reporting on doing so.<sup>12</sup> One of the most common acts of political malpractice and election fraud is the (illegal) voting in a polling in a municipality other than the one where the voter is registered, typically in exchange for bribes.

We obtained information on the money candidates expend in campaigns. A system was set in place with help of US Agency for International Development in collaboration with Transparency International, which requires by law since 2009<sup>13</sup>, to report all sources of income and expenditures in campaigns. Lack of compliance is punishable by law. This information is broken down into different sources.<sup>14</sup> In order to separate the effects of personal investment on the campaign from the party investment, we construct two measures: Individual effort which accounts for the campaign spending that is directly related to the individual using its own funds or raising own funds, and the total campaign investment which adds the sources of financing that come from the party.<sup>15</sup>

In order to estimate the effects of list choice on different positions within the lists, we devised a measurement position conditional on list placement. List placement in closed list is done by the party, while open list ranking is given by the amount of votes each candidate receives. Both of these rankings are endogenous, but they key interest for us is to compare the preferences of voters to those of the party with respect to how they rank the candidates. For example, Galasso and Nannicini (2015) argue that

---

<sup>12</sup>Registering illegally through several means: Use of a dead person fingerprint, having a duplicate ID, irregular inscription of IDs, using a fake identity, underage registration and migrating illegally to vote in another polling station. Illegal migration of IDs implies a movement to a new voting site in mass, typically in exchange of money.

<sup>13</sup>Resolution 1094 of 2009.

<sup>14</sup>Whether the campaign income comes from: 1) their own contributions or immediate relatives, 2) Contributions by private donors, 3) Financial credits. 4) Income from public events by the party. 5) State financing. 6) Party financing that comes from private resources

<sup>15</sup>Individual campaign spending consists of the first three categories in the previous breakdown and total campaigning includes all the categories present in the data. Furthermore, there are campaign limits conditional on the municipality number of registered voters. For example under 25,000 registered voters, the campaign limits are 58 M COP These campaign limits are binding: In the case a candidate spends more money than he/she is allowed to, the sanctions range from a fine to returning the money to the State or losing the seat they were elected to. In order to scale the measurement of campaign investment, we scale the absolute value amount by the corresponding campaign limits, according to the municipality the candidate is participating in. In 2011 the campaign limits are at the individual level, but for 2015 the campaign limits were established at list level. In order to establish the 2015 campaign limits comparable to 2011, we divide the total campaign limit per list by the number of candidates in each list. In order to account potential effects for the change in the law, we use year fixed effects. Furthermore we estimate the effect of open lists by each year separately and we find consistent results. On average open lists campaign more than closed lists, and this is driven by the last placed candidates in the closed lists which campaign less

in closed lists expressive or naive voters only care about the top ranked candidates in the lists, whereas rational voter groups would understand that they can only influence the election of marginal candidates in the lists, and thus, focus their attention those. Using the list ranking we define marginal candidates to be those that were two seats above and below the last seat obtained in the party, and if the party didn't not obtain a seat, the first two are considered marginal. Top safe candidates, are the ones placed above the marginal, therefore safely elected, and loser candidates are the ones that are two seats below the one seat above and below the last seat obtained in the party to define marginals, and also a scaled measurement where the size of the margin is 20% of the council size.<sup>16</sup>

Table 3.3 provides descriptive statistics for our main outcomes. In general, the parties are fairly small with the average vote share of 13% and on average, 30% of parties are left without any seat. Open list parties are larger in terms of votes, seats and number of candidates. Open lists candidates campaign more relative to the campaigning limits. We also observe that top ranked candidates (either by parties or voters) have more political experience than the other candidates and are more experienced in the open lists. Data on voting registration shows open list having less candidates with a record of illegal voting and more candidates registered to vote at the own jurisdiction. However, these results are merely descriptive and thus not to be seen as our main evidence.

Regarding municipality characteristics, we obtained a municipality level panel (between 1993-2014) with a broad range of economic, social and institutional covariates, thanks to preexisting effort by *Universidad de los Andes*. A detailed description of some of these variables is also available in Table 3.11 in the Appendix.

### 3.4 What is the effect of open-/closed list on parties' electoral success?

To estimate the effect of list type, we regress our indicators of electoral performance, the vote and seat share of the party, on a binary indicator for open list, and a full set of election year and municipality times party fixed effects, as shown in equation 3.1:

$$y_{ipt} = \beta_0 + \beta_1 OpenList_{ipt} + \alpha_i \times \rho_p + \tau_t + \varepsilon_{ipt} \quad (3.1)$$

Where  $y_{ipt}$  are the electoral outcomes in municipality  $i$  for party  $p$  in election period  $t$ ,  $OpenList_{ipt}$  is a dummy if the party chose open list in municipality  $i$  in time period

---

<sup>16</sup>The results are robust to different definitions of marginal seats and are available upon request. The mapping between marginal seats and council size is as follows: Council size 21: margin size 4; Council size: 19 margin size 4; Council size: 17 margin size 3; Council size: 15 margin size 3; Council size: 13 margin size 3; Council size: 11 margin size 2; council size: 9 margin size 2; Council size: 7 margin size 1).



$t$ ,  $\alpha_i$  are the municipality fixed effects,  $\rho_p$  are the party fixed effects, and  $\tau_t$  are the year dummies. Where  $\beta_1$  is the effect of interest.

Therefore, these models identify the effects from those parties that change from closed to open list (or vice versa) in the same municipality from one election to the next, while controlling for all time invariant differences between local parties and municipalities. While such an analysis might not completely address the endogenous choice of ballot type, it does render a large set of confounders typically present in cross-sectional comparisons impotent.

Table 3.4 presents the results from these two-way fixed effects models. The coefficients on the *Open list* indicator in Model 1 and 2 reveals that switching from closed- to open-list increases the party's vote share by 6.8 percentage points and seat share by 8.6 percentage points, respectively, which corresponds to an 88% and 117% increase at the average. Adopting open list also decreases the probability of not getting a single seat by 41 percent. This suggests that running with closed-lists is a risky strategy even for the top ranked candidates (typically the party leader), who might be willing to sacrifice seats as long as she is elected. All these effects are not only substantively large, but also highly statistically significant.

How robust are these findings? Here, we summarize a couple of robustness checks to gauge the sensitivity of our main result. First, a potential concern is that most of our results are driven by a single party that is switching between open- and closed list. To test this, we checked how many parties change the list type and find that more than 577 parties do so over the study period (see Appendix Table 3.19). In addition, we gauge the sensitivity of the estimates by iteratively dropping each of the main parties. Table 3.20 in the Appendix reports the results, and shows that estimates remain very stable and always highly statistically significant, thereby corroborating the robustness of our results.

Second, our results might be confounded by parties with more experience changing to open lists more often and achieving more electoral success due to the experience, rather than directly due to the effect of ballot type. Related concern arises if new parties switch from closed to open more often and new parties also grow more. In order to check if our estimates are confounded by party experience (or the lack of it), we coded two additional variables: the number of elections the party has participated in any municipality, and the number of elections the party has participated in the same municipality. The results presented in Table 3.21 in the Appendix show that our main estimates remain virtually unchanged when controlling for one or both of these experience indicators.

Together, these tests corroborate the robustness of the result that adopting closed-list as opposed to open-list has a massive effect on electoral performance.

### 3.5 Why Do Parties Fare Better With Open-list?

Our setting provides clear evidence that switching from closed- to open-list dramatically improves the electoral success of parties. What mechanisms might explain this effect? It is well known that isolating the precise mechanism is very difficult with observational (and even experimental) data (Imai, Keele, Tingley, and Yamamoto, 2011). Nonetheless, in the following section we provide evidence that speak to the relative importance of two main mechanisms: how list type condition the selection of high-quality candidates and how they differentially incentivize candidates to expend campaign effort. This evidence draws on insights from 32 semi-structured qualitative interviews with candidates for local councils in a representative sample of municipalities as well as further candidate-level quantitative analysis. We interviewed candidates for local councils, during January to March 2016, regarding the municipal elections that took place in October 2015, across the main 5 main regions in Colombia. The Appendix in Section 3.10 gives a detailed description of the sampling strategy, questionnaire, and findings from these interviews.

A first hypothesis is that adopting open-list incentives all candidates to campaign, while under closed list the candidates in the bottom of the list do not campaign (see e.g., Bergman et al., 2013; Shugart et al., 2005; Nemoto and Shugart, 2013). What is the empirical evidence for this mechanism? First, if this incentive mechanism is important, we would expect party officials and candidates to mention it in the qualitative interviews. This is indeed the case. For example, a second placed candidate in a closed-list stated:

*“[A] disadvantage [of closed lists] is that people lower on the list know that they won’t make it, then they don’t work as hard as people higher on the list. When the list is open one depends [more] upon oneself.”*

Candidate for the Partido de la U party in *Tamara, Casanare*.

Similarly, an elected open-list candidate criticized closed-list for making it difficult to appropriate campaign effort, since the votes would go to the top of the list and there is little recognition for the effort made by other candidates:

*“With open-lists, each candidate works, and reaps what he grows, in closed-list you work for another person, it is not fair.”*

Candidate for the Partido de la U party in *Tamara, Casanare*.

Second, we would expect that candidates on closed-lists to expend on average less campaign effort, and that this behavior is particularly pronounced among the lower-ranked with little chances of winning a seat. To test for this incentive mechanism, we replicate the two-way fixed effect model but using as outcomes candidate-level

information on campaign effort, measured as the amount of personal funds invested and the total amount of funds candidates raised. Table 3.5 shows the results. For both measures of campaign effort, Models 1 and 6 show that on average, open-list candidates invest 40% more and spend 50% more. Consistent with the hypothesis that in particular those with a small chance of being elected have few incentives to expend effort, we find that these substantively large and statistically significant results are exclusively driven by marginal and bottom-ranked candidates, with no differential effect on top-ranked and elected candidates.<sup>17</sup>

A second hypothesis is that because closed lists do not reward politicians' individual campaign effort and lower the chances of winning a seat, they fail to attract high-quality candidates. We also find qualitative and quantitative evidence for this selection mechanism. First, this mechanism was mentioned many times in the qualitative interviews. For example, when asked how the party selects the candidates at the bottom of the closed-list, the interviews revealed that these are often people affiliated with the party but little formal education and no interest in holding office. For example, a sixth-placed candidate on a closed list with no chances of winning said:

*"I don't understand the difference between open and closed list, I was just supporting our candidate (top of the list) that didn't get elected...."*

Candidate for the Partido de la U party in *Paez, Cauca*.

Similarly, we interviewed an indigenous person who was never engaged in politics before but apparently lured into signing the paperwork to run as a candidate on a closed-list, without properly understanding what that implies. It seems that in this case the party's sole goal was to simply fill the list. Another lower placed, closed-list candidate mentioned that parties sometimes "trick" people that enjoy local popularity to run as low-ranked candidates on closed lists to garner votes for the top candidate:

*"In this municipality people take advantage of others, of the peasants, honest people who want to work. (...) Sometimes a popular person who has support in their districts but doesn't know much about politics is placed low in the closed-list, so the people placed highlist win those votes without doing anything."*

Candidate for the Partido de la U party in *Tamara, Casanare*.

Another aspect of the same pattern, mentioned several times in the interviews, is the placement of women on closed lists. In 2007, Colombia enacted a gender quota that stipulated that all lists need least 30% of women. For example, we learned from one case where several female administrators working in party offices were used to fill the

---

<sup>17</sup>The differences between top-ranked and marginal, and top and bottom candidates, are also highly significant.

last places of a closed list in order to comply pro forma with the gender quota. Our candidate-level data confirms that indeed, women are disproportionately often placed at the bottom of the closed lists (during the electoral period of 2003-2015, women on average were placed in the percentile 67 in closed lists, while in open lists they were placed higher in percentile 58. This difference is statistically significant).<sup>18</sup>

Second, we can again leverage our candidate-level data to quantitatively explore the implications of this selection mechanism. First, we test whether candidates on open lists have more political experience, measured as the number of times the candidates have ran in previous municipal elections, the number of times they have been elected to the municipal council, and the number of times they participated in any other election. When interpreting the results, it is important to keep in mind that while the average differences in candidate quality between open and closed lists are the results of their differential attractiveness to high-quality candidates, the ultimate rank of the candidates under open list is an expression of the preferences of voters over candidates. We discuss both channels in turn. First, Model 1, 6, and 11 in Table 3.6 show that on average, open lists are able to attract significantly more experienced candidates. This pattern holds for all three experience measures, with effect sizes ranging between 15% for running in previous elections to 91% for having experience in office. Second, we find that voters express clear preferences for experienced candidates when they have the chance to do so. For all three outcomes, the Models 2–5, 7–10, and 12–15 show that differences in candidate quality between open-and closed list are larger for those in top seats compared to marginal and bottom-ranked candidates.

As a second measure of candidate quality, we focus on the candidate's dedication to and engagement in the municipality they seek to represent by measuring whether he/she registered to vote in the municipality he/she is running for office before the election. Table 3.7 shows the results. The overwhelming majority of candidates is indeed registered in the municipality where they are running, we find that, on average, candidates, open-list candidates are 6% more likely, a statistically significant effect. Consistent with the previous results, we also find evidence that voters are systematically punishing candidates for not registering in the same municipality close to their constituency when they have the chance to do so under open list.

As a third measure of candidate quality, we focus on two indicators of election fraud. We code whether candidates registered to vote illegally, or illegally moved to a different polling station than where they are registered before the election. Table 3.8 shows the results. While only a small minority of candidates engaged in election fraud, we find that on average, open-list candidates are 17% less likely to register to vote illegally, and 19% less likely to vote in a polling station without registering. Both effects are statistically significant effect, and mostly driven by candidates in marginal seats.

---

<sup>18</sup>We explore the compliance with and effect of these gender quotas under open and closed list in a separate paper.

In sum, we find strong qualitative and quantitative evidence for both mechanism. Open-list create more incentives for all candidates to expend higher levels of campaign effort, and parties adopting open lists manage to attract more experienced candidates, that are more committed to their constituency, and less likely to have engaged in election fraud.

### 3.6 Why Do Some Parties Sometimes Choose Closed-List?

Given their detrimental impact on electoral performance, one has to wonder why some parties sometimes decide to field closed-lists. One hypothesis is that parties can exert more control over the candidates and who gets elected. Consistent with this, several candidates mentioned in the interviews that closed-lists are a means for strong party leaders to exert control. One president of a local party even frankly admitted as much:

*“With closed lists the party has more control over candidates, and me as a president could control more the candidates, while in open list they are autonomous”*

Candidate for Polo Democrático Alternativo at *Balboa, Cauca*.

Another advantage of closed lists, proposed by several interviewees, is that they both induce and signal party cohesion. For example, one candidate sees the benefits of closed lists for ideological cohesion

*“When the list is closed the party is strengthened institutionally, because you are voting for a party and not a person, and this could lead to more ideological party cohesion.”*

Candidate for the Alianza Verde party at *Cartago, Valle*.

while another thought the signal it sends is more important:

*“We were a group of friends interested in the welfare of the municipality (...) we chose closed list because we thought it was the best way to present ourselves to the community, as a group, a family, rather than a party with people who each walked their own path. The idea is that the community would see us as a team (...)”*

Candidate for the Polo party for *Pasto, Nariño*.

But even after accounting for the benefits of closed lists for party control and cohesion, one might wonder if running with closed lists is a viable long-term strategy.

Given their massive effect on vote and seat shares, we might expect that choosing closed lists in an election, and suffering the associated electoral losses, most parties would switch to open lists in the next election cycle. The transition matrix presented in Table 3.9 explores the dynamics of adopting list types. Indeed, we find that if a party uses closed list in election  $t$ , chances are very high that it switches to open list in the next election  $t + 1$ . Moreover, and not surprising given our main findings on the electoral repercussions closed lists, the party, and the candidate, is more likely to not run again in  $t + 1$ . This suggests that open lists are absorbing state, and closed lists would be absent were it not for the new party entrants that initially adopt closed lists.

Tables 3.14 and 3.18 explore this pattern in more detail. Table 3.14 shows that among the ten major parties, closed lists are generally rarely used, and their use becomes even rarer over time. Among the major parties, only the new entrant in 2015, the Centro Democrático, decided to field almost 50% closed lists. (Our prediction is that they will also switch to open list for the next election). Table 3.18 provides further evidence that closed lists are mainly popular among new and small parties, and that they switch to open list if they survive their first election cycle.

## 3.7 Conclusions

Previous studies have shown associations suggesting that electoral rules influence political competition, selection and policy outcomes. We provide novel evidence focusing on the unique case of Colombia, where parties are free to choose between open and closed list in local PR elections. This provides rare within-country variation in election system that we use to study three questions. First, what is the effect of list type on parties electoral performance? Second, what is mechanism behind our result that open lists generate dramatically more votes than closed lists? Third, why would any party choose closed list given its detrimental effect on the election outcomes.

As the key contribution of the paper, we use a novel, candidate-level panel dataset covering all 1,100 Colombian municipalities for the 2003 – 2015 period. Exploiting variation in list type within municipalities and parties over time operationalized with a municipality times party fixed effects regressions, we find that the adoption of open list is associated with dramatically higher vote and seat shares.

In order to understand the party's and the candidates' decision making processes, we conduct extensive semi-structured interviews with a representative sample of candidates. Using the insights from these interviews, we explore the main mechanism that might explain the vote share effects. First, we find evidence of list-type induced differences in campaign effort as lower-ranked candidates in closed lists have little incentives to invest effort in campaigning. Second, closed lists have trouble attracting high-quality candidates to other than the top seats.

We go further and provide also strong quantitative evidence for both these mechanisms using data on campaigning and candidate types. We show that by influencing the incentives for all candidates to expend more effort, by attracting more experienced candidates, that are more committed to their constituency, and less likely to have engaged in election fraud, the seemingly narrow choice between open and closed has major repercussions: not only for the party facing that choice, but also for the politicians and their quality, and thus, for representative democracy more generally.

Finally we show that mainly new parties adopt closed lists, and often change to open in the next election if they do not exit. On the contrary open list parties stick to the open list also in the next elections. This indicates that new parties may make seemingly uninformed decisions regarding list type but do learn with experience.

It seems likely that our results on the incentives induced by different list types would also generalize outside the Colombian mixed system to comparing pure open list to pure closed list systems. In particular, closed lists would likely have the same difficulties in attracting high-quality candidates to low-ranked positions and to make them exert effort also in pure closed list system. However, the effects of list type on vote shares or who gets elected may vary depending on the choices of all parties, not just the own party choice, and thus, be different in a mixed than pure system. In future work, we construct a game-theoretic model of list choice to rationalize the findings of this paper and to evaluate how these results generalize.

## 3.8 Tables

**Table 3.1: Share of municipalities (of total municipalities) with number of closed lists**

Number of Closed lists							
Panel A. - All parties							
Year	0	1	2	3	4	5	6+
<b>2003</b>	59.8	22.0	9.8	4.4	2.2	1.1	0.6
<b>2007</b>	80.5	16.4	2.6	0.4	0.2	0.0	0.0
<b>2011</b>	72.9	23.6	2.9	0.5	0.1	0.0	0.0
<b>2015</b>	54.7	32.6	10.4	1.8	0.4	0.2	0.0

**Notes:** Total number of municipalities in sample: 2003: 1087, 2007: 1098, 2011: 1099, 2015: 1101. Panel A: All parties participating in elections.

**Table 3.2: Share of municipalities with %of closed lists.**

— % of Closed lists (# of Closed lists/ Total Lists in Municipality):							
Year	0-10%	10-20%	20-30%	30-40%	40-50%	50-60%	60+%
<b>2003</b>	0.62	0.14	0.11	0.05	0.02	0.03	0.02
<b>2007</b>	0.83	0.10	0.06	0.01	0.00	0.00	0.00
<b>2011</b>	0.78	0.15	0.06	0.01	0.00	0.00	0.00
<b>2015</b>	0.62	0.24	0.11	0.02	0.01	0.00	0.00

**Notes:** Total number of municipalities in sample: 2003: 1087, 2007: 1098, 2011: 1099, 2015: 1101.



**Table 3.3: Descriptive Statistics and Differences in Means Across List Type**

Variable	N	Mean	Averages within List			SD	Min	Max
			<i>Open</i>	<i>Closed</i>	<i>diff. sig</i>			
<b>A. Electoral Variables</b>								
Party Vote Share	30793	0.13	0.14	0.05	***	0.11	0	1
% of Seats Obtained by the party	30778	0.14	0.15	0.04	***	0.14	0	1
Party obtained one seat	30804	0.7	0.74	0.22	***	0.46	0	1
% of list Size filled	30766	0.81	0.83	0.55	***	0.26	0.05	3.25
<b>B. Characteristics of Candidates in the list:</b>								
<b>B1. Campaign Data (**)</b>								
Total Campaign Investment (Millions COP) - Avg	14,491	1.71	1.70	1.81		3.99	0	128
Total Campaign Investment (Millions COP) - Top	3,163	4.85	4.77	11.35	**	14.2	0	195
Personal Campaign Investment (Millions COP) - Avg	14,479	1.63	1.64	1.55		3.8	0	126
Personal Campaign Investment (Millions COP) - Top	3,163	4.62	4.58	8.07		13	0	178
% of Campaign Limit (Total Investment) - Avg	14491	0.06	0.06	0.04	***	0.09	0	3.11
% of Campaign Limit (Total Investment) - Top	3163	0.13	0.13	0.14		0.18	0	1.29
% of Campaign Limit (Personal Investment) - Avg	14479	0.06	0.06	0.04	***	0.09	0	2.57
% of Campaign Limit (Personal Investment) - Top	3163	0.13	0.13	0.13		0.17	0	1.29
<b>B1. Voting Registration (Illegal Registration)</b>								
Registered Illegally to vote (*) (Avg)	30779	0.04	0.04	0.05	***	0.09	0	1
Registered Illegally to vote (*) (Top)	7756	0.02	0.01	0.06	***	0.10	0	1
Moved Illegally to vote in another polling station (Avg)	30779	0.04	0.04	0.05	***	0.08	0	1
Moved Illegally to vote in another polling station (Top)	7756	0.01	0.01	0.06	***	0.10	0	1
<b>B2. Candidate experience</b>								
# of times cand. ran for any public elected post (Avg)	30779	1.79	1.81	1.51	***	0.53	1	6
# of times cand. ran for any public elected post (Top)	7043	2.63	2.64	2.15	***	1.12	1	6
# of times cand. in list ran for local council (Avg)	30779	1.57	1.58	1.33	***	0.40	1	4
# of times cand. in list ran for local council (Top)	7043	2.18	2.19	1.63	***	0.90	1	4
# of times cand. in list got elected to council (Avg)	30779	0.35	0.36	0.11	***	0.35	0	4
# of times cand. in list got elected to council (Top)	7043	1.76	1.77	1.33	***	0.79	1	4

**Notes:** \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Number of total party lists: 30804. Sample: Party lists in 1101 municipalities in Colombia council elections: 4 elections between 2003- 2015. (\*) Use of a dead person fingerprint, having a duplicate ID, irregular inscription of ID, using a fake identity, Underage registration, Illegal migration of IDs implies a movement of ID to a new voting site in mass, typically in exchange of money.(\*\*) Only available for 2011 and 2015.

**Table 3.4: Effect of Open lists on electoral outcomes**

	(1)	(2)	(3)
	Party vote share	% Seat party	Party one seat
Open list	0.068*** (0.003)	0.086*** (0.004)	0.410*** (0.013)
Year dummies	✓	✓	✓
Mun FE (x) Party FE	✓	✓	✓
Mean Dep. Var	0.0769	0.0734	0.392
Effect Size ( $\Delta\%$ )	88.80	117.7	104.4
95% CI LB	80.87	107.7	98.16
95% CI UB	96.72	127.6	110.7
Municipalities	1101	1101	1101
# Observations	30793	30778	30804

**Notes:** Standard errors in parenthesis and clustered at the municipality level.\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 3.5: Effects of Open List on Campaign Investment**

	(1) (2) (3) (4) (5)					(6) (7) (8) (9) (10)				
	% of Campaign limit (Personal investment)					% of Campaign limit (Total investment)				
	Avg	Top Safe	Marginal	Loser	Elected	Avg	Top Safe	Marginal	Loser	Elected
Open list	0.015*** (0.003)	-0.016 (0.039)	0.021*** (0.004)	0.017*** (0.002)	0.012 (0.011)	0.015*** (0.003)	-0.015 (0.039)	0.022*** (0.004)	0.018*** (0.002)	0.006 (0.012)
Year dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mun FE(x)Party FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean Dep. Var	0.028	0.078	0.035	0.023	0.061	0.037	0.078	0.055	0.028	0.092
Effect Size ( $\Delta\%$ )	52.05	-20.5	61.11	72.3	19.01	39.87	-19.01	39.82	64.88	6.762
95% CI LB	32.61	-117.7	39.55	54.99	-17	25.15	-115.9	25.49	50.52	-17.9
95% CI UB	71.49	76.71	82.67	89.62	55.02	54.6	77.88	54.14	79.24	31.42
Municipalities	1101	1004	1101	1099	1101	1101	1004	1101	1099	1101
# Observations	14479	2770	14040	13161	10758	14491	2770	14053	13182	10762

**Notes:** Standard errors in parenthesis and clustered at the municipality level.\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 3.6: Effects of list type on political experience.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	# of times cand. ran for any public elected post					# of times cand. in list ran for local council				
	Avg	Top Safe	Marginal	Loser	Elected	Avg	Top Safe	Marginal	Loser	Elected
Open list	0.256*** (0.013)	0.454*** (0.081)	0.264*** (0.019)	0.225*** (0.011)	0.236*** (0.056)	0.194*** (0.010)	0.457*** (0.065)	0.230*** (0.015)	0.148*** (0.009)	0.237*** (0.044)
Mean Dep. Var	1.71	2.41	1.94	1.51	2.44	1.521	2.046	1.686	1.372	2.080
Effect Size (%)	14.94	18.87	13.61	14.90	9.710	12.78	22.32	13.66	10.77	11.41
95% CI LB	13.49	12.24	11.73	13.41	5.242	11.55	16.10	11.93	9.476	7.265
95% CI UB	16.39	25.49	15.49	16.39	14.18	14.01	28.53	15.39	12.07	15.56
Municipalities	1101	1096	1101	1101	1101	1101	1096	1101	1101	1101
# Observations	30779	7043	30771	28978	21582	30779	7043	30771	28978	21582

	(11)	(12)	(13)	(14)	(15)
	# of times cand. in list got elected to council				
	Avg	Top Safe	Marginal	Loser	Elected
Open list	0.177*** (0.008)	0.342*** (0.048)	0.271*** (0.011)	0.028*** (0.003)	0.107*** (0.034)
Mean Dep. Var	0.194	1.626	0.326	0.0353	1.559
Effect Size (%)	91.14	21.01	83.14	79.31	6.837
95% CI LB	83.28	15.22	76.43	64.02	2.568
95% CI UB	98.99	26.81	89.85	94.61	11.11
Municipalities	1101	1096	1101	1101	1101
# Observations	30779	7043	30771	28978	21582

**Notes:** Standard errors in parenthesis and clustered at the municipality level.\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 3.7: Effects of list type on engagement in constituency.**

	(6)	(7)	(8)	(9)	(10)
	Registered vote same place running				
	Avg	Top Safe	Marginal	Loser	Elected
Open list	0.052*** (0.007)	0.076** (0.031)	0.059*** (0.008)	0.036*** (0.007)	0.061*** (0.017)
Year dummies		✓	✓	✓	✓
Mun FE (x) Party FE		✓	✓	✓	✓
Mean Dep. Var	0.896	0.961	0.897	0.897	0.955
Effect Size ( $\Delta\%$ )	5.842	7.898	6.577	3.975	6.365
95% CI LB	4.381	1.667	4.906	2.392	2.892
95% CI UB	7.302	14.13	8.248	5.558	9.838
Municipalities	1101	1096	1101	1101	1101
# Observations	30706	6903	30615	28781	21206

**Notes:** Standard errors in parenthesis and clustered at the municipality level.\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 3.8: Effects of list type on election fraud.**

	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
	Registered illegally to vote					Voted illegally in different polling station				
	Avg	Top Safe	Marginal	Loser	Elected	Avg	Top Safe	Marginal	Loser	Elected
Open list	-0.008*** (0.003)	-0.024 (0.019)	-0.015*** (0.004)	0.001 (0.003)	-0.008 (0.008)	-0.008*** (0.003)	-0.026 (0.019)	-0.015*** (0.004)	0.000 (0.003)	-0.008 (0.007)
Year dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mun FE (x) Party FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean Dep. Var	0.045	0.012	0.044	0.046	0.020	0.043	0.012	0.040	0.045	0.014
Effect Size ( $\Delta$ %)	-17.18	-203.4	-33.11	1.121	-41.03	-19.45	-218.1	-36.69	0.698	-53.01
95% CI LB	-29.48	-516.7	-50.02	-12.57	-117.3	-32.34	-530.9	-54.88	-13.07	-151.3
95% CI UB	-4.874	109.8	-16.20	14.81	35.20	-6.559	94.75	-18.50	14.46	45.24
Municipalities	1101	1096	1101	1101	1101	1101	1096	1101	1101	1101
# Observations	30779	7043	30771	28977	21582	30779	7043	30771	28977	21582

**Notes:** Standard errors in parenthesis and clustered at the municipality level.\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 3.9: Transition Matrices**

<b>Parties</b>				
	Closed List t+1	Open List t+1	Exit t+1	N
Closed List t	0.088	0.256	0.656	1181
Open List t	0.010	0.533	0.457	17807

<b>Candidates</b>				
	Closed List t+1	Open List t+1	Not running t+1	N
Closed List t	0.03	0.183	0.787	6881
Open List t	0.06	0.319	0.676	162543

### 3.9 Figures

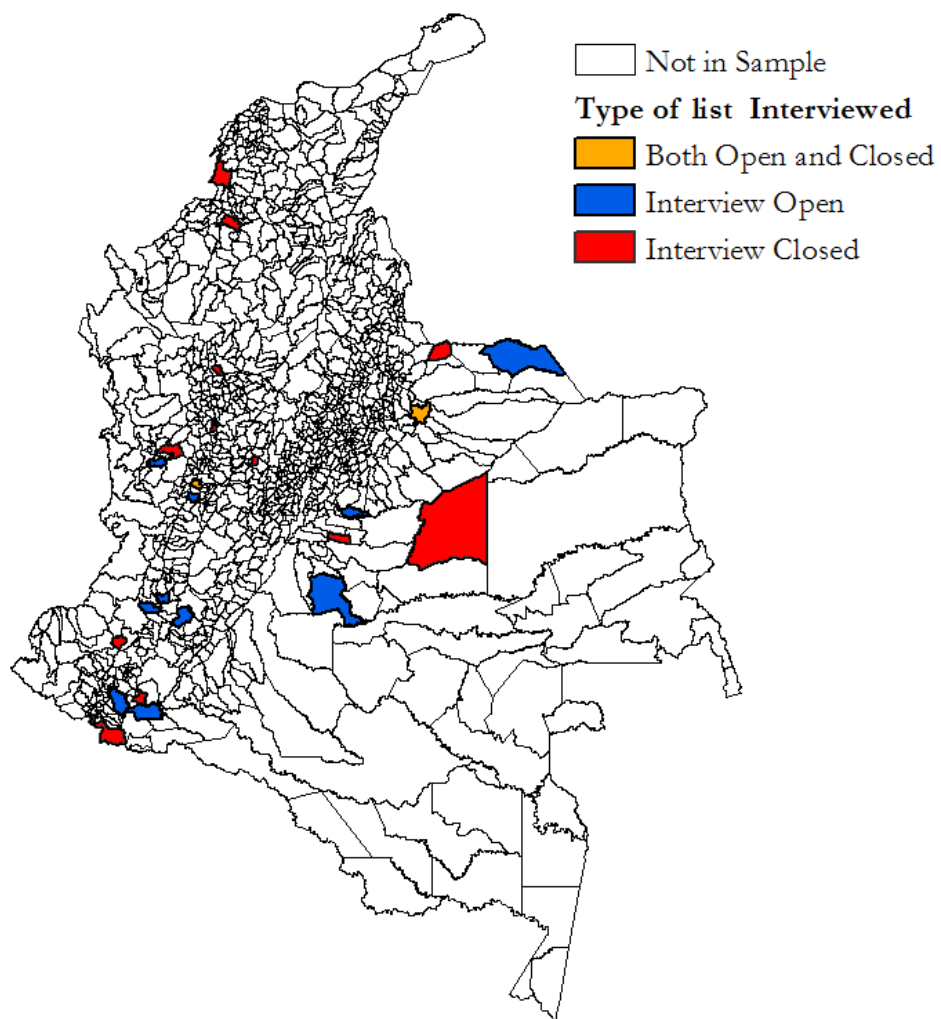
Figure 3.1: Example of a mixed ballot in Colombia.

CONCEJO DE ABEJORRAL(ANTIOQUIA) -01004CO-

<table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td colspan="2" style="text-align: center;"><b>MOVIMIENTO COLOMBIA VIVA</b></td> </tr> <tr> <td colspan="2" style="text-align: center;"></td> </tr> <tr> <td colspan="2" style="text-align: center;"><b>1</b></td> </tr> <tr> <td colspan="2" style="text-align: center;">VOTO</td> </tr> <tr> <td colspan="2" style="text-align: center;">LISTA CON VOTO PREFERENTE</td> </tr> <tr> <td>1 ALVARO MUÑOZ VILLEGAS</td> <td>5 CARLOS FABIAN VARGAS OCAMPO</td> </tr> <tr> <td>2 LUIS JAVIER OSPINA</td> <td>6 HERNAN DE JESUS OSORIO PATIÑO</td> </tr> <tr> <td>3 JAIME ALBERTO RESTREPO RESTREPO</td> <td>7 GUSTAVO DE JESUS NARANJO OSORIO</td> </tr> <tr> <td>4 ANTONIO JOSE ARANGO ARANGO</td> <td></td> </tr> </table>	<b>MOVIMIENTO COLOMBIA VIVA</b>				<b>1</b>		VOTO		LISTA CON VOTO PREFERENTE		1 ALVARO MUÑOZ VILLEGAS	5 CARLOS FABIAN VARGAS OCAMPO	2 LUIS JAVIER OSPINA	6 HERNAN DE JESUS OSORIO PATIÑO	3 JAIME ALBERTO RESTREPO RESTREPO	7 GUSTAVO DE JESUS NARANJO OSORIO	4 ANTONIO JOSE ARANGO ARANGO		<table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td colspan="2" style="text-align: center;"><b>MOVIMIENTO EQUIPO COLOMBIA</b></td> </tr> <tr> <td colspan="2" style="text-align: center;"></td> </tr> <tr> <td colspan="2" style="text-align: center;"><b>3</b></td> </tr> <tr> <td colspan="2" style="text-align: center;">VOTO</td> </tr> <tr> <td colspan="2" style="text-align: center;">LISTA CON VOTO PREFERENTE</td> </tr> <tr> <td>1 TULIA OTALVARO DE R</td> <td>7 LUZ MARINA TABARES GALLEGO</td> </tr> <tr> <td>2 LEON EDGAR JARAMILLO PATIÑO</td> <td>8 JESUS MARIA VILLA ALVAREZ</td> </tr> <tr> <td>3 HERNANDO JOSE DEL RIO GARCIA</td> <td>9 RODRIGO DE JESUS VARGAS CARDONA</td> </tr> <tr> <td>4 GUILLERMO PALACIO RESTREPO</td> <td>10 MARIA ALIJA CARMONA YDA DE CHALARCA</td> </tr> <tr> <td>5 JESUS ARSENO CARMONA MONTOYA</td> <td>11 WALTER LAZARO CARDONA RIOS</td> </tr> <tr> <td>6 JAIME ALVEIRO ARBOLEDA GOMEZ</td> <td>12 LUIS GUILLERMO BERNAL PALACIO</td> </tr> </table>	<b>MOVIMIENTO EQUIPO COLOMBIA</b>				<b>3</b>		VOTO		LISTA CON VOTO PREFERENTE		1 TULIA OTALVARO DE R	7 LUZ MARINA TABARES GALLEGO	2 LEON EDGAR JARAMILLO PATIÑO	8 JESUS MARIA VILLA ALVAREZ	3 HERNANDO JOSE DEL RIO GARCIA	9 RODRIGO DE JESUS VARGAS CARDONA	4 GUILLERMO PALACIO RESTREPO	10 MARIA ALIJA CARMONA YDA DE CHALARCA	5 JESUS ARSENO CARMONA MONTOYA	11 WALTER LAZARO CARDONA RIOS	6 JAIME ALVEIRO ARBOLEDA GOMEZ	12 LUIS GUILLERMO BERNAL PALACIO	<table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td colspan="2" style="text-align: center;"><b>MOIR</b></td> </tr> <tr> <td colspan="2" style="text-align: center;"></td> </tr> <tr> <td colspan="2" style="text-align: center;"><b>47</b></td> </tr> <tr> <td colspan="2" style="text-align: center;">VOTO</td> </tr> <tr> <td colspan="2" style="text-align: center;">LISTA SIN VOTO PREFERENTE</td> </tr> <tr> <td>RICARDO DE JESUS OCAMPO VALENCIA</td> <td>OSCAR HERNAN SOSA</td> </tr> <tr> <td>JORGE IVAN HENAO HENAO</td> <td></td> </tr> </table> <table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td colspan="2" style="text-align: center;"><b>PARTIDO COLOMBIA DEMOCRATICA</b></td> </tr> <tr> <td colspan="2" style="text-align: center;"></td> </tr> <tr> <td colspan="2" style="text-align: center;"><b>54</b></td> </tr> <tr> <td colspan="2" style="text-align: center;">VOTO</td> </tr> <tr> <td colspan="2" style="text-align: center;">LISTA CON VOTO PREFERENTE</td> </tr> <tr> <td>1 LUIS JAVIER MUÑOZ</td> <td>3 FABIO HUMBERTO HERRERA ACEVEDO</td> </tr> <tr> <td>2 AURELIO ANTONIO ACEVEDO VALENCIA</td> <td>4 GILBERTO RAMIREZ LOAIZA</td> </tr> </table>	<b>MOIR</b>				<b>47</b>		VOTO		LISTA SIN VOTO PREFERENTE		RICARDO DE JESUS OCAMPO VALENCIA	OSCAR HERNAN SOSA	JORGE IVAN HENAO HENAO		<b>PARTIDO COLOMBIA DEMOCRATICA</b>				<b>54</b>		VOTO		LISTA CON VOTO PREFERENTE		1 LUIS JAVIER MUÑOZ	3 FABIO HUMBERTO HERRERA ACEVEDO	2 AURELIO ANTONIO ACEVEDO VALENCIA	4 GILBERTO RAMIREZ LOAIZA
<b>MOVIMIENTO COLOMBIA VIVA</b>																																																																						
<b>1</b>																																																																						
VOTO																																																																						
LISTA CON VOTO PREFERENTE																																																																						
1 ALVARO MUÑOZ VILLEGAS	5 CARLOS FABIAN VARGAS OCAMPO																																																																					
2 LUIS JAVIER OSPINA	6 HERNAN DE JESUS OSORIO PATIÑO																																																																					
3 JAIME ALBERTO RESTREPO RESTREPO	7 GUSTAVO DE JESUS NARANJO OSORIO																																																																					
4 ANTONIO JOSE ARANGO ARANGO																																																																						
<b>MOVIMIENTO EQUIPO COLOMBIA</b>																																																																						
<b>3</b>																																																																						
VOTO																																																																						
LISTA CON VOTO PREFERENTE																																																																						
1 TULIA OTALVARO DE R	7 LUZ MARINA TABARES GALLEGO																																																																					
2 LEON EDGAR JARAMILLO PATIÑO	8 JESUS MARIA VILLA ALVAREZ																																																																					
3 HERNANDO JOSE DEL RIO GARCIA	9 RODRIGO DE JESUS VARGAS CARDONA																																																																					
4 GUILLERMO PALACIO RESTREPO	10 MARIA ALIJA CARMONA YDA DE CHALARCA																																																																					
5 JESUS ARSENO CARMONA MONTOYA	11 WALTER LAZARO CARDONA RIOS																																																																					
6 JAIME ALVEIRO ARBOLEDA GOMEZ	12 LUIS GUILLERMO BERNAL PALACIO																																																																					
<b>MOIR</b>																																																																						
<b>47</b>																																																																						
VOTO																																																																						
LISTA SIN VOTO PREFERENTE																																																																						
RICARDO DE JESUS OCAMPO VALENCIA	OSCAR HERNAN SOSA																																																																					
JORGE IVAN HENAO HENAO																																																																						
<b>PARTIDO COLOMBIA DEMOCRATICA</b>																																																																						
<b>54</b>																																																																						
VOTO																																																																						
LISTA CON VOTO PREFERENTE																																																																						
1 LUIS JAVIER MUÑOZ	3 FABIO HUMBERTO HERRERA ACEVEDO																																																																					
2 AURELIO ANTONIO ACEVEDO VALENCIA	4 GILBERTO RAMIREZ LOAIZA																																																																					
<table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td colspan="2" style="text-align: center;"><b>MOVIMIENTO SI COLOMBIA</b></td> </tr> <tr> <td colspan="2" style="text-align: center;"></td> </tr> <tr> <td colspan="2" style="text-align: center;"><b>2</b></td> </tr> <tr> <td colspan="2" style="text-align: center;">VOTO</td> </tr> <tr> <td colspan="2" style="text-align: center;">LISTA CON VOTO PREFERENTE</td> </tr> <tr> <td>1 ARIEL GUILLERMO CHICA RIOS</td> <td>5 BEATRIZ ELENA SEPULVEDA</td> </tr> <tr> <td>2 ALBERTO ARANGO RUIZ</td> <td>6 JOHN JAIRO BOTERO RIVERA</td> </tr> <tr> <td>3 ROBINSON DE JESUS OROZCO LONDOÑO</td> <td>7 BERNARDO ANTONIO CHICA BILBAO</td> </tr> <tr> <td>4 GERMAN DE JESUS GONZALEZ ZULUAGA</td> <td></td> </tr> </table>	<b>MOVIMIENTO SI COLOMBIA</b>				<b>2</b>		VOTO		LISTA CON VOTO PREFERENTE		1 ARIEL GUILLERMO CHICA RIOS	5 BEATRIZ ELENA SEPULVEDA	2 ALBERTO ARANGO RUIZ	6 JOHN JAIRO BOTERO RIVERA	3 ROBINSON DE JESUS OROZCO LONDOÑO	7 BERNARDO ANTONIO CHICA BILBAO	4 GERMAN DE JESUS GONZALEZ ZULUAGA		<table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td colspan="2" style="text-align: center;"><b>PARTIDO LIBERAL COLOMBIANO</b></td> </tr> <tr> <td colspan="2" style="text-align: center;"></td> </tr> <tr> <td colspan="2" style="text-align: center;"><b>18</b></td> </tr> <tr> <td colspan="2" style="text-align: center;">VOTO</td> </tr> <tr> <td colspan="2" style="text-align: center;">LISTA CON VOTO PREFERENTE</td> </tr> <tr> <td>1 LUIS JAVIER GRISALES OSORIO</td> <td>4 CARLOS EMILIO URIBE GUZMAN</td> </tr> <tr> <td>2 LUIS GUILLERMO JARAMILLO PALACIO</td> <td>5 ALCIDES DE JESUS CORREA</td> </tr> <tr> <td>3 LUIS GUILLERMO CARDONA RIOS</td> <td></td> </tr> </table>	<b>PARTIDO LIBERAL COLOMBIANO</b>				<b>18</b>		VOTO		LISTA CON VOTO PREFERENTE		1 LUIS JAVIER GRISALES OSORIO	4 CARLOS EMILIO URIBE GUZMAN	2 LUIS GUILLERMO JARAMILLO PALACIO	5 ALCIDES DE JESUS CORREA	3 LUIS GUILLERMO CARDONA RIOS		<table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td colspan="2" style="text-align: center;"><b>VOTO EN BLANCO</b></td> </tr> </table>	<b>VOTO EN BLANCO</b>																																	
<b>MOVIMIENTO SI COLOMBIA</b>																																																																						
<b>2</b>																																																																						
VOTO																																																																						
LISTA CON VOTO PREFERENTE																																																																						
1 ARIEL GUILLERMO CHICA RIOS	5 BEATRIZ ELENA SEPULVEDA																																																																					
2 ALBERTO ARANGO RUIZ	6 JOHN JAIRO BOTERO RIVERA																																																																					
3 ROBINSON DE JESUS OROZCO LONDOÑO	7 BERNARDO ANTONIO CHICA BILBAO																																																																					
4 GERMAN DE JESUS GONZALEZ ZULUAGA																																																																						
<b>PARTIDO LIBERAL COLOMBIANO</b>																																																																						
<b>18</b>																																																																						
VOTO																																																																						
LISTA CON VOTO PREFERENTE																																																																						
1 LUIS JAVIER GRISALES OSORIO	4 CARLOS EMILIO URIBE GUZMAN																																																																					
2 LUIS GUILLERMO JARAMILLO PALACIO	5 ALCIDES DE JESUS CORREA																																																																					
3 LUIS GUILLERMO CARDONA RIOS																																																																						
<b>VOTO EN BLANCO</b>																																																																						

Source: National Registry Office, local ballot for 2003. Party MOIR chose to go for closed list.

Figure 3.2: Map of council candidates interviewed. Colombia local elections 2015.



## 3.10 Appendix A: Interviews

### 3.10.1 Interviews

In order to understand how the decision makers themselves think about the list choice and its effects, we conducted a series of semi-structured interviews. We interviewed candidates for local councils, during January to March 2016, regarding the municipal elections that took place in October 2015 in every municipality of Colombia.

The candidates to be interviewed were selected from the 5 main regions of Colombia: The Andean region, the Caribbean region, the pacific region, the Orinoquia region, and Amazon region. In each region we selected a representative municipality in terms of several covariates that had only open lists, and then matched it with a closed-list municipality within the same department with similar covariates.<sup>19</sup>

In terms of parties, we selected parties that both used open and closed lists, and aimed to maintain a representative sample across the political spectrum. We selected the centrists Partido Alianza Verde, the leftist Polo Democrático Alternativo, and the center right Cambio Radical, and Partido de la U Party. Within each list we selected the candidate at the top of the list and the candidate in the middle of the list or the candidate who barely lost. All together we sampled 102 candidates.

We received their phone numbers from the national party office. Out of the 102 base sample, 4 had no phone number, of those 98 with a phone number (which is not necessarily their own private number, but may belong to a neighbor for example), 42 picked up the phone and 10 of them said they were not interested. Therefore, in total 32 interviews were conducted. Luckily, we still have a well balance sample of 16 in open lists and 16 in closed lists. 7 interviews were conducted in Cambio Radical, 8 in Partido de la U, 11 in Polo Democrático Alternativo, and 6 in Alianza Verde. A map of the interview distribution across the country can be found in Figure 2.

The interview questionnaire aimed to capture who makes the decision about list types; which factor do the decision-makers take into account when choosing between open or closed lists, and what are, from their perspective, the potential advantages and disadvantages.

### 3.10.2 Questionnaire

#### A. General characteristics:

**A1.** Who did decide to go with open or closed list in this election? Is it a decision at the national, department or local level? Is there a local party leader?

---

<sup>19</sup>Matching within the same department is to minimize the physical distance between different municipalities, which could ensure closer cultural similarity and similar policies within the department. As stated in the introduction Colombia is very diverse culturally across its regions and making comparisons within departments ensured a better comparison.

**A2.** How was that decision made? (describe the process if there are more people involved than just the party leader).

**A3.** When was that decision made? (There is a deadline for submitting list choice, let's ask what happened around that deadline).

**A4.** Was the decision pro/contra closed list a contentious issue in the party?

**A5.** Generally speaking, who was in favour of closed list?

**A6.** Generally speaking, who was in favour of open list?

**A7.** Is it difficult to attract candidates in closed (open) lists?

**A8.** Who decides the ranking? Are candidate characteristics are taken into account for the ranking? Which ones?

-Was there any special consideration of placing women within the list in different rankings?

**A9.** What do you think the voters think about the list choice? Do you think voters prefer open or closed lists? Why?

## **B. General characteristics:**

**B1.** Do you have clear idea what the other parties will do? (Before the electoral ballot is published).

**B2.** Is your list- type decision influenced by what system you think the other parties are going to adopt?

**B3.** Is the choice influences by how competitive the race is with other parties?

## **C. Advantages Open list:**

**C.** In your personal opinion, what is the advantage of open list...

1. for the party?
2. for the politicians on the list?
3. for voters?

## **D. Advantages Closed list:**

**D.** In your personal opinion, what is the advantage of closed list...

1. for the party?
2. for the politicians on the list?
3. for voters?

## **E. Open ended question:**

Is there anything that you think is important for us to know about open and closed



list and how parties decide to favour one over the other?

#### **F. On party permission:**

How was the process of obtaining the party credentials to run (*aval*)? Did you obtain the party permission *aval* with this party or you applied to others?

### **3.10.3 Who decides on list type?**

Our interviews showed that the decision to go open or closed list is typically made by the council candidates on the list. They gather before the deadline by which they have to submit the list choice to the National Registry Office, and either vote on the list choice, or discuss the issue until they reach a consensus.

However, the list choice is not always made by the candidates on the list. Often, if there is a strong leader in the party, he/she would take the decision. Similarly, if the party runs for the Mayor's office, the mayor candidate would decide the list type, and, if closed list was chosen, the ranking of the candidates on the list. In a few other cases, the central office of the party instructs which list type to use. From our interviews, we noticed that the parties are highly decentralized and most of the times they just give the party credentials for a group of candidates to run; therefore, in many municipalities the decision over open or closed list is taken by all members of the list.

### **3.10.4 Advantages of open and closed lists?**

#### **Arguments for open lists**

Party list members decide on the list type, conditional of what other parties were choosing, as part of a more general discussion on how to best compete with other parties. Most parties simply choose the same list type that other parties in their municipality adopted (or are expected to adopt). For example, competing parties were believed to choose open list, and therefore, the party in question would choose open also<sup>20</sup>. Another important consideration is the history of the type of lists that has been used in the municipality. If there is a tradition of open lists in the municipality, parties were more likely to stick to open list, not the least because they could expect voters to be proficient in using this somewhat more complex list type. In this context closed lists were seen as a costly decision to the party:

*We chose closed list (...) and, unfortunately, here there is a culture of open lists. Voters were disconcerted when we told them they only had to vote for*

---

<sup>20</sup>In fact only 9% (2837) lists presented for council elections between 2003 and 2015 were closed, is also consistent with most parties mimicking each other.

*a party and no number was necessary. I think they felt strange when no code was given to them.”*

Candidate for the Partido de la U in *Valparaiso, Antioquia*.

Considerations about internal competition are also taken into account when deciding the list type. In some cases adopting a closed list was considered at length, but no consensus could be reached about who would get the top spot on the list. In this case, open list is adopted to avoid this bargaining problem. Thus, open list can work as a device to solve possible internal party disputes:

*”We [the candidates on the list], suggested to go open list, and the suggestion was well taken. If the list were closed: Who would be the head of the list?”*

Candidate for the Cambio Radical party in *Cartago, Valle*.

Another recurring topic mentioned in favor of open lists is that under closed list, only the party head campaigns for votes. People at the bottom of the list have few incentives to put in effort, given their low probability of being elected. Along these lines, a second placed candidate in a closed list stated:

*”Another disadvantage (of closed lists) is that people lower on the list know that they won’t make it, then they don’t work as hard as people higher on the list. When the list is open one depends [more] upon oneself.”*

Candidate for the Partido de la U party in *Tamara, Casanare*.

Similarly, a winning candidate of an open list criticized closed lists because it is difficult to appropriate the vote seeking effort, since the votes would go to the top of the list and there wouldn’t be recognition for the effort made by other party members:

*”With open lists each candidate works, and reaps what he grows, in closed lists you work for another person, it is not fair.”*

Candidate for the Partido de la U party in *Tamara, Casanare*.

Furthermore, due to the lower placed candidates’ low probability of winning in closed lists, the parties struggle to recruit high-quality candidates to fill the last places of the closed lists. We asked several interviewees who the people at the end of the list are and sometimes they are the party administrative officers or people with no interest in holding office. Along these lines, candidate placed in 6th place on a closed list said:

*”I don’t understand the difference between open and closed list, I was just supporting our candidate (top of the list) that didn’t get elected....”*

Candidate for the Partido de la U party in *Paez, Cauca*.

This indigenous interviewee has never engaged in a political campaign but was lured into signing the paperwork to be put on the closed list, without properly understanding what that implies. Clearly, this was done in order to simply fill the list. A lower placed candidate in a closed list also claimed that the closed lists could be used as a mechanism to get easy votes for higher placed candidates, by tricking people into politics:

*"In this municipality people take advantage of others, of the peasants, honest people who want to work. (...) Sometimes a popular person is put lower in the closed list, that has support in their districts but doesn't know much about politics, so the people placed up in the list win those votes without doing nothing."*

Candidate for the Partido de la U party in *Tamara, Casanare*.

Finally, another aspect that was mentioned several times, is the placement of women within the list. After 2007, Colombia enacted a quota, which stipulated that lists should have at least 30% of women. In many instances, the women were placed at the bottom of the closed lists. In one list, for example, several female administrators working in party offices were used to fill the last places of a closed list in order to comply pro forma with the gender quota.<sup>21</sup>

### **Arguments for choosing closed list**

As discussed above, most parties adopt open lists, in part because other parties are doing so. In the case of the leftist *Polo*, their local party heads sometimes choose closed lists in order to differentiate themselves from the parties fielding open lists and to signal strong party cohesion. They claim:

*"We were a group of friends interested in the welfare of the municipality (...) we chose closed list because we thought it was the best way to present ourselves to the community, as a group, a family, rather than a party with people who each walked their own path. The idea is that the community would see us as a team (...)"*

Camilo Chapuel, candidate for the Polo party for *Pasto, Nariño*.

In a similar vein, several candidates manifested statements that could be interpreted such that closed lists were seen as better in order to maintain party cohesion and not fragment the party. Under open lists, personal vote would be cultivated and would lead to candidates moving from one party to the other.

---

<sup>21</sup>We explore the compliance with and effect of these gender quotas under open and closed list in a separate paper.

*"When the list is closed the party is strengthened institutionally, because you are voting for a party and not a person, and this could lead to more ideological party cohesion."*

Candidate for the Alianza Verde party at *Cartago, Valle*.

*"With closed lists the party has more control over candidates, and me as a president could control more the candidates, while in open list they are autonomous"*

Candidate for Polo Democrático Alternativo at *Balboa, Cauca*.

Also when there was a popular person in a list, some parties opted to go for closed to enjoy the benefits of placing a well known candidate among the community at the top of the list.

*"She (the party head) is in charge of an NGO in Cartago that works with women householders (...) She was the one who worked the hardest one year before the election, therefore, we all agreed she should be head of the list, she was the leader."*

Candidate for the Alianza Verde party at *Cartago, Valle*.

The other candidates less well known than the party head, could enjoy the votes that were targeted to her but would spill over into the lower people in the ranking. So for closed lists, usually the most experienced candidate (number of times elected or years in politics), would be placed on top of the list in order to attract votes.

Other aspects that were considered when taking the list choice was the invalid votes. A recurrent claim was that using open lists would lead to more invalid votes since it is more difficult for voters to vote for the party and then choose a candidate number within the party. On the other hand, closed lists would be easier to vote for since marking the party logo would be required.

Some interviewees claimed that the success of closed lists would depend on the conditions of the municipality. A list in the U party, chose closed list because it was easier for voters to vote for their party since the main voter support were indigenous people who didn't know how to read and write and it would be easier for them to vote just for a party logo and not a number.

### 3.11 Appendix C: Additional results and variable descriptions

**Table 3.10: Timeline for list type decisions**

<i>Date</i>	<i>List type decision</i>
25 July 2015	Inscription of Candidates in the Local Registry (selection of type of list)
28 July 2015	Parties can start political campaign
31 July 2105	Last day to announce changes in the party lists only if a candidate quits
2 August 2015	Publication on the web-page of final list of candidates
25 October 2015	Election date
1 January 2016	Elected officials take office

**Table 3.11: Variables and sources**

Variable	Description	Source
<b>Panel A. Electoral. Party level covariates:</b>		
Closed list	Dummy = 1 if Party chose Closed list	Electoral Data from the National Registry
Party Share	Vote Total Party Votes / Total Votes in Election	Electoral Data from the National Registry
Party Share	Seat Total Seats obtained by party / Total Seats available	Electoral Data from the National Registry
Party Obtained one Seat	Dummy =1, if the party at least got one seat	Electoral Data from the National Registry
% of party list filled	Total number of candidates in List / Maximum number of seats available in the municipality	Electoral Data from the National Registry
% of Women in party list	Number of Women in List / Total Number of Candidates in list	Electoral Data from the National Registry

Continued on next page

**Table 3.11 – Variables and sources, continued from previous page**

<b>Variable</b>	<b>Description</b>	<b>Source</b>
Left wing party	Dummy =1 if the party is was coded as left wing	Fergusson et. al. (2016)
Party is the incumbent party	Dummy = 1, if the party holds the Mayor office when elections are conducted	Own coding.
% of Incumbents in List	Total number of candidates who has held council office at least once / Total number of candidates in List	Electoral Data from the National Registry
Party experience in years	Number of years the party has participated in the municipality	Own coding
Head list experience, candidate	Number of times the head of the list was a political candidate	Own coding
Head list experience, councilor	Number of times the head of the list has elected a councilor	Own coding
Head list experience, any elected public post	Number of times the head of the list hold any elected public post	Own coding
<b>Panel B. Electoral. Municipality level covariates:</b>		
Invalid vote %	Total Invalid Votes (Null votes + Unmarked votes) / Total Votes in Election	Electoral Data from the National Registry
Unmarked vote %	Total Unmarked Votes / Total Votes in Election	Electoral Data from the National Registry
Null votes percent	Total Null Votes / Total Votes in Election	Electoral Data from the National Registry
Voter turnout	Total Votes Cast / Number of Registered voters	Electoral Data from the National Registry
Total seats available	Number of sets available for local councils	Electoral Data from the National Registry

Continued on next page

**Table 3.11 – Variables and sources, continued from previous page**

Variable	Description	Source
<b>Panel C. Additional municipality covariates:</b>		
<b>Municipality Performance</b>		
Fiscal Performance Index	Index of fiscal performance based on (+ improves the index, - deteriorates it): size of municipality's debt (-), % of income from own resources (+), % spent in investment (+), % is spent in administrative functioning (-)	Colombia's National Planning Department
Compliance with legal rules index	Index based on whether the municipality is complying with the legal rules on how to do their spending, comparing budgeted and executed resources as well as expenditure in each sector compared to what is legally permitted	Colombia's National Planning Department
Administrative Capacity Index	Index aggregating: stability of directives in the municipality, personnel qualifications, extent to which internal processes follow clear system, existence of internal controls	Colombia's National Planning Department.
<b>Demographic Covariates</b>		
Population	Number of habitants in the municipality	DANE (Colombias National Department of Statistics) 1993 National Census.
Literacy Rate	(%) of literate in the municipality	DANE (Colombias National Department of Statistics). 1993 National Census.
<b>Panel D. Individual level covariates:</b>		
<b>Campaign Data.</b>		
Campaign Effort – Total Value.	Campaign effort in the campaign. Includes contributions i) from personal income, spouses or family. ii) private donors. iii) credits from banks. Value in Colombian pesos.	National electoral commission.

Continued on next page

---



---

**Table 3.11 – Variables and sources, continued from previous page**

---



---

Variable	Description	Source
Campaign Effort - % of limit.	Campaign effort / Total Campaign limit allowed by law.	National electoral commission.
<b>Candidate Political background.</b>		
New Candidate.	Dummy =1, if the candidate is new to politics in that election.	Own coding.
Persistence in Politics.	Dummy=1, if the candidate was also a candidate in an election in the following election.	Own coding.
Reelected.	Dummy=1, if the person is reelected in the following election.	Own coding.
<b>Candidate illegal background.</b>		
Corruption.	Dummy=1, if the candidate has been accused, and sentenced by a judge for committing a fiscal crime, before running in the current election.	National Procurement office of Colombia.
Illegal registration of ID.	Dummy=1, if the candidate has registered illegally to vote. By using a dead person ID, a duplicate ID, a fake identity, registered while underage of ID (below 18), and was paid to vote at a new polling station.	National registry of Colombia.
Paid to vote in another polling station.	Dummy=1, if the candidate has been paid to vote to a new polling station.	National registry of Colombia.
Registered to vote at the same municipality where running.	Dummy=1, if the candidate registered to vote in the same municipality he is running for office.	National registry of Colombia.

---



**Table 3.12: Descriptive Statistics and Differences in Means Across List Type**

Variable	N	Mean				SD	Min	Max
		Averages within List						
		<i>Open</i>	<i>Closed</i>	<i>diff.</i>	<i>sig</i>			
<b>A. Electoral Variables</b>								
Party Vote Share	30793	0.13	0.14	0.05	***	0.11	0	1
% of Seats Obtained by the party	30778	0.14	0.15	0.04	***	0.14	0	1
Party obtained one seat	30804	0.7	0.74	0.22	***	0.46	0	1
% of list Size filled	30766	0.81	0.83	0.55	***	0.26	0.05	3.25
<b>B. Characteristics of Candidates in the list:</b>								
<b>B1. Campaign Data (**)</b>								
Total Campaign Investment (Millions COP) - Avg	14,491	1.71	1.70	1.81		3.99	0	128
Total Campaign Investment (Millions COP) - Top	3,163	4.85	4.77	11.35	**	14.2	0	195
Personal Campaign Investment (Millions COP) - Avg	14,479	1.63	1.64	1.55		3.8	0	126
Personal Campaign Investment (Millions COP) - Top	3,163	4.62	4.58	8.07		13	0	178
% of Campaign Limit (Total Investment) - Avg	14491	0.06	0.06	0.04	***	0.09	0	3.11
% of Campaign Limit (Total Investment) - Top	3163	0.13	0.13	0.14		0.18	0	1.29
% of Campaign Limit (Personal Investment) - Avg	14479	0.06	0.06	0.04	***	0.09	0	2.57
% of Campaign Limit (Personal Investment) - Top	3163	0.13	0.13	0.13		0.17	0	1.29
<b>B2. Corruption Sanctions</b>								
Corruption Sanction Before Election (Avg)	30779	0.027	0.027	0.024	*	0.067	0	1
Corruption Sanction Before Election (Top)	7756	0.033	0.032	0.072	***	0.15	0	1
<b>B3. Voting Registration (Illegal Registration)</b>								
Registered to vote (Avg)	30779	0.93	0.93	0.92	***	0.11	0	1
Registered to vote (Top)	7756	0.96	0.96	0.90	***	0.16	0	1
Regist vote same place running (Avg)	30706	0.91	0.92	0.85	***	0.16	0	1
Regist vote same place running (Top)	7612	0.96	0.96	0.90	***	0.17	0	1
Registered Illegally to vote (*) (Avg)	30779	0.04	0.04	0.05	***	0.09	0	1
Registered Illegally to vote (*) (Top)	7756	0.02	0.01	0.06	***	0.10	0	1
Moved Illegally to vote in another polling station (Avg)	30779	0.04	0.04	0.05	***	0.08	0	1
Moved Illegally to vote in another polling station (Top)	7756	0.01	0.01	0.06	***	0.10	0	1
<b>B4. Candidate experience</b>								
# of times cand. ran for any public elected post (Avg)	30779	1.79	1.81	1.51	***	0.53	1	6
# of times cand. ran for any public elected post (Top)	7043	2.63	2.64	2.15	***	1.12	1	6
# of times cand. in list ran for local council (Avg)	30779	1.57	1.58	1.33	***	0.40	1	4
# of times cand. in list ran for local council (Top)	7043	2.18	2.19	1.63	***	0.90	1	4
# of times cand. in list got elected to council (Avg)	30779	0.35	0.36	0.11	***	0.35	0	4
# of times cand. in list got elected to council (Top)	7043	1.76	1.77	1.33	***	0.79	1	4

**Notes:** \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Number of total party lists: 30804. Sample: Party lists in 1101 municipalities in Colombia council elections: 4 elections between 2003- 2015. (\*) Use of a dead person fingerprint, having a duplicate ID, irregular inscription of ID, using a fake identity, Underage registration, Illegal migration of IDs implies a movement of ID to a new voting site in mass, typically in exchange of money. (\*\*) Only available for 2011 and 2015.

**Table 3.13: Effects of use of closed lists on municipality electoral outcomes.**

	(1)	(2)	(3)
	voter turnout	% of invalid votes	% of unmarked votes
% of parties with closed lists	-0.0166 (0.0111)	-0.00289 (0.00326)	0.0321*** (0.00387)
Constant	0.540*** (0.00230)	0.0788*** (0.000392)	0.0505*** (0.000466)
Mun FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	4385	3295	3294
R2 Adjusted	0.385	0.646	-0.204
Dummy = 1 if municipality had at least one closed list	0.00412 (0.00268)	0.000829 (0.000629)	0.00217*** (0.000758)
Constant	0.536*** (0.00228)	0.0785*** (0.000393)	0.0512*** (0.000474)
Mun FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	4385	3295	3294
R2 Adjusted	0.385	0.646	-0.238

**Notes:** Standard errors in parenthesis and clustered at the municipality level. Top of the list is run using a probit model since the dependent variable is a dummy. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1 . Note that in (2) and (3) there is only data for 2007, 2011

**Table 3.14: Presence and list type choice across municipalities, 10 largest Colombian parties in local council elections 2003-2015**

Name of party	Share of:	2003		2007		2011		2015	
		Munip	Closed list	Munip	Closed list	Munip	Closed list	Munip	Closed list
<i>PART. LIBERAL COLOMBIANO</i>		0.7838	0.0012	0.8333	0.0197	0.8717	0.0167	0.8765	0.0166
	# of municip	852	852	915	915	958	958	965	965
<i>PART. CONSERVADOR COLOMBIANO</i>		0.647	0.023	0.770	0.002	0.869	0.001	0.878	0.000
	# of municip	703	703	845	845	955	955	967	967
<i>PART. CAMBIO RADICAL COLOMBIANO</i>		0.216	0.174	0.674	0.007	0.887	0.007	0.913	0.022
	# of municip	235	235	740	740	975	975	1005	1005
<i>PART. SOCIAL DE UNIDAD NACIONAL</i>				0.628	0.016	0.894	0.008	0.936	0.012
	# of municip	0	0	690	690	982	982	1030	1030
<i>POLO DEMOCRATICO ALTERNATIVO</i>				0.567	0.069	0.444	0.113	0.433	0.115
	# of municip	0	0	623	623	488	488	477	477
<i>ALIANZA SOCIAL INDIGENA .</i>						0.536	0.037	0.607	0.040
	# of municip	0	0	0	0	589	589	668	668
<i>PARTIDO VERDE</i>				0.241	0.030	0.692	0.063		
	# of municip	0	0	265	265	761	761	0	0
<i>AUTORIDADES INDIGENAS DE CO..</i>		0.046	0.300	0.085	0.022	0.284	0.035	0.360	0.038
	# of municip	50	50	93	93	312	312	396	396
<i>CENTRO DEMOCRATICO</i>								0.738	0.429
	# of municip	0	0	0	0	0	0	813	813
<i>UNION PATRIOTICA UP</i>								0.729	0.030
	# of municip	0	0	0	0	0	0	803	803
Total number of municipalities in sample:		1087		1098		1099		1101	

**Table 3.15: Effects of candidate placement on campaign investment within closed lists.**

	(1)	(2)	(3)	(4)	(5)	(6)
	Tot. Cam Invt.	Tot. Cam Invt. (% of lim.)	Camp Effort	Camp Effort (% of lim.)	Camp Effort Alt	Camp Effort Alt (% of lim.)
Top safe Candidate	7683000** (3.848e+06)	0.0766*** (0.0195)	7457000* (3.799e+06)	0.0740*** (0.0187)	7433000*** (3.786e+06)	0.0742*** (0.0187)
Marginal Candidate	2.156e+06*** (811,219)	0.0286*** (0.00508)	1.383e+06*** (471,416)	0.0195*** (0.00424)	1.435e+06*** (505,906)	0.0202*** (0.00428)
Constant	2.293e+07*** (2.485e+06)	0.0633*** (0.0113)	1.845e+07*** (2.545e+06)	0.0587*** (0.0130)	1.847e+07*** (2.523e+06)	0.0583*** (0.0130)
Year Dummies	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓
Party FE	✓	✓	✓	✓	✓	✓
Observations	3485	3485	3469	3469	3469	3469
R-squared	0.548	0.437	0.499	0.466	0.507	0.466

**Notes:** These regressions are the individual level for open lists. Marginal candidates are those that were **two** seats above and below the last seat obtained in the party, if the party did not obtain a seat the first two are considered marginal. Top safe candidates, are the ones placed above the marginal, and loser candidates are the ones that are below the marginals. Standard errors in parenthesis and clustered at the municipality level. Top of the list is run using a probit model since the dependent variable is a dummy. Campaign effort is: Money raised by the candidates, and their immediate family, donations by particulars, and financial credits obtained at a financial institution. The (alt) measurement includes the previous ones plus income generated by public activities by the party. For 2011 a candidate level campaign spending limit was established at the individual level. For 2015 the campaign limit was modified and place at the list level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 3.16: Effects of candidate placement on campaign investment within open lists.**

	(1)	(2)	(3)	(4)	(5)	(6)
	Tot. Cam Invt.	Tot. Cam Invt. (% of lim.)	Camp Effort	Camp Effort (% of lim.)	Camp Effort Alt	Camp Effort Alt (% of lim.)
Top Safe Candidate	5.356e+06*** (1.508e+06)	0.101*** (0.00642)	5.066e+06*** (1.348e+06)	0.0978*** (0.00604)	5.198e+06*** (1.405e+06)	0.0991*** (0.00621)
Marginal Candidate	1.829e+06*** (286,701)	0.0401*** (0.00197)	1.716e+06*** (255,551)	0.0388*** (0.00189)	1.744e+06*** (262,767)	0.0391*** (0.00193)
Constant	2.293e+07*** (2.485e+06)	0.0633*** (0.0113)	1.845e+07*** (2.545e+06)	0.0587*** (0.0130)	1.847e+07*** (2.523e+06)	0.0583*** (0.0130)
Year Dummies	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓
Party FE	✓	✓	✓	✓	✓	✓
Observations	115236	115236	115088	115088	115088	115088
R-squared	0.322	0.319	0.308	0.321	0.308	0.321

**Notes:** These regressions are the individual level for open lists. Marginal candidates are those that were **two** seats above and below the last seat obtained in the party, if the party did not obtain a seat the first two are considered marginal. Top safe candidates, are the ones placed above the marginal, and loser candidates are the ones that are below the marginals. Standard errors in parenthesis and clustered at the municipality level. Top of the list is run using a probit model since the dependent variable is a dummy. Campaign effort is: Money raised by the candidates, and their immediate family, donations by particulars, and financial credits obtained at a financial institution. The (alt) measurement includes the previous ones plus income generated by public activities by the party. For 2011 a candidate level campaign spending limit was established at the individual level. For 2015 the campaign limit was modified and place at the list level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

## 3.12 Appendix D: Determinants of list choice

To understand further why parties are willing to choose closed list even when it tremendously decreases their seat shares and the probability that even a single of their candidate will get a seat, we will first explain the list choice with various party and municipality characteristics in Table 3.17. We base our discussion on specification (8) that contains the richest set of explanatory variables. The other results are reported for robustness.

The first interesting finding is that choosing closed list before increases the probability that they choose closed list again. This result simply reflects what we learned from the transition matrix in Table 3.9. No one with open list at time  $t$  will change to closed list at time  $t+1$ , and even if most parties with closed list at time  $t$  change to open list at  $t+1$ , at least some of the keep the closed list. Furthermore, more experienced or important parties choose closed lists less often suggesting that closed lists are mistakes made by smaller new entrants.

Closed lists are less common in municipalities with more available seats. This is as expected because in larger districts the closed list candidates' personal ability to gain votes matters less and they have less incentives to campaign (e.g. (Carey and Shugart, 1995)). (Achury et al., 2017) report a similar result for Colombian parliamentary elections. This results is present only conditional on the number of voters. In municipalities with more voters, the closed lists are more common. Overall the variables in Table 3.17 are able to explain only very little of the variation in the list choice.

To shed further light on the list choice, we study in Table 3.18 whether previous election results influence list choice. Conditional on the previous election list choice, having received more votes in the previous election decreases the probability of choosing closed lists. Again we see that close lists are chosen by younger parties.

**Table 3.17: Determinants of choosing closed list. Colombian parties 2003-2015.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Dependent variable: Dummy =1, if party chose closed list.</i>								
	<i>Party Characteristics:</i>			<i>Municipality Characteristics</i>				<i>All</i>
Closed list before	0.149*** (0.0170)	0.178*** (0.0168)	0.177*** (0.0168)					0.171*** (0.0182)
# of Years party ran in munic.		-0.0300*** (0.00121)	-0.0291*** (0.00125)					-0.0294*** (0.00131)
Party holds the Mayor office during elec.			-0.0118*** (0.00229)					-0.0111*** (0.00239)
Total seats in the municip.				0.00293*** (0.000415)	0.00224*** (0.000557)	0.00321** (0.00159)	0.00346 (0.00228)	-0.00528** (0.00212)
log(Distance to Depart Capital, km)					-0.00584*** (0.00200)	-0.00610*** (0.00204)	-0.00588** (0.00251)	-0.00257 (0.00227)
log(# of registered voters)						-0.00277 (0.00427)	-0.00383 (0.00590)	0.0147*** (0.00534)
Years since municipality foundation							2.01e-05 (1.59e-05)	2.79e-05* (1.48e-05)
Constant	0.0489*** (0.00158)	0.117*** (0.00405)	0.116*** (0.00407)	0.0329*** (0.00511)	0.0649*** (0.0109)	0.0807*** (0.0267)	0.0842** (0.0346)	0.0463 (0.0310)
Observations	23804	23804	23804	30802	29085	29085	28679	22223
R-squared	0.013	0.036	0.037	0.002	0.001	0.001	0.001	0.036
R2 Adjusted	0.0131	0.0363	0.0365	0.00158	0.000840	0.000820	0.000838	0.0358

**Notes:** (1), (2), (3) and (8) exclude year 2003 since it is the first year in the sample, and there is no before period for them in the data. Standard errors in parenthesis and clustered at the municipality level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 3.18: Determinants of choosing closed list based on previous electoral results of parties. Colombian parties 2003-2015.**

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Dependent variable: Dummy =1, if party chose closed list.</i>						
Vote share for the party in t-1	-0.158*** (0.0151)	-0.0947*** (0.0136)	-0.0718*** (0.0143)	-0.155*** (0.0163)	-0.102*** (0.0150)	-0.0751*** (0.0157)
Party chose closed list before		0.171*** (0.0173)	0.174*** (0.0173)		0.168*** (0.0187)	0.170*** (0.0187)
# of Years party has participated in the same munic.			-0.0122*** (0.00215)			-0.0133*** (0.00221)
Constant	0.0545*** (0.00363)	0.0333*** (0.00302)	0.0684*** (0.00719)	0.0895*** (0.0343)	0.0607* (0.0317)	0.0884*** (0.0327)
Municipality controls				✓	✓	✓
Observations	11597	11597	11597	10755	10755	10755
R-squared	0.014	0.074	0.077	0.014	0.068	0.071
R2 Adjusted	0.0136	0.0739	0.0768	0.0132	0.0674	0.0709

**Notes:** The sample size is smaller since not all parties participate in the same municipality. Standard errors in parenthesis and clustered at the municipality level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

### 3.13 Appendix E: Additional robustness checks

**Table 3.19: Distribution of switchers across parties % of Total**

Party Name	2007	2011	2015	Total
MOVIMIENTO MIRA	21.51	31.84	34.96	29.98
POLO DEMOCRATICO ALTERNATIVO	0	28.49	25.22	18.72
PARTIDO CAMBIO RADICAL COLOMBIANO	18.02	8.38	10.18	11.96
PARTIDO LIBERAL COLOMBIANO	6.4	12.85	8.85	9.36
PARTIDO SOCIAL DE UNIDAD NACIONAL PARTID	0	6.7	6.64	4.68
PARTIDO NACIONAL CRISTIANO PNC	0	0	10.62	4.16
MOVIMIENTO AUTORIDADES INDIGENAS DE COLOMBIA	4.07	2.79	3.54	3.47
MOVIMIENTO ALIANZA SOC INDIGENA ASI	11.63	0	0	3.47
PARTIDO CONSERVADOR COLOMBIANO	8.14	2.79	0	3.29
MOVIMIENTO APERTURA LIBERAL	9.88	0	0	2.95
MOVIMIENTO COLOMBIA VIVA	6.98	0	0	2.08
MOVIMIENTO CONVERGENCIA CIUDADANA	6.98	0	0	2.08
PARTIDO VERDE	0	6.15	0	1.91
PARTIDO COLOMBIA DEMOCRATICA	5.23	0	0	1.56
MOVIMIENTO COMPROMISO CIVICO CRISTIANO C	0.58	0	0	0.17
MOVIMIENTO DE SALVACION NACIONAL	0.58	0	0	0.17
Total Switchers	172	179	226	577

**Table 3.20: Electoral results omitting one party at a time**

	(1) Party vote share	(2) Percentage seat party	(3) One seat party	(4) Fill per
Open List (LIBERAL PARTY)	0.059*** (0.003)	0.074*** (0.004)	0.387*** (0.012)	0.194*** (0.008)
Open List (CONSERVATIVE PARTY)	0.066*** (0.003)	0.083*** (0.004)	0.407*** (0.013)	0.205*** (0.008)
Open List (CAMBIO RADICAL)	0.067*** (0.003)	0.084*** (0.004)	0.404*** (0.013)	0.205*** (0.008)
Open List (UNIDAD NACIONAL)	0.067*** (0.003)	0.085*** (0.004)	0.407*** (0.013)	0.206*** (0.008)
Open List (POLO DEMOCRATICO ALTERNATIVO)	0.070*** (0.003)	0.089*** (0.004)	0.408*** (0.013)	0.190*** (0.008)
Open List (MOVIMIENTO ALIANZA SOCIAL INDIGENA)	0.069*** (0.003)	0.087*** (0.004)	0.409*** (0.012)	0.205*** (0.008)
Open List (PARTIDO VERDE)	0.068*** (0.003)	0.086*** (0.004)	0.403*** (0.013)	0.200*** (0.008)
Open List (AUTORIDADES INDIGENAS COLOMBIA)	0.070*** (0.003)	0.088*** (0.004)	0.413*** (0.012)	0.203*** (0.008)
Mean Dep. Var	0.0693	0.0643	0.365	0.623

Note: Omitted party in parenthesis

Robust standard errors in parentheses

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

**Table 3.21: Electoral results controlling for party experience**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Party VS	Party VS	Party VS	% Seats Party	% Seats Party	% Seats Party	One Seat	One Seat	One Seat	List fill %	List fill %	List fill %
Open List	0.068*** (0.003)	0.062*** (0.003)	0.063*** (0.003)	0.086*** (0.004)	0.076*** (0.004)	0.079*** (0.004)	0.410*** (0.013)	0.381*** (0.012)	0.391*** (0.012)	0.205*** (0.008)	0.195*** (0.007)	0.200*** (0.008)
No. of times party in municip		0.025*** (0.001)			0.038*** (0.001)			0.109*** (0.004)			0.037*** (0.002)	
No. of times party <i>any</i> municip			0.021*** (0.001)			0.030*** (0.001)			0.082*** (0.005)			0.019*** (0.002)
Year Dummies	X	X	X	X	X	X	X	X	X	X	X	X
Mun FE (x) Party FE	X	X	X	X	X	X	X	X	X	X	X	X
Mean Dep. Var	0.0769	0.0769	0.0769	0.0734	0.0734	0.0734	0.392	0.392	0.392	0.634	0.634	0.634
Effect Size (%b3)	88.80	80.39	82.50	117.7	104.2	108.2	104.4	97.21	99.67	32.29	30.77	31.59
95% Conf. Inter Lower B	80.87	72.56	74.67	107.7	94.44	98.50	98.16	91.06	93.44	29.95	28.45	29.25
95% Conf. Inter Upper B	96.72	88.21	90.34	127.6	113.9	118	110.7	103.3	105.9	34.63	33.09	33.93
Observations	30793	30793	30793	30778	30778	30778	30804	30804	30804	30766	30766	30766

Robust standard errors in parentheses

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

# References

- Acemoglu, D. and J. A. Robinson (2006). *Economic origins of dictatorship and democracy*. Cambridge: Cambridge University Press.
- Acemoglu, D. and J. A. Robinson (2008). Persistence of power, elites, and institutions. *American Economic Review* 98(1), 267–93.
- Acemoglu, D., J. A. Robinson, and R. J. Santos (2013). The monopoly of violence: Evidence from Colombia. *Journal of the European Economic Association* 11, 5–44.
- Acevedo, K. M. and I. D. Bornacelly (2014). Panel Municipal del CEDE. Documentos CEDE 2014-26, Universidad de los Andes-CEDE.
- Achury, S., M. Ramírez, and F. Cantú (2017, October). Endogenous ballot structures: The selection of open and closed lists in Colombia’s legislative elections. *Electoral Studies* 49, 136–154.
- Ames, B. (1995). Electoral Strategy under Open-List Proportional Representation. *American Journal of Political Science* 39(2), 406–433.
- Anderson, S., P. Francois, and A. Kotwal (2015). Clientelism in Indian villages. *American Economic Review* 105(6), 1780–1816.
- Ansolabehere, S., J. M. De Figueiredo, and J. M. Snyder (2003). Why is there so little money in US politics? *The Journal of Economic Perspectives* 17(1), 105–130.
- Ansolabehere, S., J. M. Snyder, and M. Ueda (2004, June). Did Firms Profit from Soft Money? *Election Law Journal: Rules, Politics, and Policy* 3(2), 193–198.
- Arrow, K. (1978). A cautious case for socialism.
- Ashworth, S. (2006, February). Campaign Finance and Voter Welfare with Entrenched Incumbents. *American Political Science Review* 100(1), 55–68.
- Ashworth, S. and E. Bueno de Mesquita (2006, February). Delivering the Goods: Legislative Particularism in Different Electoral and Institutional Settings. *Journal of Politics* 68, 168–179.
- Avis, E., C. Ferraz, F. Finan, and C. Varjão (2017). Money and Politics: The Effects of Campaign Spending Limits on Political Competition and Incumbency Advantage. Technical report, National Bureau of Economic Research.
- Bagues, M. and P. Campa (2017, July). Can Gender Quotas in Candidate Lists Empower Women? Evidence from a Regression Discontinuity Design. Technical Report 12149, C.E.P.R. Discussion Papers.
- Bakunin, M. (1882). *God and the state*. New York: CreateSpace Independent Publishing Platform, 2011.
- Baland, J.-M. and J. A. Robinson (2008). Land and power: Theory and evidence from Chile. *American Economic Review* 98(5), 1737–65.
- Baltrunaite, A. (2016). Political Contributions and Public Procurement: Evidence from Lithuania.
- Bandiera, O. and G. Levy (2011). Diversity and the power of the elites in democratic societies: Evidence from Indonesia. *Journal of Public Economics* 95(11–12), 1322 – 1330. Special Issue: International Seminar for Public Economics on Normative Tax Theory.
- Beath, A., F. Christia, G. Egorov, and R. Enikolopov (2016, July). Electoral Rules and Political Selection: Theory and Evidence from a Field Experiment in Afghanistan. *The Review of Economic Studies* 83(3), 932–968.
- Beck, T., G. Clarke, A. Groff, P. E. Keefer, and P. Walsh (2012). Database of political institutions: Changes and variable definitions. 2012. *Development Research*



- Group. *The World Bank* 15(1), 165–176.
- Benoit, J.-P. and J. Dubra (2013). On the problem of prevention. *International Economic Review* 54(3), 787–805.
- Bergman, M. E., M. S. Shugart, and K. A. Watt (2013, June). Patterns of intraparty competition in open-list & SNTV systems. *Electoral Studies* 32(2), 321–333.
- Besley, T. (2004, April). Paying Politicians: Theory and Evidence. *Journal of the European Economic Association* 2(2-3), 193–215.
- Besley, T. (2005). Political Selection. *Journal of Economic Perspectives* 19(3), 43–60.
- Besley, T., J. G. Montalvo, and M. Reynal-Querol (2011, August). Do Educated Leaders Matter? *The Economic Journal* 121(554), F205–227.
- Bhalotra, S., I. Clots-Figueras, and L. Iyer (2012). Politician identity and religious conflict in India. *Unpublished Manuscript*.
- Bird, S., E. Klein, and E. Loper (2009, July). *Natural Language Processing with Python* (1 edition ed.). Beijing ; Cambridge Mass.: O’Reilly Media.
- Blumenau, J., A. C. Eggers, D. Hangartner, and S. Hix (2017, October). Open/Closed List and Party Choice: Experimental Evidence from the UK. *British Journal of Political Science* 47(4), 809–827.
- Boas, T. C., F. D. Hidalgo, and N. P. Richardson (2014, April). The Spoils of Victory: Campaign Donations and Government Contracts in Brazil. *The Journal of Politics* 76(2), 415–429.
- Braendle, T. (2016, April). Do Institutions Affect Citizens’ Selection into Politics? *Journal of Economic Surveys* 30(2), 205–227.
- Bruce, R. and R. Rocha (2014). The reaction of elites in a democratization process: Evidence from Brazil. Working Papers, Department of Economics 2014-09, University of Sao Paulo (FEA-USP).
- Budge, I., J. Bara, A. Volkens, and H. Klingemann (2001). *Mapping Policy Preferences: Estimates for Parties, Electors and Governments, 1945-98*. Oxford University Press.
- Bushnell, D. (1993). *The making of modern Colombia: a nation in spite of itself*. Berkeley: University of California Press.
- Calonico, S., M. D. Cattaneo, and R. Titiunik (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica* 82(6), 2295–2326.
- Carey, J. M. and M. S. Shugart (1995, December). Incentives to cultivate a personal vote: A rank ordering of electoral formulas. *Electoral Studies* 14(4), 417–439.
- Casper, G. and M. M. Taylor (1996). *Negotiating Democracy: Transitions from Authoritarian Rule*. Pittsburgh: University of Pittsburgh Press.
- Castro, J. (1986). Un nuevo municipio. *Ministerio de Gobierno*.
- Centro Nacional de Memoria Histórica (2013). *¡Basta Ya! Colombia: Memorias de Guerra y Dignidad*. Bogotá: Pro-Off Set.
- Centro Nacional de Memoria Histórica (2014). *Silenciar la democracia. Las masacres de Remedios y Segovia, 1982 - 1997* (2nd ed.). Bogotá: CNMH.
- Chacón, M., J. A. Robinson, and R. Torvik (2011). When is democracy an equilibrium? Theory and evidence from Colombia’s La Violencia. *Journal of Conflict Resolution* 55(3), 366–396.
- Chandra, K. and O. Garcia-Ponce (2016). Why ethnic subaltern-led parties crowd out armed organizations. Technical report, Graduate Center at the City University of New York.
- Chang, E. C. C. (2005, August). Electoral Incentives for Political Corruption under Open-List Proportional Representation. *Journal of Politics* 67(3), 716–730.

- Chattopadhyay, R. and E. Duflo (2004). Women as Policy Makers: Evidence from a Randomized Policy Experiment in India. *Econometrica* 72(5), 1409–1443.
- Clots-Figueras, I. (2011, August). Women in politics: Evidence from the Indian States. *Journal of Public Economics* 95(7), 664–690.
- Coate, S. (2004). Pareto-Improving Campaign Finance Policy. *The American Economic Review* 94(3), 628–655.
- Collier, P. and D. Rohner (2008). Democracy, development, and conflict. *Journal of the European Economic Association* 6(2-3), 531–540.
- Collins Jr, E. and T. M. Cole (1996). Regime legitimation in instances of coup-caused governments-in-exile: The cases of presidents Makarios and Aristide. *Journal of International Law and Practice* 5, 199.
- Crisp, B. F., M. C. Escobar-Lemmon, B. S. Jones, M. P. Jones, and M. M. Taylor-Robinson (2004). Vote-Seeking Incentives and Legislative Representation in Six Presidential Democracies. *The Journal of Politics* 66(3), 823–846.
- Dal Bó, E., F. Finan, O. Folke, T. Persson, and J. Rickne (2017). Who Becomes a Politician? *The Quarterly Journal of Economics*.
- Davenport, C. (1997). From ballots to bullets: an empirical assessment of how national elections influence State uses of political repression. *Electoral Studies* 16(4), 517–541.
- Davenport, C. (2007). State repression and political order. *Annual Review of Political Science* 10, 1–23.
- Dávila, A. (1992). “¿Del bipartidismo a un nuevo sistema de partidos? Crisis, constituyente y reconstrucción de un orden político democrático en Colombia. *América Latina Hoy* 3(0), 35–42.
- Dávila, A. (1999). *Clientelismo intermediación y representación política en Colombia: ¿Qué ha pasado en los noventa?* Number 15. Universidad de Antioquia, Medellín.
- Dell, M. (2015). Trafficking networks and the Mexican drug war. *American Economic Review* 105(6), 1738–79.
- Dube, O. and S. Naidu (2015). Bases, bullets, and ballots: The effect of US military aid on political conflict in Colombia. *The Journal of Politics* 77(1), 249–267.
- Dudley, S. S. (2004). *Walking ghosts: murder and guerilla politics in Colombia*. New York, London: New York, London : Routledge.
- Duncan, G. (2007). *Los Señores de la Guerra*. Bogotá: Editorial Planeta Colombiana.
- Eggers, A. C. and J. Hainmueller (2009, November). MPs for Sale? Returns to Office in Postwar British Politics. *American Political Science Review* 103(4), 513–533.
- Eifert, B., E. Miguel, and D. N. Posner (2010). Political competition and ethnic identification in Africa. *American Journal of Political Science* 54(2), 494–510.
- Faguet, J.-P. and F. Sanchez (2008). Decentralization’s effects on educational outcomes in bolivia and colombia. *World Development* 36(7), 1294 – 1316.
- Fergusson, L., P. Querubin, N. A. Ruiz, and J. F. Vargas (2017, February). The Real Winner’s Curse. (ID 2912915).
- Fergusson, L. and J. F. Vargas (2013). Don’t make war, make elections. Franchise extension and violence in XIXth-century Colombia. Documentos CEDE 2013-19, Universidad de Los Andes - CEDE.
- Fergusson, L., J. F. Vargas, and M. A. Vela (2013). Sunlight Disinfects? Free Media in Weak Democracies. Documentos CEDE 2013-14, Universidad de Los Andes - CEDE.
- Ferreira, F. and J. Gyourko (2014, April). Does gender matter for political leadership? The case of U.S. mayors. *Journal of Public Economics* 112, 24–39.
- Flores, T. E. and I. Nooruddin (2012). The effect of elections on post-conflict peace

- and reconstruction. *The Journal of Politics* 74(2), 558–570.
- Fox, J. (1994). Latin America’s emerging local politics. *Journal of Democracy* 5(2), 105–116.
- Franco, J. C. (2001). *Elections and Democratization in the Philippines*. Comparative studies in democratization. New York: Routledge.
- Franzese, R. (2007). *Multicausality, Context-Conditionality, and Endogeneity*. Oxford University Press.
- Franzese, R. J. (2009, July). Multicausality Context Conditionality, and Endogeneity.
- Freier, R. and S. Thomasius (2012). Voters Prefer More Qualified Mayors, but Does It Matter for Public Finances?: Evidence for Germany. Technical Report 1262, DIW Berlin, German Institute for Economic Research.
- Fujiwara, T. (2015a). Voting technology, political responsiveness, and infant health: Evidence from Brazil. *Econometrica* 83(2), 423–464.
- Fujiwara, T. (2015b, March). Voting Technology, Political Responsiveness, and Infant Health: Evidence From Brazil. *Econometrica* 83(2), 423–464.
- Galasso, V. and T. Nannicini (2011, February). Competing on Good Politicians. *American Political Science Review* 105(1), 79–99.
- Galasso, V. and T. Nannicini (2015, December). So closed: Political selection in proportional systems. *European Journal of Political Economy* 40(Part B), 260–273.
- Galindo-Silva, H. (2015). New parties and policy outcomes: Evidence from Colombian local governments. *Journal of Public Economics* 126, 86 – 103.
- Gelman, A. and Z. Huang (2008). Estimating incumbency advantage and its variation, as an example of a before–after study. *Journal of the American Statistical Association* 103(482), 437–446.
- Gelman, A. and G. Imbens (2014a). Why high-order polynomials should not be used in regression discontinuity designs. Working Paper 20405, National Bureau of Economic Research.
- Gelman, A. and G. Imbens (2014b, August). Why High-order Polynomials Should not be Used in Regression Discontinuity Designs. (20405). DOI: 10.3386/w20405.
- Gibson, E. (2005). Boundary control: Subnational authoritarianism in democratic countries. *World Politics* 58(1), 101–132.
- Gibson, E. (2014). *Boundary Control: Subnational Authoritarianism in Federal Democracies*. Cambridge Studies in Comparative Politics. Cambridge University Press.
- Giraldo, F., R. Lozada, and P. Muñoz (2001). *Colombia: Elecciones 2000*. Pontificia Universidad Javeriana, Bogota.
- Giraudy, A. (2010). The politics of subnational undemocratic regime reproduction in Argentina and Mexico. *Journal of Politics in Latin America* 2(2), 53–84.
- Grembi, V., R. Freier, A. C. Eggers, and T. Nannicini (2017, May). Regression Discontinuity Designs Based on Population Thresholds: Pitfalls and Solutions. *American Journal of Political Science Forthcoming*. type: dataset.
- Grimmer, J., S. Messing, and S. J. Westwood (2012, November). How Words and Money Cultivate a Personal Vote: The Effect of Legislator Credit Claiming on Constituent Credit Allocation. *American Political Science Review* 106(4), 703–719.
- Grossman, G. M. (2002, September). *Special Interest Politics* (New Ed edition ed.). Cambridge, Mass.: MIT Press.
- Gutierrez-Sanin, F. (2008). Telling the difference: Guerrillas and paramilitaries in the Colombian war. *Politics & Society*, 36(1), 3–34.
- Gutierrez-Sanin, F. and M. Baron (2005). Re-stating the state: Paramilitary territorial control and political order in Colombia, 1978-2004. Technical report, London

- School of Economics, London. Crisis States Program, Working Paper #1.
- Hallerberg, M. and P. Marier (2004, July). Executive Authority, the Personal Vote, and Budget Discipline in Latin American and Caribbean Countries. *American Journal of Political Science* 48(3), 571–587.
- Hangartner, D., N. A. Ruiz, and J. Tukiainen (2017). Open or closed? how list type affects electoral performance and political selection. *Unpublished manuscript*.
- Horowitz, D. (1993). Democracy in divided societies. *Journal of Democracy* 4(4), 18–38.
- Hortala-Vallve, R. and H. Mueller (2015, June). Primaries: the unifying force. *Public Choice* 163(3-4), 289–305.
- Hoyos, D. (2005a). Evolución del sistema de partidos en Colombia 1972-2000. una mirada local y regional.
- Hoyos, D. (2005b, September). Evolución del sistema de partidos en Colombia 1972-2000. Una mirada local y regional. *Análisis Político* 18(55), 45–59.
- Huntington, S. P. (1991). *The Third Wave: Democratization in the Late Twentieth Century*. University of Oklahoma Press.
- Hyytinen, A., J. Merillinen, T. Saarimaa, O. Toivanen, and J. Tukiainen (2017). Does Regression Discontinuity Design Work? Evidence from Random Election Outcomes. *Journal of Quantitative Economics*.
- Imai, K., L. Keele, D. Tingley, and T. Yamamoto (2011, November). Unpacking the Black Box of Causality: Learning about Causal Mechanisms from Experimental and Observational Studies. *American Political Science Review* 105(4), 765–789.
- Imbens, G. and K. Kalyanaraman (2012). Optimal bandwidth choice for the regression discontinuity estimator. *The Review of Economic Studies* 79(3), 933–959.
- Johnson, S. and T. Mitton (2003, February). Cronyism and capital controls: evidence from Malaysia. *Journal of Financial Economics* 67(2), 351–382.
- Jones, B. F. and B. A. Olken (2009, June). Hit or Miss? The Effect of Assassinations on Institutions and War. *American Economic Journal: Macroeconomics* 1(2), 55–87.
- Klašnja, M. (2015). Corruption and the incumbency disadvantage: Theory and evidence. *The Journal of Politics* 77(4), 928–942.
- Klašnja, M. and R. Titiunik (2017). The incumbency curse: Weak parties, term limits, and unfulfilled accountability. *American Political Science Review* 111(1), 129–148.
- Klasnja, M. and R. Titiunik (2017, February). The Incumbency Curse: Weak Parties, Term Limits, and Unfulfilled Accountability. *American Political Science Review* 111(1), 129–148.
- Larreguy, H. (2013). Monitoring political brokers: Evidence from clientelistic networks in Mexico. Working paper.
- Leal-Buitrago, F. and A. Davila (1990). *Clientelismo: El Sistema Político y Su Expresión Regional*. Sociología y Política. Bogota: Instituto de Estudios Políticos y Relaciones Internacionales.
- Lee, D. S. and T. Lemieux (2010a). Regression discontinuity designs in economics. *Journal of Economic Literature* 48(2), 281–355.
- Lee, D. S. and T. Lemieux (2010b). Regression discontinuity designs in economics. *Journal of Economic Literature* 48, 281–355.
- Lehoucq, F. (2003). Electoral fraud: Causes, types, and consequences. *Annual review of political science* 6(1), 233–256.
- Madison, J. (1788). *The Federalist papers: a collection of essays written in support of the Constitution of the United States: from the original text of Alexander*

- Hamilton, James Madison, John Jay* (2nd ed., Johns Hopkins paperbacks edition; third printing, 1989. ed.). Baltimore: Johns Hopkins University Press.
- Mahoney, J. and C. Villegas (2007). *Historical Enquiry and Comparative Politics*. Oxford: Oxford University Press.
- Mahoney, J. and C. M. Villegas (2009, July). Historical Enquiry and Comparative Politics.
- Maldonado, A. (2001). Evaluación de la descentralización municipal en Colombia. avances y resultados de la descentralización política en Colombia. Archivos de Economía 2001-163, Departamento Nacional de Planeación, Bogotá.
- Martinez, L. R. (2017a). Sources of revenue and government performance: Evidence from Colombia. *Unpublished University of Chicago, Harris School of Public Policy*, 1–79.
- Martinez, L. R. (2017b). Sources of revenue and government performance: Evidence from Colombia. *Unpublished manuscript*.
- Matter, U. and A. Stutzer (2015, May). The Role of Lawyer-Legislators in Shaping the Law: Evidence from Voting on Tort Reforms. *The Journal of Law and Economics* 58(2), 357–384.
- McCrary, J. (2008a). Manipulation of the running variable in the regression discontinuity design - a density test. *Journal of Econometrics* 142(2), 698–714.
- McCrary, J. (2008b). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics* 142(2), 698–714.
- Mulligan, C. B., R. Gil, and X. Sala-i Martin (2004). Do democracies have different public policies than nondemocracies? *Journal of Economic Perspectives* 18(1), 51–74.
- Myerson, R. B. (1999, April). Theoretical comparisons of electoral systems. *European Economic Review* 43(4), 671–697.
- Naidu, S. (2012). Suffrage, schooling, and sorting in the post-bellum U.S. south. Working Paper 18129, National Bureau of Economic Research.
- Naidu, S., J. A. Robinson, and L. Young (2015). Social origins of dictatorships: Elite networks and political transitions in Haiti. *CDEP-CGEG working paper series* 31, 1–47.
- Nemoto, K. and M. S. Shugart (2013, March). Localism and coordination under three different electoral systems: The national district of the Japanese House of Councillors. *Electoral Studies* 32(1), 1–12.
- North, D. C., J. J. Wallis, and B. R. Weingast (2009). *Violence and Social Orders*. Cambridge: Cambridge University Press.
- Pachón, M. and M. S. Shugart (2010). Electoral reform and the mirror image of inter-party and intra-party competition: The adoption of party lists in Colombia. *Electoral Studies* 29(4), 648 – 660. Special Symposium: The 2008 U.S. Presidential Election.
- Pachón, M. and F. Sánchez (2014). Base de datos sobre resultados electorales CEDE. 1958 a 2011. *Documentos CEDE 2014-29(21)*, 1–30.
- Pande, R. (2003, September). Can Mandated Political Representation Increase Policy Influence for Disadvantaged Minorities? Theory and Evidence from India. *American Economic Review* 93(4), 1132–1151.
- Persson, T. and G. Tabellini (2002, February). *Political Economics: Explaining Economic Policy* (Reprint edition ed.). Cambridge, Mass.: The MIT Press.
- Persson, T. and G. Tabellini (2003, August). *The Economic Effects of Constitutions*. Cambridge, Mass: The MIT Press.
- Persson, T. and G. Tabellini (2004, March). Constitutions and Economic Policy. *Jour-*

- nal of Economic Perspectives* 18(1), 75–98.
- Regan, P. M. and E. A. Henderson (2002). Democracy, threats and political repression in developing countries: Are democracies internally less violent? *Third World Quarterly* 23(1), 119–136.
- Restrepo, J., J. Vargas, and M. Spagat (2003). The dynamics of the Colombian civil conflict: A new data set. *Homo Oeconomicus* 21(2), 396–428.
- Roberts, A. (2008). Hyperaccountability: economic voting in central and eastern Europe. *Electoral Studies* 27(3), 533–546.
- Robinson, J. A. (2013). Colombia: Another 100 years of solitude? *Current History* 112(751), 43–48.
- Robinson, J. A. (2016). La miseria en Colombia. *Desarrollo y Sociedad* 76(1), 1–70.
- Rodriguez Raga, J. C. and F. Botero (2006). Ordenando el caos: Elecciones legislativas y reforma electoral en Colombia. *Revista de Ciencia Política* 26(1), 138–151.
- Rueda, M. and N. A. Ruiz (2017). Political agency, election quality, and corruption. *Unpublished manuscript*.
- Safford, F. and M. Palacios (2002). *Colombia: Fragmented Land, Divided Society*. Latin American Histories. Oxford University Press.
- Sahin, Y. and J. J. Linz (1995). *Between States: Interim Governments and Democratic Transitions*. Cambridge: Cambridge University Press.
- Sanz, C. (2015, January). The Effect of Electoral Systems on Voter Turnout: Evidence from a Natural Experiment. *Political Science Research and Methods*, 1–22.
- Schedler, A. (2002). The menu of manipulation. *Journal of democracy* 13(2), 36–50.
- Serra, G. (2011, January). Why primaries? The party’s tradeoff between policy and valence. *Journal of Theoretical Politics* 23(1), 21–51.
- Serra, G. (2013). When Will Incumbents Avoid a Primary Challenge? Aggregation of Partial Information About Candidates’ Valence. In *Advances in Political Economy*, pp. 217–247. Springer, Berlin, Heidelberg. DOI: 10.1007/978-3-642-35239-3\_11.
- Shugart, M., E. Moreno, and L. Fajardo (2006). Deepening democracy by renovating political practices: The struggle for electoral reform in Colombia. *unpublished paper presented at the conference “Democracy, human rights, and peace in Colombia” at the Kellogg Institute, University of Notre Dame Unpublished manuscript*.
- Shugart, M. S., M. E. Valdini, and K. Suominen (2005, April). Looking for Locals: Voter Information Demands and Personal Vote-Earning Attributes of Legislators under Proportional Representation. *American Journal of Political Science* 49(2), 437–449.
- Sidel, J. (2014). Economic foundations of subnational authoritarianism: Insights and evidence from qualitative and quantitative research. *Democratization* 21(1), 161–184.
- Snyder, James M., J. and M. M. Ting (2011, October). Electoral Selection with Parties and Primaries. *American Journal of Political Science* 55(4), 782–796.
- Snyder, J. L. (2000). *From voting to violence: Democratization and nationalist conflict*. New York: Norton.
- Snyder, J. M. (1992). Long-Term Investing in Politicians; or, Give Early, Give Often. *Journal of Law and Economics* 35(1), 15–43.
- Snyder Jr, J. M. (1990). Campaign contributions as investments: The US House of Representatives, 1980-1986. *Journal of Political Economy* 98(6), 1195–1227.
- Steele, A. (2011). Electing displacement: Political cleansing in Apartadó, Colombia. *Journal of Conflict Resolution* 55(3), 423–445.

- Stratmann, T. (2002, October). Can Special Interests Buy Congressional Votes? Evidence from Financial Services Legislation. *The Journal of Law and Economics* 45(2), 345–373.
- Stratmann, T. (2005, July). Some talk: Money in politics. A (partial) review of the literature. *Public Choice* 124(1-2), 135–156.
- Szakonyi, D. (2016, November). Businesspeople in Elected Office: Identifying Private Benefits from Firm-Level Returns. (ID 2844901).
- Uppal, Y. (2009). The disadvantaged incumbents: Estimating incumbency effects in Indian state legislatures. *Public Choice* 138(1/2), 9–27.
- Vargas, J. F. (2009). Military Empowerment and Civilian Targeting in Civil War. HiCN Working Papers 56, Households in Conflict Network.
- Vargas, J. F. and J. Villaveces (2016). Rural property tax revenue in the face of economic concentration and political competition: Evidence from Colombia. Unpublished manuscript, Universidad del Rosario.
- Witko, C. (2011). Campaign Contributions, Access, and Government Contracting. *Journal of Public Administration Research and Theory: J-PART* 21(4), 761–778.