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Abstract

The discredit of democratic institutions is rooted in public disillusionment with their ability to bring about change. The misuse of public office combined with the systemic ineffectiveness of regulations to contain this phenomenon are key determinants of this condition, which is widespread across political systems. The present work analyzes corruption displacement that plays a key role in undermining policymaking and institutional change. It discuss how political actors strategically respond to institutional reforms to preserve a rent-extractive status quo, and how this affects political transparency, electoral accountability, and institutional perception - on top of a number of economic outcomes of interest. The reform areas The work address in my research are the political capture of the bureaucracy and the business capture of politicians – touching on issues as special interest collusion, personal use of one's office, and willful public resource mismanagement. The work does not only describe the effects of suboptimal policy design, it also contributes to close the knowledge gap with respect to the displacement practices by developing novel measuring techniques and testing approaches to different phenomena - happening at the margin of the legal framework, or in open violation of it. With respect to each area of discussion, a blueprint for action is defined as a result. The work rests on causal identification achieved through the use of quasi-experimental design.

Section 3 works as an introduction and presents the overarching theme of the inquiry, stating more precisely the scope of the project and presenting its articulation. Sections 4 to 6, constitute the main body of the dissertation and work autonomously as separate essays. Section 7 concludes.

Introduction

The ever-growing distrust of democratic institutions and the actors who inhabit them is a central concern of the political science scholarship, and of the broader public discourse (Hetherington 1998; Kaase 1999; Mishler and Rose 2001). The rescission of the bond of trust between rulers and ruled rests on a three-folded experience: the realization that a number of actors are willing to capture political offices and distort their function to their benefit – both from within and without the institutions; the realization that regulation put in place to contain misconduct is ineffective at the task (Rothstein and Stolle 2012; Morris and Klesner 2010); and the realization that the two aspect a intimately connected in any political arena where legislation is not produced in the void and strategically respond to policy intervention, when not preemptively directing it. If the rescission of this bond is indeed deemed to be problematic for the functioning of institutions (Putnam, Leonardi, and Nanetti 1994; Rothstein and Uslaner 2005), research efforts should be concentrated on, first, better understanding and measuring the main forms of abuse of office; and, second, bridging the knowledge gap regarding the mechanisms through which effective policy interventions can be carried out – in this openly considering the pivotal role of status-quo preserving actors.

With reference to the first goal, this project identifies three main embodiments of misconduct, by different actors, all negatively affecting the working of institutions, not in a gradient of threat degree to the working of democrat institutions: i. the bypass of regulative institutions; ii. the rent-extracting use of public offices; and iii. the shortsighted management public resources.

With reference to the second goal, this work analyzes the listed forms of misconduct and tries to understand to what extent they are expression of (possibly different) political actors respond strategically to policy regulations and maintain their entrenchment. In presence of a status-quo improving intervention, I try to disentangle the strategies through which actors go beyond classical public policy attenuation Olken and Pande 2012 to overtly nullify an intervention. A result of event greater impact to distrust and institutional deterioration.

The present work is composed of three separate studies, which bring together the two stated goals of the overall research. All the studies are empirical examinations of real-world phenomena observed in the context of Brazil or Italy. The implications are valid beyond the specific context in which they are drawn. The work aims at talking to the greater literature of comparative political science. It is strongly situated in causal inference research and makes ample use of frontier quantitative methodology.

3.1 Theoretical framework

In setting the presented goals, this project situates itself within the classical research on corruption in political science and public policy scholarship. In particular, it has clear connections to that part of the scholarship that aims at both isolating social and economic determinants of public sector malfeasance, and at devising a reform strategy to improve efficiency and fairness of government. More specifically, I approach several instances of malfeasance aiming at understanding to what extent relevant corruption curbing programs and legislative interventions have been designed with formal or practical loopholes so that they ultimately result detrimental to their very goal. In this, I following the pivotal teachings of Rose-Ackerman 1978, 2007.

The analyzes that I will carry out are sector specific. This is in line with the guidelines of the discipline, that recommends to study corruption in middle-to-high income countries by devoting attention to specific dysfunctional sectors in otherwise sufficiently well-functioning economic and political contexts.

I focus on local governments. As specified, in that context devote attention to three different form of misconduct carried out by different actors. All different forms undermine functioning and perception of the state machinery working, and ultimately of the state itself.

As first, I tackle how actors manage to bypass of specific regulative institutions and maintain their status quo. I study how, an abrupt change in campaign finance regulation fail to displace extra-political actors' incentives to seek high-return political connection. I discuss how shortsighted policy design led to a decrease in transparency and electoral accountability – this favoring the very actors that set off to dis-empower.

In the second case, I tackle some limit case of self-serving public office use, devoting my attention to nepotistic discretionary hiring. I study how entrenched actors managed to leverage personal an political networks to maintain their rent extracting tool, in the face of legislation openly blocking it.. Corruption in the administration of the bureaucracy follows a classical pattern and result in capture, detrimental to the administration's autonomy and its very ability to operate.

In the last case I discuss shortsighted management public resources. Malfeasance takes the form of mismanagement of governmental finances. There political actors proactively engage in unsavory accounting practices, undermining limits on spending, getting around the types of spending that are permitted, and subverting controls on the sources of funds themselves. Under scrutiny passes the inability of regulatory grip and the set of incentives that put local administration to jeopardize the economic stability of a municipality.

The scope of each piece might look narrow, but their relevance should not be undeplayed - as, for any form of "[c]orruption [...] is a symptom that state/society relations operate in ways that undermine the fairness and legitimacy of the state [...]" (Rose-Ackerman 2007, p. xvi). Besides, all are compelling example of 'grand' corruption Rose-Ackerman 2007, and well deserve scrutiny withing the scope of the project and in accordance to the its conceptual framing adopted. The present work addresses some "pathologies in the principal-agent relation" (Rose-Ackerman 1999), which have been identified as be-

ing at the heart of many corrupt transactions, but which are also highly important for the mismanagement cases I address. In the case of corruption practices, how politicians violate their principals' mandate is straightforward and substantially in line with the literature; in the case of resource mismanagement, the violation is less clear-cut. An overview of how each study connects with the well known representation conundrum (Ferejohn 1986, Achen and Bartels 2017) is, thus, important for an understanding of the connections between the different topics.

The deviation of the mandate is evident when considering the relationship between politicians and the bureaucracy when nepotistic practices are employed. A politician who employs a technology of rent extraction for personal returns clearly exemplifies a control issue. The violation of the mandate has to do, on one hand, with the introduction of a form of attrition in the workings of the bureaucracy that directly affect citizens; on the other, by even marginally overstepping blurring the lines of its bestowed power one can undermine the whole institutional credibility (Reid and Scott 1994). The issue of control is even more evident when, as in the cases I study, the politician displays a deviant behavior, taking advantage of a legislative opportunity (e.g. the license to make discretionary hires), and points at the fact that more complete contracts, or more capillary institutional control should be exerted. My work also reveals the illusory nature of such recommendations when taken at face value: even when legislation becomes pervasive, agents can openly coordinate to escape the control of principals - leaving the status quo unchanged or worsened. Myopic policy intervention is hardly capable of enforcing principal-agent mandates. To make it so, a deep understanding of the incentives and opportunities open to politicians must be acquired.

By considering the relation between politicians and corporate donors, I address another classical issue of principal-agent models: the contemporary existence of multiple principals for the same agent. Politicians, in the context of this study, start off by responding to both their citizens and the corporate actors who sponsored their electoral campaign. The policy intervention described –the introduction of a corporate donation ban– is devised to curtail the corporate influence. However, the interven-

tion strengthen the principal-agent bond between contributors and elected at the expenses fo the one berween electors and elected. From this emerges the ability of specific groups of principals to (more) readily adapt their strategies to preserve the status quo in the face of an institutional rearrangement.
I also find that politicians violate their principals' mandate by willfully making unsound financial choices. These choices violate both present citizens' mandate, as well as future citizens' mandate – both directly, and indirectly. The violation consists, on the one hand, of undertaking choices that are not specified in the representation contract and, on the other hand, the violation is embodied in the limiting potential these decisions have over others in the same principal agent (possibly across time). The limiting aspect is linked to the debt accumulation that follows from financially unsound choices. The direct limitation is embodied by the constraint that debt repayment imposes on the agent's action – in the present or and in the future. High levels of debt can jeopardize the very existence of a principal-agent relations: financially irresponsible choices can result in the removal of a politician and to her substitution by a non elected bureaucrat over which citizens have no control nor accountability.
In all these scenarios the subsistence of the relationship, and –within reasonable limits– the subsistence of an unconstrained relation, has absolute priority over the actions an agent should undertake on behalf of the principal. In this sense, it makes little sense to argue that a politicians is not in violation of her duties as an agent if she makes choices to maintain electoral promises, especially debt-accruing ones. The situation is further aggravated by the fact that these mandate violations are generally driven by self-interest, and undertaken to increase one's ability to expand expenditures for monetary or electorally return.

If the ultimate scope of any anti-corruption intervention is to limit the overall social cost of corruption (Rose-Ackerman 1999), legislators need to consider all the possible consequences of any such intervention before implementation. This ceases to be a truism, as soon as it is clear that anticipating strategic response of key actor is not straightforward. All intervention presently understudy fail to an

extent specifically for this reason. The consideration is at the basis of the policy suggestions common to many scholars who invite to target anti-corruption programs, driving them away from a high level of generality, to successful implement them (i.g. Rousso and Steves 2007). Program targeting requires a refined knowledge of both incentives and resources of targets. As Rose-Ackerman 2007, p. xxxvii writes, "Clever technical solutions, based on economic incentives, may not be enough. If corruption is one of the pillars supporting a political system, it cannot be substantially reduced unless an alternative source of revenue replaces it. Powerful groups that lose one source of patronage will search for another vulnerable sector. [...] It is little wonder that effective and long-lasting corruption control is a rare and precious achievement". Correctly carried out reforms might well introduce changes to political structures and changes the incentives that elected officials face (Kunicová 2006). Politics remains central in the understanding of the origins, articulation, and persistence of corrupt practices, especially with reference to embedded conflict of interest.

Relevant to the present discussion, are also considerations over malfeasance displacement. Anti-corruption reform might produce unexpected outcomes, also in sectors other than the one specifically targeted (Yang 2008). This increases once again the complexity of the task of anticipating actors' reactions to regulation. A limited literature discusses how partial enforcement and limited information lead to corrupt activities displacement and creation of new *loci* of corruption (Mishra 2002). If the creation of new *loci* of corruption is pertinent in all studies, it is a combination of partial enforcement and limited information that gives room for corruption of reproduce and evolve. This should taken specifically into consideration when designing and assessing policy intervention – especially as actors can recognize the value of disinformation and use it to their benefit.

3.2 Overview

The thesis is subdivided into three distinct essays, each covering a major facet of the mismanagement of public office. Each chapter presents the relevant question and delves into the dataset specifics in further detail.

The first essay is a solo paper titled “Political scandals and corporate political participation An analysis of straw-man donations in Brazil". It investigates the straw-donor phenomenon, as adopted by corporations to keep participating in the political game in the face of a restriction. Empirically, I focus on Brazil where corporate donations have been banned. I track individual donations back to firms using a dataset covering the universe of employment data. Then, leveraging on donation pattern information, I show how some firms might have bypassed the ban using their employees as donors. I adopt a Difference-in-Differences strategy taking advantage of heterogeneity in pre-intervention donation patterns. I conclude that these interventions incentivize non-transparent ways for corporate actors to participate in the political arenas, reducing transparency and accountability, without otherwise changing the status quo. The second essay is a joint work with F. Daniel Hidalgo and Gabriel Cepaluni titled “Reforming the Nepotistic State? How familial and political networks undermine bureaucratic reform in Brazil" . It studies nepotism as a rent-seeking technology that politicians use to capture local bureaucracies. It tackles the issue of measuring nepotism, and exploring to what extent top-down interventions are effective ad eradicating it. Empirically, it studies Brazilian municipalities over the years 2002-2012, before an after a Supreme Court ruling that banned all forms of nepotism. It discuss the extent to which the reform is effective at curbing the practices, especially in the face of an entrenched political class that can create new *loci* of corruption. Winning has a positive effect on nepotistic appointments. The ban has a relative and absolute effect of the ban. The piece points to the existence of strategic responses to the ban in the form of “relatives exchange” across politicians.

The third essay is a solo paper titled: “The color of the public purse Ideological camp effects on resource misallocation”. It considers mismanagement of public in Italian local governments. The study focuses on the use of accounting tricks to expand expenditure capacity which are in violation with respect to administrative regulation. The paper explores the role of contingent political determinants on the municipal financial health. Empirically, the piece explores a Regression Discontinuity design on margins of victory at the local level. It presents previously unavailable evidence regarding a lack of heterogeneous degree of mismanagement across ideological camps families, and the across ideological and non ideological ones..

Political scandals and corporate political participation. An analysis of straw-man donations in Brazil

4.1 Abstract

Curtailing corporate participation in electoral campaigns, in principle, improves electoral accountability by reducing the number of principals to whom politicians have to respond. Not surprisingly, the practice is widespread. Reforms in that direction, however, are not straightforward to implement and present clear conundrums with respect to corruption displacement. In settings with high-returns to political connections, corporate actors are unwilling to relinquish control over politicians and resources willfully, and might oppose change, directly and indirectly. I approach the issue of campaign regulation effectiveness by focusing on the latter, specifically addressing the emergence of straw donor schemes – in which corporate actors circumvent restriction by employing private donors as intermediaries.
Empirically, I exploit a corporate donation ban in Brazil and study how it affected corporations' ability to signal support by means of private-for-corporate donations of their employees. Employing a Difference-in-Differences strategy and leveraging pre-intervention heterogeneity in firms' donation patterns, I show that firms with a history of political participation orchestrate a clearer signal to politicians with respect to comparable firms without such record. Two measure of donation orchestration are developed as adaptation of the Herfindahl-Hirschman Index: a within- and an across-candidates contributions concentration index. The papers shows that drastic campaign reform does affect the ability of firms to maintain speech in the political arena. The study concludes that the all-encompassing ban was counterproductive, especially because implemented at the cost of electoral transparency and accountability.

4.2 Background and theory

Interest groups' campaign contributions bring with them, alongside the promise of free expression, the risk of disproportionate influence of corporate actors in the democratic process. In political contexts where money's role is critical, corporate participation in politics has been perceived as a threat to the integrity of electoral dynamics and governmental processes (Stratmann 2005; Scarrow 2007). Interest groups making large contributions have been found to gain undue influence on electoral outcomes, on legislation shaping, and on the regulatory proceedings outcomes (Wright 1990; Romer and Snyder Jr 1994; Ansolabehere, Gerber, and Snyder Jr 2001; Shapiro 2009; Hasen 2016). Regulation of corporate participation in election is, as a result, unsurprisingly regulated worldwide (IDEA 2018). Despite practitioners' rush to implement contribution caps to limit these *quid pro quo* exchanges , the academic literature is divided on the extent to which these reforms are effective with respect to their stated scope (Levitt 1995; Che and Gale 1998; Smith 2009). Presently, I study the focus on corruption displacement dynamics, specifically focusing on strategic responses corporate actors give to regulation attempts. I try to overcome the normative attitude underpinning financial regulation reform, towards a more comprehensive analyses, relevant for political scientists and practitioners alike.
If – in an attempt to preserve the status quo– actors strategically respond to regulation, this should be key in corruption curbing policy evaluations, as well as in jurisprudence (Hasen 1998; Butrymowicz 2009). Presently, I explore to what extent an all-encompassing corporate campaign ban, in a context of high-return to contributions, have stimulated straw donor schemes. This novel unexpected outcomes of reforms lays the ground to discuss to what extent these strategies negatively effect transparency electoral accountability.

Studying responses to policy regulations that formally comply, while *de facto* bypassing them, appears beyond the scope of campaign contribution regulation assessment and modeling. A rigorously discussion of legal loopholes exploitation is crucial to be able to provide the meaningful overlook of a

phenomenon that is both diffuse and worrying. This piece presents a novel approach to the study of straw-man schemes, that rests on data-mining of campaign finance disclosure reports. The method is novel to the discipline and to public discourse alike. In a context with high-returns to political connections (Boas, Hidalgo, and Richardson 2014), straw-man donations substitute for special interest donations when these are banned. I discuss the elusive practice, its diffusion, and its relevance in preserving the rent-seeking ability of interest groups. The analysis rests on the development of different minimal and scalable measures that are usable in multiple contexts.

Brazil is an exceptionally fit context in which to study straw donor schemes. Despite having a sufficiently comprehensive jurisprudence (Samuels 2001a, 2001b), the country displays overt signs of a dysfunctional campaign finance system - for corruption, (Samuels 2001c; Fleischer 2002), costs (Aguiar 1994; Zovatto 2003) and returns on investment (Boas, Hidalgo, and Richardson 2014). On top of this, in the wake a major political scandal, the Country formally ruled out corporate donations, creating the perfect setting in which to study whether coordinated private signals did substitute for corporate ones, and to what extente.

On top of this, a major political scandal in the Country led to a drastic, all-encompassing reform that banned all corporate donations that just before fueled the whole system. Pundits hold that the change pushed corporations to develop 'creative' strategies to maintain the status quo, among which the use of indirect donations was prominent (Douglas 2015; Benjamin and Caruso 2016; Borges and Marcela 2016). To study the universal ban treatment, I explore firms pre-intervention donation patterns matching private and corporate donations within a firm the determine pseudo-control groups. Employing a Difference-in-Differences strategy, I gauge the impact of the intervention observing the changes in within- and across-candidates concentration contribution indexes, taken as a proxy for donations orchestration. I create both indexes modeling on the Herfindhal-Hirschman Index (Dharmapala and Palda 2002). Tracing back individual donations to specific corporations is made possible thanks to

of unique identifiers' present in a dataset spanning the universe of formal employment contracts of Brazil.

Restricting corporate campaign contributions in the manner described pushes firms to restructure their donation portfolios with the goal to maintain their political and speech ability, pivotal to extract rents from politicians (Koger and Victor 2009; McMenamin 2013; Bertrand, Bombardini, and Trebbi 2014). Firms with a history of campaign contributions – those actually affected by the treatment – display significantly higher concentration indexes than the rest of the firms – both within- and across-candidates. As the measures are taken as a proxy of forced donations orchestration, whether or not in overt violation of the law, the piece concludes that treated firms were better able to navigate the post-scandal political landscape, maintaining their level of signalling. The use of private donations, could have been key to maintain speech, but also unlawful favours exchange relation down the line. In a sea of private donations, concentrated donations can be recognized and decoded by a politician as a unitary signal of support and, thus, can substitute for a corporate contribution in its scope.

To advance an ownership claim over its employees' donations, at a minimum, a firm needs to make sure these individual donations display a peculiar pattern. Producing concentrated donations is a sufficiently easy and effective way to produce such pattern. A firm can secure it by means of simply orchestrating its employees donations or proactively investing funds to achieve such a goal. The production of the pattern precedes and is irrespective of a politician's ability to decode the signal unaided. My original measures of the phenomenon provide a rigorous, quantitative, and externally valid solution to detect such efforts. They provide a good indication of the effectiveness of a ban, going beyond the mechanical elimination of a source of donations.

Blindly advocating for campaign finance regulations often fails to take into consideration several unintended consequences of the policy - even in an ideal implementation scenario. The present paper discusses the effects of such an intervention in a relevant scenario and delves into how legislation's

shortfalls are taken advantage of by corporate actors. The paper presents a more realistic picture of the issues and challenges open to any political scientist modelling campaign finance welfare effects, and any legislator planning to curb donors' influence. Overlooking strategic reactions to donation bans comes at the cost for voters of reduced information, transparency and electoral accountability. This is unacceptable when designing effective legislation.

The rest of the paper is organized as follows. In Section 5.3, I present a brief literature review on corporate political participation and the effectiveness of policy intervention at curtailing it. In Section 5.4, I provide details concerning institutional context, especially discussing campaign finance regulation. In Section 5.5, I present an overview of the data discussing face-value effectiveness of a the corporate donation ban. In Section 5.6, I present a measure of donation coordination proxying straw-man schemes. In Section 5.7, I present the empirical strategy and the main estimations results. Section 5.8 concludes.

4.3 Literature review

The present paper contributes to a recent body of research on campaign finance regulations, the role of money in politics, and the ability of actors to displace corruption practices.

The study aim at expanding our current understanding of how campaign funding and corruption interlink, especially in Latin American settings. The specificity of the Latin American case has been explored in a general comparative perspective (Austin and Tjernstrom 2003) and in isolation (Castillo Vera and Zovatto 1998; Carrillo, Lujambio, and Navarro 2003). In both a clear connection emerges between the way in which political parties fund their election campaigns and the degrees of corruption they experience; with transparency and reporting being positively correlated to corruption, and restriction to access of different actors negatively correlated to it. These studies mainly present campaign funding systems in terms of the limitations they impose on private contributions - at the center of corruption

dynamics, as in the present research. Brazil emerges as a remarkably interesting case, with both limits on sources of private contribution, and contribution ceilings - making it one of the most restrictive systems in the region - though still reporting high level of corruption. Zovatto 2003 presents some of the major problems regarding enforcing anti-corruption restrictions, and explains some of the limitations of the 'textbook solutions' to corruption. In particular, it states that in the context of Brazil "imposing restrictions generally encourages parties to resort to 'creative' accounting and practices designed to stretch these limits". This dynamic, in a form that has previously been overlooked, will be at the center of the present study.

Considering how corporations revert to straw-man donations in response to a all-encompassing policy intervention, the paper presents a further example of how reforms might produce unexpected outcomes. Here, no new *loci* of corruption are generated –as in Mishra 2002, or Yang 2008–, instead, the same kind of activity is carried out through a new channel. The corporate ban, in this sense, becomes one of those topic solutions unable to affect informal institutions, nor the incentives that political and non-political actors face (Kunicová 2006); as such, it is revealed to be ineffective.

Despite focusing on a specific form of political participation of corporations, the paper contributes to a broader literature discussing the rationale and dynamics behind more classical forms of political participation of corporations, both in general (Ansolabehere, De Figueiredo, and Snyder Jr 2003; McMenamin 2013), and with specific reference to Brazil (Samuels 2001a; Arvate, Barbosa, and Fuzitani 2013; Bourdoukan 2010, Boas, Hidalgo, and Richardson 2014[1]). The paper, moreover, contributes to the exiguous literature examining the effects of regulating campaign finances from an empirical perspective, including Stratmann, Francisco, et al. 2006 that discusses how campaign limits lead to closer elections for future incumbents other than the promoters, and to an increase in candidates in the US state elections; Milligan and Rekkas 2008 that finds that large limits on spending leads to less competitive elections, fewer candidates, and lower voter turnout in Canadian federal elections; and Avis et al. 2017 discusses the increase in political competition and incumbency advantage reduction brought

1. For a comprehensive review of more classical literature, see: Stratmann 2005

about by spending limits, in the same context as the present one.

In addition, the paper contributes to a vast scholarship in jurisprudence that discusses the potential undemocratic consequences of campaign finance reforms. The US-centered contributions focus substantially on the Federal Election Campaign Act of 1971 and the subsequent Supreme Court *per curiam* opinion (*Buckley v. Valeo* 1976) that struck down limits on independent expenditures[2]. At the core of this literature lies the discussion of the role that political contributions play in expressing citizens' opinions in US politics (Smith 1995, 2009). This speaks directly to the "donations-as-speech" perspective in political science (McMenamin 2013). Besides this, the scholarship addresses the regularity according to which excessive regulation incentivizes the creation of new *loci* of corruption – aptly labeled the 'First Law of Political Thermodynamics' in Issacharoff and Karlan 1999: "[...] every reform effort to constrain political actors produces a corresponding series of reactions by those with power to hold onto it" (*Ibidem.*). The literature is generally skeptical regarding the possibility to destroy the desire for political power, raising the issue of monitoring with respect to practices that circumvent fundraising restrictions. The issue of entrenchment is also explored to a certain, though informal degree (Klarman 1996). Several contributions in this strand of literature suggest to cope with the phenomenon modest legal reforms – such as the introduction of public financing and disclosure measures – as a panacea. The present work, however, calls into question the effectiveness of such measures, at least for some contexts.

To conclude, given that straw-man donations rely on the mobilization of employees, the paper also speaks to a strand literature in the field of management on the role played by corporations in employees' activism, as well as in fostering ideological alignment among them (Beyer 1981, Rosenstone and Hansen 1993, King 2008, Gupta, Briscoe, and Hambrick 2017). However, the sudden change in policy and the substantial impact it had on corporations' signaling ability, suggests that the coordination effects I find

2. For an overview of more recent reforms in U.S. campaign-finance policy, as well as an overview of the role political scientists played in the process, please refer to Mann 2003.

(as revealed by the increase in donations' concentration) should be attributed to the will of corporations to tinker with their employees' donations rather than to an ideological realignment produced through culture change or nudging.

4.4 Institutional context

In this section, I describe the main features of Brazilian institutions key to approach the study of straw donor schemes in the country locally.

4.4.1 Brazilian municipalities

Brazil has 5,570 municipalities[3], which are constitutionally part of the Brazilian Federation and divided across 26 States[4]. Brazilian municipalities are governed by a mayor (*prefeita/prefeito*), and a municipal chamber, made up council members (*vereadores*) whose number varies according to population. The mayor and the chamber are elected at the same time and simultaneously across the Country. Mayors are elected by plurality rule and face a limit of two consecutive terms. Their mandate lasts 4 years. The mayor performs both political and administrative duties, with no other appointed public manager in charge (Avellaneda and Gomes 2017). The mayor performs not only political but also administrative functions and is considered the most prominent and identifiable political actor among elected officials: the mayor is the key decision maker in Brazilian municipalities (Avellaneda 2008; Avellaneda and Gomes 2017; Rocha, Orellano, and Bugarin 2018). In addition to this, Brazilian municipalities display a high degree autonomy – among the highest in South America (Nickson 1995; Samuels 2004; Titiunik 2009) – and local governments are directly responsible for the provision of key goods and services - including management of health services, primary education, transportation, and sanitation[5] . It is not

3. Municipalities vary substantially in population, development, and other political and socio-economic indicators (Estatística 2010). These factors are kept into consideration in the analyses that follow.

4. For information regarding the administration of the Distrito Federal please access: `http://www.df.gov.br/`

5. Brazilian Constitution of 1988, Article 30, Chapter IV establishes municipal responsibilities. *See*: Avellaneda and Gomes 2017 for an in-depth discussion.

surprising that the practice of corporate actors to cozy up to mayors in search for a an economic return in the form of contracting is widespread[6].

4.4.2 Campaign finances

Formally, Brazil strictly regulates campaign finance: bans have been historically in place on donations coming from government contractors, trade unions, foreign entities, and anonymous sources (Falguera, Jones, and Ohman 2014). Donations limits are in place and historically were set at 2% of a one's gross annual revenues for private donations, and at 10% of previous-year gross revenues for corporate donations (Zovatto 2003). Regulations apply to all races across levels of government, and candidates are requested o publicly report both campaign donations and spending. Legal source of finances include corporations, private individuals and parties – self-financing is allowed. However, given a number of flaws in the implementation of political finance controls, incentives for reckless fundraising practices has emerged in the Country making these commonplace across the country (Casas-Zamora 2016). In particular, Brazil's electoral regulations have been deemed permissive toward excessive corporate donations. As a result, campaign financing has long emerged as a marketplace fo firms to buy political influence with high returns (Boas, Hidalgo, and Richardson 2014, p. 417).

Campaign finance regulation changed abruptly in 2015 with the new Electoral Reform (Law No. 13.165 / 2015, or '*minirreforma*', Eleitoral 2016) . In March of the previous year the largest-ever corruption scandal in the Country was exposed: the Operation Car wash - *Operação Lava Jato*) uncovered a complex collusion and corruption network linking Brazilian business and political elites, which led to an extensive and successful police investigation of even some of the most prominent figures in the country (Kamm 2015; Sotero 2018). The scandal opened a window of opportunity for the Supreme Court (*Supremo Tribunal Federal*) to pronounce unconstitutional the involvement of corporate actors in cam-

6. For anecdotal evidence access: `https://www.bbc.co.uk/news/world-latin-america-34104124,https://g1.globo.com/rio-de-janeiro/noticia/obras-da-odebrecht-foram-superfaturadas-em-mais-de-r-3-bilhoes-no-rj.ghtml,https://www1.folha.uol.com.br/poder/2015/08/1668585-pf-deflagra-operacao-de-combate-ao-superfaturamento-em-estadio-da-copa.shtml`.

paign financing (‘*Ação Direta de Inconstitucionalidade*’ 4650 Federal 2015). Corporate donations, it was claimed, distort electoral fairness and translate in undue influence over elected officials. On this basis[7], the subsequent electoral reform banned corporate donations at all levels of government for both candidates and parties. The ban was effective beginning from the 2016 municipal election.

4.4.3 Straw donor schemes

The corporate donation reform was met with skepticism from the onset[8]. In a country with the extremely expensive elections (Zovatto 2003; Casas-Zamora 2016) and where a system of economic *do ut des* practices underpinned the electoral machine, the fear of corrupt practices displacement was very high[9] . Both the proliferation of off-the-books slush funds (‘*caixa dois*’), and the use of straw man donors (‘*laranjas*’) – two sides of the same coin – were discussed both in the local (Borges and Marcela 2016; Ramalho 2016; Av.Vv. 2016) and international press (Douglas 2015; Benjamin and Caruso 2016). Investigative pieces on the phenomenon only appeared in the subsequent federal election, in 2018, when it became clear that some businessmen, as private individuals, were participating in the campaign donations in open substitution of their firm (Breno and André 2018; Josette 2018).

Straw man donations are the focus of the present analysis. Despite its enormous reach, the ban leaves open more than one channel through which parties can still funnel money to candidates. In the presence of alternative donation channels a legal loophole opens up for corporations that are (still) willing to influence politicians. The extent to which said loophole has been taken advantage of by corporations is precisely what the present piece aims to assess. Displacement of this kind raises a number of key issuer with respect to the transparency and accountability trade-offs that come with all-encompassing reg-

7. the pronouncement was briefly upheld by the Congress, and then passed into low thanks to the veto by President Rousseff (Ramalho 2015; Passarinho and Calgaro 2015; Matoso 2015).

8. Questioned on the efficacy of the policy, the President of the Chamber of Deputies Eduardo Cunha, replied: ‘People will go out ‘hiring’ individuals to donate. Will the campaign cost less? No. You will have to pay the hiring’ (Borges and Marcela 2016. Whereas, Justice José Celso de Mello declared: “If some donation is made from now on all that we can be sure of is that we won’t know about it, but we can never be sure that it does not exist." (Douglas 2015)

9. In popular culture, the displacement was framed as an example of ‘*Jeitinho Brasileiro*’, or ’the Brazilian way’, referring to way to accomplish something by circumventing or bending the rules or social conventions (Wikipedia 2019).

ulation[10]. As these kind of regulation appear to be the go to option for legislators with small window of opportunities in otherwise gridlocked corrupt scenarios (Casas-Zamora 2016), it is important to thorough discuss their advantages, by design and test measure to ascertain the phenomenon. In other contexts, data mining campaign finance disclosure reports is started being listed among the options available to analysts to pick up irregularities in the donation process (Kelner 2018). Presently we try to use a novel methodology to approach the detection of straw donor schemes.

4.5 Data

In this section, I describe the main sources of data needed to assess the effectiveness of the corporate donation ban and that are used to operationalize of straw man donations. The section includes preliminary results describing the effectiveness of the ban.

4.5.1 Electoral data

Electoral data for all political races in Brazil are available in the *Repositório de dados eleitorais*, a repository provided by the Superior Electoral Court (*Tribunal Superior Eleitoral*, TSE). Presently, we will focus exclusively on the local races for the years 2004-2016, because of data availability and quality concerns further explained below. The 16,058 candidates runing for one of the 5568 majoral positions open are identified by an 11-digits social security number (*Cadastro de Pessoas Físicas*) number. The data report a number of socio-economic characteristics, including: gender, age, education, marital status, employment (profession and status). Among the race-specific information, the data report party to which the candidate is directly affiliated, the coalition of parties that supports her, and the result of the election. Races contestability changes substantially averaging out at 2.91, and reaching a peak of 12 candidate for São Paolo.

10. Similar considerations were raised with respect to the ban on public contractors' donations: Falguera, Jones, and Ohman 2014 underlines how the challenge of regulating 'undesired donors' is to determine whether a ban would merely drive an donor to operate further into the shadows.

In addition to these, the IBGE (*Instituto Brasileiro de Geografia e Estatística*) and IPEA (*Instituto de Economia Aplicada*) provide socio-demographic data on Brazilian municipalities. Data are public and openly available[11]. These are integrate in the analyses to control for some aspects

4.5.2 Campaign finance data

Contribution reports for any political race in Brazil are publicly available through the Electoral Data Repository (*Repositório de dados eleitorais*),that is maintained by the Superior Electoral Court (*Tribunal Superior Eleitoral*, or TSE). Since law No. 8713/1993, it is a requirement for candidates to submit to the Court a detailed overview of all the campaign contributions received in a given electoral campaign. Presently, only electoral reports from years 2004-2016 will be employed: data quality for earlier reports is poor, and using them would intoduce too much noise in the analysis.[12].

With respect to donors, the reports include two identifiers: name and tax identification number – for both individuals (natural person, or 'pessoa física') and corporations (legal person, or *' pessoa jurídica'*). Tax identification numbers are unique and distinct across the two legal entities: for individuals is the already mentioned CPF number, for firms is the 14-digits CNPJ (*Cadastro Nacional da Pessoa Jurídica*) number – each will be used as personal identifiers for the respective category. Additional information is reported with respect with the kind of donations received. For in-kind donations a brief (non-standardized) description of the service provided is listed. For private donations, no information is reported with respect to the origin of the donation or labor market infomation of the donor, as it is customary in other settings (i.e. the US).

Candidates in mayoral races have historically received support from both corporations and private individuals. Regarding private donations (self-donations excluded), they accounted for 20.8% of

11. Data can be accessed at the institutions' respective websites: `www.ibge.gov.br` and `www.ipeadata.gov.br`

12. Contribution data for 2004 partially suffer form the same issues: for election year 2004 reports data from around 20% of municipalities are not reported. Missing municipalities will be dropped in a robustness check aiming at producing a more balanced panel data-set.

the total value of donations, amounting to BRL 474m (USD 231m). This is particularly impressive as only 0.28% of Brazilians (562,619 people) appeared as individual donors in last election prior to the reform (2012), used for these calculations. The reform shrunk total donation by 34.6.%After the electoral reform the volume of donations contracted, at around -34.6% for mayors, despite the number of individual donors surged by 17% – accounting for population change. Banned corporate donations, individual ones accounted for 41.9% of donations.

Regarding corporate donations, they accounted for 28.9% of the total value of donations, amounting to BRL 658m (USD 202m) in the last election prior to the reform. In the campaign 47,169 (47,670) unique corporations (corporation branches) participated by means of 119,848 distinct contributions. The remaining donations in municipal races came from party transfer and self-donations.

Candidate-level statistical research on campaign finance in Brazil are sufficiently thorough and, in the years considered, easily available. The ever-growing comparativist interest on the setting, at least since Samuels 2001a, 2001b contribution, comes at no surprise.

4.5.3 Labor Market Data

The major source of employment data for Brazil is the administrative database produced by the Brazilian Ministry of Labor and Employment (*Ministério do Trabalho e Previdência Social*, MTE/MTPS): the RAIS (*Relação Anual de Informações Sociais*) which covers the universe[13] of individual formal market contracts for the whole country, including both the private and the public sector. It is considered a high quality census of the Brazilian labour market (Dix-Carneiro and Kovak 2017, Brollo, Forquesato, Gozzi, et al. 2017). The data-set made available by the MTE to research institutions upon request.

The RAIS provides detailed demographic and labour market information about each person who

13. The Brazilian institute of Geography and Statistics (*Instituto Brasileiro de Geografia e Estatística*, or IBGE) maintains that RAIS covers about 97% of the Brazilian formal market. `http://ces.ibge.gov.br/base-de-dados/metadados/mte/relacao-anual-de-informacoes-sociais-rais.html`

holds a formal labour market contract in a given year. This information includes demographic characteristics, employment status, and educational achievement. What is essential for the present analysis is that the data-set provides information on each worker's individual tax identification number (CPF), as well as that of the corporation she was employed by (CNPJ). As mentioned in Section 5.5.1, these numbers are unique and distinct across individuals and corporations. In the electoral years considered (2004 - 2016), the RAIS includes around 67m contracts in the Brazilian formal market that can be traced back to about 54m uniquely identifiable employees. On average, employees sign 1.2 formal contracts per head, with only 18% of the employees' population having more than one contract registered in the data-set each year. The RAIS dataset will be used to match individual and corporate donations, and it is crucial for the resent analysis.

The RAIS data-set is limited in one major respect: in the data-set only formal employment contracts are recorded. Business owners, consultants and self-employed workers[14] are not present. Given that it is of fundamental importance to my research to be able to track donations back to each firm using the database, their absence leads to a substantial amount of unmatched donations, which are dropped from the analyses (*See*: Table 4.3). Besides this, it is important to note that in some firms the higher up a worker moves in the firm's hierarchy, the more likely it is that she moves from a dependent worker's contract to an independent one. This means that the people likely to have more power and access in a given firm are also those missing from the data-set, which prevents me from tracing back to a firm specifically their (potentially high) donations. If the straw-man donation dynamics involves this category of workers – as anecdotal evidence shows –, it is important to realize that my ability to fully capture the dynamic is limited. The present needs to be taken as a lower-bound estimate, though still very much informative regarding the straw-man donation dynamic. Anything captured by my estimations is likely to be representative of a larger and more significant phenomenon[15]

14. Self-employed professionals were not affected by the court ruling banning corporate participation, so that one might hypothesize that the reform did not affected their signalling ability – as discussed in the next section –, nor their behaviour.

15. People involved in straw-man donations are – according at least to one story – might be workers with a history

4.5.4 Primary effects

In assessing the effect of banning corporate actors from directly participating in electoral campaigns, two levels of effectiveness should be assessed: on one hand, one needs to provide evidence that the ban has*prima facie* effectiveness, meaning corporate donations do disappear from the data-set and no clear substitution effect occurs across funding channels. On the other hand, one needs to move beyond this superficial level and try to assess whether this level of effectiveness is stable upon deeper scrutiny – this will be done in the next Session, discussing straw man donations.

As discussed in Section 5.5.1, municipal election resources come from five main sources: personal donations, party donations[16], donations from other candidates[17], private donations, and corporate donations[18]. Figure 4.1 presents the trends for the total amount of donations across all municipal races for the years 2004-2016.

From the graphical evidence, the ban appears to be effective in eliminating corporate donations, with no corporate actor remaining in the data also under other denomination[19]. This evidence also proves well-founded of some of the fears among pundits (Douglas 2015): the ban left the system lacking resources, with parties and prominent candidates incapable of redistributing resources – also indirectly coming from corporations. Overall, the policy intervention managed to substantially shrink the contribution pie. As reported in the Appendix, the number of candidates who received any form of funding

within a firm. It appears unlikely that non-vetted newcomers, though in a position of power in a firm, would be involved in illegal arrangements of the kind described. Reputation and transaction costs loom very large in this respect. Because of this, it might be interesting to look into long term or fast-rising employees that disappear from a firm organization without appearing in another one and assume they stayed on in a role that is not considered by the data-set. However, the risk of false positive attribution is extremely high and does not appear to be worth pursuing at this time.

16. These are formally registered as such donations only from the 2008 campaign onward. These donations can be detected – with a certain degree of error – also for previous years by constructing a vocabulary of donors' names.

17. Same as above.

18. In Brazil campaign PACs or similar organizations do not exist, and the law explicitly prohibits contributions from peak associations (Samuels 2001a), as well as from any other body receiving public funding. Through-party donations were also a feature of the Brazilian campaign context, these donations were produced by individuals to parties and were legally anonymized. The piece of legislation presently considered as intervention, also eliminated such aspect of the ban

19. The law is mechanically effective otherwise, because after the elections firm cannot donate and are formally bound not to appear in such graph.

diminished as a result of the reform (*See*: Figure 4.12). This is clearly the case for entrenched parties, and particularly so for the *Partido dos Trabalhadores* (PT) – the party at the centre of the scandal, that more than any other suffered from the negative shock, and for whom the number of candidates taking part in races plummeted (Figure 4.11)).[20]

From a global perspective, the ban does not appear to induce any substitution across supporting channels: lost corporate donations do not appear to be funnelled to candidates through other (legal) channels at this level of analysis. This also seems to be true for both of the offices. Subdividing the sample into population ventiles, it is possible to appreciate how heterogeneous the picture is different for municipalities in the right tail of the population distribution. Figure 4.2 reveals that corporate donations play a disproportionate role only in large and very large municipalities. Moreover, the same Figure reveals an up-tick in private donations when all the other channels are shrinking or are unaffected. This suggests the presence of marginal substitution within large municipalities.These figures demonstrate the necessity to distinguish dynamics specific to large municipalities, those attracting more candidates, parties, and resources[21].

20. This response can be explained in two ways: either PT candidates, at the time of the election, were under investigation/arrest and the party was not in the position to have them running nor had the time to substitute them; or previously PT-affiliated candidates fled the party to avoid a personal negative shock. This last hypothesis is in line with the known fluidity of party affiliation in Brazil, though it is not presently discussed.

21. Figure 4.5 and 4.6 also suggest that the results, and especially those in the last decile of the population might be driven by outliers such as the municipality of São Paulo, and other large cities present in the South-East region of Brazil, which is hardly representative of the rest of the country in terms of population, wealth, and political relevance (in the top ventile, municipalities from the South-East amounts to 49.8% of entries, despite only accounting for 29.9% of the total). Robustness checks and subgroup analyses will be devised to account for this.

	2004	2008	2012	2016
No. candidates	13,041	19,538	20,039	21,344
No. donations	113,347	399,408	413,913	433,620
No. donors	464,984,257	1,358,737,466	2,280,510,591	1,490,380,877
Avg. donation (BRL)	4,102	3,402	5,510	3,437
Max donation (BRL)	400,000	5,886,411	4,500,000	17,248,000
SD donation	11,390	24,306	31,623	30,861
Donations from self				
No. donations	23,102	49,613	61,979	72,656
No. donors	6,833	11,587	15,898	18,883
Tot. donation (BRL)	113,122,007	239,463,409	429,153,187	562,041,001
Avg. donation (BRL)	4,897	4,827	6,924	7,736
Max donation (BRL)	400,000	4,000,000	1,800,000	17,248,000
SD donation	10,954	28,629	24,504	68,148
Donations from individuals				
No. donations		247,251	240,190	310,233
No. donors		197,180	200,284	245,888
Tot. donation (BRL)	143,019,996	298,432,858	475,244,393	624,657,051
Avg. donation (BRL)	2,074	1,207	1,979	2,014
Max. donation (BRL)	400,000	536,701	800,000	1,000,000
SD donation	4,489	4,278	5,771	7,016
Donations from corporations				
No. donations	21,287	35,461	38,421	
num_donors_corp	12,414	19,088	21,971	
Tot. donation (BRL)	208,842,253	392,481,517	520,126,076	
Avg. donation (BRL)	9,811	11,068	13,538	
Max donation (BRL)	400,000	800,000	1,500,000	
SD donation	21,183	25,715	35,761	
Estimated share	0.29	0.58	0.54	0.40

Table 4.1: Descriptive statistics - Campaign contributions, Mayor

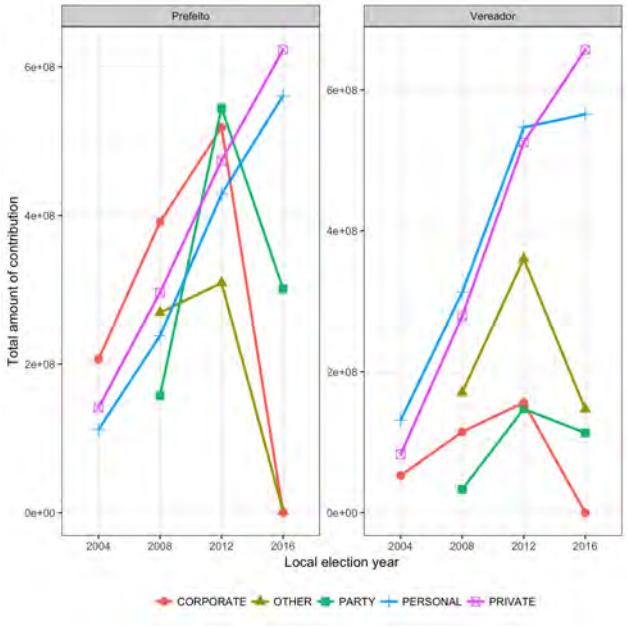


Figure 4.1: Total campaign donations, by source and office

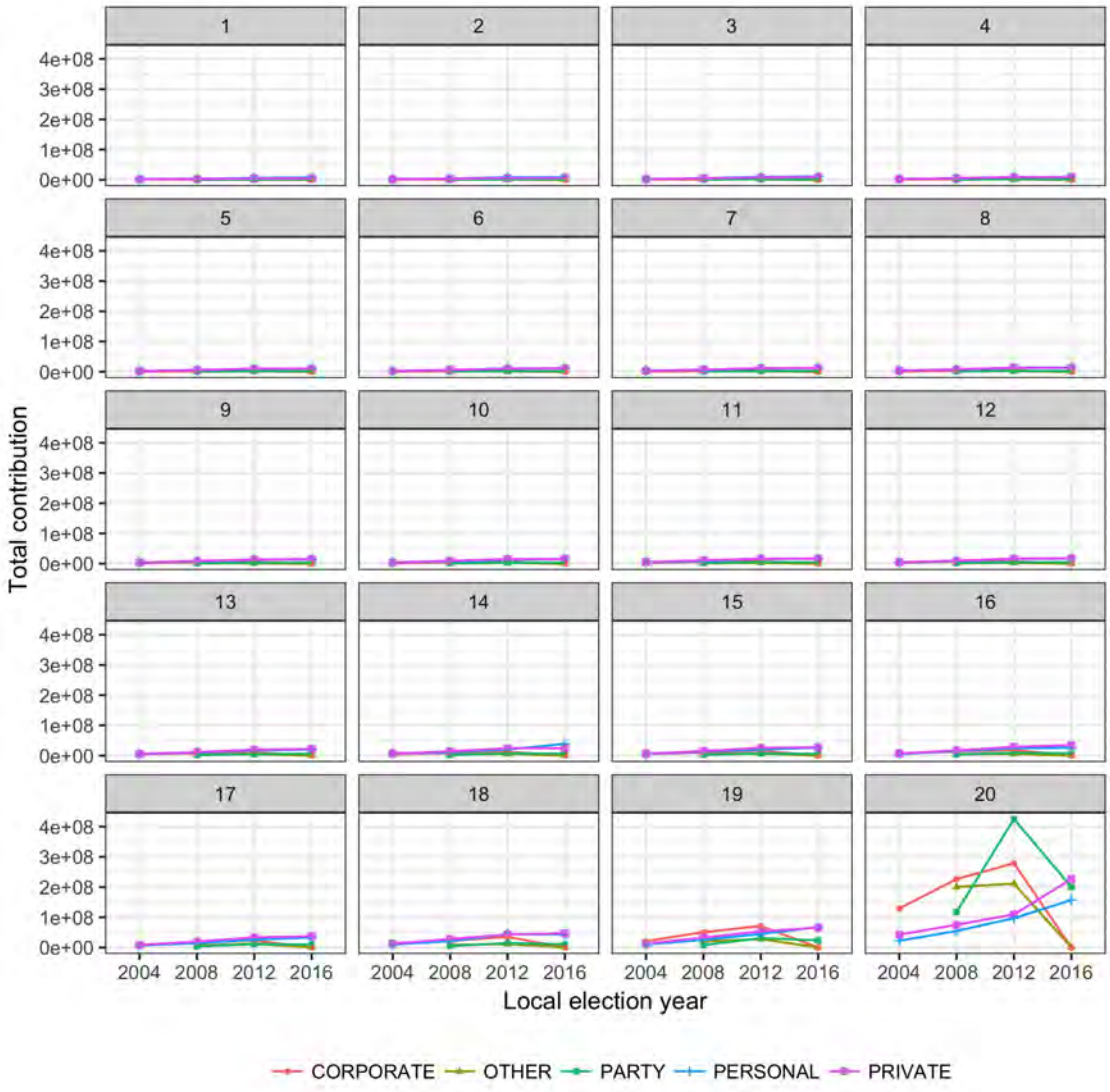


Table 4.2: Mayor - Total campaign donations, by source (population ventile)

It is important to notice that whatever up-tick is registered at the margin, is very limited in the great scheme of things. As mentioned in the Section, the ban imposed a contraction of resources available to candidates running in municipal elections[22]. At this level of analysis only visual evidence can be provided to describe the ban's effectiveness in eliminating corporate donations, as well as in preventing

22. The resource shrinking should not be underestimated. It might have substantially changed the set of incentives different donors were facing in entering the political game, as well as the modalities according to which they eventually participated. This will be discussed at length later, especially with reference to the change in the amount of resources needed to produce through donations, a detectable support signal to an individual politician.

substantial substitution effects across channels. It is however not possible to estimate a credible treatment effect: any of the non-corporate channels is equally subjected to the policy treatment and one should refrain from carrying out the analysis at this level. No control group really exists for private donations among the ones already presented – not even self-donations that might appear to be generally unaffected by the policy. A more refined solution to address the re-channeling of donations is then required; to move beyond a face-value assessment of policy effectiveness.

4.6 Straw-man donations

In this section, develop a novel approach to proxy straw-man donation and devise a methodology to identify their emergence as a locus of corruption displacement.

4.6.1 Operationalization

In discussing the effect of the 2015 campaign finance reform, the evidence provided in Section 5.5.3 only considers corporations as able to participate *directly* and openly in the political game. This overlooks corporations' ability to take part also *indirectly* in the game, as mobilizing agents: creating the conditions for third parties to participate, or altering the ways in which they participate, corporations can achieve their original campaign finance goals having other to take action on their behalf.

Presently, I want to explore specific subset of this indirect form of participation and for which the mobilizing dynamic happens within the firm itself and by means of donations orchestration. In the US context, such dynamics has been best practically discussed with respect to the Thornton Law scandal uncovered by the Boston Globe's Spotlight Team and Center for responsive politics (Estes and Novak 2017a; Estes and Novak 2017b). In 2017, a Massachusetts law firm was discovered having its employees' private donations to prominent politicians timely matched to the dollar[23] by the firm itself in bonuses.

23. This was true for a substantial part of the donations, not for the universe (**??**).

The multi-million dollar over-billing scheme remains the largest straw donor scandal in the country to date. The scheme at its minimum, involves a firm passing funds down to its employees with the sole scope of using them as figurehead private donors. A firm might want to explore this solution if, though interested in participating in the political arena, finds itself legally unable to do so, or anticipates excessive costs to directly participate – for example, during a political scandal, when displaying support for a politicians might negative affect the business' image. Brazil presents a clear set of incentives for firms to explore straw-man donation strategies of this kind, especially when the ban on corporate donations – described in Section 5.5.1 – was imposed. The concerns raised among pundits[24] further justify this exploration.

To uncover a straw donor scheme, the legal literature has recently hinted at the possible use of automated analysis of campaign finance disclosure reports (Kelner 2018), on top of routine review by law enforcement agents. Data mining offers significant advantages, especially when coupled with other red-flagging systems, in terms of speed of detection and scalability of efforts. In its basic form it rests on then identification of an 'initiator' and explores to what extent in her influence network a number of individuals – or 'conduits' – are displaying unusual contribution patterns: donating disproportionate amounts of money, given their contribution history or their job position. In extremely naive cases, conduits can be identified as by checking whether they are maxing out their contribution capacity or even hyper-coordinate donations in space and time[25]

In the context presented, it appears reasonable to focus on a firm's body of employees as its influence network, and study alterations in their donation pattern of donations produced within each firm. To do so, one needs to trace each individual donations to the corporate environment in which it was 'produced'; namely each donor needs to be matched with the firm she was working for at the

24. *See*: Section 4.4.3.

25. The investigation of Dinesh D'Souza – another famous example in the US context – appears to have used some of these flags (FBI 2014).

time of her participation in the campaign. To do so, I match the TSE private donations data-set and the RAIS data-set using employees' individual tax identification numbers. Given the coverage of the RAIS, to minimize false positives, I consider only contracts active between the beginning of September and the end of October - the window in which citizens can contribute to a local level electoral campaign. In case of multiple contracts, moreover, i keep the contract reporting the highest salary. Robustness checks are performed by selecting the contract reporting the longest hours, or produced by picking it at random[26]. On average, I am able to trace donations back to a specific firm 32.8% of the time across years – with the share of matching improving with the quality of the labour market dataset. As mentioned before, individuals unmatched are likely owners, self-employed or people working outside of the formal market – specific consideration will be given to these later in the study.

Table 4.3: Mayor - Donations matches (cleaned)

	2004	2008	2012	2016
No. private donations	68,958	247,251	240,190	310,233
No. private donations matched	20,832	64,072	72,022	139,889
share private donations matched	30.21%	25.91%	29.98%	45.09%
No. distinct corporation	15,516	50,910	67,139	93,568

Once private donations are linked to their corporate setting, it is possible for each firm to be labelled as having a 'corporate', 'private', or 'mixed' profile - according to whether historically the firm participated in the political contribution marked *only* directly, *only* indirectly, or using both channels. In my analyses, corporations with a 'corporate' profile are those firms that throughout the available election years never had employees who donated; on the other hand corporations with a 'private' profile are those that never donated as a corporation. 'Mixed' profiles are those that fall in between these

26. Available upon request

two categories[27]. For the years considered, table 4.4 illustrates in detail the different donation profiles for firms in the Brazilian electoral contribution market. Across the election years, on average, slightly more than a half of the companies have 'private' profile, slightly less than half has a 'corporate' one , and the remaining few a 'mixed' profile. The results are even more consistent if one considers only the 2008 and 2012 elections to attribute a firm to either profile[28]

The matching of individuals and corporate donations creates room for a heterogeneity in pre-treatment donation patterns to emerge. In the Section 5.7, this pre-treatment heterogeneity will be used to define a treatment and a pseudo-control group to overcome the identification challenge posed by the universal reach of the reform. The idea is that companies with a 'private' profile – so, playing a strategy that did not involve 'direct' corporate donations – should not have been affected by the corporate donation ban - the only strategy sanctioned by the policy.

Table 4.4: Donation profiles II

	2012	2008	2004
N	112,330	98,797	39,818
Private	64,660	56,163	16,249
Corporate	44,328	39,409	22,738
Mixed	3,342	3,225	831
Private (%)	57.56	56.85	40.81
Corporate(%)	39.46	39.89	57.10
Mixed (%)	2.98	3.26	2.09

27. Refer to Table 4.9 in the Appendix for an overlook of different donation profiles – were different options are explored in terms of definition (consider less years, to be ascribed to a profile, be less categorical with the definitions, etc.).

28. As it has been mentioned elsewhere, the quality of data prior to 2008 is significantly lower, and all the analyses will be conducted both including and excluding said data as a robustness check.

The construction of these profiles helps one understand how donors react to the ban, going beyond superficial evaluation. Figures 4.2 and 4.3 present contributions from employees whose firm has been attributed to one of the three profiles[29]. As expected, donations coming from 'private' firms are not affected by the introduction of the ban. On the other hand, 'mixed' and 'corporate' profiles display - respectively - a negative and a positive deviation from their trajectory.

Before focusing on the 'private' and 'mixed' profiles only, it is important to underline how the ban clearly induced a contribution strategy change in some companies: private donations from firms with a history of only 'corporate' contributions should have remained at 0, whatever change in the regulation. Instead, the graph shows an up-tick of circa 9000 private donations – amounting to more than BRL 33m – for the corporate profile. This involvement remains a change at the margin, covering only 1.66% of the count of donations. Nevertheless, it remains among the tangible unforeseen effects of the ban. In all the analyses that follow, donations coming from 'corporate' profile will not be considered.

To study straw donor schemes, one needs to move beyond generic consideration based on volume of donations: simply looking at donation amounts one cannot disentangle patterns of genuine employment engagement from an orchestrated one. In this, the overall pattern of donation is crucial, and should be central to the analysis of the phenomenon. To pinpoint the use of straw man donation, one must look into the characteristics of a firm's donations as a whole. In particular, one should try to get a sense of the extent to which donations are orchestrated in a firm, towards the goal of producing a support signal for the recipient politician. Not all signals are equal: coordinating a costly signal is the key – one can argue – to keep intact the spoils system connecting politicians and firm. Section 5.6.2 sheds

29. In the Appendix, Table 4.14 reproduce the analyses though excluding from the analysis the private donations coming from the public administration. Donations from within the public sector can be dropped from the analyses as they are not openly responding to the investment dynamics that guides the rest of the firms in the market, even if this remains open to discussion.

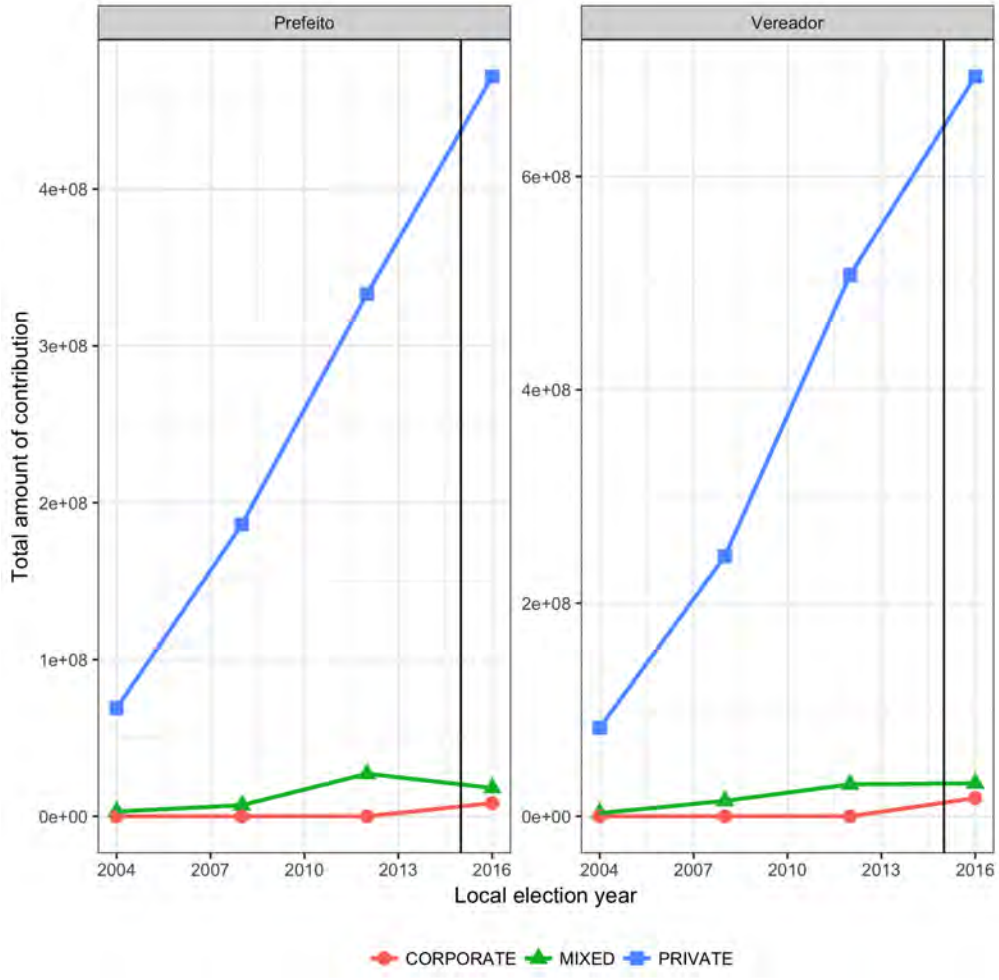


Figure 4.2: Total donations by donation profile

light on how to tackle these issues and effectively measure possible secondary effects of the reform.

4.6.2 Measurement

As discussed in Sections 5.6.1 and 5.6.2, devising a credible methodology that enables a researcher to distinguish straw-man donations from simple, private ones is crucial to provide evidence of campaign regulation fraud and, thus, to evaluate whether campaign finance reforms might have generated corruption displacement dynamics of this kind.

If a firm wants to enter into a profitable *do ut des* relation with a politician, it needs to produce a suffi-

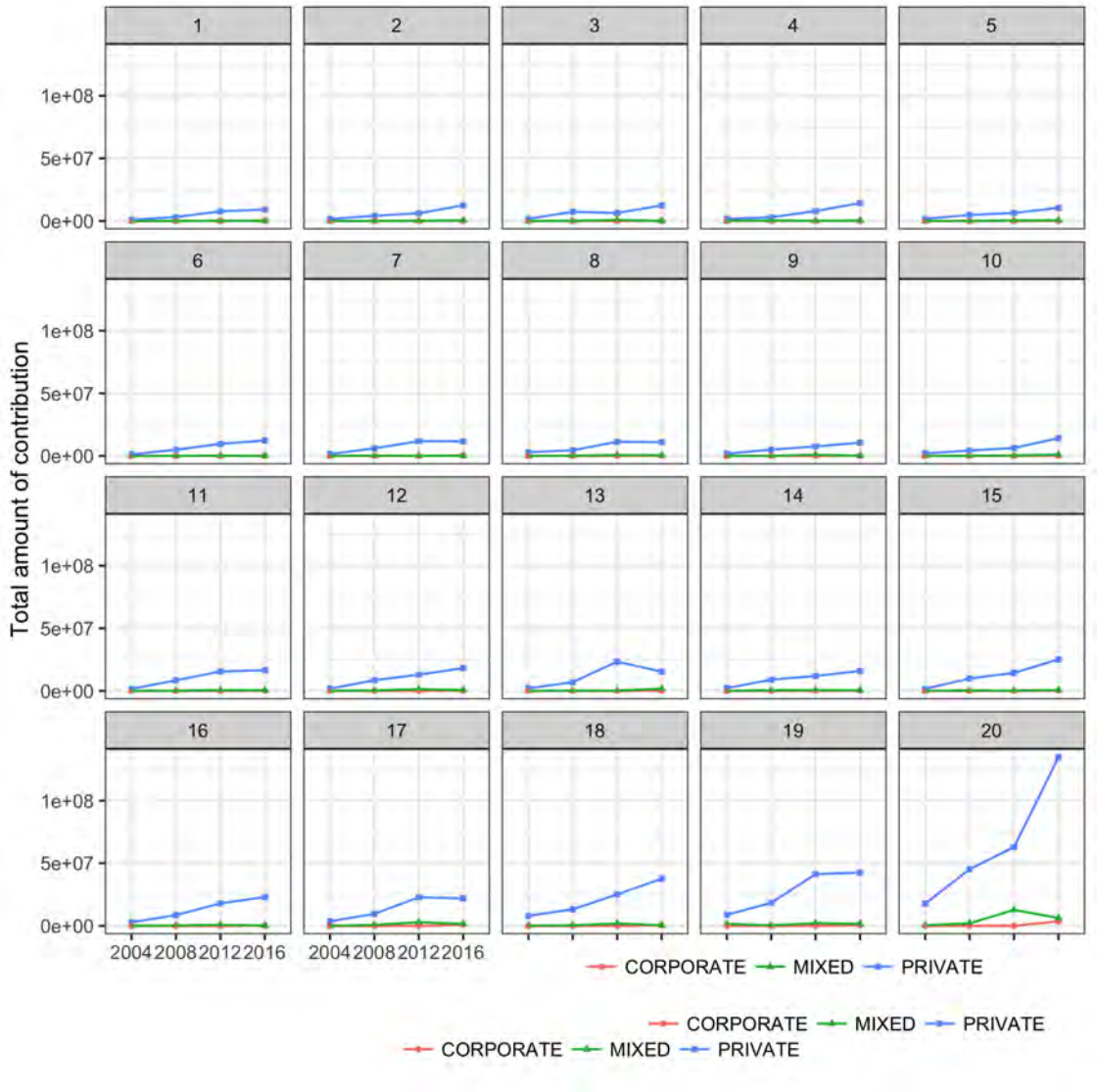


Figure 4.3: Total donations by donation profile (by population ventile)

ciently strong and clear signal of its political support through its donations - that is to say, it must make money 'talk' (McMenamin 2013). In general, the size of a firm's contributions achieve the signaling job just fine (Boas, Hidalgo, and Richardson 2014). However, changes in the campaign finance regulation may force firms to adopt different strategies to achieve the same goal under a new set of constraints. Universal ban on corporate donations is at the extreme of the possible constraints. A straw donor scheme has the potential to achieve exactly that, under certain conditions.

Simply dividing up a large corporate donation into smaller ones to be paid by employees – granted their willingness to do so – solves the issue of conforming to the regulation, but not per se the signaling one. To *de facto* conserve the status-quo, a firm needs to produce a sufficiently strong signal, so that whomever receives it is capable of decode its message[30], to re-aggregate a bunch of sounds into speech.

Such signal, coming form a careful orchestration of donations, and lost in the extreme noise of small donations, can be detected approaching donation data systematically.

At a minimum, donations coordinated to produce a political signal by firm (noise included) should result more 'concentrated' than random donations, across and within candidates. Firms are likely allocate resources on one candidate among the contenders – the one they would have otherwise directly financially supported. This allocation would produce more concentrated donations for that candidate, and across candidates, than random allocation would have.Grasping this pattern is key to figure out which companies might have to proactively produce a signal, and it is the basis for straw-man donations here developed.

To do so, I adapt the Campaign Contribution Concentration Index (CCCI) described in Dharmapala and Palda 2002, modeled on the classic Herfindhal-Hirschman Index (Hirschman 1964). I develop two different indexes to try to gauge the two concentrations dynamics just explained: a Within Candidate Coordination Index (WCC), and an Across Candidates Coordination Index (ACC). The measure clearly does not allow one to qualify individual donations as the product of illegal exchange – as only direct prosecution could –, however it allows one to uncover broad donation patterns that reveal heterogeneous behaviour across groups affected by the policy, and grasps with no fail the emergence of orchestration practices in the within-firm marketplace. Though the measure remains an imperfect proxy for actual straw-man donations, it is nonetheless a solid way to start the use of such an elusive practice. A description of the indexes follows.

30. This is particularly true in contexts where small donations are a staple of electoral campaign, as in the setting (Zovatto 2003).

Within candidate contributions concentration index (WCC_{cf}).

$$WCC_{cf} = \sum_{i=1}^{n} \left(\frac{C_{icf}}{\sum_{j=1}^{n} C_{jcf}}\right)^2 \tag{4.1}$$

Where, C_{icf} is a private (physical person's) contribution made to candidate c by an individual working for a firm f; n is the number of donations a candidate c receives from firm f. $WCC_{cf} \in [0, 1]$.

Across candidate contributions concentration index (ACC_f).

$$ACC_f = \sum_{c=1}^{m} \left(\frac{\sum_{i=1}^{n} C_{icf}}{\sum_{d=1}^{m} \sum_{j=1}^{n} C_{jdf}}\right)^2 \tag{4.2}$$

Where C_{icf} is a private (physical person's) contribution made to a candidate c by an individual working for firm f; n is the number of donations candidate c receives from firm f; m is the number of candidates to whom employees of the firm f give a contribution. $ACC_f \in (0, 1]$

In isolation, the indexes help characterize different results of the financing activities a firms engages into through its employees, intentionally or not. Despite the attractive straightforwardness of the indexes, some warnings are necessary: (i) index values carry little meaning in absolute terms: high scores can be reached by simply having few employee-donors, up to degenerate cases of only one donation for whom the index always takes the value of 1[31]; (ii) donations should be sector- and region-specific, as the required strength of the signal produced via a donation depends on the strength of the signal of all the competing firms (as signal-producers), and is to be controlled for accordingly; and (iii) it might be useful to consider the indexes described in combination, especially when trying to identify individual indirect donations, such as straw-man donations – though how to practically do so remains to be studied and in not presently explored.

Discussing how indexes' levels and the magnitude of donations affect the strength that a donation pro-

31. Robustness checks are produced including and excluding these degenerate cases.

duces it is not straightforward. Tables 4.9 to 4.10 present an overview of index density according to the donation portfolio of a firm, focusing on the number of donors. Donations by straw-man are likely those donations that display a high index despite a large number of donations, or precisely where donations are numerous and the mechanics of the calculation would otherwise produce a low index. Focusing on general orchestration dynamics, this aspect is left to a specific inquiry to come.

To use campaign finance concentration indexes to gauge a phenomenon of this kind presents many risks, however, such a methodology remains at the same time extremely promising in terms of automating the analysis of campaign donation in terms of pattern search. This could very much be the starting point of any inquiry into straw donor schemes. Highly concentrated donations produce by large donor sets can be red-flagged, no matter what the amount of money involved is. This instrument can be pivotal in a next phase of wrongdoing detection.

4.7 Empirical Strategy

In the present section, I analyze how the policy intervention affected within- and across-candidates concentration index, using a standard causal identification strategy whose application is reasonable based on the discussion on treated and pseudo-control group carried out previously. These results reveal insightful with respect to the corruption displacement dynamics triggered by the electoral campaign reform.

4.7.1 Identification strategy

Relying on the discussion in Sections 5.6.1 and 5.6.2, the effect of the corporate donation ban is explored by assessing how firms' contribution concentration indexes change, comparing firms with and without a pre-treatment history of corporate donations. Indexes are generated using private dona-

tions only, as they are the sole possible vector of straw-man donations as presently discussed. Changes in concentration indexes across groups should reveal how the introduction of a corporate ban changed corporations' strategies for political participation – formally, if not substantively. To measure the average treatment effect, I estimate a Difference-in-Differences model, with lags of the treatment and Fixed Effects.

$$Y_{ist} = \gamma_s + \lambda_t + \delta_j \sum_{j=-3}^{0} (Treat_{is} * Post_{it+j}) + \epsilon_{ist} \tag{4.3}$$

Where i indexes a firm, s its treatment status, and t an election year. Y is either the WCC index or the ACC one. $Treat_{is}$ is an indicator taking the value of 1 for treated units; $Post_{it}$ an indicator taking the value of 1 in the post-reform period; δ_j is the coefficient of interest and captures the effect of being a firm with an affected portfolio in the election year $t + j$ relative to the introduction of the reform. Leads are accounted for to formally support the parallel trend assumption, as a complement to graphical evidence. γ_s indicates State Fixed Effects, and λ_t time Fixed Effects. among the robustness checks a vector of fixed effects are introduced, to control for both firm, race and municipal characteristics. The introduction of Fixed Effects is justified by the necessity to control for idiosyncratic trends that might affect contribution practices especially across an extremely varied economy such as the one of Brazil.

In the estimation, each firm enters with a unique, year-specific, value that accounts for a firm's donation structure and move away from possible criticisms regarding the undue influence of degenerate indexes.

Regarding the within-candidate concentration index (WCC), the present analysis considers two weighting scheme for the multiple candidate- and firm- specific WCCs, where the weights are respectively determined by the number of donations given in a race to a candidate (WCC 1), or the amount of donations given in the same race to the same candidate (WCC 2).Some firms, through their employees donations, appear to donate in multiple races. The two index are therefore further weighted using a

similar scheme (number of donations, or value of donations) across races when the case arises[32].

Regarding across-candidate concentration index (ACC), the present analysis considers three specification, each firm- and year-specific. The indexes are calculated as weighted means of race-specific ACCs, where the weights are determined, respectively, by the number of donations (ACC 1), the number of candidates (ACC 2) and the overall amount of donations given in a race (ACC 3). The same caveat as before is valid. Results are substantially robust across specifications. Results and discussion are reported in the Appendix.

In the analyses, donations under the level of BRL 100 are dropped because of noise reduction. Results are robust to the inclusion of these donations, at the only cost of reduced precision in the estimates.

In Table 4.7.1 and 4.7.1 are reported descriptive statistics that highlight how the two groups are not substantially different from each other despite the great unbalance in respective market-share. A more refined version of the paper will call for the adoption of a matching strategy on pre-treatment characteristics, especially sector and engagement ability in the donation market. For the time being, given the exploratory nature of the piece, this is not yet done.

32. Between 80-85% of firms support candidates in one race only, so that the across races weighting is of little impact. Robustness checks would see the definition of a primary race per firm, based on historic data, to eliminate the necessity of such a scheme.

Table 4.5: Pre-Treatment Summary Statistics

(1)

	Mixed profiles					Private profiles				
	mean	sd	min	max	count	mean	sd	min	max	count
WCC (1)	0.874	0.233	0.004	1.000	4631	0.824	0.269	0.007	1.000	36000
WCC (2)	0.883	0.223	0.004	1.000	4631	0.846	0.245	0.007	1.000	36000
ACC (1)	0.994	0.045	0.500	1.000	4631	0.976	0.091	0.224	1.000	36000
ACC (2)	0.994	0.045	0.500	1.000	4631	0.977	0.088	0.224	1.000	36000
ACC (3)	0.994	0.046	0.500	1.000	4631	0.977	0.090	0.224	1.000	36000
Donations (BRL)	0.009	0.075	0.000	2.401	4631	0.015	0.098	0.000	5.188	36006
No. donations	2.498	9.637	1.000	303.000	4631	3.742	16.164	1.000	1670.000	36006
No. candidates	1.536	4.718	1.000	161.000	4631	1.791	6.921	1.000	635.000	36006
Salary (BRL)	1.852	2.383	0.000	40.115	4631	1.932	2.531	0.000	65.032	36006
Age	39.439	8.695	16.000	73.000	4631	41.228	8.585	13.000	88.000	36006
White, share	0.593	0.440	0.000	1.000	4614	0.648	0.447	0.000	1.000	26228
Male, share	0.801	0.341	0.000	1.000	4631	0.693	0.370	0.000	1.000	36006
Employment	39.439	8.695	16.000	73.000	4631	41.228	8.585	13.000	88.000	36006
No. elections	1.612	0.863	1.000	4.000	4631	1.990	1.109	1.000	4.000	36006
Race candidates	4.727	2.661	1.000	14.000	4338	4.184	2.458	1.000	14.000	33822
Margin of victory	0.162	0.161	0.000	0.985	4558	0.161	0.161	-0.029	0.998	35129
Population in 2010	875.636	2268.819	0.805	11253.503	4631	628.188	1843.952	0.805	11253.503	36006
HDI	0.771	0.065	0.513	0.919	4627	0.753	0.078	0.467	0.919	35886
Dist. capital (Km)	171.296	173.423	0.000	1161.143	4627	186.412	170.161	0.000	1476.277	35886

Note: Population and monetary values are reported in thousands units. Employment measures the number of months an employee worked in a firm at the time of the donation.

Table 4.6: Pre-Treatment Summary Statistics - Sectors (BRL)

	(1)									
	mixed					private				
	mean	sd	min	max	count	mean	sd	min	max	count
Agriculture, Livestock	0.011	0.052	0.000	0.414	134	0.005	0.027	0.000	0.551	1461
Fishery	0.009	.	0.009	0.009	1	0.003	0.005	0.000	0.015	13
Extraction Industry	0.003	0.005	0.000	0.041	87	0.021	0.102	0.000	0.852	174
Transformation Industry	0.009	0.084	0.000	2.215	1074	0.004	0.031	0.000	0.687	2712
Electricity, Gas, Water	0.007	0.033	0.000	0.342	124	0.018	0.091	0.000	1.771	682
Construction	0.006	0.024	0.000	0.316	514	0.004	0.017	0.000	0.307	1064
Commerce	0.006	0.039	0.000	0.733	1290	0.004	0.028	0.000	0.799	5285
Food and Housing	0.010	0.031	0.000	0.152	68	0.003	0.014	0.000	0.198	680
Transport and Telecom	0.006	0.029	0.000	0.386	290	0.004	0.026	0.000	0.622	1356
Financial services	0.117	0.383	0.000	2.401	74	0.009	0.076	0.000	1.317	308
Real Estate services	0.007	0.050	0.000	0.868	582	0.006	0.046	0.000	1.316	2609
PA and Defense	0.057	0.134	0.000	0.535	30	0.028	0.146	0.000	5.188	14097
Education	0.020	0.103	0.000	1.173	145	0.013	0.069	0.000	1.885	2101
Health	0.003	0.006	0.000	0.030	64	0.009	0.061	0.000	1.290	1177
Social services	0.005	0.034	0.000	0.411	154	0.006	0.039	0.000	1.051	2281
Domestic Services	.	.	.	.	0	0.001	0.001	0.000	0.002	2
IOs	.	.	.	.	0	0.005	0.006	0.002	0.014	4
Observations	40637									

Note: Monetary values are reported in thousands units.

4.7.2 Graphical evidence

Figures 4.4a and 4.4b present the Difference-in-Differences parallel trend graph for the WCC and ACC indexes, as they appear according to the preferred specification – WCC 1 and ACC 1 –, as discussed in Section 5.7.1. The graphs are reported to visually demonstrate pre-treatment parallel trends assumption. This can be also checked in the Model 1 of Table 4.7 and 4.8, where the leads are included with that specific goal in mind. In the Tables, the lagged interaction despite being reported as statistically significant are substantial[33] irrelevant, amounting, respectively, to 5% and 3% of a SD, when the main

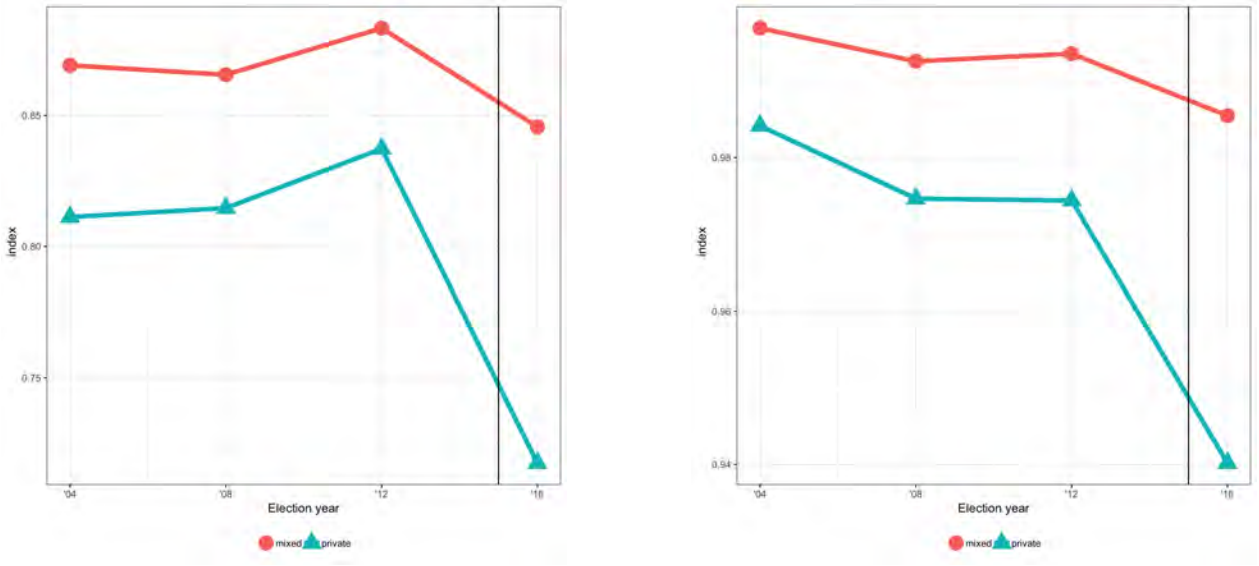
33. Please refer to the discussion on how to interpret statistical significance in extremely large dataset in the next paragraph.

effect amounts to 16% of a SD – for the WCC – , and to 14% and 11% of a SD, when the main effect amounts to 72% of a SD – for the ACC.

Both Figure 4.4a and 4.4b shows how companies with a 'private' profile, after the reform display substantially lower levels of concentration in their donations, an a drop for post-scandal election. This is justified with reference to the post-scandal political scenario in Brazil – described in Section 5.4. The scenarios was one in which many (relatively) prominent politicians disappeared from the political arena because of their involvement in the corruption scandal. To make things worse, the parties more involved in the scandal – PT and PTB – went through mayor party organisation disruption that translated into them struggling[34] to have mayors and city council candidates running in the first place (**??**). Before the scandals, these reference politicians were potentially focal points for donations by employees from 'private' firms. Their presence had kept high the concentration index of a firm despite the lack of any proactive effort to orchestrate donations. The deep change in the political scenario, with the (self-)removal of these figures left to distribute their donations evenly across candidates – with the consequent drop in concentration indexes across and within candidates.

On the other hand, companies with a 'mixed' profile, on average, managed to maintain a significantly stable level on both indexes in the face of a political scenario altered in the way described. Firms were able to maintain unaltered support signaling abilities after the reform. This ability can be attributed to donations orchestration – as both kind of companies were facing the same abrupt changes in the political landscape, and private donations produced within the companies with a 'mixed' profile should have otherwise responded similarly to their counterparts . To what extent this ability can be pinpointed to new coordinating schemes in the form of straw man donation, remains to be proven. However, the measure devised provides an interesting insight in the coordination ability of firms in terms of political signaling.

34. PT candidates across the election shrunk by -47,19%. *See*: Table 4.11



(a) WCC (b) ACC

Figure 4.4: Concentration Indexes, preferred specification

4.7.3 Estimation

Moving into a more formal analyses, it is worth remarking that given the number of observations is very large, the standard errors of estimates become mechanically extremely small. On the one hand, this means that the model used has power to detect even very small changes, which is useful given the nature of the present dependent variable; on the other, however, *t*-statistic numerators are so inflated that even very small changes are deemed statistically significant even when no practical significance is there to be detected. This presents a trade-off that must be addressed with a pinch of salt[35]: the discussion on the Difference-in-Differences interaction coefficients is carried out looking primarily at the magnitude of coefficients, and in the present case in terms of standard deviation. The estimation is consistent with the graphical analysis of Section 5.7.2, and substantially robust to the inclusion of State and Sector Fixed Effects. Focusing, on the general interaction therm for the WCC index this,

35. Their statistical significance – partially void of meaning with respect to more classical analyses – is assessed by means of considering series of ever smaller random re-samples to see up to what point the estimations remain significant. These are available upon request.

registers – in the most complete specification – a value of 0.0202*** (0.00730), which in substantive terms, account for a 16% of a standard deviation; whereas the ACC registers at value of 0.0150*** (0.00221) and amounting to a 72% of a standard deviation. By design index values are hard to make sense by themselves and disregarding the overall pictures that they give. Nonetheless, the results appear consistent with the data presented and the anecdotal evidence discussed. In the same specification, it is worth underline the impact of municipality development (HDI) in accounting for the base line value of the indexes. This is not surprising as mode developed contexts are also those in which competition is higher and participation broader. The same idea is conveyed looking at the margin of victory of the village in which firm are located and towards whose municipal race mainly donates. In these scenarios, highly concentrated indexes are hard to achieve the first place, despite results are consistent accounting for them. The general results appear worth of further enquiries.

VARIABLES	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4	(5) Model 5
Treat = 1	0.0580***	-0.0387***	-0.0393***	-0.0354***	-0.0356***
	(0.0107)	(0.00385)	(0.00386)	(0.00372)	(0.00375)
Post = 1	-0.0938***	-0.0669***	-0.0666***	-0.0491***	-0.0436***
	(0.00457)	(0.00285)	(0.00285)	(0.00285)	(0.00297)
1.treated#1.post	0.0703***	0.0413***	0.0421***	0.0206***	0.0202***
	(0.0125)	(0.00746)	(0.00747)	(0.00720)	(0.00730)
Donations (BRL)				0.0693*	0.0643*
				(0.0356)	(0.0349)
No. donations				-0.00854***	-0.00838***
				(0.000825)	(0.000820)
No. candidates				0.0170***	0.0166***
				(0.00171)	(0.00174)
Margin of victory					0.0209***
					(0.00622)
Salary (BRL)					-0.00423***
					(0.000458)
Popuation					0.00102*
					(0.000613)
HDI					0.282***
					(0.0336)
Dist. capital (Km)					-8.51e-06
					(8.11e-06)
Urban, share					-0.0135
					(0.00867)
Post 2 = 1	0.0261***				
	(0.00417)				
1.treated#1.post_2	-0.0119				
	(0.0118)				
Post 3 = 1	0.00342				
	(0.00425)				
1.treated#1.post_3	-0.00700				
	(0.0120)				
Constant	0.811***	0.917***	0.917***	0.901***	0.713***
	(0.00386)	(0.00472)	(0.0125)	(0.0116)	(0.0244)
Observations	54,122	54,122	54,122	54,122	52,619
Number of firm_id	35,473	35,473	35,473	35,473	34,770
Sector FE		YES	YES	YES	YES
State FE			YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
SE Clustered at firm level

Table 4.7: Difference-in-Difference - WCC, preferred specification

VARIABLES	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4	(5) Model 5
Treat = 1	0.0127***	-0.00435***	-0.00405***	-0.00347***	-0.00354***
	(0.00163)	(0.000745)	(0.000755)	(0.000761)	(0.000779)
Post = 1	-0.0440***	-0.0257***	-0.0258***	-0.0240***	-0.0253***
	(0.00156)	(0.00122)	(0.00122)	(0.00124)	(0.00130)
1.treated#1.post	0.0325***	0.0181***	0.0178***	0.0160***	0.0150***
	(0.00267)	(0.00218)	(0.00218)	(0.00218)	(0.00221)
Donations (BRL)				0.0441***	0.0402***
				(0.00918)	(0.00946)
No. donations				-0.000815***	-0.000784***
				(9.16e-05)	(9.09e-05)
No. candidates				0.000571*	0.000364
				(0.000293)	(0.000353)
Margin of victory					0.0168***
					(0.00244)
Salary (BRL)					0.000357***
					(0.000120)
Popuation					0.00104***
					(0.000172)
HDI					0.136***
					(0.0139)
Dist. capital (Km)					-1.34e-06
					(3.54e-06)
Urban, share					-0.00677*
					(0.00363)
Post 2 = 1	-0.00976***				
	(0.00123)				
1.treated#1.post_2	0.00643***				
	(0.00205)				
Post 3 = 1	-0.00952***				
	(0.00124)				
1.treated#1.post_3	0.00520**				
	(0.00208)				
Constant	0.984***	0.995***	0.983***	0.983***	0.888***
	(0.00102)	(0.00150)	(0.00468)	(0.00460)	(0.00987)
Observations	54,122	54,122	54,122	54,122	52,619
Number of firm_id	35,473	35,473	35,473	35,473	34,770
Sector FE		YES	YES	YES	YES
State FE			YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
SE Clustered at firm level

Table 4.8: Difference-in-Difference - ACC, preferred specification

Overall, the estimation reveals an untold ability by firms already active in the donation market ('mixed' profiles) to produce private donations in a more coordinated fashion that their counterpart ('private' profiles). Donations that display such features can be pivotal in a busy political speech arena where having one's voice heard can easily translate in economic returns. The effectiveness of concentrated signal to generate returns is left to future examinations.

The results presented here reveal to a point the ineffectiveness of the policy intervention aimed a containing the corporate influence in politics. Through donation orchestration corporate actors appeared to have managed to maintain to an extent the upper hand within the campaign donation market with respect other firms. In the sea of private donations the ones produced by coordination stand out and can be used as non-generic prove of commitment and support of a firm – not only of its employees – towards a politicians. This might have opened a channel of communication between the two, leading to exchanges corrupt in nature and scope. This is done whilst firms *de jure* complied with the new restrictive regulatory environment, and without considering the wealth of illegal channels otherwise open to both actors to engage in the same practice. The electoral reform and its corporate donation ban could become even less effective and meaningful. Donation patterns of this kind cannot be openly uncovered by the public who lack easy access to detailed data, as a result, the practical consequence of the ban is a clear reduction in transparency with no overt gain in accountability.

4.8 Conclusion

The paper studies straw donor schemes, a strategy corporations use to indirectly contribute to political campaigns when banned from directly doing so. Empirically, I explore the issue using the context of Brazilian local government elections, where a relevant reform was implemented. I conceive straw donors schemes as a form of corruption displacement and I raise the questions on the ultimate effectiveness of all-encompassing reforms in corporate financing.

First, I analyze donation portfolios by firms matching individual donations records and labour market

information to link each donor to a specific firm. Then, I divided companies in treated and pseudo-control groups leveraging their pre-treatment heterogeneous donation pattern and solely focusing on private donations – key in identify the effect of a universal corporate donation ban.

Second, I constructed a measure of donations orchestration within firms, using and adapted version of the the Herfindahl-Hirschman Index. The measure is minimal, scalable and exportable.

Third, using a Difference-in-Differences identification strategy, I show how firms with a history of direct corporate donations are able to maintain high donation coordination, thus signaling ability, in the face of a disrupted political scenario. Given information asymmetries, the ability to produce clear and decodable signals is not irrelevant, especially on a large scale and when return to investment is expected.

Overall, straw-man donations are a phenomenon difficult to observe, trace, and account for. They are a cunning answers to abrupt changes in campaign finance law, emerging in contexts already negatively affected by a dysfunctional relation between corporation and politicians. The study of the phenomenon deserve more attention – across settings – from academics and practitioners alike.

This piece advocates for a closer consideration of the phenomenon and produce usable instruments and strategies to account for it. The use of data mining, coupled with solid identification strategy, appears an ideal instrument to better design interventions, to contain corruption displacement, and to finally question the value of all-encompassing interventions usually ultimately detrimental to transparency and electoral accountability.

4.9 Appendix

4.9.1 Geographical specification

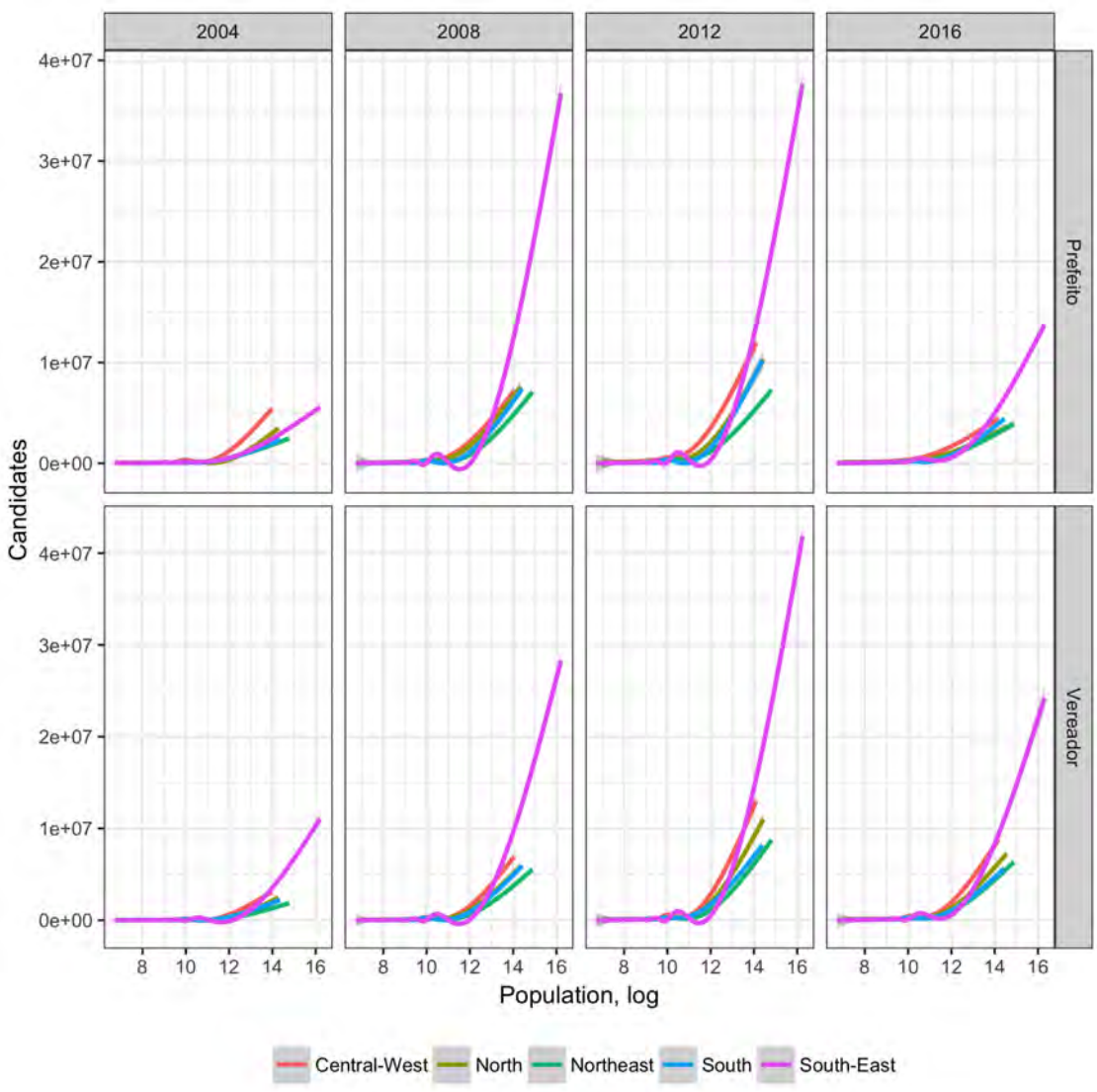


Figure 4.5: Contribution by population

Figure 4.6: Mayor contributions, by municipality

4.9.2 Donation Profiles

Table 4.9: Donation profiles

	Option I	*Option II*	*Option III*
N	112,330	186,178	211,111
Private	64,660	103,164	109,769
Corporate	44,328	74,819	90,907
Mixed	3,342	8,195	10,435
Private (%)	57.56	55.41	52.00
Corporate (%)	39.46	40.19	43.06
Mixed (%)	2.98	4.40	4.94
Matches 2016	23,766	30,033	31,357
Matches 2016 (%)	20.70	26.15	27.31

These profiles are used in the identification strategy to track how banning one of these profiles actually affects some measures of coordination. Profiles are assessed *before* the introduction of the ban. There are three different options to attribute a firm to specific profile: *Option I* - using the firm profile in the election prior to the ban (in this case 2012); *Option II* - using a constructed profile that considers two elections prior to the ban (in this case 2012 *and* 2008); *Option III* - using all three elections prior to the ban (in this case going back to election year 2004)[36]. Going back to consider more than one election to assign a firm to a participation profile, the probability that a firm is assigned a 'mixed' profile over one of the other two increases. This follows by construction, as any firm that displays an inconsistent behaviour over time is labelled thus. This has the potential to (marginally) muddle the profile assignment, for example assigning a 'mixed' profile to an otherwise 'corporate' player because back in time

36. 'Pure' profiles were also considered. In that case, a firm was attributed to a specific profile only in the case where it was absolutely consistent throughout the elections in its pattern of donations. The specification was, however, extremely demanding on the data, driving the number of companies in all profiles but the 'private' one down substantially.

one of its employees donated even a little amount of money[37]. However, considering more than one year to define a firm's profile has the advantage to increase the sample of firms one is able to assign to groups in the post-treatment election. This gain is potentially irrelevant: a significant number of firms - given the time-span between elections (let alone more than two elections) - go through a complete life-cycle and disappear from the economy[38]. However, it might also be the case that changes in political landscape over a long period of time might discourage some companies to participate in the political game whilst attracting others to it, in a cyclical fashion. If this is the case, considering a longer period of time might be more informative. Table 4.9 presents synthetically the company profile options alongside the number of matches they are capable to produce in the 2016 post-treatment election. Presently, analyses will be conducted with the *Option II* specification. Using this specification, a firm having a 'private' profile has not engaged in donations as a company ('direct' donations) *both* in 2012 and 2008 elections, a firm having a 'corporate' profile has not seen its employees engaged in donations('indirect' donations) in the same elections. All other have experienced a mix of the two at least once, or being inconsistent in its participation behaviour.

37. This will be mitigated by excluding small donations in the robustness checks

38. According to the SDBS Business Demography Indicators, the 3-year survival rate of Brazilian firms is 60.9% (data for 2015). Data are made available by the OECD and can be accessed at `https://stats.oecd.org/Index.aspx?data-setCode=SDBS_BDI_ISIC4`

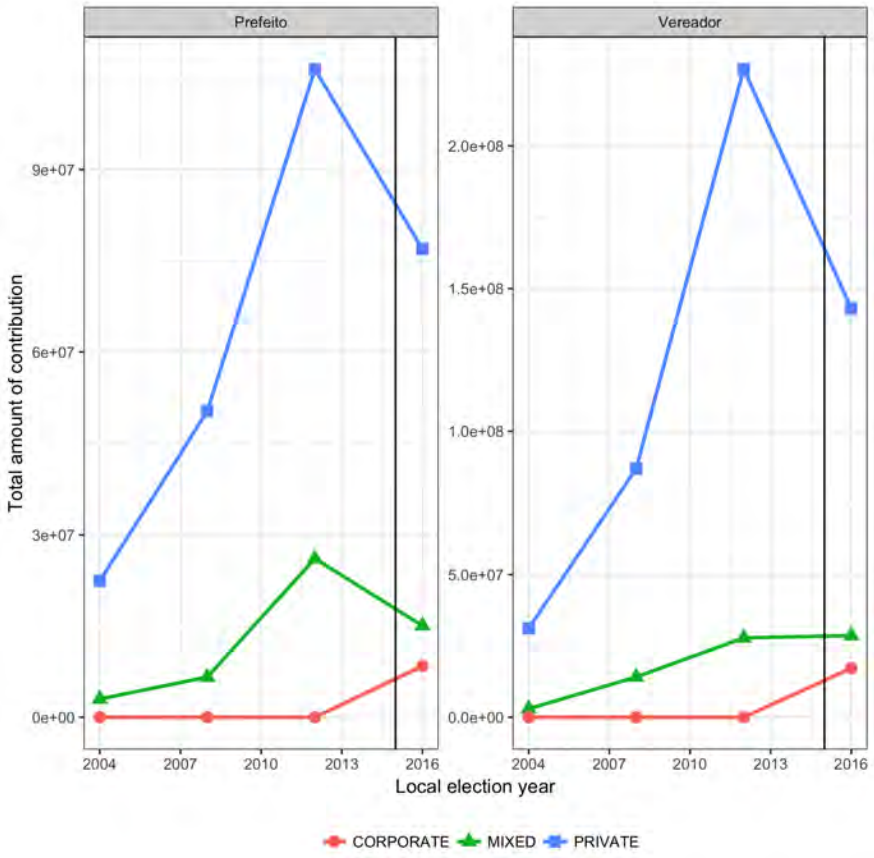


Figure 4.7: Total donations by donation profile - No Public Administration

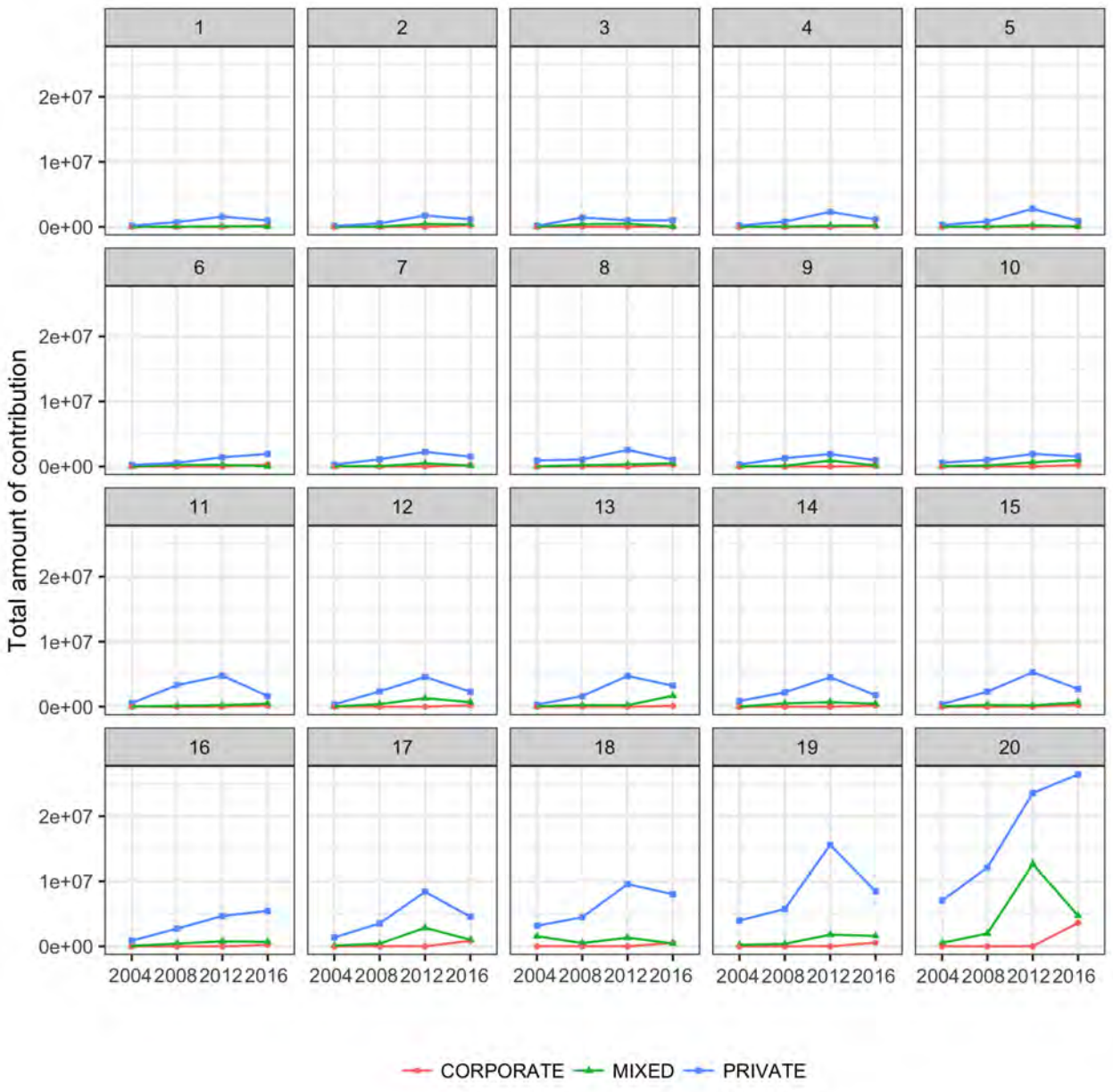


Figure 4.8: Total donations by donation profile (by population ventiles) - No Public Administration

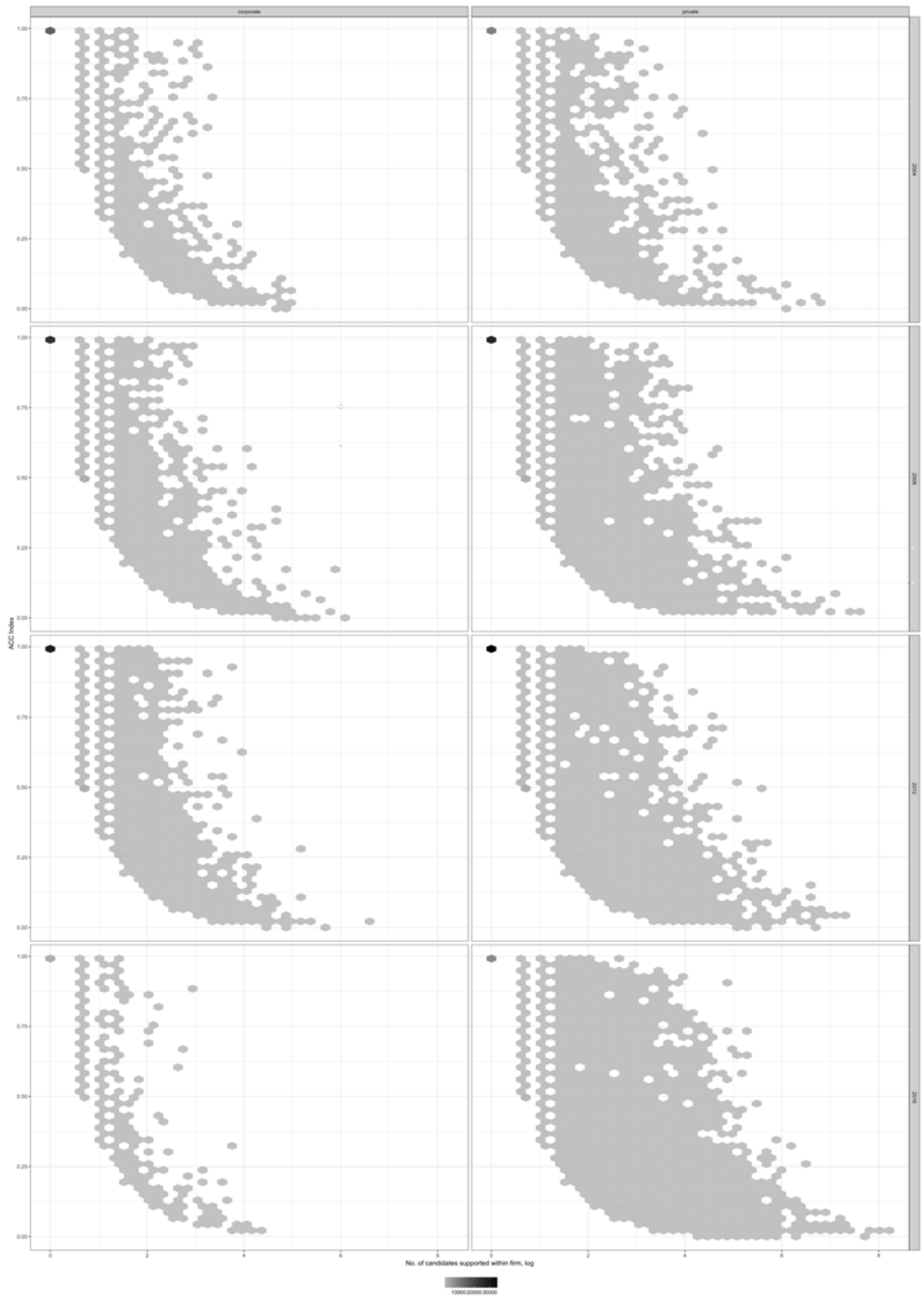


Figure 4.9: ACC - Hexbin, >100 BRL

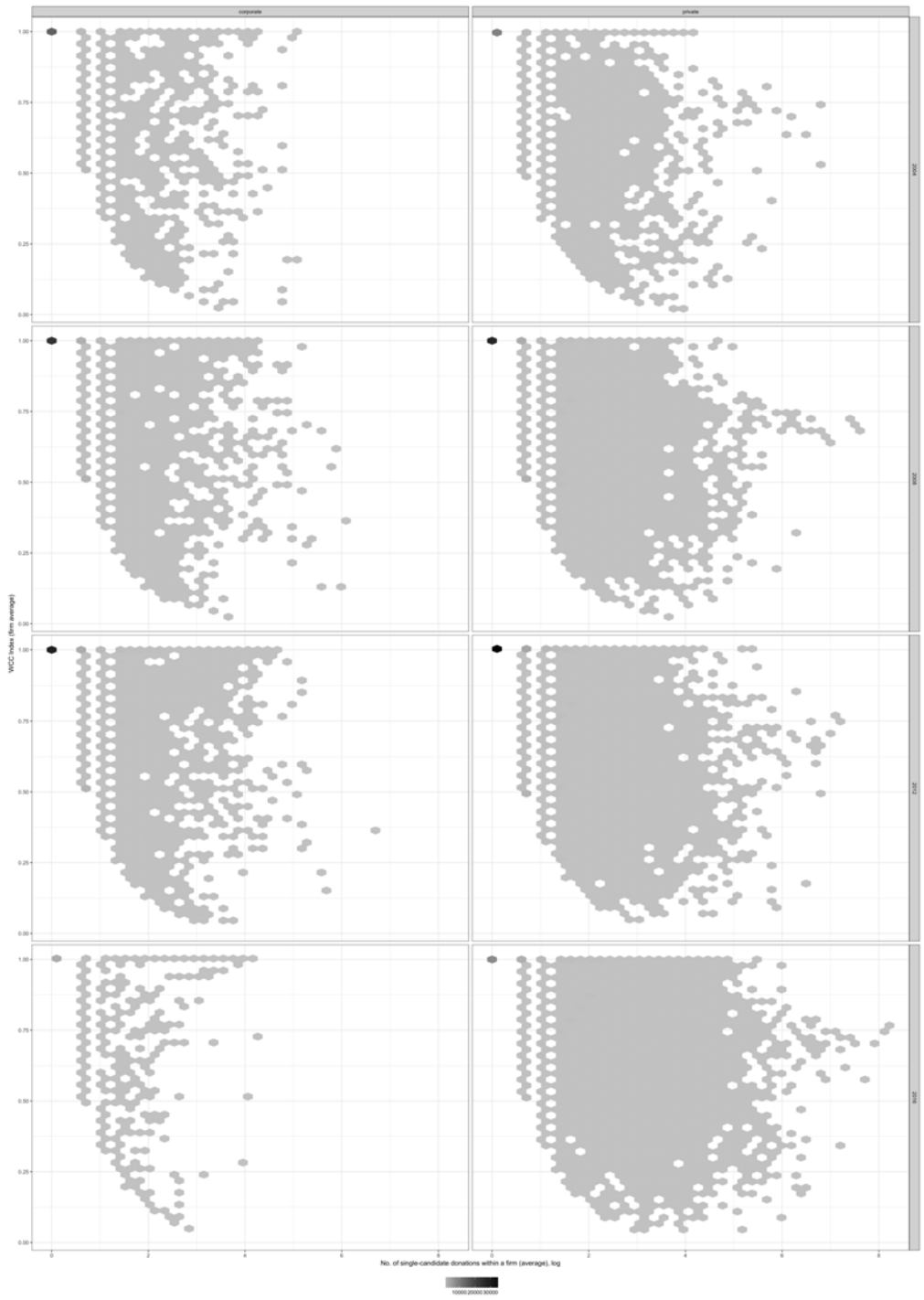


Figure 4.10: ACC - Hexbin, >100 BRL

4.9.3 Party dynamics

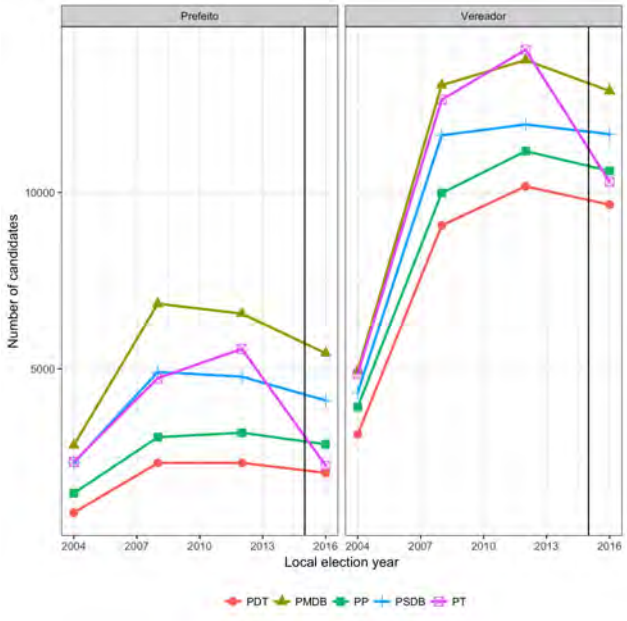


Figure 4.11: Total candidates per party, by office (Top 5 parties)

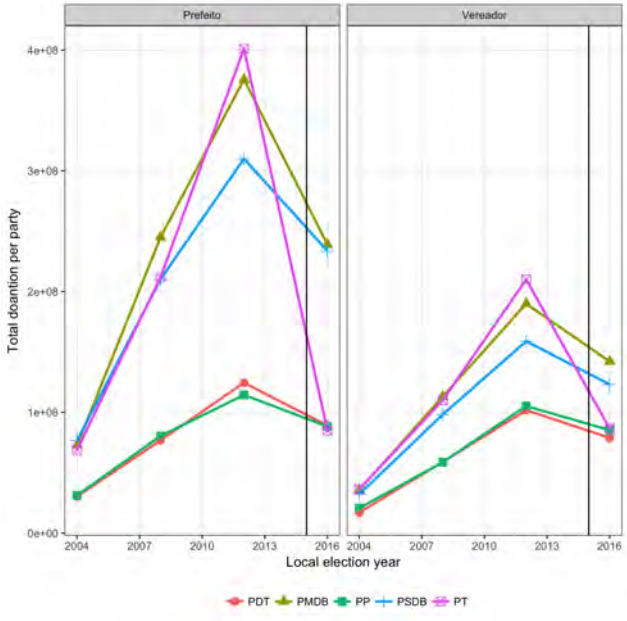


Figure 4.12: Total donations per party, by office (Top 5 parties)

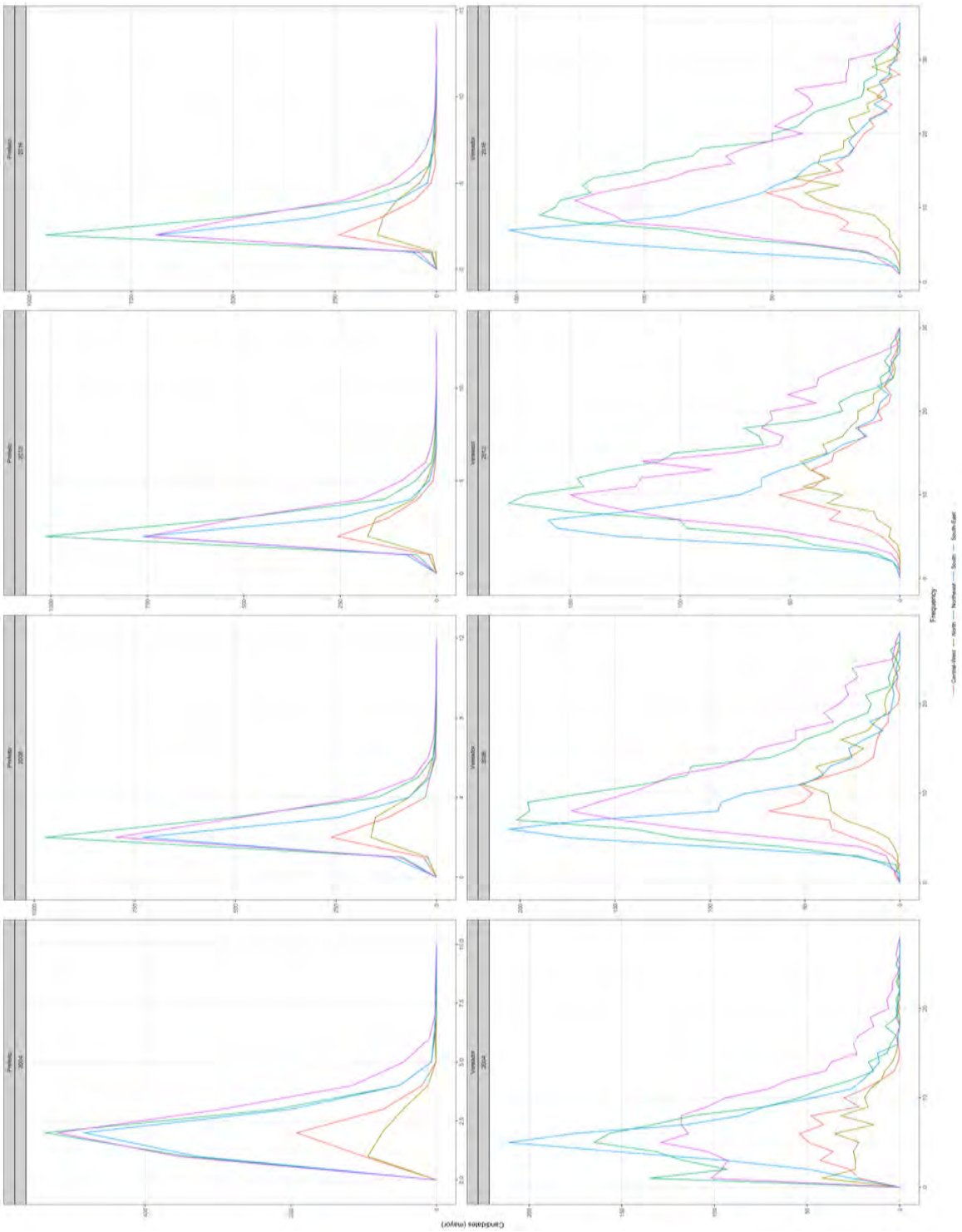


Figure 4.13: Parties representation frequency

4.9.4 Public sector exclusion

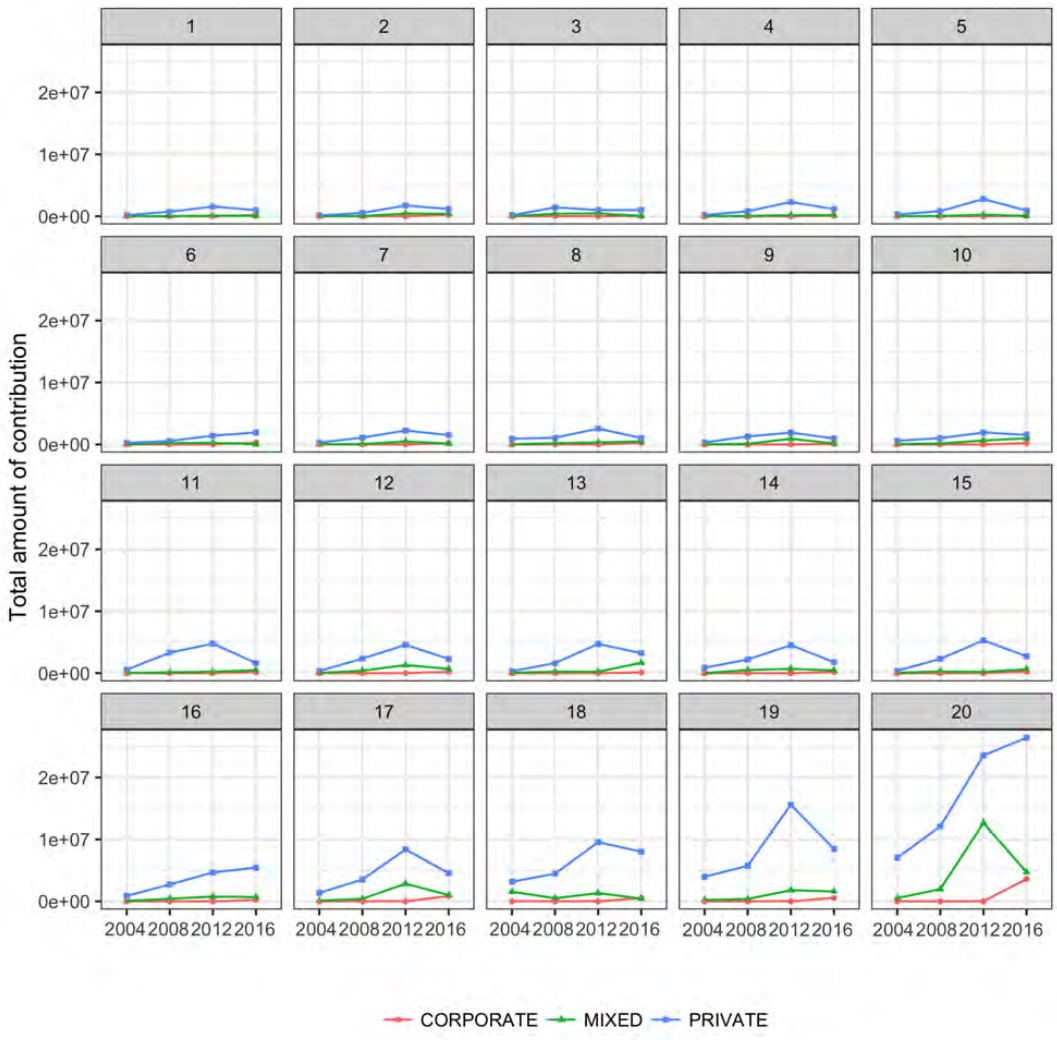
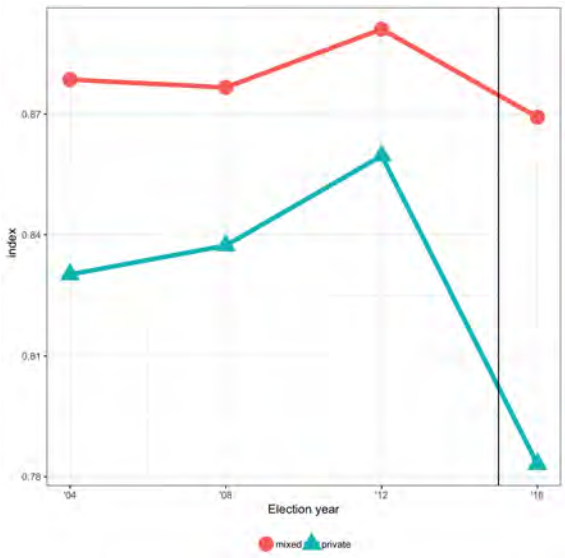
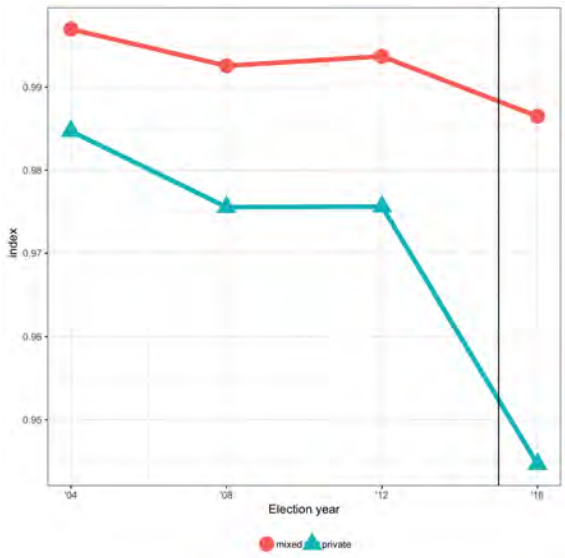


Figure 4.14: Total donations by profile, ventiles (no public sector)

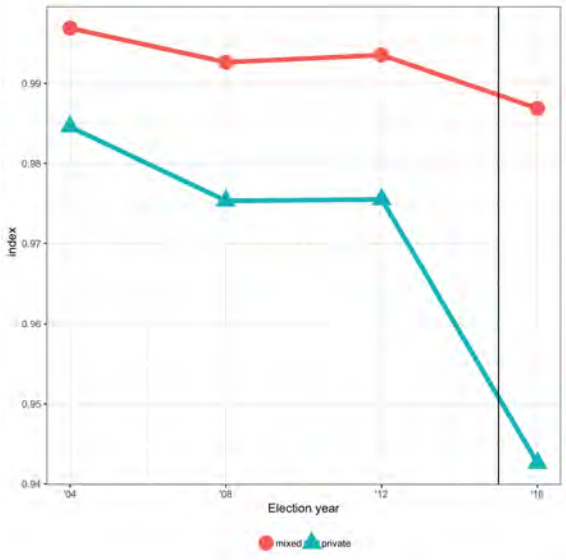
4.9.5 Additional Indexes



(a) WCC (2)



(b) ACC (2)



(c) ACC (3)

Figure 4.15: Concentration Indexes, alternative specification

VARIABLES	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4	(5) Model 5
Treat = 1	0.0485***	-0.0315***	-0.0321***	-0.0302***	-0.0308***
	(0.0103)	(0.00365)	(0.00366)	(0.00356)	(0.00358)
Post = 1	-0.0470***	-0.0324***	-0.0325***	-0.0222***	-0.0198***
	(0.00417)	(0.00253)	(0.00254)	(0.00255)	(0.00266)
1.treated#1.post	0.0377***	0.0203***	0.0212***	0.00701	0.00632
	(0.0119)	(0.00689)	(0.00690)	(0.00675)	(0.00683)
Donations (BRL)				0.0666***	0.0651***
				(0.0204)	(0.0209)
No. donations				-0.00567***	-0.00555***
				(0.000623)	(0.000617)
No. candidates				0.0126***	0.0122***
				(0.00131)	(0.00130)
Margin of victory					0.0182***
					(0.00586)
Salary (BRL)					-0.00279***
					(0.000385)
Popuation					0.00146***
					(0.000549)
HDI					0.239***
					(0.0308)
Dist. capital (Km)					-1.74e-05**
					(7.48e-06)
Urban, share					0.0102
					(0.00808)
Post 2 = 1	0.0295***				
	(0.00392)				
1.treated#1.post_2	-0.0169				
	(0.0114)				
Post 3 = 1	0.00719*				
	(0.00400)				
1.treated#1.post_3	-0.00914				
	(0.0115)				
Constant	0.830***	0.917***	0.914***	0.902***	0.734***
	(0.00361)	(0.00441)	(0.0115)	(0.0110)	(0.0226)
Observations	54,122	54,122	54,122	54,122	52,619
Number of firm_id	35,473	35,473	35,473	35,473	34,770
Sector FE		YES	YES	YES	YES
State FE			YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
SE Clustered at firm level

DiD Estimates: Across Candidate Concentration Index (ACC 2)

VARIABLES	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4	(5) Model 5
Treat = 1	0.0123***	-0.00397***	-0.00371***	-0.00328***	-0.00337***
	(0.00157)	(0.000731)	(0.000739)	(0.000743)	(0.000760)
Post = 1	-0.0400***	-0.0228***	-0.0229***	-0.0217***	-0.0230***
	(0.00150)	(0.00117)	(0.00117)	(0.00118)	(0.00124)
1.treated#1.post	0.0295***	0.0161***	0.0158***	0.0147***	0.0136***
	(0.00253)	(0.00207)	(0.00208)	(0.00208)	(0.00211)
Donations (BRL)				0.0408***	0.0372***
				(0.00773)	(0.00807)
No. donations				-0.000560***	-0.000527***
				(7.49e-05)	(7.39e-05)
No. candidates				0.000164	-4.16e-05
				(0.000243)	(0.000297)
Margin of victory					0.0157***
					(0.00234)
Salary (BRL)					0.000394***
					(0.000113)
Popuation					0.00101***
					(0.000163)
HDI					0.127***
					(0.0134)
Dist. capital (Km)					-2.05e-06
					(3.38e-06)
Urban, share					-0.00487
					(0.00350)
Post 2 = 1	-0.00909***				
	(0.00120)				
1.treated#1.post_2	0.00582***				
	(0.00199)				
Post 3 = 1	-0.00915***				
	(0.00121)				
1.treated#1.post_3	0.00474**				
	(0.00202)				
Constant	0.985***	0.994***	0.982***	0.981***	0.892***
	(0.000999)	(0.00149)	(0.00464)	(0.00459)	(0.00956)
Observations	54,122	54,122	54,122	54,122	52,619
Number of firm_id	35,473	35,473	35,473	35,473	34,770
Sector FE		YES	YES	YES	YES
State FE			YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
SE Clustered at firm level

DiD Estimates: Across Candidate Concentration Index (ACC 3)

VARIABLES	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4	(5) Model 5
Treat = 1	0.0123***	-0.00442***	-0.00415***	-0.00373***	-0.00385***
	(0.00161)	(0.000757)	(0.000766)	(0.000770)	(0.000787)
Post = 1	-0.0420***	-0.0245***	-0.0246***	-0.0232***	-0.0247***
	(0.00155)	(0.00122)	(0.00122)	(0.00123)	(0.00129)
1.treated#1.post	0.0320***	0.0182***	0.0180***	0.0164***	0.0153***
	(0.00261)	(0.00215)	(0.00215)	(0.00215)	(0.00219)
Donations (BRL)				0.0542***	0.0525***
				(0.00831)	(0.00858)
No. donations				-0.000782***	-0.000754***
				(8.90e-05)	(8.78e-05)
No. candidates				0.000732***	0.000556*
				(0.000257)	(0.000301)
Margin of victory					0.0154***
					(0.00243)
Salary (BRL)					0.000410***
					(0.000119)
Popuation					0.00107***
					(0.000169)
HDI					0.135***
					(0.0136)
Dist. capital (Km)					-2.40e-06
					(3.49e-06)
Urban, share					-0.00612*
					(0.00358)
Post 2 = 1	-0.00909***				
	(0.00122)				
1.treated#1.post_2	0.00573***				
	(0.00204)				
Post 3 = 1	-0.00926***				
	(0.00123)				
1.treated#1.post_3	0.00499**				
	(0.00207)				
Constant	0.985***	0.995***	0.984***	0.983***	0.889***
	(0.00101)	(0.00149)	(0.00460)	(0.00456)	(0.00973)
Observations	54,122	54,122	54,122	54,122	52,619
Number of firm_id	35,473	35,473	35,473	35,473	34,770
Sector FE		YES	YES	YES	YES
State FE			YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
SE Clustered at firm level

Reforming the Nepotistic State? How Family and Political Networks Undermine Bureaucratic Reform in Brazil

with **F. Daniel Hidalgo and Gabriel Cepaluni**

5.1 Abstract

Rampant nepotism within government bureaucracies is possibly a cause of poor government performance. Politicians can degrade governmental performances through the discretionary hiring of their staff from a sub-optimal pool of candidates. Despite governments around the world have taken efforts to pass anti-nepotism laws, the effectiveness of administrative reforms to curb the practice is not well established. Presently, we evaluate one such intervention in Brazil: a universal ban by the Supreme Court affecting all levels of government. We use large scale administrative data on the universe of local bureaucrats in the country. Using a Regression Discontinuity (RD) design on the margin of victory at the municipal level, we –first – measure the extent to which winning candidates employ discretionary hiring compared to runner-ups, and – second – we evaluate the effect of the reform to tackle the issue. In the analysis we develop a minimal and scalable measure of nepotist links based on surname patterns and address its few shortcoming through econometric adjustments. We find that politicians who win local offices do appoint bureaucrats from their familial pool at a rate that is 15% higher than their counterparts. The reform partially effective, at face value, bringing this number down to 11%. However, we also show that the very same reform is partially offset due to the strategic reaction of local politician. Elected politicians manage to evade the reform by making deals with officials in neighboring jurisdictions, and performing a relative exchange classifiable as 'cross-nepotist'. The dynamic – identified through a similar strategy – emerges only at the onset of the reform, and is sustained only between close municipalities ruled by the same party. Cross-nepotism is a novel example of corruption displacement that should be brought to the attention of policymakers and political scientist alike. In light of the evidence produce, the piece initiates a discussion on the limits of top-down, all-encompassing mandates for reforming local democratically-elected governments.

5.2 Introduction

Resorting to nepotist hiring is a common practice even in democratic settings (Mulcahy and Finn 2015). Politicians use nepotism practices – the discretionary hiring of relatives or friends in public roles – to appropriate rents and redistribute public resources while increasing their control of the bureaucracy. Public sector nepotism has detrimental effects on social welfare, especially in connection to the degradation of state performance and perception (Rauch and Evans 2000, Dahlström, Lapuente, and Teorell 2012). Calls to regulate the practice are not new: in several settings, policy interventions have attempted to contain and overcome the practice. Few have analyzed the effectiveness of these regulations, and about the strategic response on the part of political actors. We present a specific set of strategies that could undermine anti-nepotist interventions. In particular, the use of political networks to preserve this tool of rent-appropriation is discussed and measured. In this paper, we develop a novel set of nepotism measures, and examine the extent to which policy reforms can curb nepotism practices. Finally, we show that politicians strategically employ family members in neighboring municipalities.

Brazil represents an excellent test-ground to study the introduction of anti-nepotist bans. Despite its historical push to develop into a modern, professional bureaucracy, the country still displays remarkable levels of nepotism at all levels of the public machine (Abrucio, Pedroti, and Pó 2010; Brollo, Forquesato, and Gozzi 2017) - that recently received international coverage when President Bolsonaro attempted to appoint his son ambassador to the United States. Crucial to our research, in 2008, the Supreme Court passed a resolution banning the hiring of family members at all government levels. The main goal of the reform was to reduce privileges and professionalize Brazil's bureaucracy. This study is the first to attempt to measure the effectiveness of this intervention. Here, we show that the policy reduced the discretionary hiring of relatives, although with some limitations. Specifically, we find evidence that the ban induced elected politicians to evade the effects of the reform by making deals with officials in neighboring jurisdictions to sustain cross-nepotist practices. This is particularly visible among jurisdictions governed by the same party.

Our empirical investigation faces many challenges – from developing a sound measure of nepotism hiring to the pinpointing of an identification strategy to overcome endogeneity threats. First, we measure politician-bureaucrat family ties to analyze nepotism in the public sector quantitatively. We use last name 'tokens' to identify family links (cf. Monasterio 2017), and focus on unusual last names and within-municipality over-time variation to limit measurement error to a reasonable extent. With the last name tokens, we compute the number of candidate-bureaucrat matches for the 63,566 mayoral candidacies (winners and losers) for 2004, 2008, and 2012 elections. Second, we measure the effect of winning on family member appointments, adopting a regression discontinuity design on the margin of victory at the municipal level.

The outcome variable of interest is the difference in surnames-matches between the elected politicians and the runner-up, in each municipality. First-difference is used instead of absolute number of matches to control for several factors, including surname clustering, personal network, and so on. We find that elected mayors *do* appoint their relatives to bureaucratic positions before the onset of the ban. However, these estimates fall after 2008, suggesting that the Supreme Court ruling did reduce nepotism, although not eliminating the phenomenon. Specifically, we measure the unintended consequences of the policy intervention, asking a fundamental question related to top-down interventions: does the ban merely push politicians to make deals with other politicians to hire their relatives? To check "cross-nepotism," we look at whether neighboring mayors governed by the same party hire the relatives of neighboring politicians when he or she wins a close election. We find some evidence in favor of a strategic reaction to the nepotism ban. Before the court decision, we find no evidence of mayors in neighboring municipalities hiring elected candidates' relatives; however, we do find evidence of this happening after the intervention. Evidence of this strategic response to the policy intervention appears to disappear over time. This paper contributes to a recent body of literature on returns to political connections, explaining how individuals can benefit from familial connections when attempting to gain access to otherwise competitive positions. It contributes to the discussion of the technologies of rent appropriations that

connects the ownership of public office and rent-seeking attitudes. It contributes to the literature on bureaucracy personnel economics, by describing how politicians manage to maintain control of the state machinery in the face of anti-nepotism policy interventions.

The rest of the paper is organized as follows. In Section 5.3, we present a brief literature review on the issue of nepotism. In Section 5.4, we provide details about the institutional context with respect to local elections and hiring dynamics. In Section 5.5, we present an overview of the data employed. In Section 5.6, we discuss and describe the empirical strategy adopted and the main results. In Section 5.7 we discuss forthcoming research avenues. Section 5.8 concludes.

5.3 Background and Theory

A politician must be able to offer economically superior discretionary job opportunities to loyal personnel in order to capture the bureaucracy. Several institutions usually constrain this capacity by hiring through a merit-oriented screening of civil servants, coupled with the possibility for successful individuals to obtain job security through tenure. Formal and merit-oriented hiring is crucial to insulate the bureaucracy from political and economic pressures (Weber 1947[1]). Second, the existence of a large, well-functioning labor market where the chances of matching one's skills to job opportunities are high. In such market, workers' outside options are numerous and the attractiveness of public sectors is limited.

Nepotism is the discretionary appointment of an individual due to a familial connection, and is a subset of discretionary appointment practices. The literature discusses the positive impact that some forms of discretionary appointment might have on government efficiency and effectiveness – ranging from candidate selection improvements (Lewis 2010) to agency friction reductions (Grindle 2012).

1. See Lynn Jr 2001 for a critical evaluation of the evolution and veracity of the 'bureaucratic paradigm' in the Public Administration scholarship.

However, under nepotist, candidate selection is unlikely to improve government efficiency and effectiveness due to self-selection bias. nepotist hiring remarkably reduces the pool of candidates. Then, it is among the most restrictive selection practices, even among other non-meritocratic forms of public hiring (e.g. ethnic appointment) (Burkart, Panunzi, and Shleifer 2003; Caselli and Gennaioli 2013). nepotist hiring also rests on an open anti-meritocratic basis. Then, it is again among the most callous selection practices concerning ability display, even among similar system blind to merit (e.g. machine patronage) (Pérez-González 2006). Besides, nepotism sustained across time generates adverse selection: relatives of politicians, anticipating a favorable selection criterion, are less incentivized to accrue human capital (Becker and Tomes 1986). [2] .

We argue that nepotism is a technology of rent appropriation on the part of politicians and explores its consequences. In doing this, it taps into a vast economic literature discussing labor market inefficiencies deriving from the combination of ownership and rent-seeking attitudes (Shleifer and Vishny 1993; Banerjee, Mullainathan, and Hanna 2012; Brollo, Forquesato, and Gozzi 2017).[3] From a public policy perspective, the paper addresses the role played by individuals in determining policy effectiveness (Best, Hjort, and Szakonyi 2017), specifically focusing on a deliberate willingness to circumvent regulation so as to maintain the status quo. In so doing, it leverages on the literature of bureaucracy personnel economics (Dal Bó, Finan, and Rossi 2013; Ashraf, Bandiera, Lee, et al. 2014), especially concerning the political capture of the bureaucracy (Iyer and Mani 2012; Rogger 2014; Akhtari, Moreira, and Trucco 2017; Gulzar and Pasquale 2017).

The paper discusses how individuals benefit from the electoral appointment of family members. In

2. The literature is generally silent on the benefits of nepotism, but one cannot exclude them completely, in particular regarding effort, loyalty, and recruiting costs. As a critical defense, nepotism appears unlikely to foster effort. As a family member's job security is unrelated to performance, morally hazardous behavior cannot be written off. One cannot assume loyalty either. Although one might expect family members to be more loyal than professional bureaucrats, this does not rule out the possibility of agency friction between politicians and bureaucrats, even though appointed family members are likely to display ideological alignment or share policy views with the officeholder. With reference to cost, nepotism might well reduce recruiting and searching costs, but this comes at the offsetting expense of demotivating the workforce (Laker and Williams 2003), at least in the private sector

3. See also the literature on the economics of discrimination (Becker 1957; Goldberg 1982; Gelb, Knight, and Sabot 1991; Singell Jr and Thornton 1997), where nepotist practices are taken as an example of preference-derived inefficiencies, usually in private sector firms.

this, it contributes to the substantial literature on returns to political connections. The vast scholarship focuses on monetary and non-monetary returns to firms' connections (Fisman 2001; Faccio 2006; Ferguson and Voth 2008; Cingano and Pinotti 2013; Acemoglu et al. 2016) – exploring financial (Khwaja and Mian 2005; Li et al. 2008); access-related (Amore and Bennedsen 2013; Goldman, Rocholl, and So 2008); and policy-specific favors (Bunkanwanicha and Wiwattanakantang 2008; Johnson and Mitton 2003). The scholarship also investigates individuals' returns to connections in the private (Wang 2013; Gagliarducci and Manacorda 2016; Markussen and Tarp 2014) and public sectors (Fafchamps and Labonne 2017).

Discretion in the use of public office, which underpins politicians' ability to generate individual returns, tangentially connects the present paper to the literature on patronage (Johnston 1979; Folke, Hirano, and Snyder 2011; Colonnelli, Teso, and Prem 2017) and on the reproduction of elites (Michels 1915; Dal Bó, Dal Bó, and Snyder 2009; Bragança, Ferraz, and Rios 2015). Our paper also presents a less explored kind of public office misuse that requires coordination among politicians across neighbor municipalities:"cross-nepotism".

Methodologically, the article builds upon previous studies that use surname patterns and naming conventions to determine familial links between politicians and outer groups (Fafchamps and Labonne 2017; Gagliarducci and Manacorda 2016), thus circumventing the need for inter-generational panel data as is done in inter-generational mobility studies (Güell, Rodríguez Mora, and Telmer 2007). In doing so the paper contributes to the literature also providing a minimal and scalable approach to the issue, though being built on the Brazilian surnames pattern, it is not bound to it.

5.4 Institutional Context

5.4.1 Local government

Brazil has 27 Federal Units (26 states and a federal district) and 5,570 municipalities[4] (*See*: Figure 5.7 in the Appendix). Municipalities vary substantially in terms of population, ranging from 800 inhabitants up to nearly 11 millions, and averaging at 33,145 inhabitants (Estatística 2010). Local governments display a high degree of decentralization and a conspicuous level of autonomy, among the highest in South America (Nickson 1995; Samuels 2004; Titiunik 2009). A mayor and a municipal chamber are the main responsible for governing municipalities. The mayor performs both political and administrative duties, with no other appointed public manager in charge (Avellaneda and Gomes 2017). The mayor directly controls the provision of key goods, services, and positions in the bureaucracy. Majors are elected simultaneously across the country under plurality rule, their mandate lasts four years and they face a two mandates term limits.

5.4.2 Nepotism in Brazil

Brazil makes for a perfect empirical setting in which to study nepotism. The country historically displayed rampant levels of nepotism at all levels of government (Mulcahy and Finn 2015; Rodrigues 2012), despite being the first country in Latin America to advocate for and successfully design a formal merit-based career civil service (Longo and Iacoviello 2010; Cortázar Velarde et al. 2014; Brollo, Forquesato, and Gozzi 2017).[5]. Despite this, concerning bureaucratic capture, Brazil has openly displayed an overt institutional dissonance, already recorded in the academic literature regarding patronage practices (Grindle 2012; Colonnelli, Teso, and Prem 2017; Brollo, Forquesato, and Gozzi 2017).Nepotism

4. `https:/cidades.ibge.gov.br/`

5. Brazil's civil service is considered one of the region's most extensive and professional ones. The American Development Bank assigned Brazil a 95/100 civil service merit index for the years 2012-2015. The Brazilian position is well above the 45/100 OECD average (OECD 2016).

remained for a long time a recurring theme in the political and administrative culture of Brazil, as well as in its society (Holanda 1984). The legal framework surrounding the issue in the country, however, started to evolve in 2005, when the National Council of Justice prohibited nepotism in the judiciary (Resolution No. 7, October 18, 2005). On August 21, 2008, the Supreme Court issued the Binding Precedent No. 13 ('*Súmula Vinculante*' 13 (SV1 3), or *Súmula do Nepotismo*). The reform aimed to abolish nepotist practices at all government branches. Hiring relatives who did not pass a public service entry exam became a violation at the federal, state, and municipal levels. In particular, nepotism was framed as an act of administrative misconduct, directly confronting the principles of morality and impersonality; in this was claimed to violates the principles of Art. 37 of the Brazilian Constitution. Despite the fact that from a strictly jurisprudence standpoint, the doctrine remains scarce and confusing even after SV 13 (Rodrigues 2012), a sufficiently clear set of sanctions were put in place, though leaving lots of discretionary power in the hands of single judges. For the official caught in nepotist practices, the punishment included: removal from public service, suspension of political rights for three to five years; payment of civil fines of up to 100 times the amount of compensation perceived and full damage compensation; and a clear prohibition of contracting with the Government. The doctrine remained nonetheless scarce and confusing (Silva and Matioli 2018; OLIVEIRA 2018; Reis 2018), with substantial grey areas and little guidance for practitioners. Anecdotal evidence covering the phenomenon across the country is widely available (**??**).

5.5 Data

5.5.1 Local Administration Data

IBGE (*Instituto Brasileiro de Geografia e Estatística*) and IPEA (*Instituto de Economia Aplicada*) provide socio-demographic data on Brazilian municipalities. Data are public and openly available[6]. Electoral

6. Data can be accessed at the institutions' respective websites: `www.ibge.gov.br` and `www.ipeadata.gov.br`

data for all political races in Brazil are available in the *Repositório de dados eleitorais*, a repository provided by the Superior Electoral Court (*Tribunal Superior Eleitoral*, TSE). Here, we will focus exclusively on the local races for the years 2002-2012, because of employment data availability as determined by the RAIS dataset described below.

5.5.2 labor Market Data

The Brazilian Ministry of labor and Employment (*Ministério do Trabalho e Previdência Social* MTE/MTPS) administrative database (RAIS) is the primary source of employment data of Brazil. RAIS (*Relação Anual de Informações Sociais*) covers about 97% of Brazil's formal market.[7], including private and public sectors. RAIS is a high-quality census of the Brazilian formal labor market (Dix-Carneiro and Kovak 2017; Brollo, Forquesato, and Gozzi 2017), and the individualized data set is only available to research institutions upon a formal request to the Brazilian government.[8]

The RAIS provides detailed demographic and labor market information about each person that held a formal labor market contract in a given year – including demographic characteristics (gender, age, race), employment status (employer, sector, sub-sector, position, salary), and educational achievement.

Relevant for the analysis is the presence in the dataset of all Brazilian citizens employed in the public administration at the local level. The dataset is subset to include only this class of worker. Each entry (contract) identifies by the employee's and employer's full name and tax identification number. Presently, only public employees' surnames will be used to produce the match count described in the following section.

7. See Brazilian Institute of Geography and Statistics (*Instituto Brasileiro de Geografia e Estatística*, or IBGE) `http:/ces.ibge.gov.br/base-de-dados/metadados/mte/relacao-anual-de-informacoes-sociais-rais.html`

8. First, research institutions should request the data to the Ministry of labor and Employment (MTE). Currently, they request to the Ministry of Economy.

5.5.3 Family Ties Data

The key difficulty in quantitatively analyzing nepotism in the public sector is *measuring* it.[9], says: "the major difficulty with misuse of power is its proof, since the agent does not state his true intention; he seeks to conceal it to produce the misleading impression that the act is legal. That is why the misuse of power is proved by means of evidence" (En.tr. from Portuguese).

Multiple approaches have been tested in different contexts, with varying levels of success.

These, can be organized into three groups: surveys (Olken 2007; Scoppa 2009), analyses of shared family names (Allesina 2011; Fafchamps and Labonne 2017), a mixture of the two (Lesné and Gauthier 2014). The first method rests on a respondent's willingness to disclose family ties. Given that, such disclosure, though, might lead to guilt, reproaching, and even punishment, it is not surprising surveys have been deemed a biased measure of connection to public officials (Fafchamps and Labonne 2017)[10]. The second method rests on a keen understanding of naming conventions, as well as on the researcher ability to disentangle social capital, professional networking and demographic effects all affecting the analysis of shared last names that some scholars claim should not be confused with true nepotism (Ferlazzo and Sdoia 2012). The third method has been more recently developed to overcome some some of the shortcomings of the other two approaches. However, it rests on lengthy preliminary assessments, self-administered questionnaires, and cross-checking of information, which make it hardly implementable outside of contained bureaucratic contexts.

make use of last names to identify family links between bureaucrats and politicians within each municipality. Our approach can be scaled up to comprise the entire public sector. However, it introduces the risk of both false positives and false negatives among the identified matches *vis-á-vis* alternative methodologies based on interview-supported network constructions, previously discussed.

9. Di Pietro 2007, a prominent administrative law scholar in Brazil

10. Some exceptions exists with respect to self-administered anonymous questionnaires, that in some context have showed to produce credible results (Sequeira and Djankov 2014)

We adopted two strategies to mitigate matching measurement biases. First, we exploit variation in matches over time and, second, we focus on unusual last names. Besides, it is unlikely that mismeasure of family ties correlates with the treatment variable. As a result, measurement errors would likely only decrease precision.

To identify family ties between politicians and bureaucrats, we calculate the number of bureaucrats that share at least one last name with the elected mayor in each municipality. We use a modified version of the algorithm proposed by Monasterio 2017 to pre-process Brazilian administrative records in order to overcome the lack of precise specification between first and last names. The last name generated by this algorithm is a one or two-word last name ('last name token').

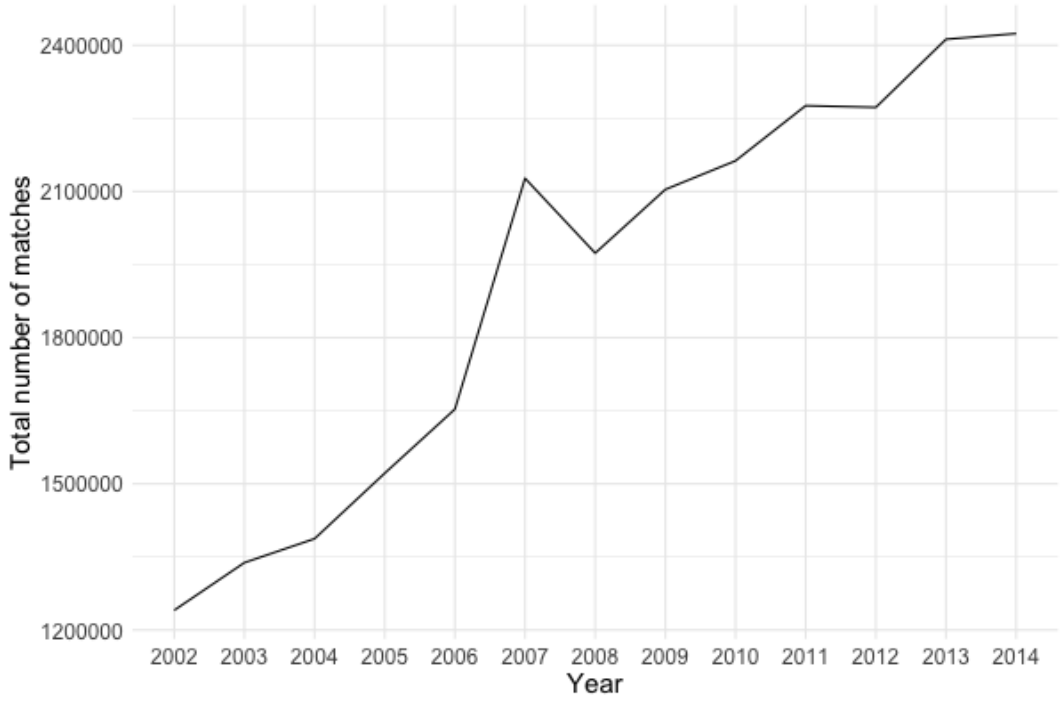


Figure 5.1: Total Number of Politician-Bureaucrat Matches by Year

Using the last name tokens, we compute the number of candidate-bureaucrat matches for the 63,566 mayoral candidacies (winners and runners-up) for the local elections in the years 2004, 2008, and 2012.

For every year we compute the number of bureaucrats in a given municipality who share at least one last name token with a competing politician. The total number of matches is displayed in Figure 5.1. The overall increase in the number of matches is likely due to two factors. First, the public sector expanded massively over the decade under study. Second, in the same years, there was an increase in RAIS compliance with reporting requirements implemented across the country, readily ascertainable by the notable enlargement of the same dataset.

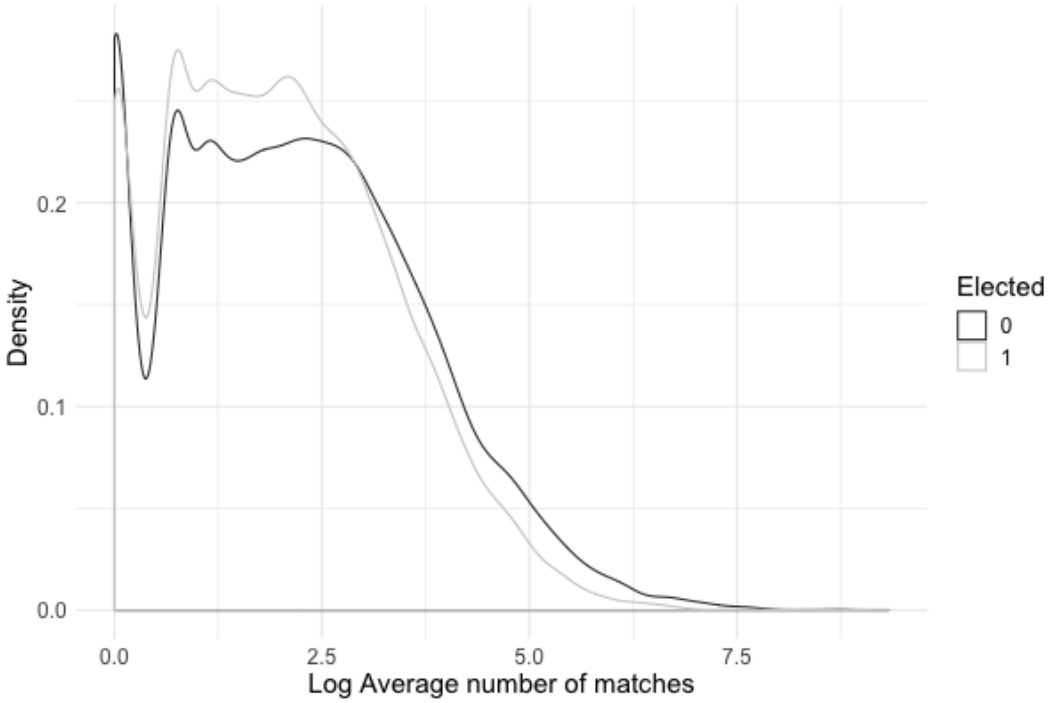


Figure 5.2: Log No. of Matches per Candidate - distribution

We then proceed to calculate the distribution of the mean number of matches across years by a politician. Figure 5.2 shows that the distribution of bureaucrat-candidate matches is long-tailed. While the modal politician has 0 matches, there are a substantial number of politicians with many matches. The median number of matches is 7 in any given year, and about 30 overall. The systematic error of dozens of real family ties between bureaucrats and a politician is unlikely, as well as the result of

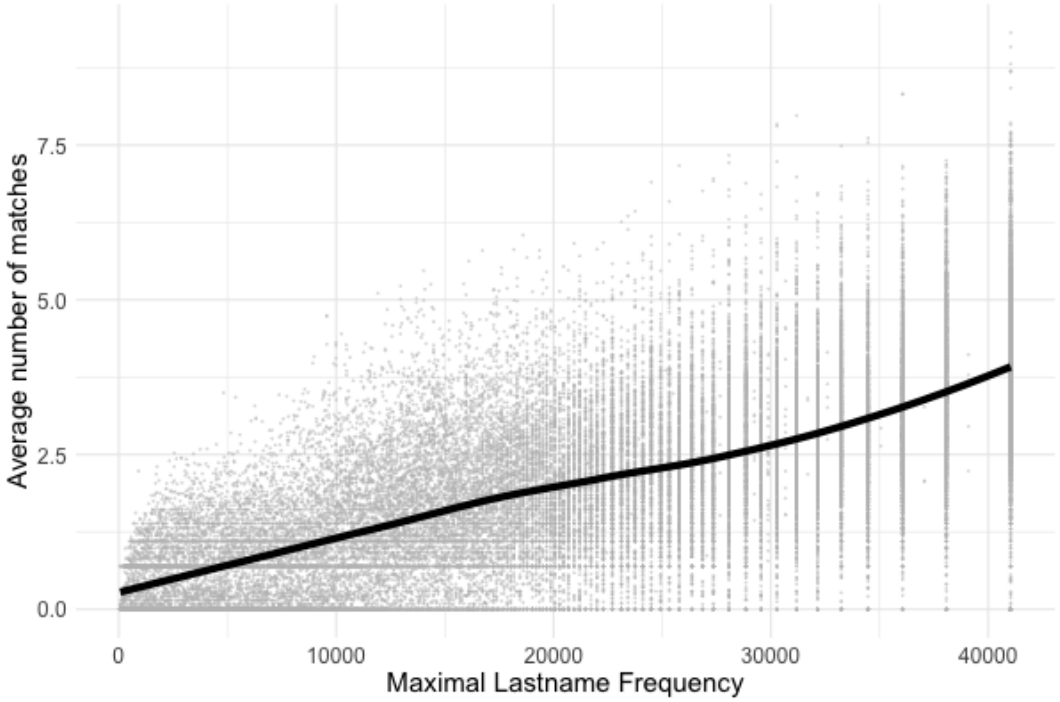


Figure 5.3: Lastname Frequency and Number of Matches

false-positive matches generated because of frequent surnames. To show that this is likely the case, for each full candidate surname, we assign the name frequency of the most frequent surname token. For example, the surname 'Silva Barradas' has two last name tokens – 'Silva' and 'Barradas' – with name frequencies of 7% and .000016%, respectively. The surname 'Silva Barradas', according to the procedure, is thus assigned a maximum name frequency of 7%. Figure 5.3 shows the correlation between surname frequency and many bureaucrat-politician matches. Hence we can infer considerable measurement errors in our outcome variable. To overcome this potential issue, we will conduct robustness checks excluding the most common names from our sample. In sum, last name matches are the primary dependent variable of this paper. As a consequence of these explorations, in the main estimations reported in Section 5.6.1 onward, the most frequent surnames are dropped from the sample to reduce noise. Operationally, we subset the data by removing mayors with the last name frequency above the 80% percentile. In the Appendix results are robust to the inclusion increasingly restricting last name

sub samples, in particular dropping surnames of female candidates and bureaucrats – which presents more difficulties in their ability to be matched with one another, due – again – to naming convention linked to birth registration and marriage-related naming updates.

Statistic	N	Mean	St. Dev.	Min	Max
Election year	16694	2008.00	3.27	2004	2012
Male	16693	0.91	0.29	0.00	1.00
Age	16690	47.92	9.78	21.00	90.00
Education level	16671	5.45	1.74	0.00	7.00
Votes 1st round	16691	9245.17	45444.51	0.00	2686396.00
Votes 2nd round	122	287524.00	523560.50	68469.00	3790558.00
Max match probability	16694	0.01	0.02	0.00	0.07
Margin of victory	16563	15.79	16.91	0.00	100.00
No. bordering villages	14422	13.87	8.63	0.00	76.00
Distance Km, mean	14422	35.47	33.55	0.92	517.74
No. matches within, mean	14792	27.47	74.97	1.00	3404.75
No. matches across, mean	14422	32.70	92.52	1.00	4512.00
No. aligned majors	14142	2.23	3.25	0.00	28.00
No. matches across, mean (<100Km)	14314	32.40	93.32	1.00	4512.00
No. aligned majors (<100Km)	14167	2.14	3.18	0.00	28.00

Table 5.1: Descriptive statistics - Winning candidates

Statistic	N	Mean	St. Dev.	Min	Max
Election year	16349	2008.00	3.27	2004	2012
Male	16349	0.88	0.33	0	1
Age	16349	48.91	10.26	20	107
Education level	16317	5.38	1.78	1.00	7.00
Votes 1st round	16225	6410.01	33693.92	0.00	2209264.00
Votes 2nd round	120	248119.40	463493.20	26131.00	3387720.00
Max match probability	16349	0.02	0.02	0.00	0.07
Margin of victory	16219	−16.12	16.93	−100.00	0.00
No. bordering villages	14410	14.06	8.52	0.00	65.00
Distance Km, mean	14410	35.35	32.80	0.76	435.89
No. matches within, mean	13778	30.02	79.23	1.00	2269.50
No. matches across, mean	14410	34.48	100.80	1.00	4872.00
No. aligned majors	14142	2.02	3.14	0.00	32.00
No. matches across, mean (<100Km)	14311	34.27	102.62	1.00	4872.00
No. aligned majors (<100Km)	14166	1.92	3.05	0.00	32.00

Table 5.2: Descriptive statistics - Runner-up candidates

5.6 Empirical Strategy

5.6.1 Identification strategy

To estimate the effect of the ban on nepotist appointments, we start by assessing the effect of winning a close election on family member appointments to a job in the municipal bureaucracy. This would produce a baseline to quantify the degree of political capture mayor have on the local bureaucracy in Brazil. Precisely, we employ a Regression Discontinuity (RD) strategy on the margin of victory, locally comparing surname-matches of close winners and losers above and below the election thresholds in a sharp design. The procedure we use for the estimation is in line with the classic literature on incumbency advantage (Lee 2008; Caughey and Sekhon, n.d.; Eggers et al. 2015), and it is justified by the wealth of endogeneity concerns the same literature faces - selection on unobservable characteristic and non random treatment assignment in non-close elections.

$$y_{im}^{post} - y_{im}^{pre} = \alpha + \tau E_{im} + f(V_{im}) + \epsilon_{im} \tag{5.1}$$

More precisely, the dependent variable used in the analyses is the first difference ($y_{im}^{post} - y_{im}^{pre}$) in last name-matches between a candidate and the group of public employee of the municipality she governs. Using the first difference as the dependent variable increases precision by reducing the impact of measurement error. Most importantly, first difference adjusts for false positive matches due to name frequency, since this is likely a fixed source of measurement error. Using new hires matches as dependent variable has been used as a robustness check, besides being a less demanding specification, it cannot claim to have any mitigating effect on the measurement error - results are nonetheless robust to the change in dependent variable (analyses not included, available upon request).

\{}vspace{1.5cm} The number of matches is averaged over the four years prior and after each election, corresponding – as explained – to a full local government mandate in Brazil. The forcing variable is the local election margin of victory: E_{im} measures whether the candidate i was elected in municipality m; $f(V_{im})$ is a measure of the forcing variable.

Results are produced with a local linear specification with data-driven automatic-bandwidth selection (Calonico, Cattaneo, and Titiunik 2014). The paper exploits a classic Regression Discontinuity framework on margins of victory - specifically, we adopt the, now standard, *continuity-based* framework relying on local polynomial least-squares method to model separate regression functions on each side of the cut-off - not assuming neither a quadratic model nor constant standard deviations. Local polynomial fits follow the standard procedure for data-driven, automatic empirical work. Our preferred model presents local linear estimations with standard errors clustered at the municipal level, and the inclusion of pre-treatment covariates (Calonico et al. 2019 - alongside a number of variations, weighting scheme, local polynomial and covariates inclusion. All models relies on robust nonparamet-

ric bias-corrected inference in the RD design which is the most restricting option available (Calonico, Cattaneo, and Titiunik 2012). Our results are robust to more largely adopted inference approaches, especially the standard method for MSE-optimal bandwidths selection with triangular Kernel Weights (Not included in the Appendix, available upon request).

5.6.2 Estimation - Intended consequences

We separately estimate the effects of being elected in 2004, 2008, and 2012 on the hiring of relatives during the subsequent mandates (precisely the municipal mandates over years 2005-2008, 2009-2012, and 2013-2014, as a mayor elected in the fall of a given year does not take office before the 1st of January of the subsequent year[11], respectively). Given the pundit discussion presented here, we should expect that elected mayors do indeed appoint significantly more relatives once in office, than their counterfactual - at least before the introduction of the ban. At the same time, at the onset of the ban, we should expect mayors to be somewhat constrained in their ability to appoint relatives. These are informally the first two hypothesis of the paper.

The first hypothesis results correct. Figure 5.4 show that elected mayors do indeed appoint their relatives to bureaucratic positions, with respect to their counterparts. Table 5.3 estimates the nepotist capture of the bureaucracy as being 15% higher for mayors than for runner-ups. Runner-ups in close elections are a good control for observable[12] and non observable characteristics - among which the ability to manipulate directly or indirectly the hiring process in the municipality to favor their relatives is particularly relevant for the present study. The result is robust to the usual robustness checks and placebo test (*See*: Appendix).

11. It is to be noted that for the 2012 candidates, at the present moment, we only have access to RAIS data for 2013 and 2014. This makes the results for the last period not strictly comparable. In the Appendix analyses are carried out considering only the first two years of the mandate - i.e. 2005-2006, 2009-2010, and 2013-2014 to force the comparability of results across the three election.

12. In the Appendix, Figures 5.10 and 5.11 present visually the lack of discontinuity at threshold of pre-existing covariates

The second hypothesis results correct, also. Figure 5.4b shows that after the ban the 'nepotist advantage' of winners shrinks and so does the the overall level of nepotism in the country. Table 5.4 shows the nepotist capture of the bureaucracy by mayors being reduced to 11.6%, in the course of a single mandate. In assessing the effectiveness of the ban, it is important though to consider the matches shrunk also in absolute terms, with fewer matched for both winners and runner-up. This aspect is and cannot be not captured through an RDD identification strategy. Nonetheless, it might be taken into consideration as a further, non-causal evidence of the effectiveness of the policy. The the Supreme Court ruling appears to have a substantial effect[13], at least *prima facie*. The result is robust to the usual robustness checks and placebo test and does not seems to be driven by a specific cluster of municipalities (*See*: Appendix).[14]

13. The magnitude of the result, given the characteristics of the settings, should not be overlooked. Elimination of the practice was never imaginable in such a short period of time

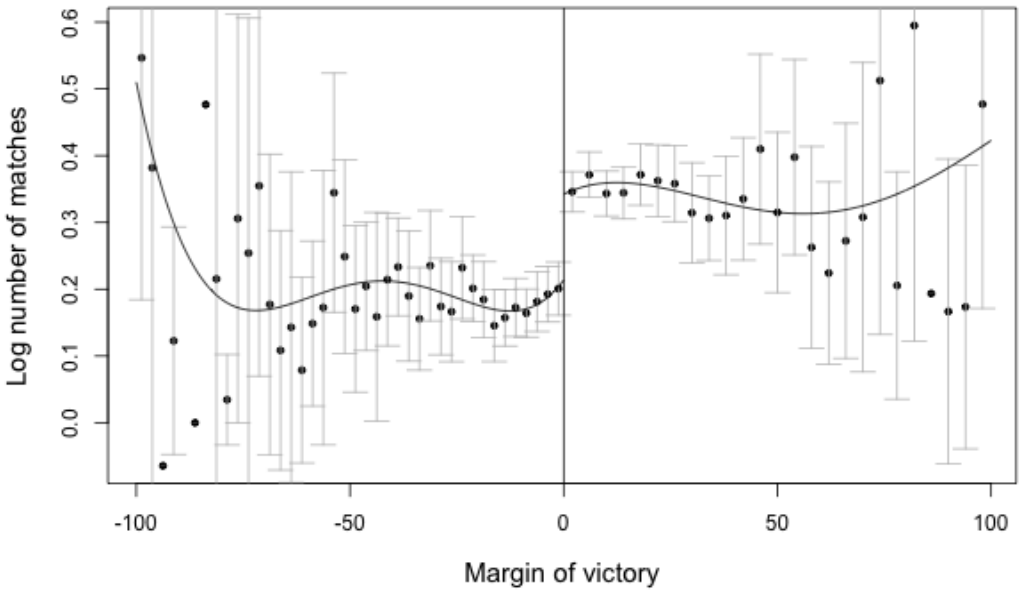
14. Please notice that traditional RD estimations produced graphical evidence including confidence intervals for a quadratic least-squares fit, assuming a constant standard deviation on each side of the cut-off (Calonico, Cattaneo, and Titiunik 2014). Here, we binned the data so that we can estimate a different standard deviation for each bin, with confidence intervals calculated on it. These are the confidence intervals reported in all graphs, in all the subsequent analyses.

	Model (1)	Model (2)	Model (3)
Coeff.	0.154	0.154	0.156
S.E.	0.029	0.029	0.032
z	5.389	5.388	4.951
P>|z|	0.000	0.000	0.000
95% C.I.	0.098 , 0.210	0.098 , 0.210	0.094 , 0.218
N	5057 , 5079	5057 , 5079	4216 , 4162
N (effective)	3080 , 3092	3081 , 3093	2550 , 2517
h	14.83	14.84	14.77
b	24.21	24.21	23.70
p	1	1	1
p (bias)	2	2	2
Cluster		YES	YES
Covariates			YES

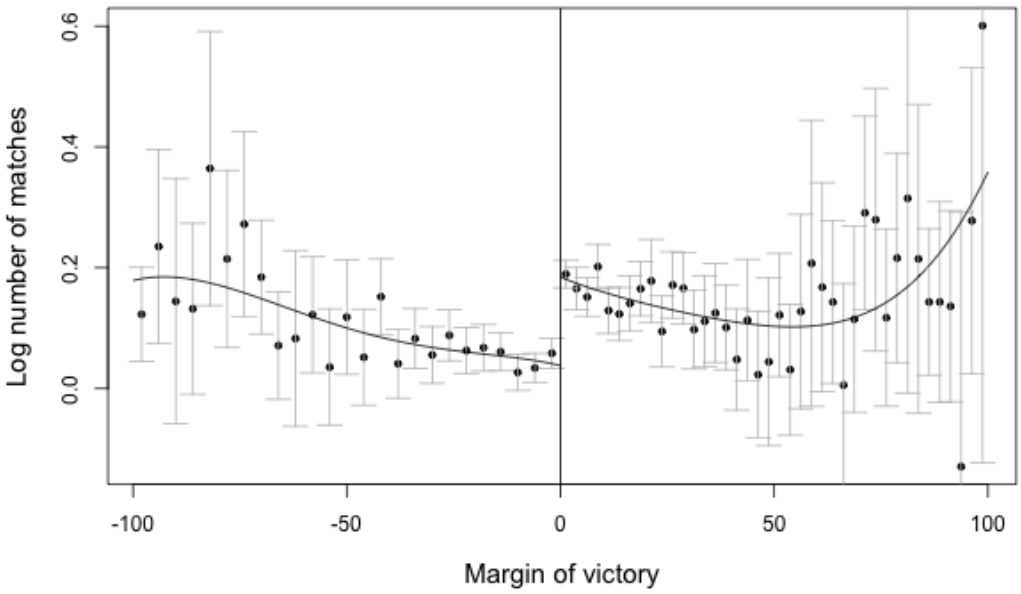
Table 5.3: RD Estimates - Election 2004 within municipalities

	Model (1)	Model (2)	Model (3)
Coeff.	0.133	0.133	0.119
S.E.	0.023	0.023	0.024
z	5.903	5.903	4.889
P>|z|	0.000	0.000	0.000
95% C.I.	0.089 , 0.178	0.089 , 0.178	0.071 , 0.167
N	4483 , 4509	4483 , 4509	3904 , 3794
N (effective)	2686 , 2703	2687 , 2704	2360 , 2302
h	15.00	15.00	15.23
b	24.90	24.91	26.22
p	1	1	1
p (bias)	2	2	2
Cluster		YES	YES
Covariates			YES

Table 5.4: RD Estimates - Election 2008 within municipalities



(a) Election 2004



(b) Election 2008

Figure 5.4: GLOBAL RDD - Number of matches within municipalities

5.6.3 Estimation - Unintended consequences

In considering the effectiveness of the ban, one should non forget the kind of actors engaging in the practices: politicians with an non-naïve understanding of local government machinery, with the need and/or will to extract rents from it. This kind of actors is likely react against the regulation to maintain their rent-extraction technology, also to the point of developing a new *loci* for it (Stokes 2007). The present paper focuses on one of these reactions: cross-nepotism - a permutation of classical nepotism with politicians leveraging their position in a political network to see their relatives hired by connected politicians - with an adjustment for reciprocal designations or appointments.The phenomenon is well-known in Brazil (‘*nepotismo cruzado*’ or ‘*por reciprocidade*’ - Silva and Matioli 2018). Already Resolution No. 07/2005 of the Brazilian National Council of Justice discusses and regulates the practices - there with respect to nepotism across government branches. This nepotism has the potential to offset any effect of the intervention registered in Section 5.6.2.

Cross-nepotism is an appealing second-best solution for politicians willing maintain their bureaucratic capture, as it equally easy to implement but more difficult to observe and to timely regulate[15].

In this Section we advance the hypothesis that politicians resorted to cross-nepotism, at least at the onset of ban, to reproduce their instruments of rent appropriation. To test this hypothesis, we exploit a similar identification strategy to the one discussed in Section 5.6.1, looking this time at the number of surname of matches between politicians and bureaucrats across neighboring municipalities ruled by mayors of the same party. To restrict the scope and reduce the noise of the analysis, we include only

15. As previously mentioned, cross-nepotism presupposes an adjustment for reciprocal designations or appointments. However, as it is more difficult to (timely) regulate against: from a legal perspective such adjustment must be proven to specifically enter the nepotist appointment category and the existence of such circumstances is very difficult to prove in practice (Rodrigues 2012).

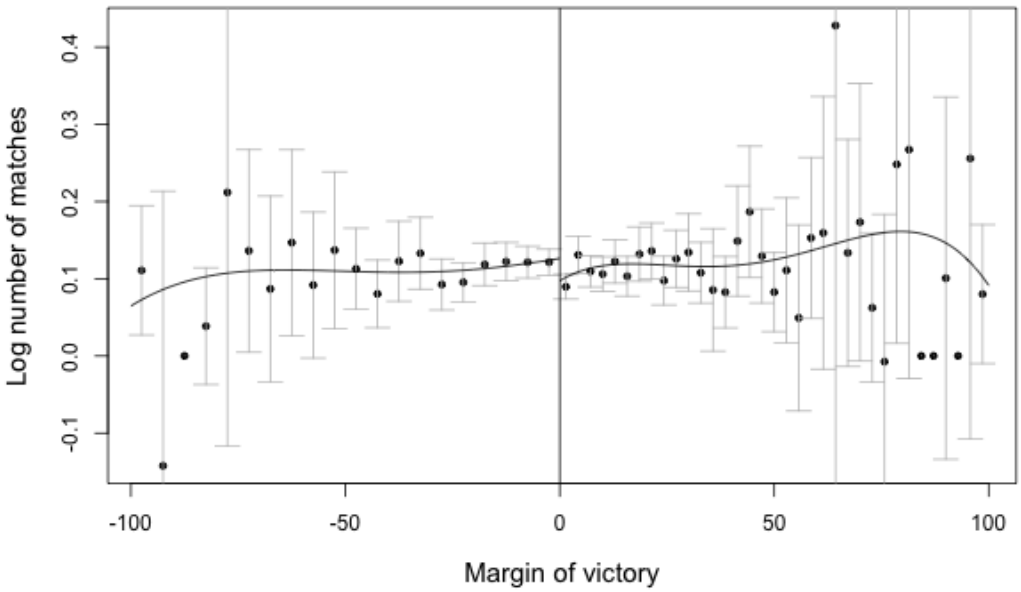
directly neighboring municipalities[16] and dropping any match beyond 100km from their centre[17] (*See*: Table 5.1 and 5.2 for descriptive statistics on the selected municipalities). As discussed in the appendix, the results are robust to even more restrictive radius. In the scenario profiled, we expect mayors to engage in cross nepotist exchanges at the onset of the ban, but not before - as the practice remains less than ideal, with increasing transaction costs and reduced political returns to investment as the capture chain lengthens.

	Model (1)	Model (2)	Model (3)
Coeff.	-0.017	-0.017	-0.002
S.E.	0.015	0.015	0.017
z	-1.128	-1.128	-0.099
P>|z|	0.259	0.260	0.921
95% C.I.	-0.047 , 0.013	-0.047 , 0.013	-0.035 , 0.031
N	5057 , 5079	5057 , 5079	4216 , 4162
N (effective)	2992 , 3003	2993 , 3004	2372 , 2334
h	14.25	14.26	13.34
b	24.23	24.24	22.45
p	1	1	1
p (bias)	2	2	2
Cluster		YES	YES
Covariates			YES

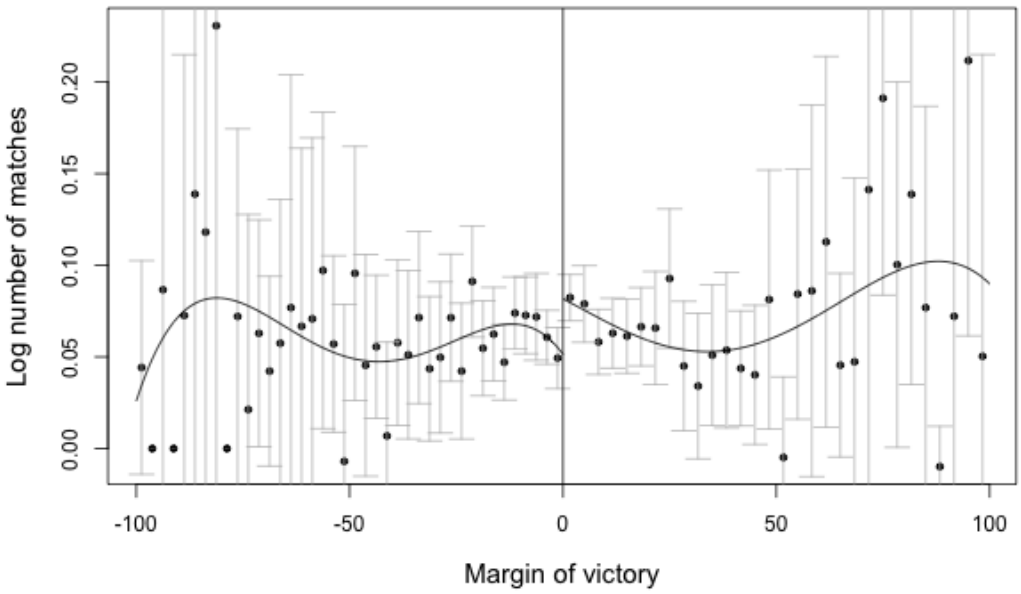
Table 5.5: RD Estimates - Election 2004 across municipalities

16. The inclusion of second-order exchanges – exchange across municipalities beyond those directly bordering the one of interest– has been developed but not yet included in this version of the paper. It makes sense especially for the South East region of the country, with highly urbanized areas where considering only direct municipalities might lead to underestimates the number of mayors politically aligned likely to have undertaken cross-nepotist practices.

17. Distance between municipalities is calculated by taking Earth distance from municipal urban centres, using a shapefile made available by the *IBGE*. The precision of the measure can be improved using actual centres of the municipalities via Google Maps API, and actual travel distances between villages within the municipal territory. To date this still remains to be developed.



(a) Election 2004



(b) Election 2008

Figure 5.5: GLOBAL RDD - Number of matches across municipalities

	Model (1)	Model (2)	Model (3)
Coeff.	0.030	0.030	0.030
S.E.	0.012	0.012	0.013
z	2.540	2.539	2.357
P>|z|	0.011	0.011	0.018
95% C.I.	0.007 , 0.053	0.007 , 0.053	0.005 , 0.056
N	4483 , 4509	4483 , 4509	3904 , 3794
N (effective)	3192 , 3210	3193 , 3211	2811 , 2747
h	19.89	19.89	20.54
b	42.29	42.30	43.85
p	1	1	1
p (bias)	2	2	2
Cluster		YES	YES
Covariates			YES

Table 5.6: RD Estimates - Election 2008 across municipalities

Our expectations are met, as comparing Figures 5.5 and 5.6 graphically shows. The nepotism ban pushed mayors to engage in cross-nepotism practices only after the onset of the ban. We find no evidence of mayors in neighboring municipalities hiring other elected representative's relatives before the Supreme Court pronouncement (Table 5.5), but we do observe a statistically significant up-tick to 3.4% of these hiring afterwards (Table 5.6). The result is robust to the usual robustness checks and placebo test and does not seems to be driven by a specific cluster of municipalities (*See*: Appendix). In additions, as robustness test we expanded the number of municipality considered among which the exchange might happen. Results are robust to the inclusion of municipalities two- and three-degrees removed[18].

Some further analyses reveal that this nepotist exchange exists only across municipalities governed by mayors of the same party, as the result disappear when attempting to match on non politically aligned municipalities for placebo testing (*See*: Appendix). The findings supports the idea that politicians have to leverage on their political network to keep extracting rents[19].

5.6.4 Estimation - Stability of the results over time

In this section we discuss to what extent the results thus far discussed are consistent over time. To do so, we focus on the next municipal election, the 2012 one, for which we replicate all the previous analyses. As time passes, one might expect two things: with respect to standard nepotism – seen the early effectiveness of the policy – one might expect the policy to gain even more traction with internalization of the ban and standardization of the controls; with respect to cross nepotism, similar expectation building is not clear. To the observer is not clear how effective (strictly political) cross nepotism was to maintain capture of the bureaucracy, especially considering the set of costs previously discussed, for

18. Data available upon requests

19. Being Brazil on an extreme in terms of politicians party loyalty, these results might have been even stronger in otherwise similar contexts.

the politicians and for the bureaucrats. On top of this, it is yet not clear the extent to which authorities had been able to catch up with the strategic reconfiguration of the politicians.

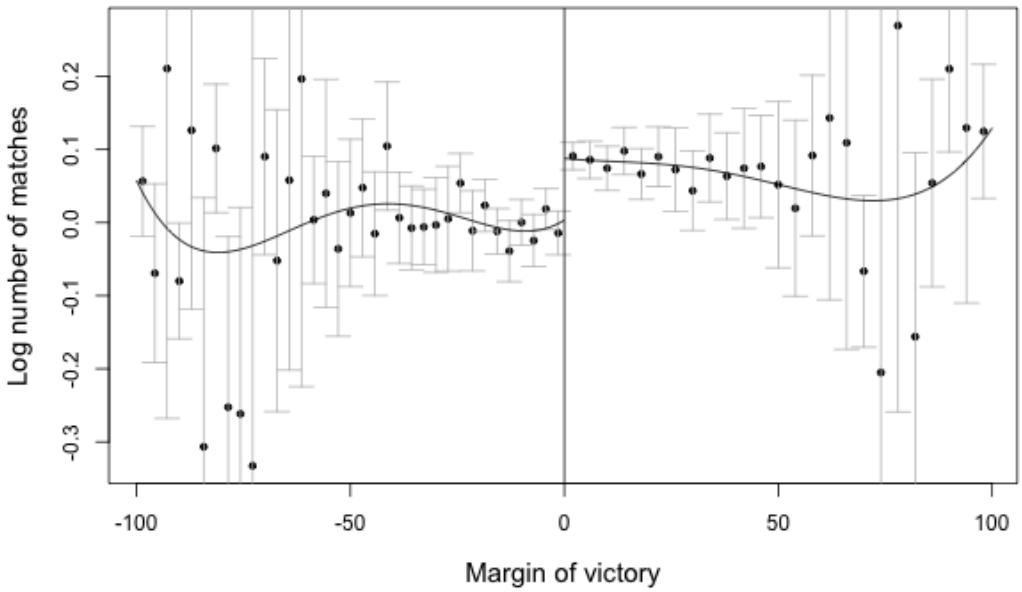
From the analyses it is possible to say that indeed the policy manages to get further traction, as Table 5.7 shows. During the second mandate after the introduction of the ban, the level of nepotist capture advantage is reduced to 7.3% for the winner over the runner up. That might also be true for cross-nepotism that goes back to be undetectable across municipalities with a mayor of the same party, as Table 5.8 shows[20].

If this specific instance of strategic reactions to the anti-nepotism policy disappear over time, we cannot completely exclude that it did not further evolve to evade the detection attempts, as discussed in Section 5.7.1.

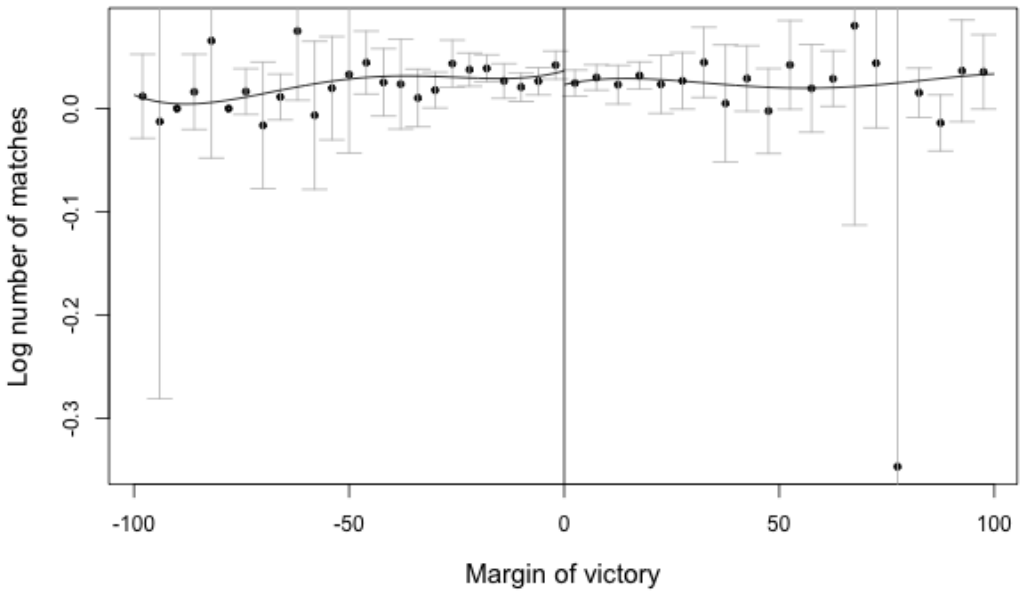
	Model (1)	Model (2)	Model (3)
Coeff.	0.085	0.085	0.079
S.E.	0.021	0.021	0.023
z	4.078	4.077	3.501
P>|z|	0.000	0.000	0.000
95% C.I.	0.044 , 0.126	0.044 , 0.126	0.035 , 0.124
N	4796 , 4870	4796 , 4870	4155 , 4176
N (effective)	3046 , 3089	3047 , 3090	2685 , 2723
h	15.99	16.00	16.43
b	28.86	28.87	29.61
p	1	1	1
p (bias)	2	2	2
Cluster		YES	YES
Covariates			YES

Table 5.7: RD Estimates - Election 2012 within municipalities

20. It is important to remark once more the data data for 2015 and 2016 RAIS were not made available at the time of estimation – as explained in Section 5.7.2. The unavailability of RAIS makes the data not strictly comparable with the other two regressions. Thus, we cannot ascertain if nepotist hiring follows unwritten rules, such as the need to wait until later in the mandate to undertake such an action. We will re-estimate all the previous analyses only considering the first two years to force – to an extent – comparability across the three elections.



(a) Election 2012



(b) Election 2012

Figure 5.6: GLOBAL RDD - Number of matches across municipalities

	Model (1)	Model (2)	Model (3)
Coeff.	-0.017	-0.017	-0.019
S.E.	0.011	0.011	0.013
z	-1.521	-1.520	-1.533
P>|z|	0.128	0.129	0.125
95% C.I.	-0.039 , 0.005	-0.039 , 0.005	-0.044 , 0.005
N	4796 , 4870	4796 , 4870	4155 , 4176
N (effective)	2836 , 2873	2837 , 2874	2406 , 2448
h	14.41	14.41	14.15
b	29.05	29.06	28.70
p	1	1	1
p (bias)	2	2	2
Cluster		YES	YES
Covariates			YES

Table 5.8: RD Estimates - Election 2012 across municipalities

Cross-nepotism remains not an ideal solutions: besides being explicitly prohibited by the law (OLIVEIRA 2018), it is more costly, less effective, and overall less appealing than standard nepotism for all the benefiting actors involved. This leaves room for optimism: despite the practice is harder to observe and requires more resources to be tackled, it is likely to die out in the form here discussed on its own, after being tested out just one time. This though might be limited to the Brazilian case, were geography, bureaucratic arrangements and outside-options beneficiary of nepotist favors, all contribute to the unprofitably of the arrangement. The value of the viability of the same solution should be tested in other contexts to test this final hypotheses.

5.7 Further analyses

In this concluding section we sketch out some additional avenues of research that we are currently exploring, in an attempt to produce a picture of the politicians' reaction to the ban as complete as it is possible. We present a refinement of the within-party cross-nepotism analysis that is potentially more in line with the reality of local politics in Brazil.

5.7.1 Ideological exchange

In previous sections, we assessed the extent to which a cross-nepotism took place among jurisdictions governed by mayors member of the same same *party*. In the future, we will try to refine the analysis by looking at whether a cross-nepotist dynamic unfolds among jurisdictions governed by mayors sharing the same *ideology*, being them members of the same party or not. In looking beyond party affiliation when defining ideological positions, we try to engage more closely with the reality of local politics in Brazil. Classic comparative politics at length discusses the autonomy politicians are granted *vis-à-vis* their parties in Brazil and how this has led to individualistic behaviours, undermine parties' effectiveness, but also voiding party affiliation of meaning (Mainwaring 1991; Desposato 2006).

Given the low degree of party loyalty , party affiliation might be a poor proxy of a mayor ideological position and – more important for the present analysis – a poor marker to pinpoint couples of politicians willing to engage in illegal behaviour. Moving away from party affiliation might increase the precision with which we can detect cross nepotist exchanges. Ideological closeness can be calculated using common-space campaign finance scores (CF-Scores), a measure of candidates' ideology estimated using campaign finance data described in Bonica 2013 for the US context. The methodology is deemed better suited for the task than other fast dimension reduction methods and it has already been explored in the multi-party context of Brazil by Desposato and Cunow 2011. The CF-Scores ideology measure leverages on donation patterns from contributors to politicians, assuming that funds are distributed in accordance with one's evaluation of a recipient's ideological position and that preference is given to more proximate ones. The measure represents an improvement in that it accounts for contributions' magnitude, with the information component having been previously overlooked. We use campaign finance data provided by the TSE using all levels of politics for the years under consideration. The fact that we include donation data beyond that of the local level, is mainly as a response to the calibration requirements of the statistical analysis. However, it also allows us to look into the ideological distributions of local-level politicians, and to compare them with the national ones – a task previously left unfulfilled by the scholarship.

Once these ideological distributions are defined, an analysis comparable to the previous one will be carried out, comparing the differential increases in surnames' token matches across constituencies that are sufficiently close and that are ruled by sufficiently close politicians with respect to ideology. A regression discontinuity design will be then exploited to gauge the magnitude of this effect. At the time of writing these analyses are being undertaken and the measurement of the effect of anti-nepotist policy on the magnitude of ideology-based cross-nepotism remains unknown. Using political donations as a way to measure ideological affinity has the potential to shed light on the role played by private sector actors in policy response.

5.7.2 Extra-political cross-nepotism

In previous sections, we assessed the extent to which a cross-nepotism took place as a response to the nepotism ban, considering it as unfolding strictly within the realm of the public administration. The research's focus was on the ability and willingness of politicians to engage *themselves* in discretionary hiring of other politicians' relatives. In the future, we consider as a possible avenue of research the dynamic according to which politicians, once limited in their ability to carry out nepotist practices in the public sector, might look to the private sector as an alternative 'marketplace' for cross-nepotism - in line with the most common understanding of the practice.

The relation between politicians and corporate actors has been shown to be, to a certain extent, driven by the expectation of exchanging money for political favors – both in general (Milyo 1999, Stratmann 2005), and with specific reference to the Brazilian context (Boas, Hidalgo, and Richardson 2014). Along this line of research we explore how hiring contracts for relatives of a politician might follow from a similar dynamic. Incapable of hiring a relative himself or herself, a politician might ask a close donor to hire one or more of his or her relatives. This might be done in partial or complete substitution of the donor's contributions, in exchange for further political favors. In this light, some private sector jobs might be considered as extra-office rents that politicians might be able to appropriate, in a certain policy context. The social burden of these hiring is not carried by the society *directly*, but externalized to the private sector. Upon superficial scrutiny this might appear to be a positive consequence of the ban; however, any exchange of resources for policy between politicians and corporate actors – whatever the currency – has the potential to ultimately harm society and should be taken into consideration when assessing a ban on nepotism.

5.8 Conclusion

In the present article we discuss the issue of nepotism in the public sector – the discretionary appointment of bureaucrats by politicians due to a familial connection –, and the extent to which top-down reform attempts are effective at curbing such practices. Specifically, we look at Brazilian local level government in the years following the introduction of a Supreme Federal Court ban of nepotist practices. Brazil ia

We first assess the effect of winning an election on the number of relatives appointed to jobs in the local bureaucracy and find evidence in favor of this practice. The finding supports the theory that sees nepotism as a technology of rent appropriation to which politicians in office resort. Second, we evaluate the effect of the ban on the curbing of such practices and find evidence in support of the ban's effectiveness despite full eradication not being achieved through the ban alone. Finally, we study the strategies politicians might have adopted in response to the ban to maintain the status quo – mainly what we describe as the practice of cross-nepotism –, we find evidence of the practice being used in response of the policy intervention. To conclude, we present avenues for future research that focus on alternative reacting strategies. The paper sheds light on a phenomenon of paramount relevance, highlighting the strengths and shortcomings of a major legislative intervention attempt. To measure the magnitude of the extra-political cross-nepotism, we plan to employ a similar strategy to the one adopted previously, looking at the increase in the differential number of matches between a candidate's surname and those of the people employed by his or her top donors and explore, again, a regression discontinuity design on margins of victory. If a candidate is capable of forcing these hires in the private sector only when occupying an elected office, this would strongly suggest that such a practice is part of the favor exchanges discussed in the literature and should, as such, be considered attentively.

5.9 Appendix

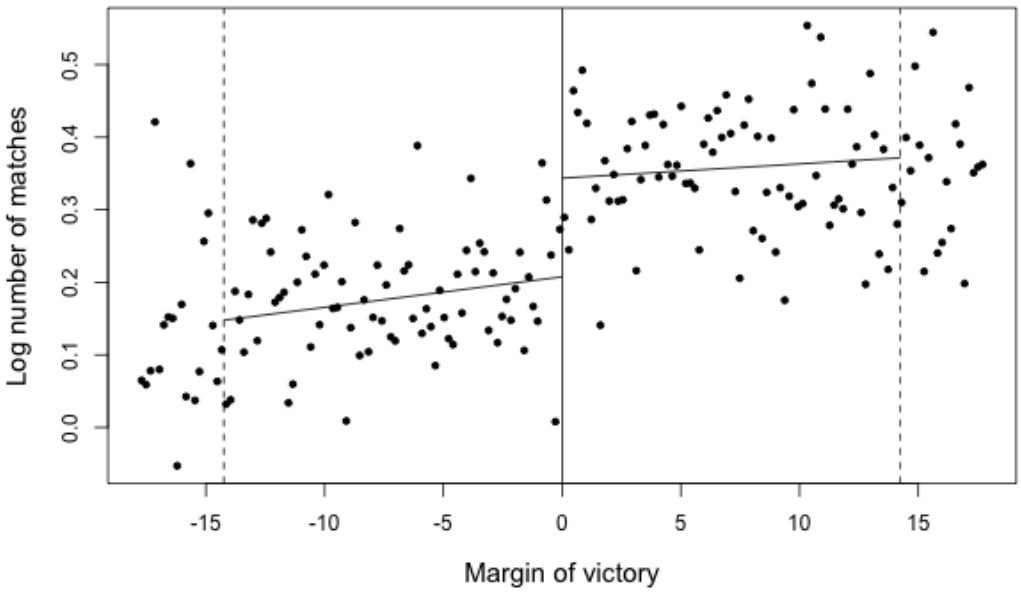
5.9.1 Brazil maps

Figure 5.7: Brazil Municipalities, by State

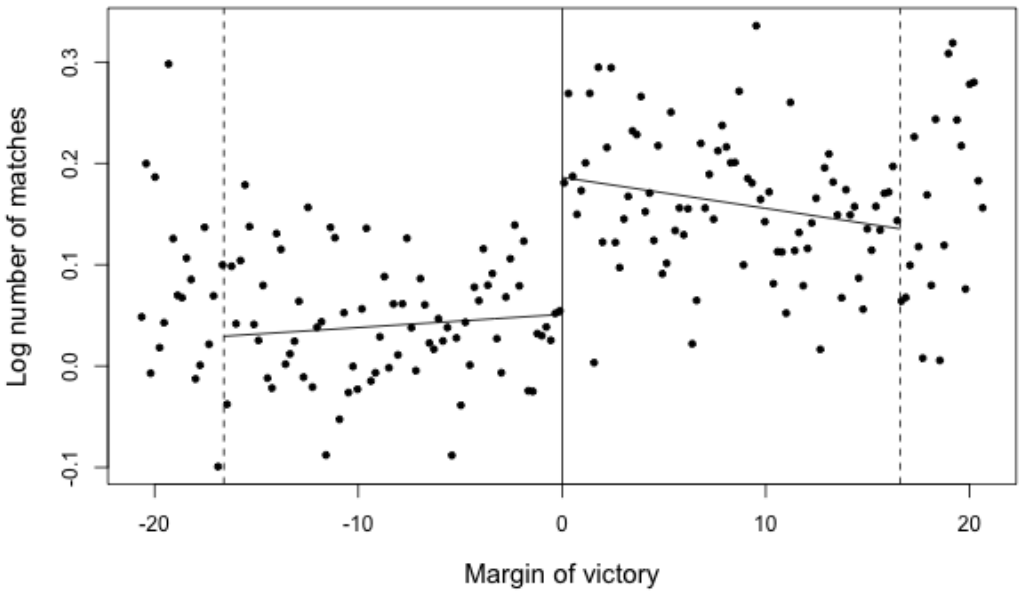
5.9.2 Non-polynomial fits graphs

5.9.3 Discontinuity on set of pre-determined covariates

All pre-determined covariates results substantially continuous at the threshold. Exceptions to the rule are for the 2008 election the Age, Education level and share of Males among winners that are all lower

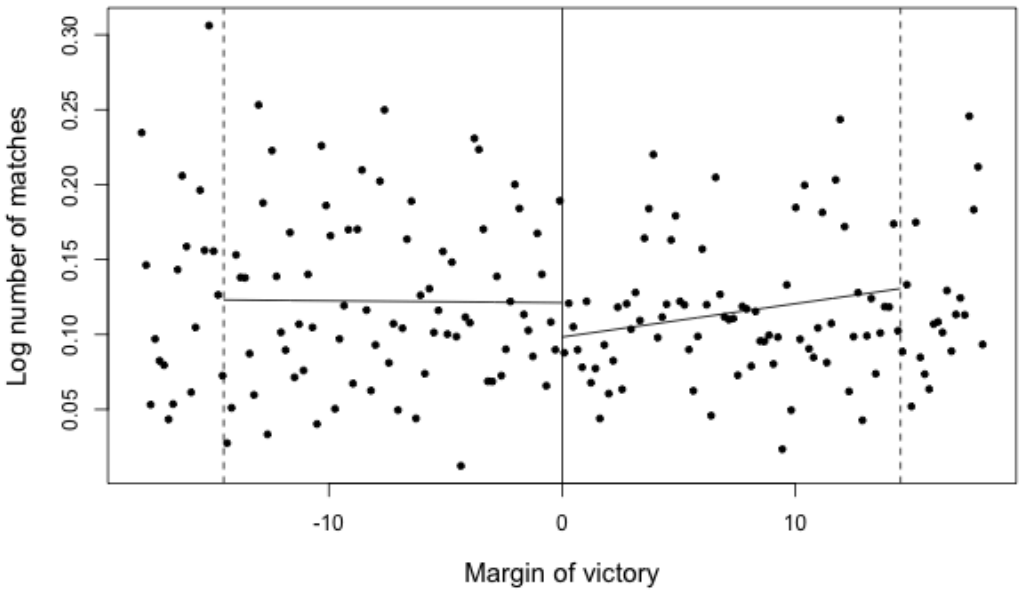


(a) Election 2004

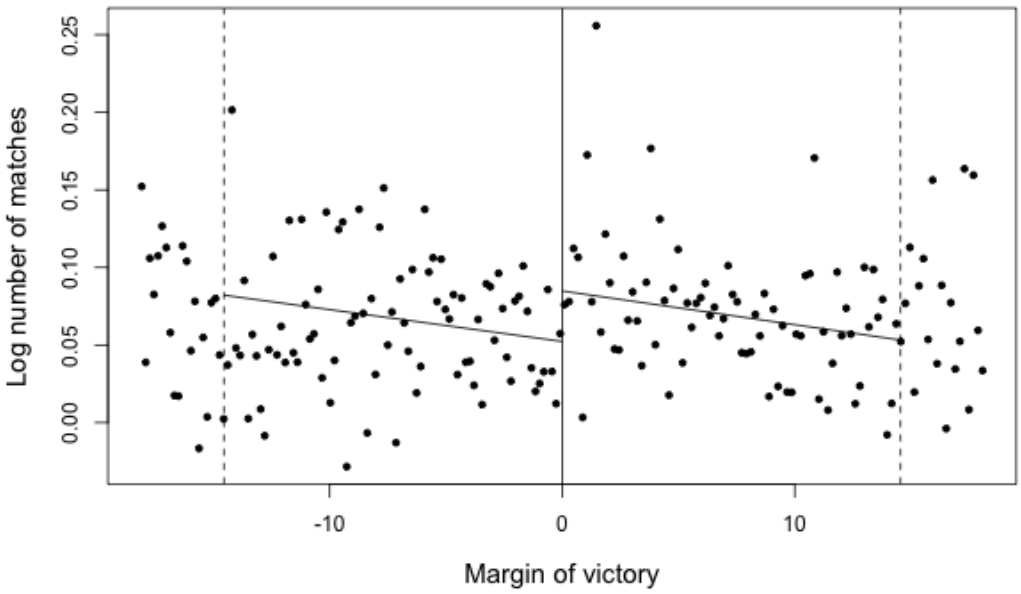


(b) Election 2008

Figure 5.8: LOCAL RDD - Number of matches within municipalities



(a) Election 2004



(b) Election 2008

Figure 5.9: LOCAL RDD - Number of matches across municipalities

than than their counterpart. Despite the result, no major jump at the threshold is registered at the threshold for global, non parametric estimates.

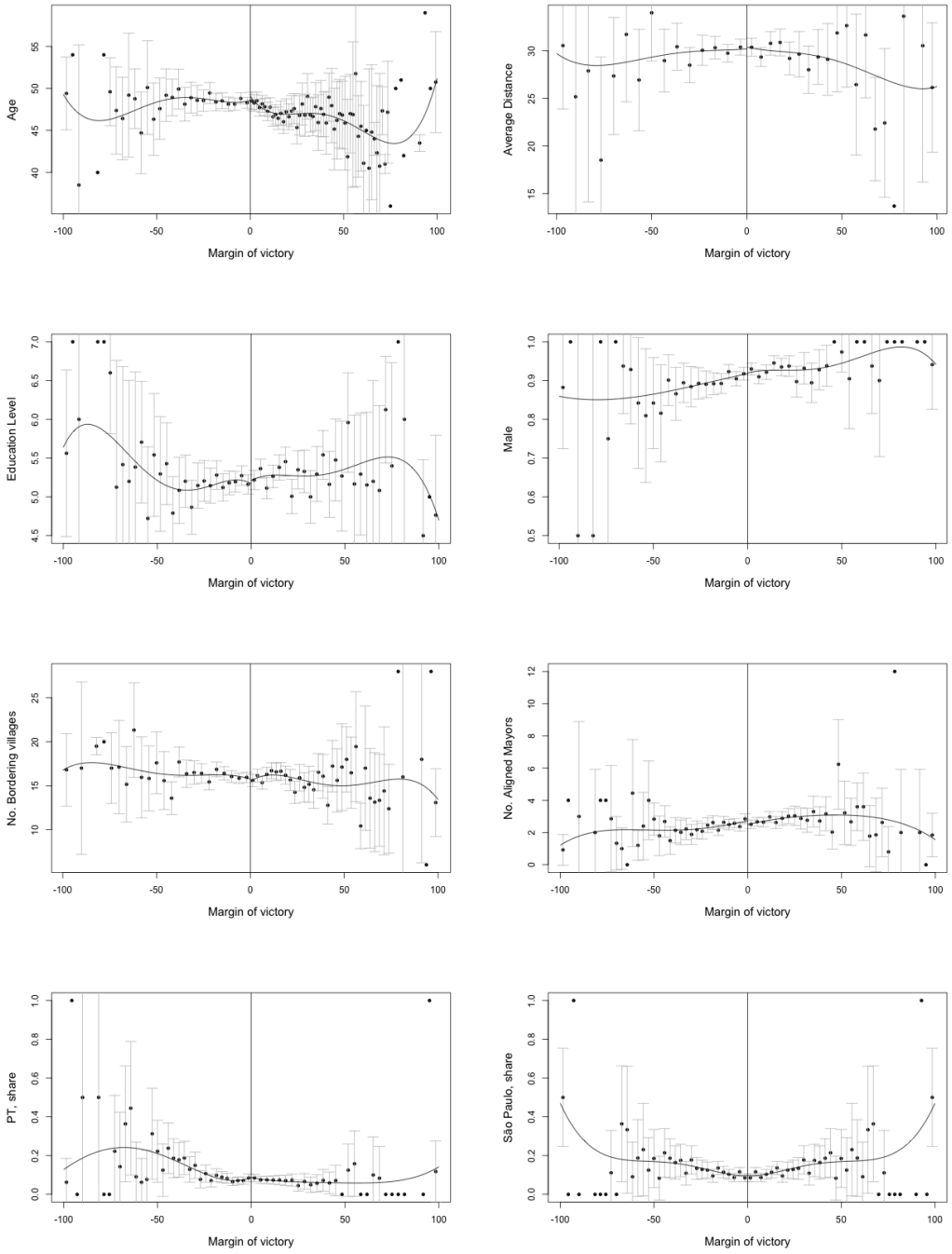


Figure 5.10: Discontinuity on pre-determined covariates - 2004

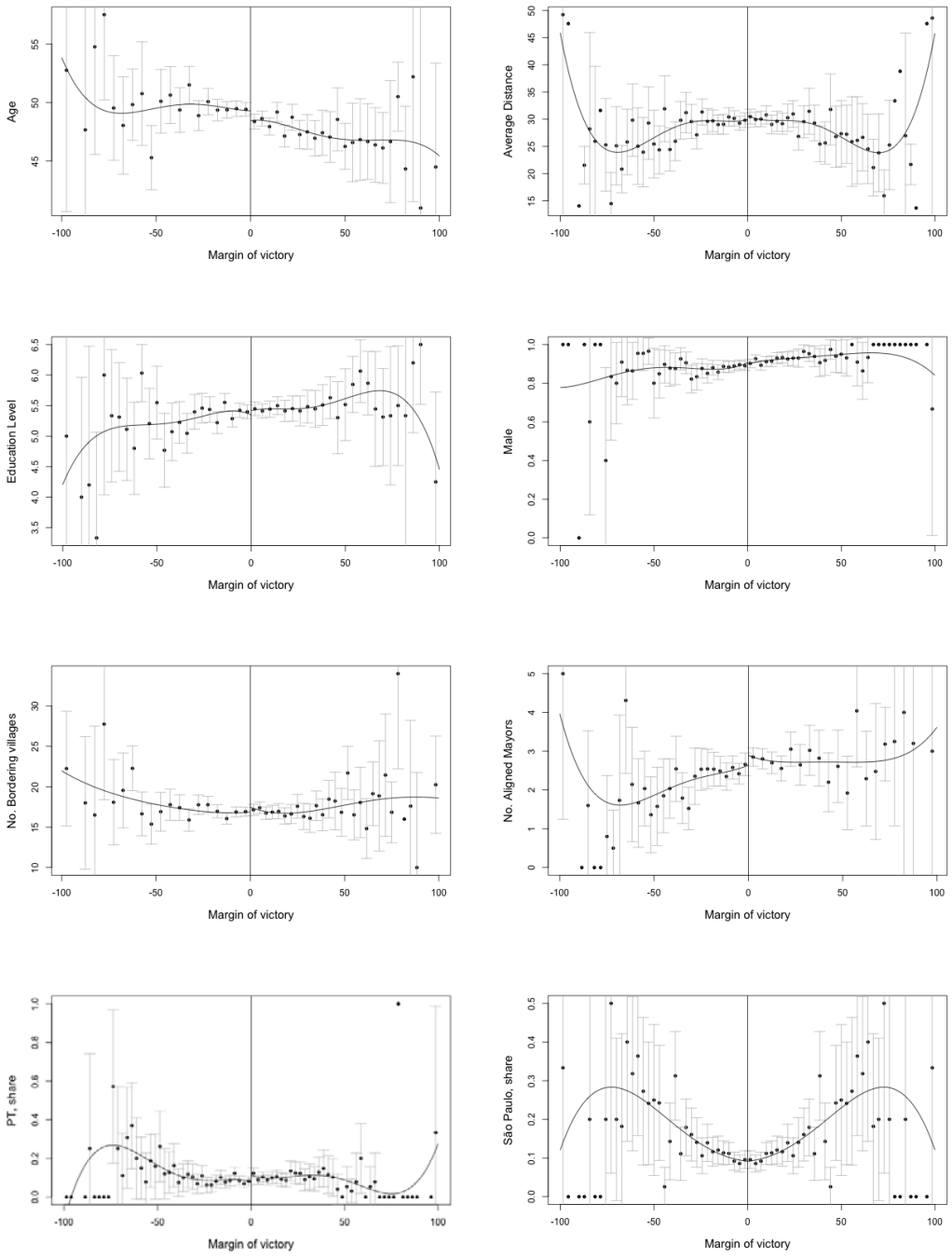


Figure 5.11: Discontinuity on pre-determined covariates - 2008

5.9.4 Re-estimation of main regressions

The key regressions are re-estimated dropping entries with exceedingly frequent last name tokens. Results remain robust across both sub-samples and specifications. Exception is 2008 across municipalities measure that becomes statistically not significant when considering only surnames below the 60th percentile. Reported estimates are for basic regressions with S.E. clustered at the municipal level.

5.9.5 Placebo tests

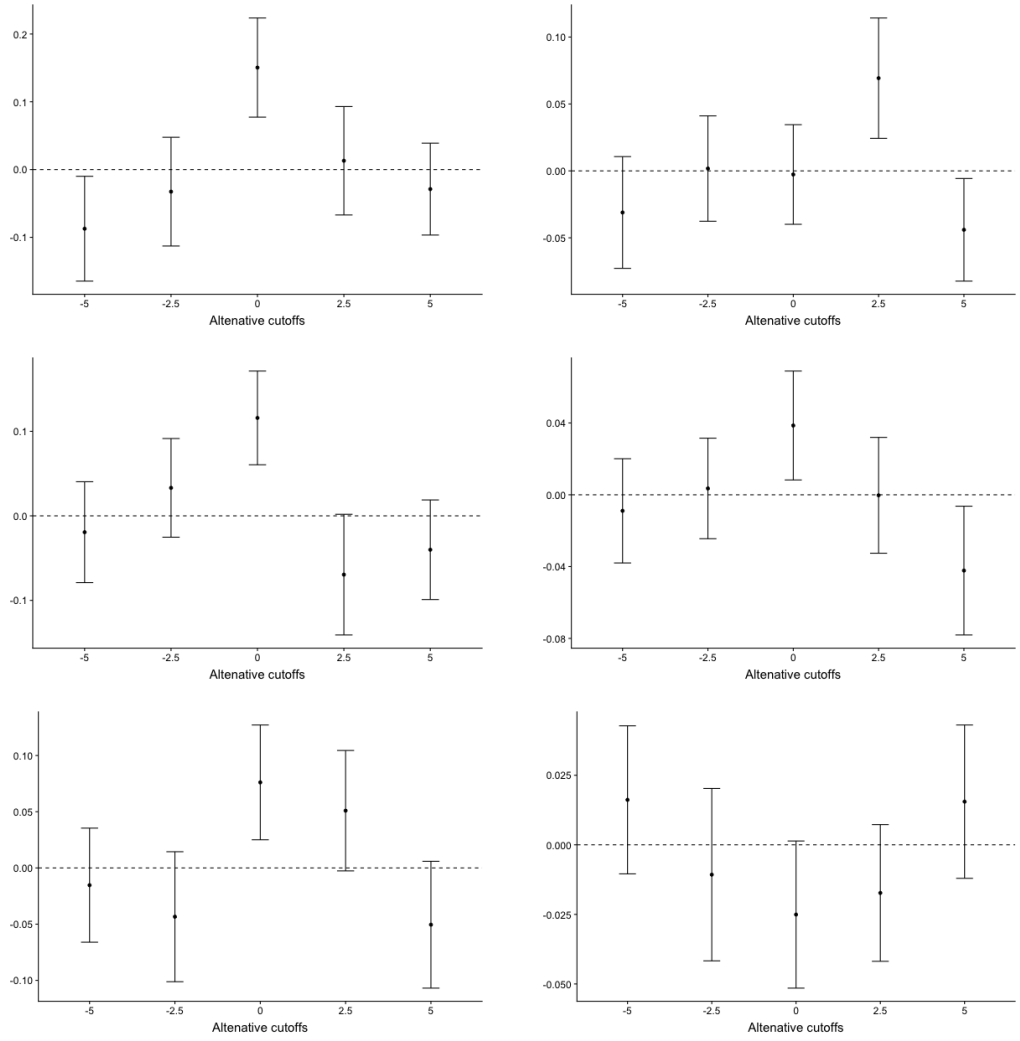


Figure 5.12: Placebo test: estimation on altenative cutoffs

5.9.6 Placebo tests - Alternative Bandwidth

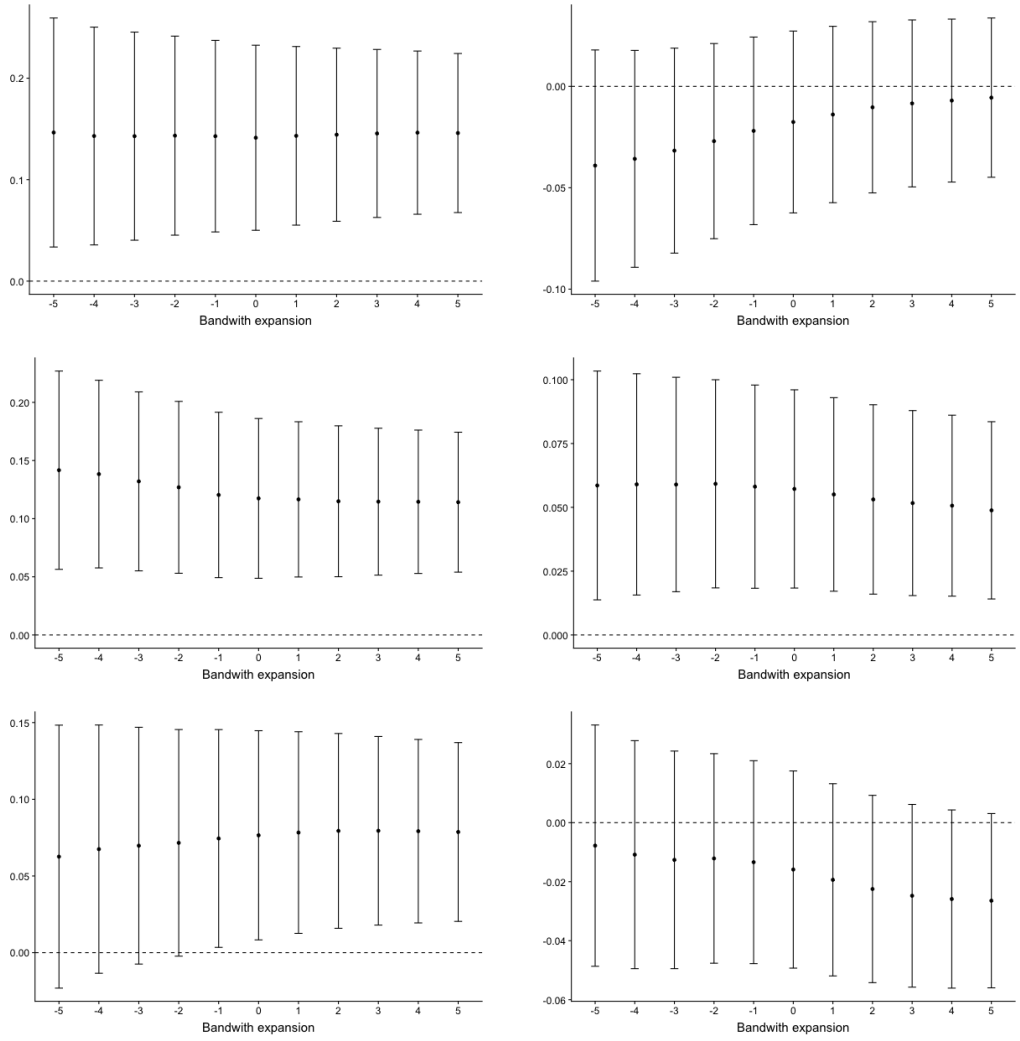


Figure 5.13: Placebo test: estimation with altenative bandwidth

5.9.7 Robustness check - Alternative donuthole

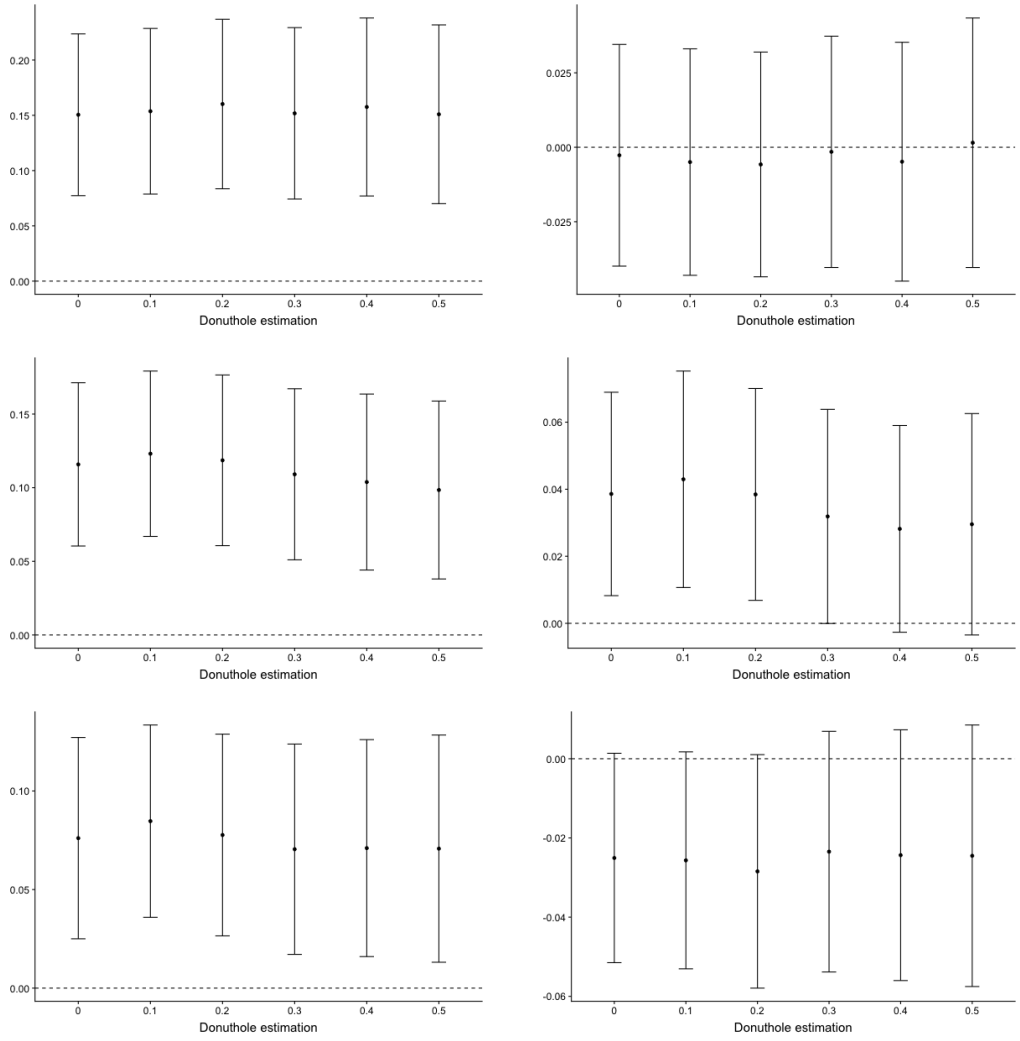


Figure 5.14: Placebo test: estimation with altenative donutholes

5.9.8 Placebo tests - Alternative Frequency Selection

	<90 cnt.	<80 cnt.	<70 cnt.	<60 cnt.	<50 cnt.
Coeff.	0.146	0.178	0.200	0.200	0.232
S.E.	0.027	0.027	0.030	0.035	0.048
z	5.403	6.657	6.616	5.803	4.855
P>|z|	0.000	0.000	0.000	0.000	0.000
95% C.I.	0.093 , 0.199	0.126 , 0.230	0.141 , 0.259	0.133 , 0.268	0.138 , 0.325
N	4962 , 5149	3944 , 4148	2706 , 2959	1615 , 1786	784 , 917
N (effective)	2893 , 3039	2523 , 2669	1701 , 1842	1089 , 1170	546 , 619
h	14.02	15.98	15.32	16.76	17.25
b	24.22	27.45	27.65	30.06	26.98
p	1	1	1	1	1
p (bias)	2	2	2	2	2
Cluster	YES	YES	YES	YES	YES

Table 5.14: Re-estimation by lastname frequency - Election 2004 within municipalities

	<90 cnt.	<80 cnt.	<70 cnt.	<60 cnt.	<50 cnt.
Coeff.	0.139	0.137	0.133	0.145	0.138
S.E.	0.020	0.023	0.027	0.028	0.039
z	7.063	6.030	4.896	5.176	3.515
P>|z|	0.000	0.000	0.000	0.000	0.000
95% C.I.	0.100 , 0.178	0.092 , 0.181	0.079 , 0.186	0.090 , 0.200	0.061 , 0.216
N	4589 , 5253	3722 , 4363	2614 , 3060	1544 , 1864	743 , 961
N (effective)	3050 , 3680	2251 , 2789	1564 , 1920	995 , 1220	479 , 601
h	18.17	15.36	15.07	16.42	15.22
b	30.69	25.99	27.25	29.06	23.33
p	1	1	1	1	1
p (bias)	2	2	2	2	2
Cluster	YES	YES	YES	YES	YES

Table 5.15: Re-estimation by lastname frequency - Election 2008 within municipalities

	<90 cnt.	<80 cnt.	<70 cnt.	<60 cnt.	<50 cnt.
Coeff.	0.087	0.107	0.110	0.064	0.066
S.E.	0.019	0.019	0.024	0.027	0.037
z	4.640	5.535	4.670	2.368	1.804
P>|z|	0.000	0.000	0.000	0.018	0.071
95% C.I.	0.050 , 0.124	0.069 , 0.145	0.064 , 0.157	0.011 , 0.117	-0.006 , 0.138
N	4749 , 5292	3859 , 4332	2677 , 3081	1579 , 1880	782 , 949
N (effective)	3228 , 3750	3029 , 3446	1961 , 2278	1015 , 1241	521 , 637
h	17.82	23.23	19.37	15.87	16.20
b	29.30	39.03	31.25	30.77	30.87
p	1	1	1	1	1
p (bias)	2	2	2	2	2
Cluster	YES	YES	YES	YES	YES

Table 5.16: Re-estimation by lastname frequency - Election 2012 within municipalities

	<90 cnt.	<80 cnt.	<70 cnt.	<60 cnt.	<50 cnt.
Coeff.	-0.024	-0.030	-0.033	-0.029	-0.070
S.E.	0.014	0.014	0.016	0.017	0.022
z	-1.741	-2.081	-2.089	-1.720	-3.157
P>|z|	0.082	0.037	0.037	0.085	0.002
95% C.I.	-0.051 , 0.003	-0.058 , -0.002	-0.063 , -0.002	-0.061 , 0.004	-0.114 , -0.027
N	4962 , 5149	3944 , 4148	2706 , 2959	1615 , 1786	784 , 917
N (effective)	3092 , 3241	2532 , 2676	1723 , 1862	1061 , 1144	503 , 576
h	15.52	16.10	15.61	15.86	14.72
b	25.34	26.93	24.65	25.50	27.16
p	1	1	1	1	1
p (bias)	2	2	2	2	2
Cluster	YES	YES	YES	YES	YES

Table 5.17: Re-estimation by lastname frequency - Election 2004 across municipalities

	<90 cnt.	<80 cnt.	<70 cnt.	<60 cnt.	<50 cnt.
Coeff.	0.039	0.042	0.034	0.010	0.019
S.E.	0.011	0.012	0.013	0.013	0.015
z	3.434	3.440	2.655	0.781	1.223
P>|z|	0.001	0.001	0.008	0.435	0.221
95% C.I.	0.017 , 0.061	0.018 , 0.065	0.009 , 0.060	-0.016 , 0.037	-0.011 , 0.049
N	4589 , 5253	3722 , 4363	2614 , 3060	1544 , 1864	743 , 961
N (effective)	2390 , 3003	1933 , 2455	1509 , 1857	1085 , 1324	479 , 604
h	12.67	12.37	14.03	19.26	15.40
b	23.69	24.63	26.97	32.70	27.63
p	1	1	1	1	1
p (bias)	2	2	2	2	2
Cluster	YES	YES	YES	YES	YES

Table 5.18: Re-estimation by lastname frequency - Election 2008 across municipalities

	<90 cnt.	<80 cnt.	<70 cnt.	<60 cnt.	<50 cnt.
Coeff.	-0.021	-0.007	0.000	0.010	0.018
S.E.	0.012	0.010	0.013	0.013	0.015
z	-1.677	-0.726	-0.035	0.787	1.240
P>|z|	0.094	0.468	0.972	0.431	0.215
95% C.I.	-0.045 , 0.004	-0.027 , 0.012	-0.026 , 0.025	-0.015 , 0.035	-0.011 , 0.047
N	4749 , 5292	3859 , 4332	2677 , 3081	1579 , 1880	782 , 949
N (effective)	3161 , 3679	2797 , 3225	1685 , 2009	991 , 1221	521 , 637
h	17.17	19.60	15.35	15.18	16.21
b	31.21	35.45	27.40	24.70	27.33
p	1	1	1	1	1
p (bias)	2	2	2	2	2
Cluster	YES	YES	YES	YES	YES

Table 5.19: Re-estimation by lastname frequency - Election 2012 across municipalities

5.9.9 Placebo tests - Alternative Cutoffs

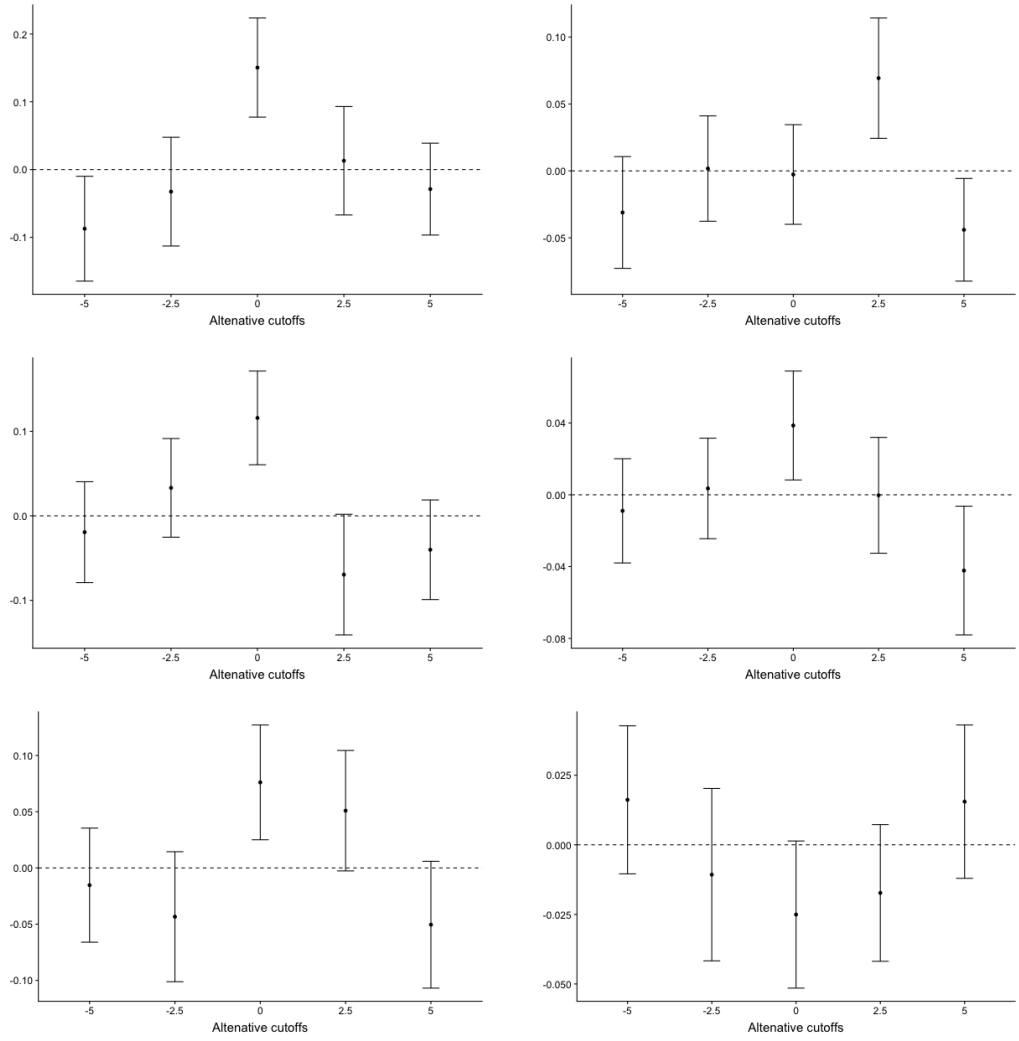


Figure 5.15: Placebo test: estimation on altenative alternative cutoffs

5.9.10 Placebo tests - Alternative races (2nd Vs. 3rd)

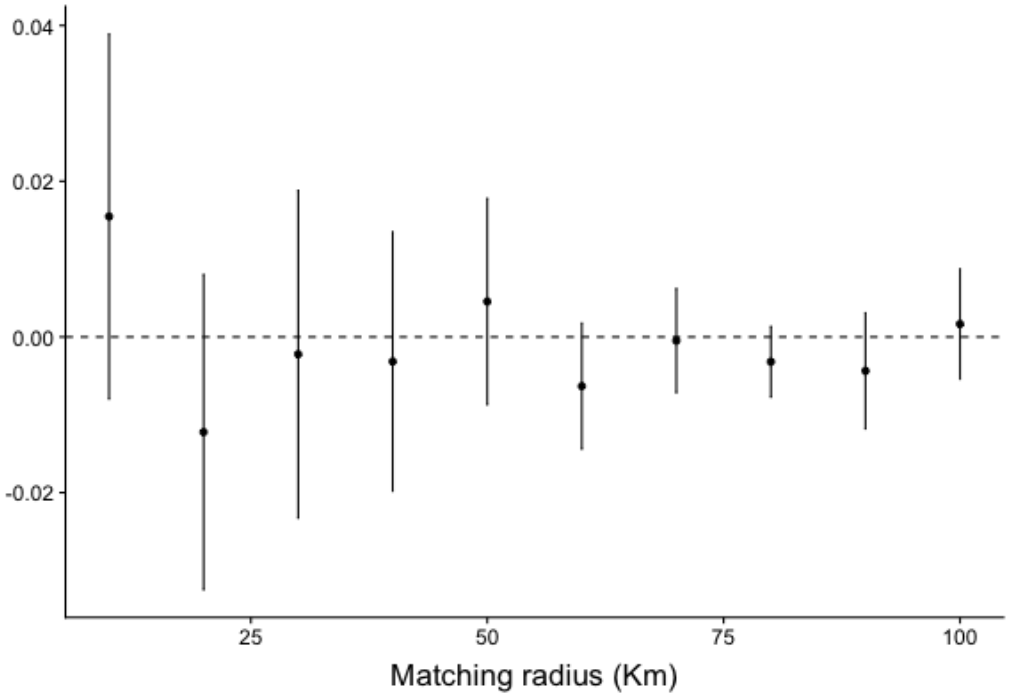
	Model (1)	Model (2)	Model (3)
Coeff.	-0.004	-0.004	-0.009
S.E.	0.019	0.019	0.021
z	-0.190	-0.190	-0.457
P>|z|	0.849	0.849	0.648
95% C.I.	-0.042 , 0.034	-0.042 , 0.034	-0.050 , 0.031
N	5079 , 5079	5079 , 5079	4234 , 4162
N (effective)	3455 , 3455	3456 , 3456	2784 , 2739
h	17.70	17.71	16.85
b	30.82	30.83	29.33
p	1	1	1
p (bias)	2	2	2
Cluster		YES	YES
Covariates			YES

Table 5.21: RD Estimates - Election 2004 across municipalities (2nd vs. 3rd)

	Model (1)	Model (2)	Model (3)
Coeff.	0.012	0.012	0.016
S.E.	0.017	0.017	0.018
z	0.725	0.725	0.898
P>|z|	0.468	0.468	0.369
95% C.I.	-0.020 , 0.044	-0.020 , 0.044	-0.019 , 0.052
N	4509 , 4509	4509 , 4509	3928 , 3794
N (effective)	2951 , 2951	2951 , 2951	2535 , 2458
h	17.26	17.27	16.97
b	30.79	30.80	30.48
p	1	1	1
p (bias)	2	2	2
Cluster		YES	YES
Covariates			YES

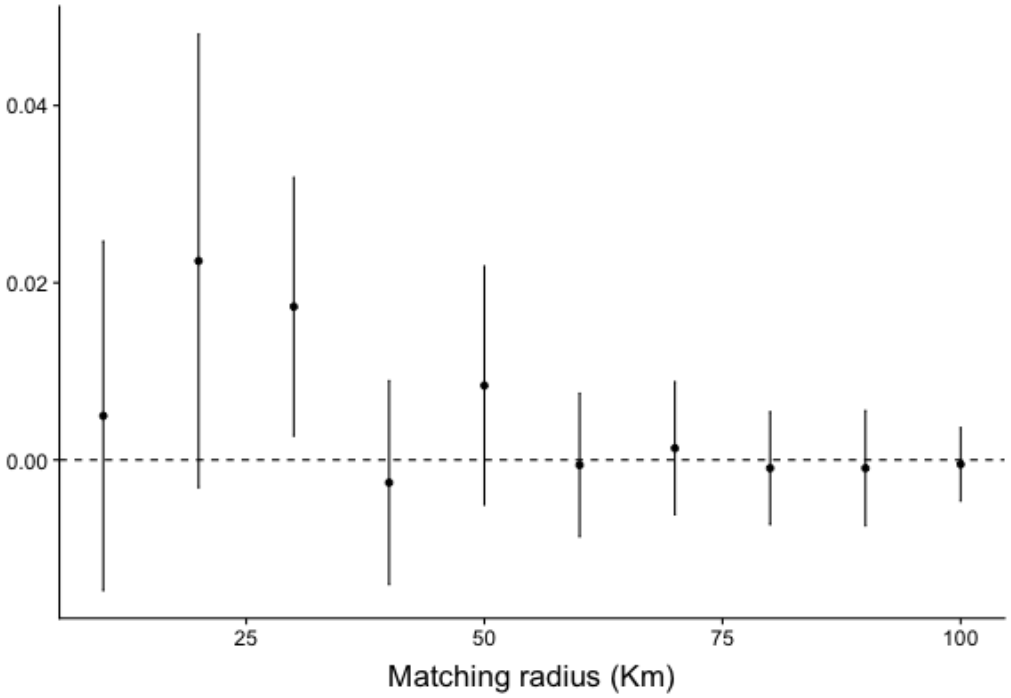
Table 5.22: RD Estimates - Election 2008 across municipalities (2nd vs. 3rd)

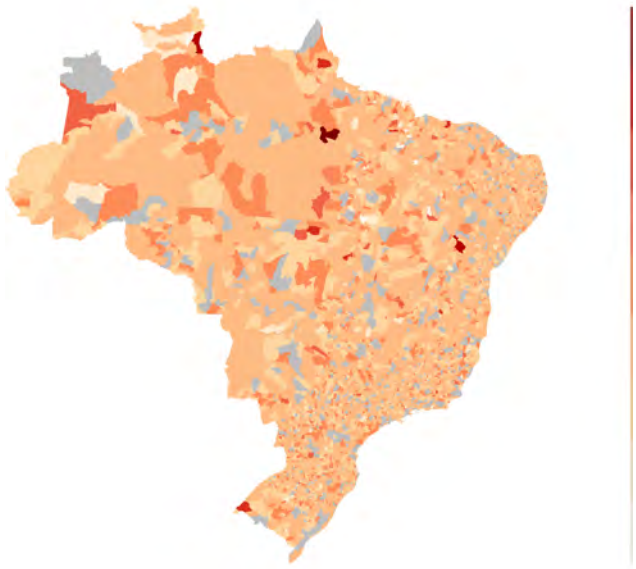
5.9.11 Further analyses - Radius subsection



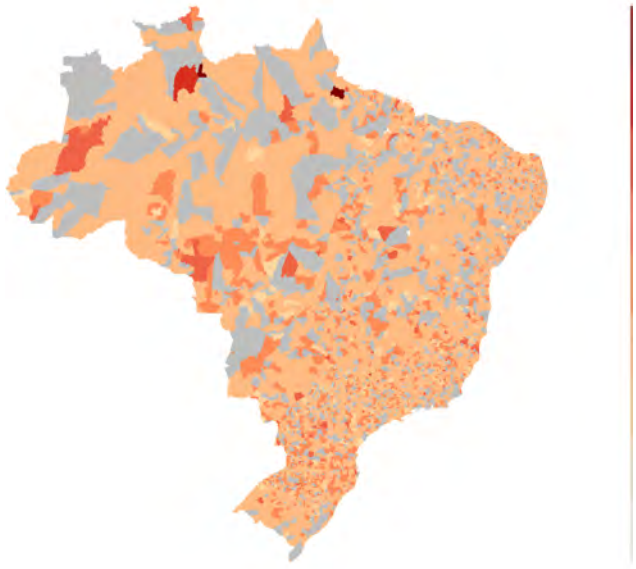
	Model (1)	Model (2)	Model (3)
Coeff.	0.018	0.018	0.010
S.E.	0.014	0.014	0.016
z	1.317	1.317	0.629
P>|z|	0.188	0.188	0.529
95% C.I.	-0.009 , 0.046	-0.009 , 0.046	-0.021 , 0.041
N	4870 , 4870	4870 , 4870	4219 , 4176
N (effective)	3444 , 3444	3446 , 3446	2955 , 2950
h	18.91	18.92	18.65
b	34.11	34.12	31.80
p	1	1	1
p (bias)	2	2	2
Cluster		YES	YES
Covariates			YES

Table 5.23: RD Estimates - Election 2012 across municipalities (2nd vs. 3rd)





(a) Election 2004



(b) Election 2008

Figure 5.16: Geographical clustering of log nepotist capture (Nepotism)

(a) Election 2004

(b) Election 2008

Figure 5.17: Geographical clustering of log nepotist capture (Cross-Nepotism)

5.9.12 Further analyses - Frequency reduction

	<90 cnt.	<80 cnt.	<70 cnt.	<60 cnt.	<50 cnt.
Coeff.	0.146	0.178	0.200	0.200	0.232
S.E.	0.027	0.027	0.030	0.035	0.048
z	5.403	6.657	6.616	5.803	4.855
P>|z|	0.000	0.000	0.000	0.000	0.000
95% C.I.	0.093 , 0.199	0.126 , 0.230	0.141 , 0.259	0.133 , 0.268	0.138 , 0.325
N	4962 , 5149	3944 , 4148	2706 , 2959	1615 , 1786	784 , 917
N (effective)	2893 , 3039	2523 , 2669	1701 , 1842	1089 , 1170	546 , 619
h	14.02	15.98	15.32	16.76	17.25
b	24.22	27.45	27.65	30.06	26.98
p	1	1	1	1	1
p (bias)	2	2	2	2	2
Cluster	YES	YES	YES	YES	YES

Table 5.25: Re-estimation by lastname frequency - Election 2004 within municipalities

	<90 cnt.	<80 cnt.	<70 cnt.	<60 cnt.	<50 cnt.
Coeff.	0.139	0.137	0.133	0.145	0.138
S.E.	0.020	0.023	0.027	0.028	0.039
z	7.063	6.030	4.896	5.176	3.515
P>|z|	0.000	0.000	0.000	0.000	0.000
95% C.I.	0.100 , 0.178	0.092 , 0.181	0.079 , 0.186	0.090 , 0.200	0.061 , 0.216
N	4589 , 5253	3722 , 4363	2614 , 3060	1544 , 1864	743 , 961
N (effective)	3050 , 3680	2251 , 2789	1564 , 1920	995 , 1220	479 , 601
h	18.17	15.36	15.07	16.42	15.22
b	30.69	25.99	27.25	29.06	23.33
p	1	1	1	1	1
p (bias)	2	2	2	2	2
Cluster	YES	YES	YES	YES	YES

Table 5.26: Re-estimation by lastname frequency - Election 2008 within municipalities

	<90 cnt.	<80 cnt.	<70 cnt.	<60 cnt.	<50 cnt.
Coeff.	-0.024	-0.030	-0.033	-0.029	-0.070
S.E.	0.014	0.014	0.016	0.017	0.022
z	-1.741	-2.081	-2.089	-1.720	-3.157
P>|z|	0.082	0.037	0.037	0.085	0.002
95% C.I.	-0.051 , 0.003	-0.058 , -0.002	-0.063 , -0.002	-0.061 , 0.004	-0.114 , -0.027
N	4962 , 5149	3944 , 4148	2706 , 2959	1615 , 1786	784 , 917
N (effective)	3092 , 3241	2532 , 2676	1723 , 1862	1061 , 1144	503 , 576
h	15.52	16.10	15.61	15.86	14.72
b	25.34	26.93	24.65	25.50	27.16
p	1	1	1	1	1
p (bias)	2	2	2	2	2
Cluster	YES	YES	YES	YES	YES

Table 5.27: Re-estimation by lastname frequency - Election 2004 across municipalities

	<90 cnt.	<80 cnt.	<70 cnt.	<60 cnt.	<50 cnt.
Coeff.	0.039	0.042	0.034	0.010	0.019
S.E.	0.011	0.012	0.013	0.013	0.015
z	3.434	3.440	2.655	0.781	1.223
P>|z|	0.001	0.001	0.008	0.435	0.221
95% C.I.	0.017 , 0.061	0.018 , 0.065	0.009 , 0.060	-0.016 , 0.037	-0.011 , 0.049
N	4589 , 5253	3722 , 4363	2614 , 3060	1544 , 1864	743 , 961
N (effective)	2390 , 3003	1933 , 2455	1509 , 1857	1085 , 1324	479 , 604
h	12.67	12.37	14.03	19.26	15.40
b	23.69	24.63	26.97	32.70	27.63
p	1	1	1	1	1
p (bias)	2	2	2	2	2
Cluster	YES	YES	YES	YES	YES

Table 5.28: Re-estimation by lastname frequency - Election 2008 across municipalities

The color of the public purse. Ideological camp effects on resource misallocation

6.1 Abstract

The political determinants of public resource allocation are a topic of central importance for both the political science literature and public interest. Despite the relevance of this topic for any government, the effect that ideological affiliation has on budgetary decisions remains substantially under-explored at the local level. Given the paramount importance of local government for individuals' experience of the state and for a country's administration, the topic calls for renewed attention. This study advances the discussion on ideology-driven resource allocation and presents previously unavailable evidence regarding the degree of mismanagement of public resources across ideological camps locally. The use of accounting tricks that surreptitiously dismantle spending constraints - at the cost of debt accumulation and at the risk of economic default - proxies the phenomenon. The present study focuses on Italian local government elected over the decade 2001-2010. It exploits a data-driven Regression Discontinuity design on margins of victory at the municipal level, comparing camps pairwise - including non-ideological camps. Non-ideological administrations are expected to fare better than their counterparts, but causal evidence of this is not found in the data. Moreover, locally liberal and conservative camps do not differ in the abuse of accounting instruments, which is in line with a substantial part of the literature. Overall, this piece sheds light on unexplored budgetary mechanics underpinning government consumption and advances the discussion about the costs of reproducing an ideologically driven public purse configuration at different levels of government; presenting a novel setting with a unique ideological camp configuration, it contributes substantially to an emerging literature worldwide.

6.2 Introduction

The assumption that ideological preferences direct governments' actions underpins all democratic practices and informs the common understanding of how political parties work. Ideological determinants of government consumption have long been at the core of political economy discussions (Persson and Svensson 1989; Alesina and Tabellini 1990) and debates in the public realm. If party affiliation – within reason – guides politicians' choices at the national level (Aldrich 1995), it remains unclear to what extent this occurs at lower levels of government. Among other things, the scholarship fails to shed light upon the cost of replicating partisan policies locally, where governments face tighter resources and have limited scope as well as shorter time-horizons. Partisan impact on policy decisions at the municipal level seems to be extremely context-specific (Pettersson-Lidbom 2008; Ferreira and Gyourko 2009; Casal, Gómez, and Liste 2014), and the literature openly calls for further attention to be paid to the topic.

The present study contributes to this debate by testing a standard set of hypotheses on budgetary allocation choices, although it does so by focusing on more fundamental budgetary decisions regarding the use of accounting instruments with dire potential effects on municipal financial health. Such instruments – that can be reduced to the excessive commitment of receivables that are unlikely to turn into cash flows – are not an abstract financial health construct (*See*: Casal, Gómez, and Liste 2014) and straightforwardly translate into public resource mismanagement, the abuse of which is well known to, and openly used by the political actors who are directly involved in these practices. I focus on Italian local government which displays a significant degree of freedom in budgetary decision-making with respect to higher levels of government, resource collection, and resource management, so that single administration responsibility are sufficiently clear. Italy is, moreover, chosen as a context worth studying for two additional reasons: on one side, Italy displays a significant number of non-ideologically leaning local governments which allows for hypothesis-testing beyond standard ideological camps. On the other, the accounting tricks at the core of the present study are made extremely compelling by

the financial status of the whole country: in dire economic times, the abuse of the accounting practices under study here is particularly hard to justify (legally and otherwise), thus making their abuse a clear sign of wilful mismanagement of the public purse.

In the study, I exploit a Regression Discontinuity (RD) design on margins of victory at the municipal level, comparing Italian ideological camps pairwise. This allows for a causal exploration of the link between ideological ruling and adoption of good accounting practices in government. In particular, it helps in exploring the connection between non-ideological management and good practices, which is usually taken for granted but is plagued by endogeneity issues. The results reveal that besides liberal and conservative camps not differing with regards to the abuse of accounting instruments, in line with a substantial part of the literature, I also find that non-ideologically leaning parties also do not fare any better than their ideological counterparts. This finding opens a discussion regarding the cost at which parties pursue their different policy preferences and regarding the limitation for non-ideological camps to respond to financial health requirements. The identification strategy allows me to exclude the relevance of other factors besides ideological affiliation, but leaves room to explore differences across race types and within the country.

Given that local-level politics represent the primary experience of government workings for many people, the relative lack of discussion regarding the conditions under which ideological preferences are translated into policy decisions at the municipal level is problematic. The paper thus contributes to a growing literature addressing this missed opportunity, exploring a novel and insightful case.

The rest of the paper is organized as follows. In Section 6.3, I present a brief literature review on partisan impact over policy decisions. In Section 6.4, I provide details about the relevant institutional context, especially with respect to local election and budgeting dynamics. In Section 6.5, I present an overview of the data employed. In Section 6.6, I present the empirical strategy and the main results. Section 6.7 concludes.

6.3 Background and Theory

The paper discusses how the ideological affiliation of local-level politicians in Italy affects the degree of budget mismanagement, in this way it contributes to the substantial literature on the political determinants of malfeasance. The scholarship relies heavily on cross-country studies and devotes its attention mainly to explicitly corrupt practices. It focuses mainly on the role of legal systems (Treisman 2000; Persson and Tabellini 2005), of electoral rules (Persson, Tabellini, and Trebbi 2003; Kunicova and Rose-Ackerman 2005; Chang and Golden 2007), and of other political institutions (Ades and Di Tella 1999; Lederman, Loayza, and Soares 2005; Testa 2010). However, this literature seldom considers local-level determinants of malfeasance. Some exceptions focus on the local effect of institutional changes, involving specifically federalization processes (Fisman and Gatti 2002; Prud'homme 1995), or monitoring initiatives (Ferraz and Finan 2011; Reinikka and Svensson 2005).

The present work contributes specifically to the debate on whether government's ideological leaning affects the economic and financial outcomes of local-governments. The debate appears to be inconclusive thus far and, at best, extremely setting-specific. From the studies based on solid methodological underpinnings,[1] two contradictory findings have emerged: on one hand, Pettersson-Lidbom 2008, using a large panel data set from Swedish local governments, arrives at a positive conclusion regarding the relevance of party labels for economic and financial outcomes. Focusing on major economic outcomes more than budgetary allocation choices, the paper shows that liberal governments spend more, have lower unemployment rates, and employ more people. On the other hand, Ferreira and Gyourko 2009 arrives at the opposite conclusion, using US data. This paper uses information from

1. *See*: Ferreira and Gyourko 2009 for a list of studies that are methodologically weak and, thus, excluded from the present discussion.

a novel panel data base of mayoral elections to find no evidence of a strong partisan influence on a number of budgetary decisions (e.g. the size of city government, the allocation of local public spending across important functions, or on property or violent crime rates). Both pieces use a RD design, to control for endogeneity issues related to which party wins mayoral office. In line with the latter, Casal, Gómez, and Liste 2014 discusses how various political factors influence the financial situation of Spanish municipalities, to conclude that conservative and progressive parties do not present different behaviors in relation to any financial dimension. This project is especially interesting as it moves away from measuring some specific budgetary choice dimensions to develop a synthetic global health measure of public institutions, despite using a weak identification strategy as compared to the other two papers I have cited.

The present inquiry contributes to the debate advancing the analysis of a setting that presents some important differences with respect to the other three. In particular, studying municipal level government in Italy allows one to study camp effects non only across opposite, ideologically charged sides, but also across ideological and non-ideological ones given the widespread presence of municipal lists that are not supported by, nor affiliated to, official political parties and that often emerge on the political stage to provide a purposefully non-ideological alternative to the ideologically-directed administration. In such a context, the study of ideological camp effects can become tripartite: comparing liberals to conservatives, but also both to non-ideological governments. This has the potential to test some hypotheses regarding the role, quality and scope of non-ideological governments – often depicted as a superior solution to any ideologically-driven resources allocation decision.

A second important feature of the Italian setting is the way in which local accounting is managed, as it provides some straightforward measures of financial mismanagement. These are far from being abstract measures, susceptible to the way in which financial data are pooled, and are simple accounting statistics that are known and managed directly by the respective administration. These statistics

measure the extent to which administrations tamper with (if not overtly cook) books and can be taken, as will be explained, as a measure of wilful mismanagement of public resources – especially in the context of one of the most serious local debt crises experienced by a Western country to date (Trovati 2017, 2018; Netti 2018).

The method used borrows heavily from the recent accounting literature on municipal fiscal responsibility in Italy (Barbera, Guarini, and Steccolini 2016; Barbera et al. 2017), which is part of a global trend of research on fiscal discipline and the sustainability of local-level government, embraced both by scholars in political science (Rodden et al. 2003) and international organizations (Ianchovichina, Nagarajan, and Liu 2006; Canuto and Liu 2010; Liu and Webb 2011). The politicization of fiscal performance malfeasance is not discussed in terms of retrospective voting behavior (Brender 2003; Bågenholm 2009), but is presented as a function of party ideology, and the organizational features that descend from it. This is a novel and insightful approach to ideological camp-effects, that it is worth analyzing for its compelling real world implications. The article connects political malfeasance with public debt accrual. In this, it contributes to the debate that tries to untangle the two phenomena globally (Cooray, Dzhumashev, and Schneider 2017) and at the country level (Del Monte and Papagni 2001). The paper marginally addresses the literature on the effectiveness of fiscal rules vis-á-vis ideological and political incentives (Persson and Tabellini 2002; Maskin and Tirole 2004; Grembi, Nannicini, and Troiano 2016).

A vast literature in public economics discusses the role of fiscal rules' implementation on local administration budgetary choices and behavior (see Heinemann, Moessinger, and Yeter 2018; Wyplosz 2012; Gregori 2014 for a detailed overview). Of tangential interest for the present study is the discussion regarding how fiscal rules affect the likelihood of over-borrowing (Dovis and Kirpalani 2017), and deficit spending when targeting budget balance (Debrun et al. 2008), and to a point composition decisions (Foremny 2014; Grisorio and Prota 2015). Despite thoroughly discussing the direct effects of such

policies across different contexts, the issue of accounting tricks that in practice hide debt accumulation and budget imbalances is never directly discussed. This constitutes another key contribution of the present piece by means of linking two disciplines discussing the same topic from different perspectives.

Overall, the present study tackles a set of hypotheses with reference to the Italian context, but these findings can be applied to a range of other settings outside of Italy. A question of paramount importance to address is to whether ideological camps differ in their financial management capacities within local governments, especially with reference to unsavory accounting decisions. As discussed, the study delves into whether ideological and non-ideological camps differ along the same dimension in a tripartite fashion. The hypotheses are underpinned by expectations over the impact of political affiliation in a short time horizon (e.g. ideologically driven budget allocation choices, heterogeneous ability to collect money from higher levels of government, etc.), or in long time horizon (e.g. political career-driven budget allocation choices, multiple principal dynamics, etc.), although these issues remain beyond the scope of the present study.

6.4 Institutional Context

6.4.1 Local government

In Italy there are around eight-thousand municipalities ruled by a mayor and a city council. In the time-frame considered,[2] mayors are directly elected under plurality rule, with a single round for municipalities below 15,000 inhabitants, and with a runoff system above that threshold. As described in Bordignon, Nannicini, and Tabellini 2016, below the population threshold, each party, or coalition of parties, presents one candidate for mayor and a list of candidates for the city council. Voters cast

2. Law 81/1993 marked the passage from a pure parliamentary system, where citizens voted for party lists under proportional representation to elect the legislative body (i.e., the city council), which, in turn, appointed the mayor and executive office.

a single vote for the mayor and his supporting list. The mayoral candidate who gets the most votes becomes mayor and her list gains 2/3 of all seats in the council. The remaining 1/3 of the seats are divided among the runner-up lists according to their vote shares, conditional on having gained more than 4% of the vote share. Above the 15,000 threshold, parties, or coalitions of parties, present lists of candidates for the council, and declare their support to a specific candidate for mayor. Each mayoral candidate can be supported by more than one list. There are two rounds of voting. In the first round, voters cast two votes, one for a mayoral candidate and one for a party list[3]. If a candidate for mayor gets more than 50% of the votes in the first round, he is elected, otherwise a run-off election is held in two weeks' time between the two leading candidates only (with other lists capable of displaying support for one of the two candidates). The rules for the allocation of council seats entail the same majority premium for the lists supporting the winning candidate for mayor. In the time-frame considered, the electoral mandate lasts 5 years[4].

In Italy, mayors are key figures in local government: they can appoint and dismiss executive officers at will and have the prerogative of shaping all municipal policies (Caciagli and Di Virgilio 2005). Mayors have a prominent role in determining budget allocation,[5] which is crucial for the present analysis as the political leaning of the mayor, not the whole council, will be used as a sorting criteria for a given administration (*See*: Section 6.4.2).

3. In case a preference is expressed for a list only, the preference is automatically extended to the candidate this list supports. The opposite is not true. The two votes may be disjoint.

4. A term might end early in three cases: i. a vote of no confidence from the city council; ii. resignation of the council; iii. judicial dissolution of the government upon evidence of collusion with organized crime. All three are rare events, accounting, respectively, for 1.11%, 1.69%, and 0.06% of cases in the 1993-2007 period over the universe of mandates (Bordignon, Nannicini, and Tabellini 2016)

5. The sectors on which local administration have specific competence are vast and range from general administration, justice, local police, public education (primary school and part of secondary school), culture, sport, tourism, local transportation, urban development, social sector, economic development, productive local services.

6.4.2 Local budgeting practices

In Italy, local governments' budgeting practices are defined within the framework set out in Law n. 367/2000 (*Law for Local Authorities*). The law discusses in detail the functions of local government and provides an overview of the instruments made available to fulfill them. On top of this, in 1999 the national government introduced a set of rules, the *Domestic Stability Pact*, that more strictly determined local budgeting practices. Of particular interest for the present study are three aspects of this last ruling: (*i*). the request for local municipalities to maintain a balanced budget, with the total amount of expenditures not exceeding the total amount of revenues; (*ii*). the possibility to impose local level revenue ceilings by the central government; and (*iii*). limits on the stock of debt or the issuance of new debt (*See*: Gregori 2014 for a complete overview of the rulings imposed by the Domestic Stability Pact).

In this legal framework, Italian municipalities adopt a traditional budgetary accounting system on a commitment-basis, in which an entity appropriates resources based on when the right or obligation to incur (future) recoveries or payments (Barbera, Guarini, and Steccolini 2016). In such a system, at the end of the fiscal year the surplus/deficit is calculated as the cash held, plus revenues that have not yet been received ('receivables', it.tr. *residui attivi*) and minus commitments yet to be paid (*residui passivi*), that enter positively and negatively in the surplus/deficit calculation (*Ibid.*). Examples of receivables are due fines and tickets[6], taxes, canons, and tariffs that a municipality is structurally incapable of collecting[7].

The increase via receivables is not abnormal, but substantial debt issues arise when these receivables become 'unsound': when these receivables are kept in the budget beyond the time they are reasonably

6. Far from being an irrelevant budget item, tickets are by far the single most missed source of revenues: in 2016 alone cities produced EUR 1.7bn in tickets and were capable of collecting only EUR 599m, 35.1% of the total (Trovati 2017)

7. The Italian court of Audits, refers that some outliers, such as Naples, have been able to collect only around the 1.75% of their receivables

expected to produce the corresponding cash flow. The use of this blunt accounting trick is not inconsequential. During the time period considered by this study, local Italian finances were in an unprecedented, and unparalleled state of disarray. A EUR 23bn budget gap – amounting to *half* of the overall municipal current expenditures – [8] came specifically in the form of an implicit debt (Netti 2018). Such debt, if settled, would substantially undercut local governments' ability to support expenditures and would lead several municipalities to bankruptcy (Bordignon 2017).

Explicit regulation[9] and general good practice require administrators to write off 'unsound' receivables from municipal balance sheets (Barbera, Guarini, and Steccolini 2016; Barbera et al. 2017).[10] However, in Italy the overestimation of 'unsound' receivables is key to allowing for the budgeting of much sought-after additional expenditures over the following fiscal year. The surreptitious inflation of expected revenues is a well-known accounting trick upon which local administrations can rely (Barbera, Guarini, and Steccolini 2016). For all these reasons, the presence and accrual of 'unsound' receivables over time is taken presently as a measure of wilful budget mismanagement, ascribed to a clear political will that goes beyond bureaucratic decision-making. It will be used as my core measure of the financial health of a municipality.

8. In 2014, the debt stock was at EUR 19.8bn, but grew by around 15% between 2015 and 2016 (*See*: study by Cerved Centre in Netti 2018).

9. In recent years – beyond the data available for the present analyses – Italian municipalities were explicitly required to check the subsistence of their revenues and commitments before accounting for the annual surplus/deficit and withdraw them from the balance if they are not expected to produce the corresponding cash flow (Barbera, Guarini, and Steccolini 2016 ref. Corte dei Conti, 2013, pp. XII-XIII, 466). The initiative has some limitations that I unfortunately do not have room to expand upon here.

10. The Italian General Accounting office after having imposed the removal of EUR 29.3 bn in certified 'unsound' receivables from public budgets, but estimates that there are still EUR 30.9bn in receivables in them (Trovati 2017)

6.5 Data

6.5.1 Local Election Data

Local election data have been collected by web-scraping the Italian Ministry for Internal Affairs' website (`http://www.elezionistorico.interno.gov.it/`). In the period considered – for races taking place between 2001 and 2010 – there are 12,385 municipal races involving 33,418 candidates, over 15 regions[11]. The web archive reports for each race statistics regarding contenders and results. Besides the name of the candidates and of the supporting lists, the number of votes, the share of votes (for each round, in the cases specified in Section 6.4.1), the seats obtained, and general race information (such as the number of voters, the number of effective voters, and the number of invalid or blank ballots) are reported. Logos of the list are also always present[12].

Of fundamental importance to the present analysis is the ability of the researcher to ascribe each supporting list to a clearly defined ideological camp or to ascribe it to a non-ideological one. For what concerns the ascription to an ideological camp, a minimal ('exclusive') ascription procedure is adopted. Lists are ascribed to one few ideological camps only if there is an explicit expression linking the list of an ideologically connoted national party. These groups are: *Left*, *Center-left*, *Center*, *Center-Right*, and *Right*[13]. As a robustness check, a less-than-minimal ascription procedure is tested ('Inclusive'). Lists are ascribed to one of 5 ideological camps if they are affiliated to a national party but *also* in cases their

11. A set of regions are missing from the Ministry's database from 2005 onward – when they acquired 'Special Statute' statue, they dropped from the database. These are: Friuli-Venezia Giulia, Sardinia, Sicily, Trentino-Alto Adige/Südtirol, and Val d'Aosta/Vallée d'Aoste. Further basic cleaning has been conducted of the dataset, such as dropping shorter that normal mandates, and invalid elections.

12. Further work will be directed to use neural networks to distinguish the ideology of each list using the logo, with sorting relying on symbols (e.g. the Italian flag, for nationalistic conservative parties) and the color palettes (e.g. the light blue for list close to the Berlusconi-led conservative coalition: *'gli azzurri'*) is central to the brand identity of ideological camps. This is not presently explored and the sorting uses uniquely the lists' names.

13. For ease of notation, these are reported in Tables 6.1 and 6.2 as: 'SX', 'CSX', 'CEN', 'CDX', and 'DX'. There, 'CIV' indicates non-ideological candidates and 'OTHER', anti-establishment ones without a clear ideological connotation, such as the then emerging Five Stars Movement..

name uses ideologically connoted language, identified by a simple and reproducible expert-coding procedure.[14] Analyses that rest on the second coding procedure are not reported as they do not differ from the more basic ones. They can be used to discuss the relative value of official party endorsement, although this is not part of the present analysis[15]. In the 'Exclusive' allocation of the 32,851 unique list names, 95.6% were non-ideological lists (SX: 0.39%, CSX: 1.13% , CEN: 0.52%, CDX: 1.77%, DX: 0.17%), in the 'Inclusive' only 90.2% (SX: 1.19%, CDX: 2.54% , CEN: 1.56%, CDX: 3.85%, DX: 0.24%).

Once each list is attributed to an ideological camp, the candidates' ideal points are calculated. Camps are given a score by positioning them at equal distance along a range. Lists belonging to a camp are given the respective camp score. Candidates' ideal points are calculated averaging these scores using the relative lists' vote share as a weighting scheme[16]. In this calculation, even supporting lists below the 4% threshold are kept in the sample as they give nonetheless a solid signaling of the ideological leaning of the candidate they support. Finally, candidates are attributed to a camp giving their ideal score and a segmentation of the spectrum.[17]. With respect to non-ideological camp a negative definition is adopted: any list not assigned to any of the camps following the first procedure is deemed non-ideological, or 'civic', following the Italian jargon ('*liste civiche*'). Only candidates supported exclusively by non-ideological lists are deemed truly non-ideological.

In assigning lists to camps, it is taken into consideration that its name is not always a clear giveaway of a list's ideological positioning, as the information may be otherwise disclosed to the voters. As a result, relying only on names to assign lists to camps, one would overestimate the amount of non-ideological lists in the sample. A way around this is to assign the best non-ideological list to one ideological camp,

14. To give an example: the made-up list '*Comunisti di Milano*' would be assigned to the Left camp in the 'Inclusive' definition but not in the 'Exclusive' one, as the name of the list does not refer to a formalized party but presents one or more ideology-revealing terms. On the contrary, '*Rifondazione Comunista*' would (is) ascribed to the Left camp under both definitions.

15. These analyses are available upon request, along with the coding procedure.

16. Robustness checks are carried out using the number of seats per lists, instead of the vote share. Also, different values are attributed to specific ideological camps – especially the Christian Democrats, centrist but then aligned with the Center-Right.)

17. The segmentation of the spectrum, has been as well tested for robustness.

in all races where at least one representative of the opposite camp is present[18]. The assumption that local supply of ideological candidates is likely balanced, regardless of observer's ability to infer it from lists' names, underpins this procedure. The procedure is prone to produce both false positives and negatives and should be adopted with a grain of salt and possibly only in robustness checks – especially for large villages where is generally the case that real non-ideological lists runs side by side with ideological ones. This sorting scheme produces results reported in Table 6.1, where 'Exclusive' and 'Inclusive' columns refers to the number of lists for each ideological camp using the respective definition (1 and 3), and the remaining columns the same statistic after the re-assignment procedure just described (2 and 4).

	Exclusive	Exclusive (R)	Inclusive	Inclusive (R)
CIV	22507	12529	22507	12643
SX	579	579	821	821
CSX	3122	12377	3443	12333
CEN	2706	2706	981	981
CDX	3875	4598	4976	5950
DX	433	433	494	494
OTHER	196	196	196	196
TOTAL	33418	33418	33418	33418

Table 6.1: Candidates' ideological camps, by definition

Table 6.2 lists the number of ideological camps' face-off races. It provides power reasons (in addition to conceptual relevance) to focus the present analysis only on the comparison between Center-Left and Center-Right camps ('CSX-CDX'), as well as the two against non-ideological groups ('CSX-CIV' and 'CDX-CIV', respectively). By choosing this restrictive method of attributing candidates to ideological or non-ideological camp some issues emerge.

18. A moderating assumption is made, so the non-ideological list is always re-coded as belonging to either the Center-Left or Center-Right camp, as the supply of extreme candidates is limited in the data.

	Exclusive	Exclusive (R)	Inclusive	Inclusive (R)
CSX-CDX	1036	2790	1439	3469
CSX-CIV	725	6898	921	6856
CDX-CIV	1381	323	1568	452
SX-DX	NA	NA	NA	NA
SX-CIV	23	4	54	6
DX-CIV	30	NA	33	NA
OTHER	9190	2370	8370	1602
TOTAL	12385	12385	12385	12385

Table 6.2: Ideological camps face-offs, by definition

6.5.2 Budgetary Data

As mentioned in the introduction, the present analysis does not propose a fully-fledged account of budgetary decision-making as seen in the literature. Instead, it focuses primarily on the role of those budgetary items positively included in the surplus/deficit calculation though not yet received ('receivables', it.tr. *'residui attivi'*). Focusing on these items allows me to gauge the general health of municipal budgets, especially in terms of hidden (although not hidden to the involved politicians and bureaucrats) debt. In particular, the present study uses the share of receivables declared by an administration as a measure of this health.

The *Openbilanci* initiative (`https://openbilanci.it/`) – a transparency initiative run by the association OpenPolis that was established with the goal of making all Italian public administration balance sheets available, accessible, and free of charge.[19] The period 2005-2014 has a health score for each Italian municipality, based specifically on the share of receivables declared by an administration. OpenPolis' index, is a three-years average and, by design, provides a clean measure of financial behav-

19. It is worth underlining that the organization collecting data and promoting the transparency initiative, does not have any stake in the matter – in particular they appear not to have any conflict of interest (e.g. doing business for the parties or its members). As a matter of fact, the association is not in charge of auditing municipalities, it only collects, cleans, and elaborates publicly available data. No threat to the identification strategy emerges here to the best of the author's knowledge.

ior that is not openly affected by electoral cycle incentives.[20] Having a financial health assessment over a long period of time helps substantially with avoiding the issues that come with 'one-shot' measures and giving a specific insight into 'unsound' receivables, that part of the receivables that an administration is incapable or, more likely, unwilling to eliminate from the budget and that accrues over time.

To be precise, the receivables score adopted for year t (RS_t) is calculated as the percentage of receivables collected during a year ($totR^C_{t-i}$) with respect to the total amount of receivables assessed at the end of the previous year ($totR^A_{t-i-1}$), averaged over the three preceding years and multiplied by 100. It is considered only for the fourth-mandate year of government and, thus, it reflects an average measure of the budgeting behavior of an administration sufficiently far away from elections.

$$RS_t = \frac{1}{3}\sum_{i=0}^{2}\frac{totR^C_{t-i}}{totR^A_{t-i-1}} * 100 = ... \frac{R^C_t}{R^A_{t-1} + \alpha(R^A_{t-2} - R^C_{t-1}) + ...} ... \tag{6.1}$$

As it is, the index does not allow to assess the extent of which each of the three administrative years drives the averaged result, for example making it impossible to distinguish municipality that are moving towards a more (less) virtuous management of resources. In addition, the synthetic nature of the index makes it impossible to disentangle two dynamics taking place at the same time, both contributing to the index. As a matter of fact, the 'unsound' receivable score can improve either if the effort to collect more receivables increases ($R^C_t \uparrow$), or if a correct estimation of 'unsound' receivables ($\alpha \downarrow$) is produced – which corresponds to the writing off of receivables from the budget. However, both dynamics pull in the same direction and are symptomatic of a healthy behavior on the behalf of the administration. In practice, there is no upside per se in reducing the value of receivables if not for pursuing a substantially balanced budget as, given the timings considered, it is unlikely that a logic of

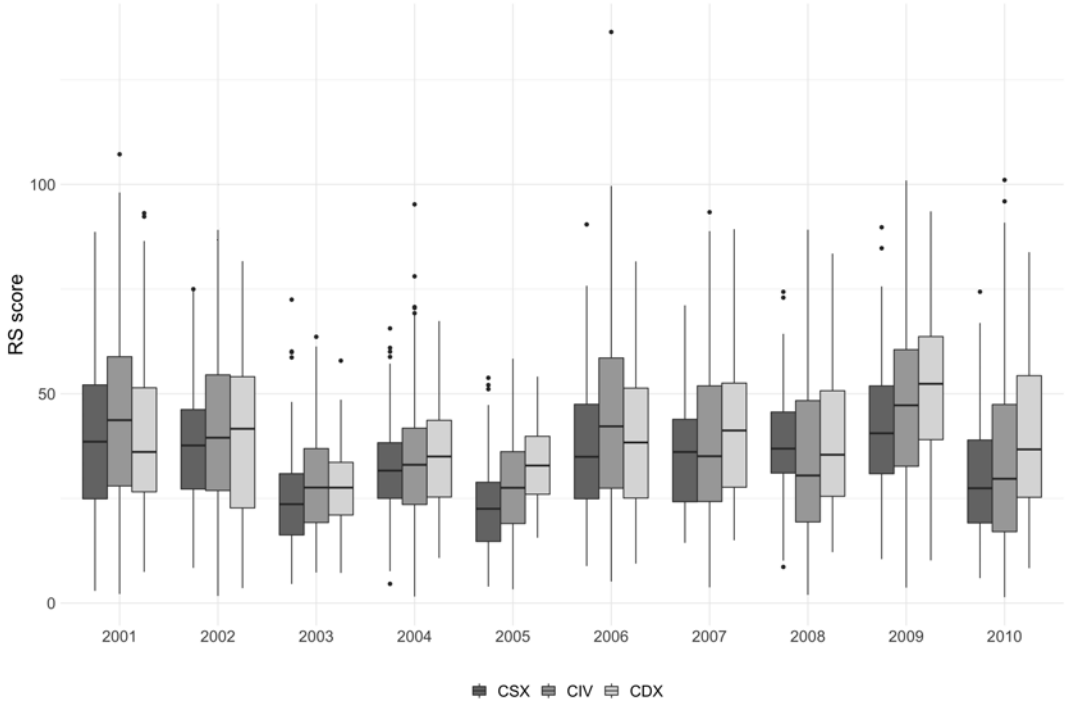
20. Presently, each administration is attributed the index averaging its behavior over the central three years of its mandate.

prospective resource constraint (like that described in Brollo and Nannicini 2012) is at play here .

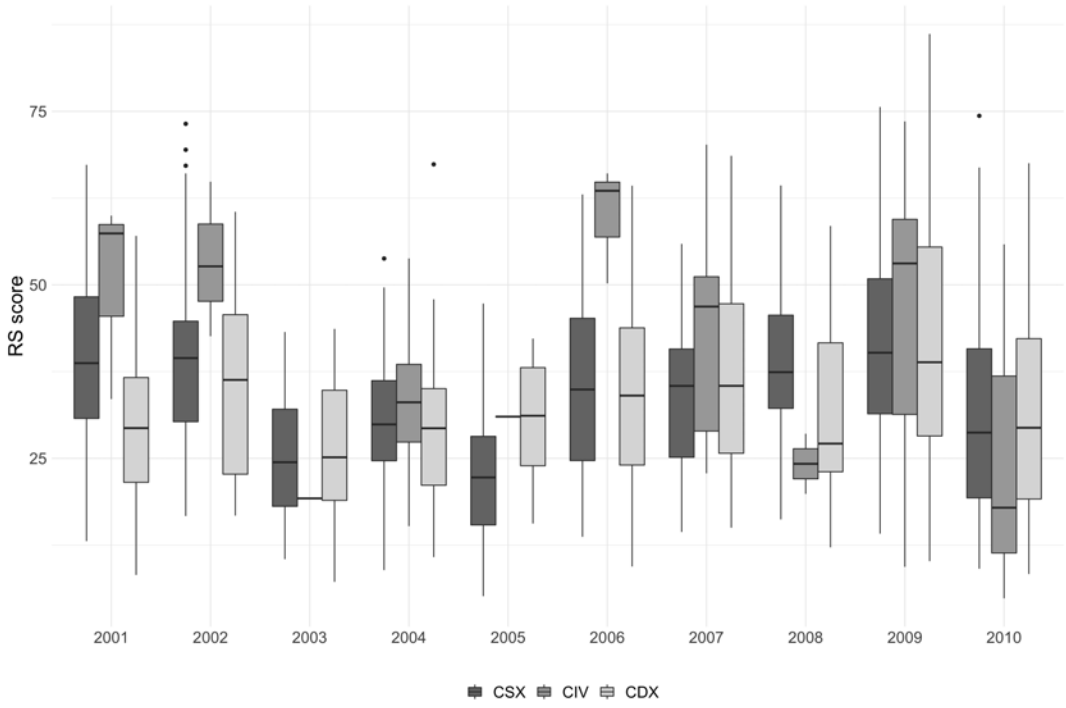
Table 6.3: Descriptive statistics

Statistic	N	Mean	St. Dev.	Min	Max
Year	12385	2005.78	2.78	2001	2010
Population (tho)	12385	7.17	43.15	0.03	2572.49
Votes - 1st rnd	12385	2159.88	11913.59	1	800275
Share - 1st rnd	12385	58.62	15.52	17.10	100.00
Votes - 2nd rnd	438	12831.68	43140.52	10.00	783725.00
Share - 2nd rnd	438	55.74	4.70	50.03	80.29
No. lists	12385	1.36	1.38	1	15
No. effective lists	12385	1.25	0.96	0	12
Mandate length	12385	5.00	0.00	5	5
Incumbent	12373	0.34	0.47	0.00	1.00
Financial Health Score 'RS'	12385	39.13	17.85	1.41	136.41
Collected wrt. Balance	11714	10.30	137.36	0.13	10841.52
Paid wrt. Balance	11714	10.32	137.67	0.14	10782.08
Revenue, Final balance (mil)	11714	74.17	23.07	3.35	179.14
Expenditures, final balance (mil)	11714	74.53	23.47	3.32	183.56

Graph 6.1a and 6.1b show the financial health index evolution over time, in an attempt to display heterogeneity across ideological camps. At this level of analysis, municipalities led by non-ideological mayors appear more capable than their ideological counterparts in accounting terms, especially in large municipalities – whenever they happen to successfully run. This piece of evidence somehow fosters the idea that non-ideological administration are indeed better at taking care of local government finances overall. However, the same evidence is plagued by endogeneity as ideological camp selection is not random. The discussion would require a more solid identification strategy carried out in Section 6.6.1. Graphs 6.2a and 6.2b show the heterogeneity in financial capabilities across Italian regions (NUTS 1). The North of the country consistently display higher (better) indexes than the regions in the Center, and in the South – in respective order. This is reproduced similarly across different subsection of the database, especially above the population threshold of 15,000 inhabitants. This gradient in ability will



(a) Exclusive - All races



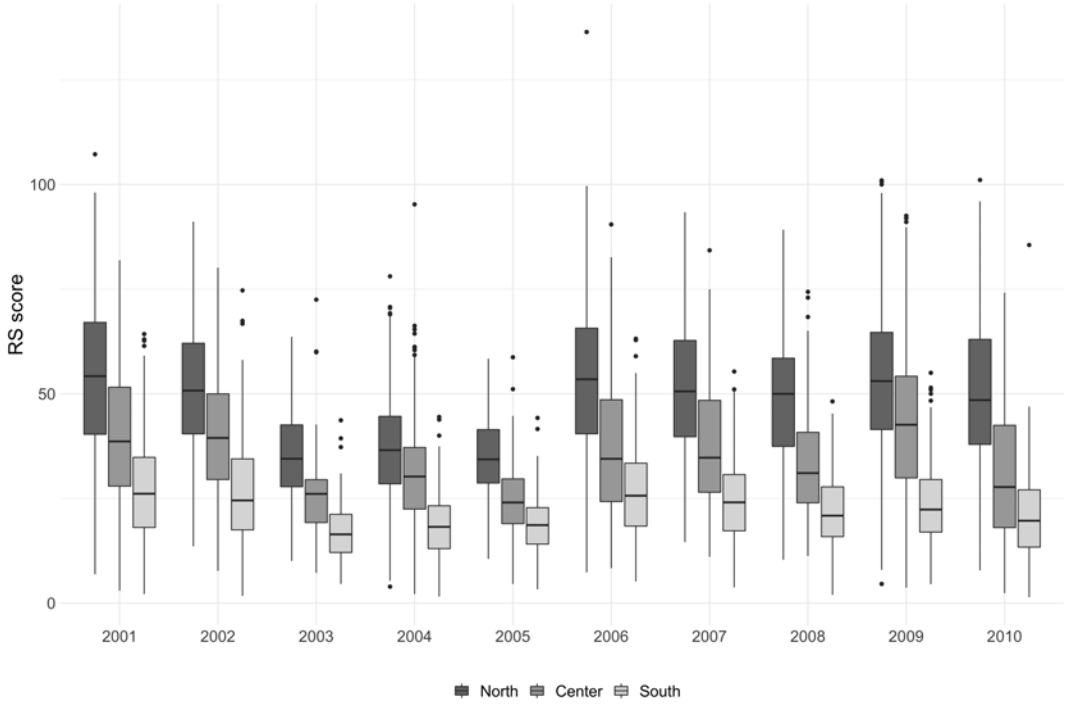
(b) Exclusive - Races above 15,000 in.

Figure 6.1: Residual Score, evolution over time (main ideological camps)

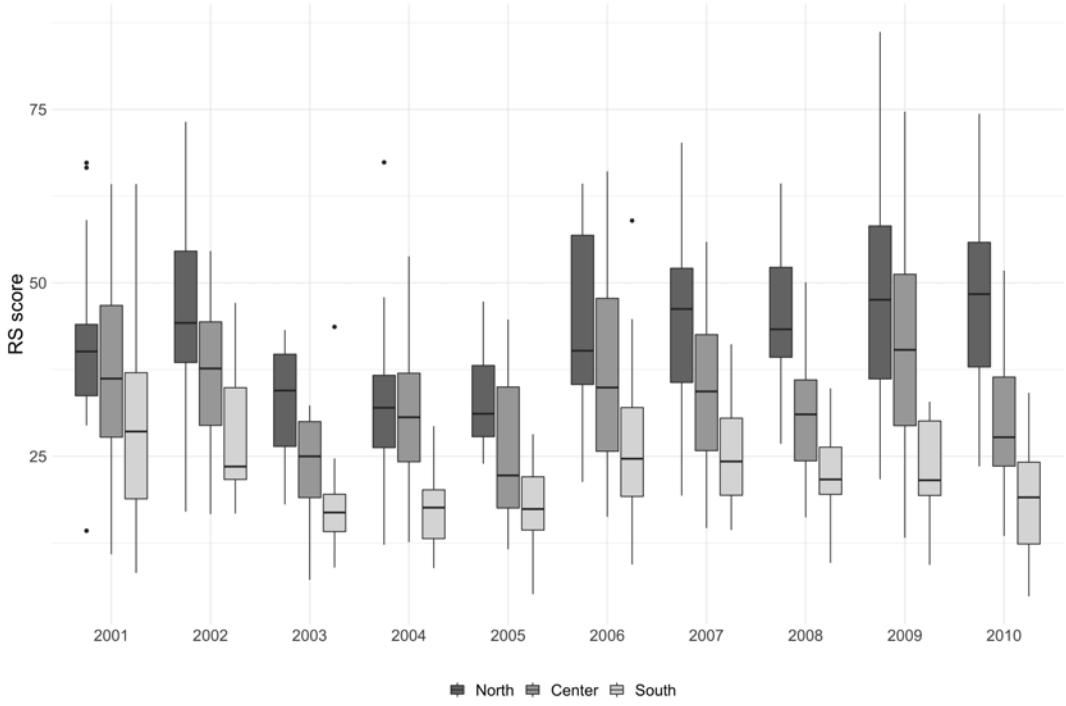
be controlled for in the subsequent analyses. The piece of evidence, is not surprising to readers aware of the differential in human capital and attitudes towards public institution in the Italian context, specifically it is in line with the pundits discussion of the topic that usual references data from the Italian Court of Audits (*See*: Trovati 2017).

From both sets of graphs, it is clear that the index experienced a general decrease for administration elected in during years 2003-2005, no matter the region. This could intuitively be explained making reference to the Stability Pact regulations discussed in Section 6.4.2, which might have had a substantial though lagged effect when into force. This specificity will as well be accounted for in the analyses.

In order to test further hypotheses, as well as to produce balance tests for the original RD estimations, data has been obtained from the Bureau Van Dijk's AidaPA database (`http://aidapa.bvdinfo.com`). The database records important socioeconomic characteristics and financial variables regarding each municipality, especially resource allocation. The same database provides an alternative measure of financial mismanagement that will be used to provide a robustness check to the findings presented here.



(a) All races



(b) Races above 15,000 in.

Figure 6.2: Residual Score, evolution over time (NUTS 1)

6.6 Empirical Strategy

6.6.1 Identification strategy

The present piece aims at exploring the impact of ideological camp effect on budget health for local level governments. It does so by estimating the impact of a mayor's ideological camp on the municipality's ability to write off 'unsound' receivables from their accounts during the course of their time in office. Given that local government ideological camp affiliation is endogenous to several administrative dimensions, not secondarily those pertaining to financial management – both prospective, and retrospectives–, issues emerge with respect to identify a causal link between ideology and financial health. To overcome the issue, as been deemed fit in the literature, I will exploit a data-driven Regression Discontinuity (RD) design on margins of victory at the municipal level, comparing ideological groups pairwise. The present work focus on the main ideological camps' comparison, and them in turn against non-ideological camp. Focusing on these specific comparison (thus overlooking extremists and anti-system parties), besides having the advantage of underpinning estimations with sufficiently large sample sizes, is theoretically justified as the two camps called the shots in Italian local politics in the years considered and were sufficiently distant from an ideological perspective to see their policy preferences translated into heterogeneous practices.

To estimate the effect of ideological affiliation on the degree of engaging in unhealthy budgetary choices, the following equation is estimated:

$$RS_i = \alpha + \tau E_i + f(V_i) + \epsilon_i \tag{6.2}$$

The dependent variable is the absolute (unsound) receivable score for a municipality i described in Section 6.5.2. Given that the score is a three-year average, only the score for the fourth year of local government is considered (spanning years two, three, and four of each government office time) so that previous government fiancial legacy and electoral considerations are minimized. The forcing variable is the local election margin of victory: E_i measures whether the candidate i was elected in municipality m, taking positive or negative values according to the pairwise comparison under consideration; $f(V_i)$ is a measure of the forcing variable. An additional specifications where a set of control variables are introduced to refine the estimates are also considered without substantial changes in terms of estimations.

Methodologically, the piece rests on the exploitation of a classic Regression Discontinuity design on margins of victory: a *continuity-based* RD framework relying on local polynomial least-squares methods to model separate regression functions on each side of the cutoff (Calonico, Cattaneo, and Titiunik 2014). The local (nonparametric) polynomial fits follow the standard procedure for data-driven, objective, and automatic RD empirical work: MSE-optimal bandwidth are selected for each side separately, with triangular Kernel Weights (MSE-optimal choice for point estimation). Results are reported for local linear estimation ($p = 1$) with Standard Errors clustered at the municipal level. All results and graphs are produced with the `rdrobust` software package for `R`.

It is important to notice that, in older RD estimation, graphical evidence was produced including confidence intervals for a quadratic least squares fit. To do so, it was assumed a constant standard deviation on each side of the cutoff. However, any estimation made with `rdplot` assumes neither a quadratic model, nor constant standard deviations. In it, data are binned so that a different standard deviation can be estimated in each bin, with confidence intervals calculated on it. These are the confidence intervals reported in the following graphs.

The RD design presently exploited should produce a balanced representation of regions above and

below the threshold. Non-discontinuity of pre-treatment covariates. Robustness checks are nonetheless performed. This is important because of the described existing gradient in unsavory budgetary practices across Italian regions described[21]. This, together with the quasi-random assignment of the method employed, should dismiss the issues of the results being driven by a particular region, or set of thereof. Coming to the interpretation of the results, however, it is important to underline how different regional-level social capital level might determine the rational underpinning the practiced observed.

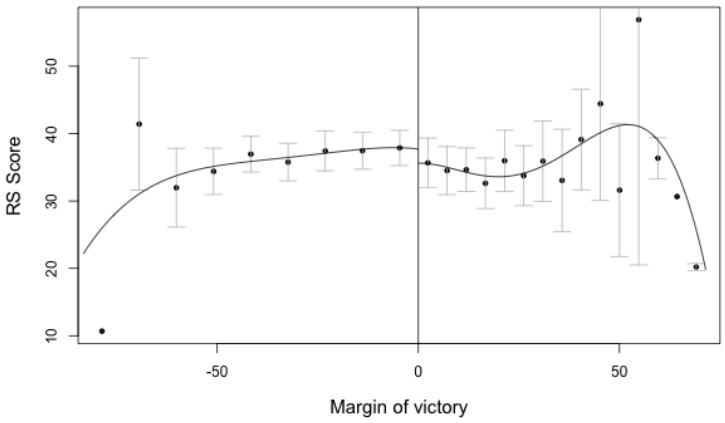
6.6.2 Estimation

Global fit (parametric) plots with binned local means for camp-pairs are reported in Figure 6.3a to 6.3c. The plots fail to reveal any jump at the cut-off of apparent statistical significance. Local fit (non-parametric) plots are reported in Figure 6.4a to 6.4c. They confirm the original impression: not statistically significant local effects appear for all specifications. All the graphs and estimations are carried out presently using the Exclusive definition, without reassignment of lists. Estimation produced with the inclusive definition do not change substantially to be reported.

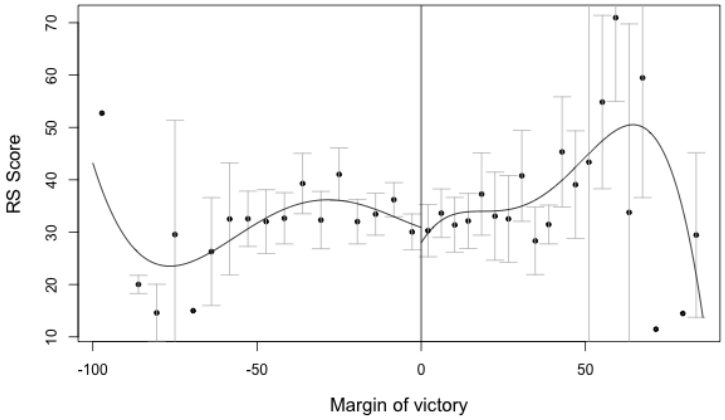
Estimations are reported in Tables 6.4 to 6.6. No substantial jumps are registered across model specifications. Non-ideological candidates prevailing over ideological ones of either camp do not appear to causally induce a better financial management of local resources. Results are robust to the clustering of S.E. at the municipal level (Model 2); to the inclusion of a number of covariates - Including population, race year, incumbency, and number of lists running (Model 3). The exploration of different donuts-hole result non-consequential to the estimates (Model 4)[22]. Local analyses are also robust to the alterations of the local polynomial order (1:4), and to the selection of alternative (automatic) bandwidths. All predetermined covariates appear continuous at the threshold for the

21. As a side note, in an analysis more specifically about corruption, Del Monte and Papagni 2007 dismisses the issue of potential systematic bias due to differences among regions between the actual and reported number of corruption crimes, referencing to the statistically non-significant relation between an index of judicial efficiency and corruption crimes. This should apply also for the present case in terms on unreported residuals.

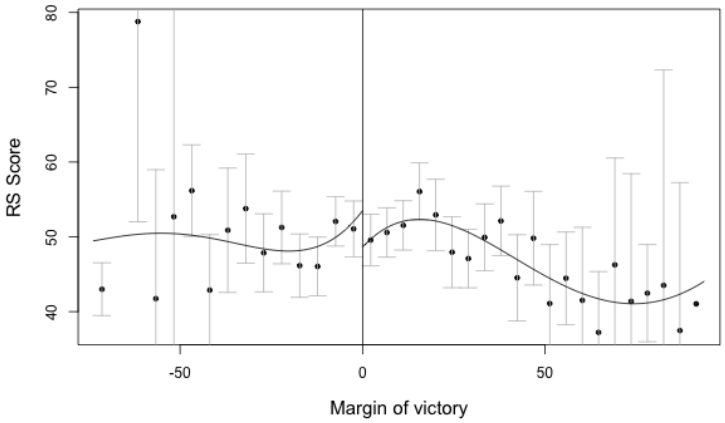
22. Only the average hole of 1.5 on both sides is reported.



(a) Center-Left Vs. Center-Right

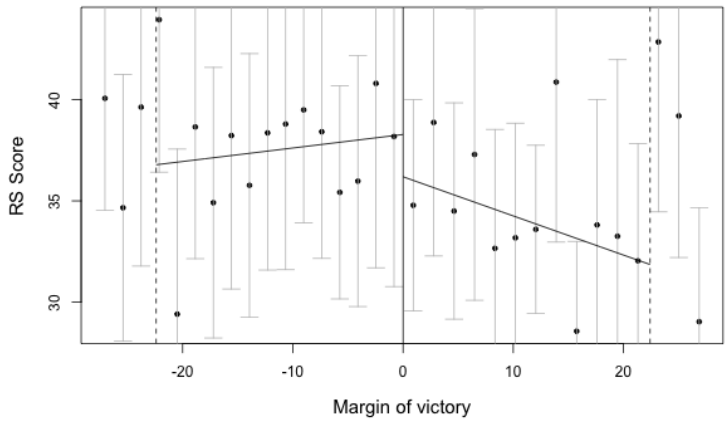


(b) Center-Left Vs. Non-ideological

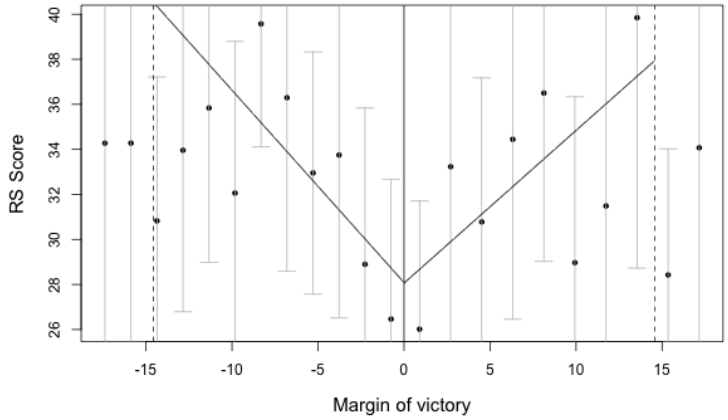


(c) Center-Right Vs. Non-ideological

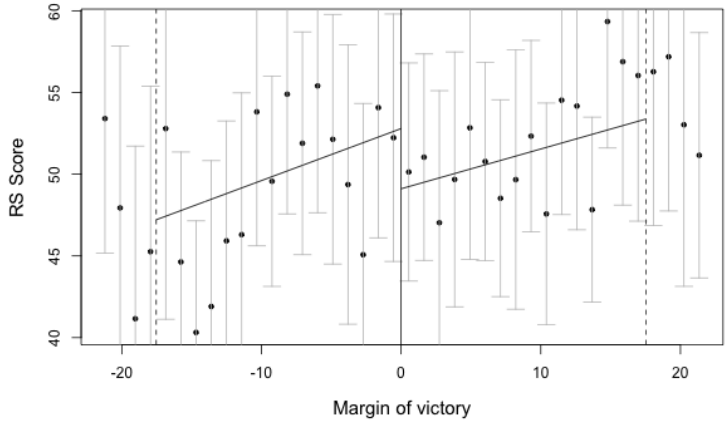
Figure 6.3: Residual Score, Regression Discontinuity Plots (GLOBAL)



(a) Center-Left Vs. Center-Right



(b) Center-Left Vs. Non-ideological



(c) Center-Right Vs. Non-ideological

Figure 6.4: Residual Score, Regression Discontinuity Plots (LOCAL)

main analysis[23], with the sole exception of Center-Right Vs. Non-ideological candidates, where non-ideological administrations are likely to be incumbents just above the threshold .

	Model (1)	Model (2)	Model (3)	Model (4)
Coeff.	-1.652	-1.646	0.629	0.397
S.E.	2.694	2.681	2.798	3.517
z	-0.613	-0.614	0.225	0.113
P>|z|	0.540	0.539	0.822	0.910
95% C.I.	-6.932 , 3.628	-6.900 , 3.607	-4.856 , 6.114	-6.496 , 7.290
N	651 , 385	651 , 385	651 , 385	635 , 363
N (effective)	307 , 246	307 , 247	244 , 204	216 , 176
h	22.41	22.51	17.17	16.51
b	36.90	36.95	28.30	32.34
p	1	1	1	1
p (bias)	2	2	2	2
Cluster		YES	YES	YES
Covariates			YES	YES
Donuthole				YES

Table 6.4: RDD Estimates: Center-Left VS. Center-Right

From the analysis of co-produced budgetary variables no particular insight emerges. Revenues, Expenditures, overall Budget and Deficit are per-capita measured, whereas the amount of resources collected and paid are measured as the share revenues and expenditure, obtained and cleared respectively.

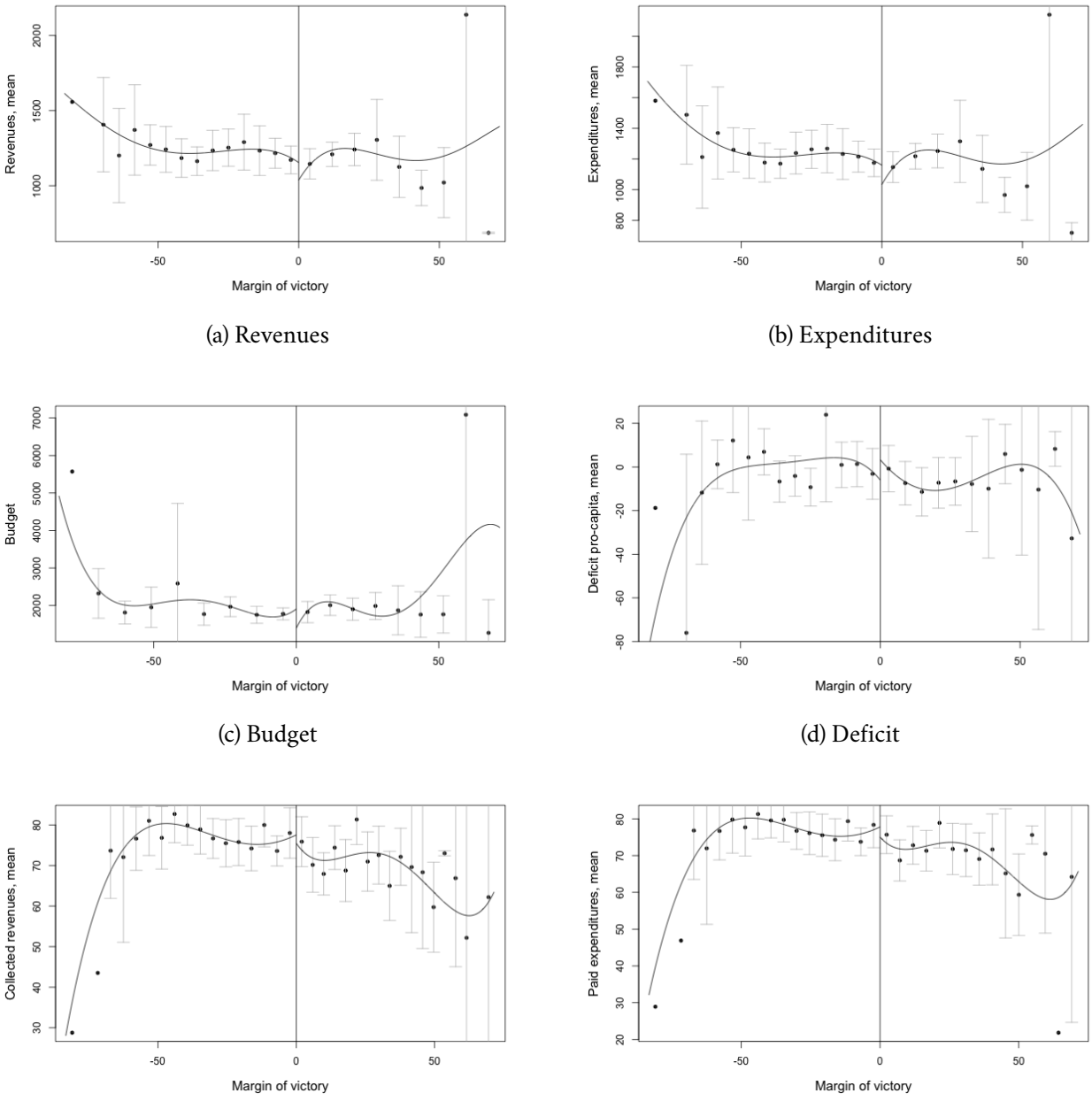
23. *See*: Appendix for the parametric graphs. All estimation not included are available upon request

	Model (1)	Model (2)	Model (3)	Model (4)
Coeff.	0.664	0.568	1.357	3.457
S.E.	3.465	3.500	3.044	4.542
z	0.192	0.162	0.446	0.761
P>|z|	0.848	0.871	0.656	0.447
95% C.I.	-6.127 , 7.454	-6.292 , 7.428	-4.608 , 7.322	-5.446 , 12.359
N	437 , 288	437 , 288	436 , 287	421 , 272
N (effective)	165 , 146	166 , 148	166 , 147	151 , 130
h	14.57	14.71	14.73	14.60
b	23.81	24.02	24.53	26.77
p	1	1	1	1
p (bias)	2	2	2	2
Cluster		YES	YES	YES
Covariates			YES	YES
Donuthole				YES

Table 6.5: RDD Estimates: Center-Left VS. Center-Right

	Model (1)	Model (2)	Model (3)	Model (4)
Coeff.	-4.774	-4.795	-2.167	-1.881
S.E.	2.891	2.891	2.703	3.603
z	-1.651	-1.658	-0.802	-0.522
P>|z|	0.099	0.097	0.423	0.602
95% C.I.	-10.440 , 0.892	-10.461 , 0.872	-7.463 , 3.130	-8.942 , 5.181
N	545 , 836	545 , 836	544 , 836	518 , 798
N (effective)	338 , 410	338 , 410	345 , 420	292 , 351
h	17.54	17.59	18.30	16.28
b	33.25	33.43	29.74	36.21
p	1	1	1	1
p (bias)	2	2	2	2
Cluster		YES	YES	YES
Covariates			YES	YES
Donuthole				YES

Table 6.6: RDD Estimates: Center-Left VS. Center-Right



(a) Revenues

(b) Expenditures

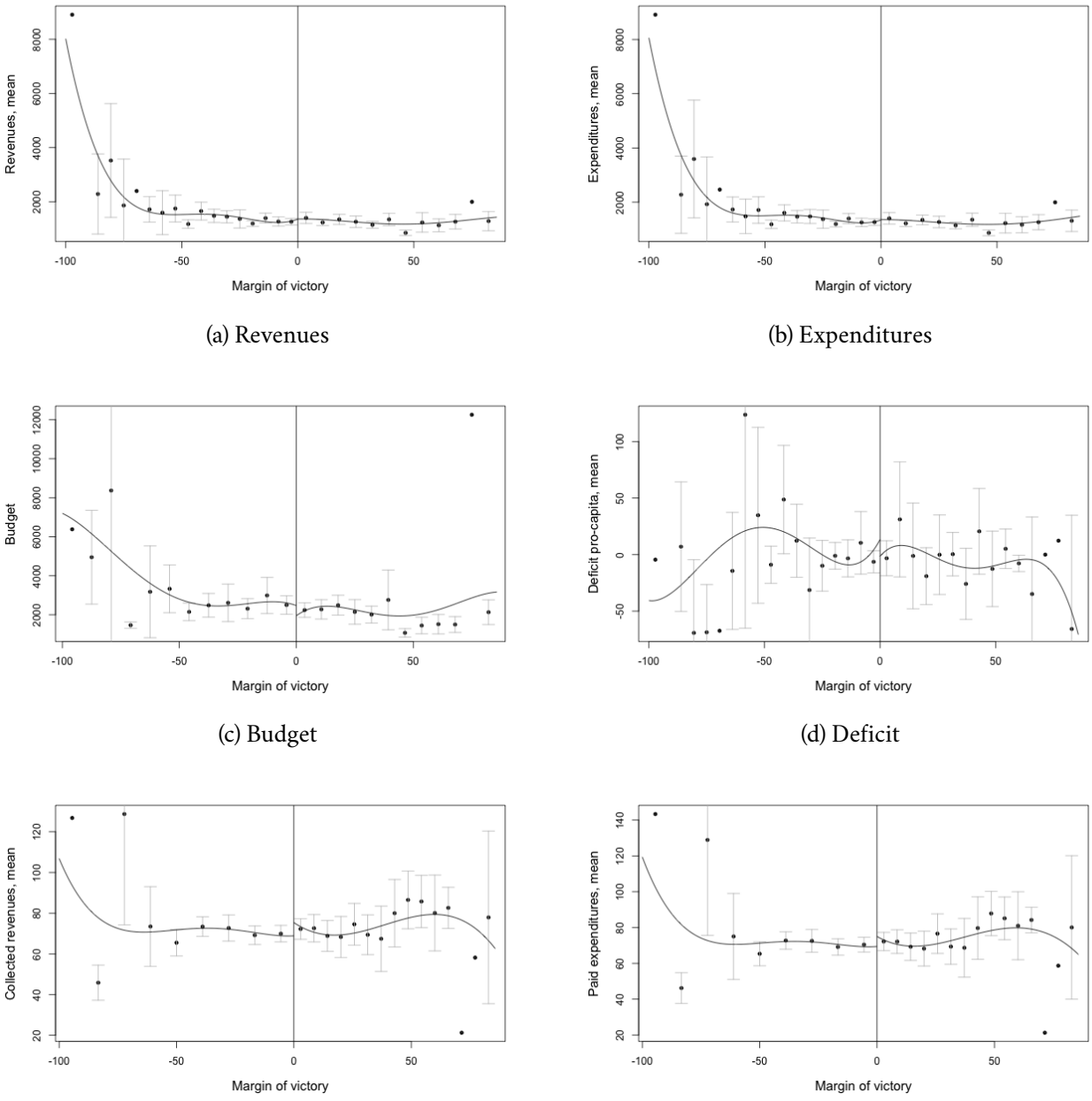
(c) Budget

(d) Deficit

(e) Collected

(f) Paid

Figure 6.5: Discontinuity of co-produced covariates - Center-Left Vs. Center-Right



(a) Revenues

(b) Expenditures

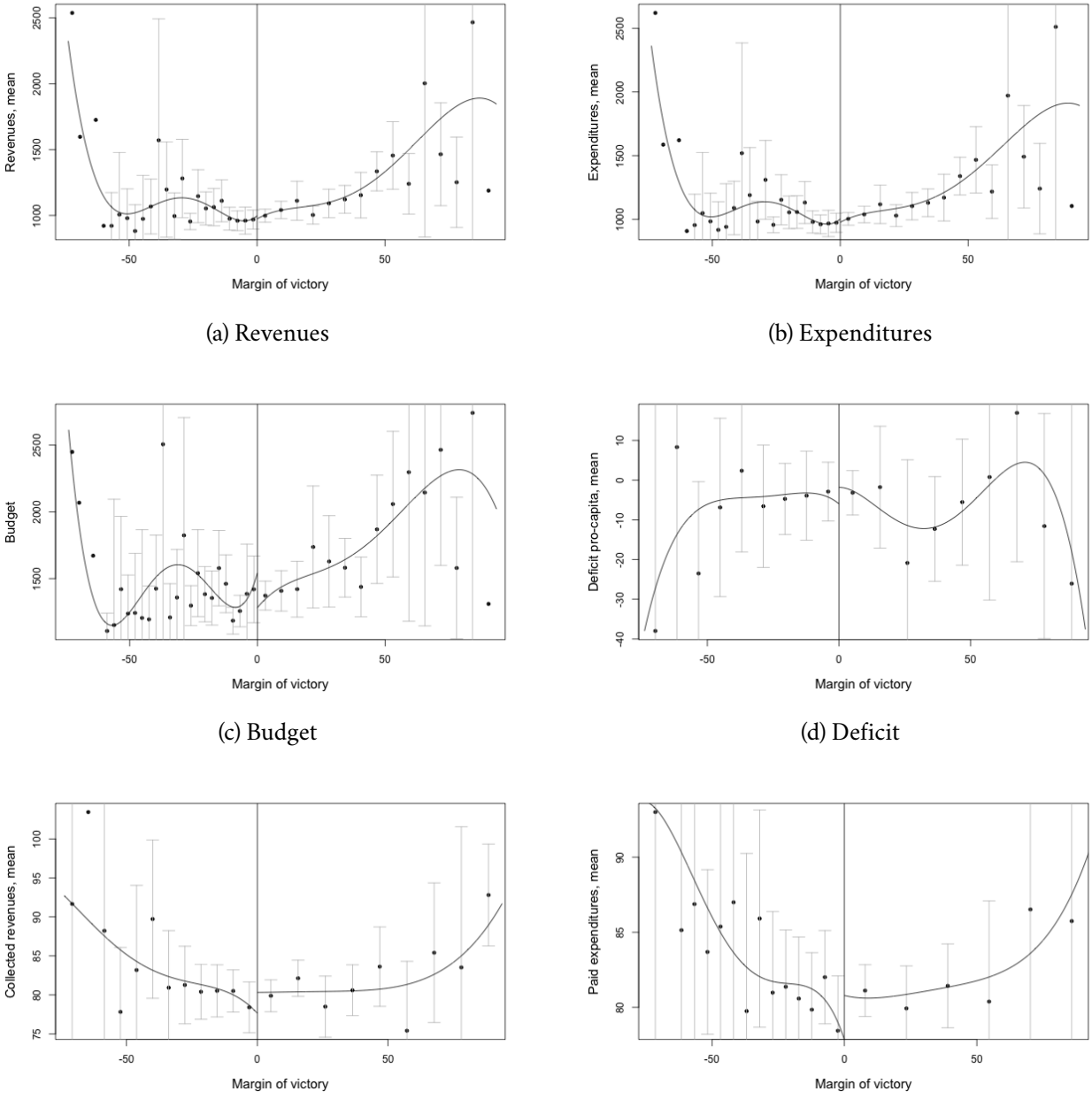
(c) Budget

(d) Deficit

(e) Collected

(f) Paid

Figure 6.6: Discontinuity of co-produced covariates - Center-Left Vs. Non-ideological



(a) Revenues

(b) Expenditures

(c) Budget

(d) Deficit

(e) Collected

(f) Paid

Figure 6.7: Discontinuity of co-produced covariates - Center-Right Vs. Non-ideological

6.7 Conclusion

The paper assess the impact of ideological affiliation of local governments on fundamental budgetary choices key to the health and sustainability of local finances. Accounting tricks to defer spending constraints which are highly consequential for debt accrual is used as the at the center of the analysis. Relying on an RD design to overcome standard endogeneity concerns, the study presents previously unavailable evidence of regarding the degree of malfeasance across ideological camps in Italy. The setting allows for testing hypothesis comparing not only ideological camps, but also non-ideological ones – which remains a central contribution of the piece. In the time-framed considered, local administration affiliated ideologically with the center-left appear not differ in their budget management with respect to center-right ones. Moreover, neither of the two camps under-performs with respect of non-ideological ones. Positive jumps are registered, but not statistically significant in most specifications. The piece contributes to substantial series of non-results in this line on inquiry, breaking down the unsubstantial myth of the relevance of ideological leaning of administration when it comes to fundamental administrative decisions. The results expands this judgment irrelevance also to non-ideologically administrations, taken sometimes as a panacea to corrupt and incapable ideological counterparts. Such result, is tentatively explained by pointing at the substantial lack of long-horizon incentives for the majority of local-politicians, that do not respond to career-incentives beyond their mandate (e.g. developing a vote-base on over-expansive spending, or making the party 'look good' locally). It is important to underline how external financial constraints might have played a role in the time-frame considered. Further analyses over a period non vested by generalized economic crisis that globally tighten budgets, might be suggested. The paper does not delve into budget items discontinuities analyses, in this it remains silent regarding how comparable financial health scores at the threshold are be achieved in the face of possible heterogeneous, camp-specific budget priorities. Though interesting this remains beyond the scope fo the present analysis.

6.8 Appendix

6.8.1 Robustness checks - RDD with Inclusive definition of ideological camps

	Model (1)	Model (2)	Model (3)	Model (4)
Coeff.	-2.582	-2.572	-1.413	-1.642
S.E.	2.654	2.664	2.373	3.313
z	-0.973	-0.965	-0.596	-0.496
P>|z|	0.331	0.334	0.551	0.620
95% C.I.	-7.785 , 2.620	-7.793 , 2.649	-6.065 , 3.238	-8.135 , 4.852
N	900 , 539	900 , 539	900 , 539	874 , 505
N (effective)	350 , 299	350 , 302	351 , 303	284 , 233
h	16.47	16.53	16.64	14.29
b	25.98	26.02	26.32	26.69
p	1	1	1	1
p (bias)	2	2	2	2
Cluster		YES	YES	YES
Covariates			YES	YES
Donuthole				YES

Table 6.7: RDD Estimates: Center-Right VS. Non-Ideological

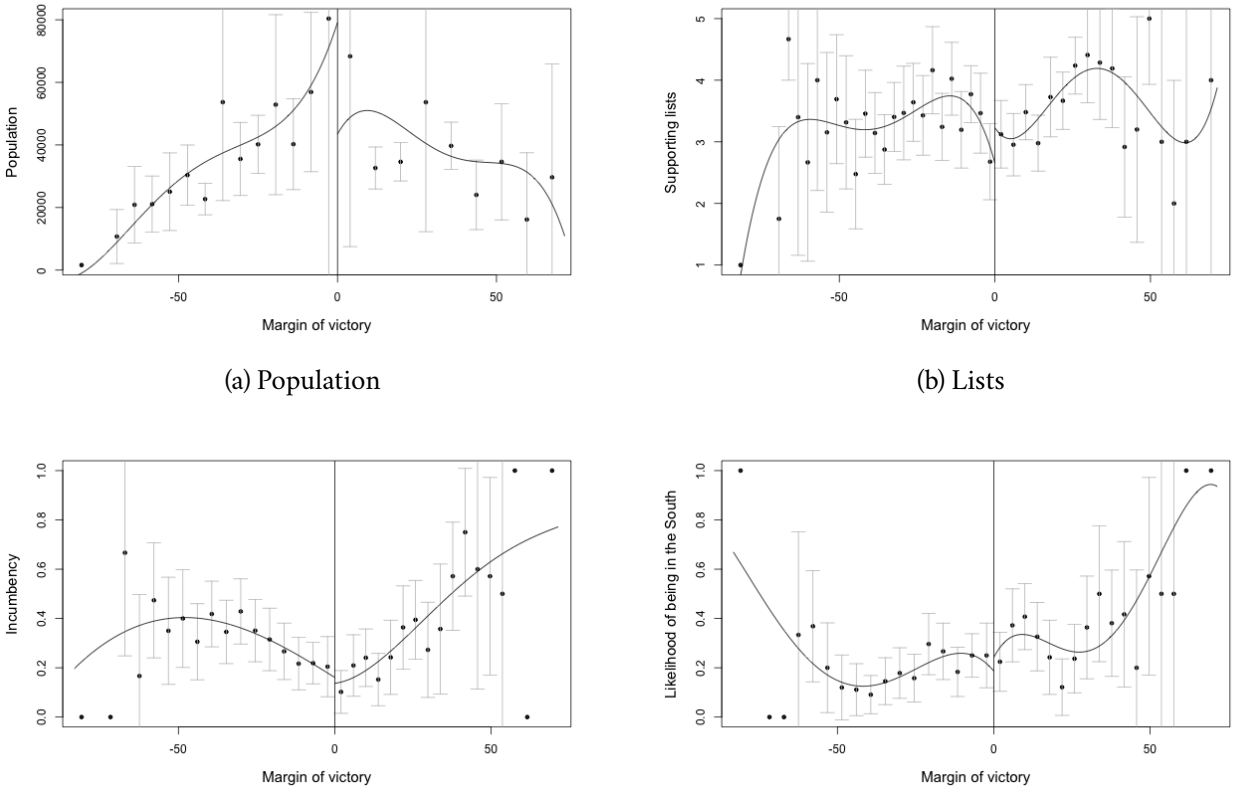
	Model (1)	Model (2)	Model (3)	Model (4)
Coeff.	2.640	2.524	4.474	3.051
S.E.	3.093	3.101	2.729	4.041
z	0.853	0.814	1.639	0.755
P>|z|	0.393	0.416	0.101	0.450
95% C.I.	-3.423 , 8.703	-3.554 , 8.601	-0.875 , 9.823	-4.870 , 10.971
N	552 , 369	552 , 369	551 , 368	525 , 346
N (effective)	222 , 190	223 , 190	205 , 177	193 , 165
h	14.34	14.51	12.78	14.24
b	24.94	25.22	24.35	26.98
p	1	1	1	1
p (bias)	2	2	2	2
Cluster		YES	YES	YES
Covariates			YES	YES
Donuthole				YES

Table 6.8: RDD Estimates: Center-Right VS. Non-Ideological

	Model (1)	Model (2)	Model (3)	Model (4)
Coeff.	-4.093	-4.150	-1.391	-0.192
S.E.	2.799	2.812	2.698	3.608
z	-1.462	-1.476	-0.515	-0.053
P>|z|	0.144	0.140	0.606	0.958
95% C.I.	-9.580 , 1.393	-9.663 , 1.362	-6.679 , 3.898	-7.263 , 6.880
N	623 , 945	623 , 945	622 , 945	593 , 903
N (effective)	361 , 439	363 , 439	342 , 424	288 , 348
h	16.33	16.40	15.57	14.02
b	26.34	26.42	24.52	25.63
p	1	1	1	1
p (bias)	2	2	2	2
Cluster		YES	YES	YES
Covariates			YES	YES
Donuthole				YES

Table 6.9: RDD Estimates: Center-Right VS. Non-Ideological

6.8.2 Discontinuity of pre-existing covariates



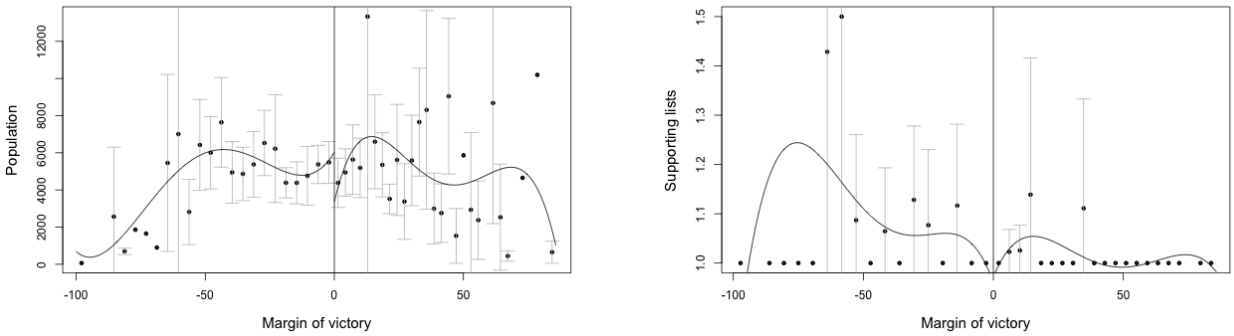
(a) Population

(b) Lists

(c) Incumbency

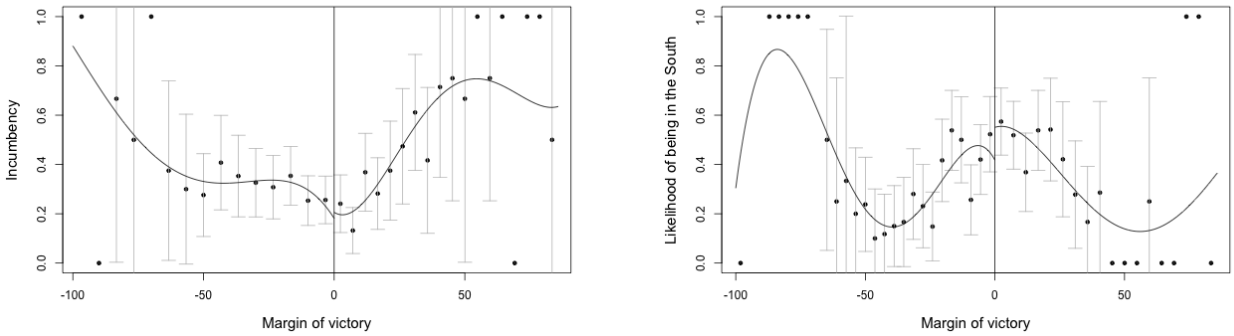
(d) South

Figure 6.8: Discontinuity of pre-existing covariates - Center-Left Vs. Center-Right



(a) Population

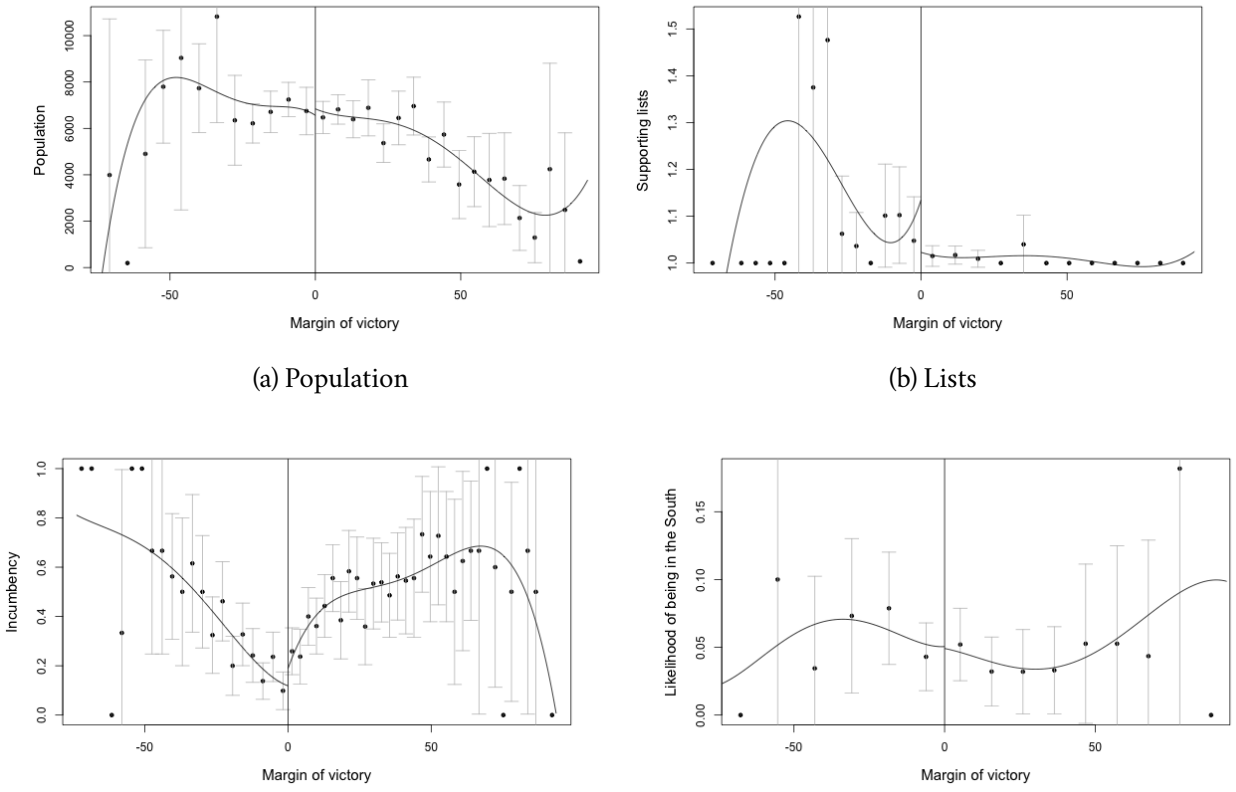
(b) Lists



(c) Incumbency

(d) South

Figure 6.9: Discontinuity of pre-existing covariates - Center-Left Vs. Non-Ideological



(a) Population

(b) Lists

(c) Incumbency

(d) South

Figure 6.10: Discontinuity of pre-existing covariates - Center-Right Vs. Non-Ideological

Conclusions

The present work is composed of three separate essays, each developed to fill a knowledge gap regarding different forms of local public office mismanagement, conceptually connected through corruption displacement dynamics. Each piece makes a number of advances regarding how to compensate for the theoretical and practical shortcomings of the actual policy approach, including the development of neater, scalable ways to measure each phenomenon, and a blueprint of how to tackle it, always keeping in mind optimal policy design guidelines. In the different contexts under analysis, actors' ability to strategically respond to a reform process proves to be an ultimate determinant of the success of the process itself. The ability to promptly create new corruption channels is a feature shared by actors operating in and around political centers of power across the scenarios considered. This pattern is striking as one approaches the study of anti-corruption intervention. Dismantling a rent-extractive system put in place by actors who, by dint of their positions, are entrenched, resourceful and committed in their preservation efforts, is not an easy task. At the same time, the extent of the corruption displacement described, reveals the need to question the logics behind current policy design, implementation and monitoring. If the lessons taught in the essays are mainly to do with assessing interventions' effectiveness, that is the starting point to eventually achieve optimally designed policy solutions.

Despite the best efforts of the scholarship, corruption practices of the kind described here are hard to measure and –due to their ingenuity – to keep up with. In this, political science lags behind real world challenges and runs the risk of providing only a minor, marginal, and late contributions. With this in mind, the present essays go beyond telling cautionary tales about misdirected interventions on corruption and propose strategies to overcome some of them. The range of straightforward and reproducible quantitative tools presently discussed can be of interest to practitioners to measure some otherwise elusive phenomena. If anti-corruption efforts can be undermined in the implementation phase, their risk is even greater when preservation efforts move up the process and affect the very design of a policy intervention.

In term of policy design – given that full anticipation of actors' re-orientation efforts can hardly be built into it – two lessons can be learned from the essays here reported to avoid ineffective anti-corruption actions. The first lesson is that under-articulate design should be avoided. The displacement strategies analyzed in the essays, although novel for the specific settings, were sufficiently customary in other contexts and, to an extent, were also anticipated by local pundits. These could have been included in the reform package. Incomplete reform efforts led to actors being able to displace corrupt practices promptly. Though, a clear efficiency-effectiveness trade-off emerges when devising reforms within narrow windows of opportunity, all three cases studied display an unjustifiable imbalance towards policymaking efficiency – that is to say, preferring a minimal law over an articulated one for fear to miss a legislative opportunity – that makes all unmistakably all-encompassing, headline-making solutions. These failed to consider a number of first order stopgap strategies to which, unsurprisingly, actors resorted. If the corrupt actors had been more limited in their option set, their search of a new corruption locus would have been slower, allowing the monitoring and prosecuting bodies to catch up in time with the development of truly new solutions.

The second lesson is that policy needs to change the actors' set of incentives. The reforms discussed

appear all to be superficial fixes incapable of affecting deep dynamics of the political system. The original policy sin appears again to be the short-sighted desire to be efficient in bringing about reform, regardless of its impact or long term effect. All solutions failed to address the issues that foster the creation and perpetuation of a corruption-plagued system - from the cost of politics, to the limited outside options and the excess of discretionary power – all feeding one another. It is not surprising, in the end, that actors resorted to other forms of corruption to respond to the very same incentives. These two suggestions – in addition to the classical and widely discussed anti-corruption toolkit – have the potential to produce a marginal improvement in the quest to tackle corruption.

In the cases considered, not only did the anti-corruption interventions fail to fully dismantle rent-extractive solutions, they also led to the emergence of corruption arrangements that are harder to account for and monitor. Interventions of this kind are thus, at best, ineffective and, at worst, detrimental to the status quo – affecting political transparency, electoral accountability, and institutional perceptions – on top of a number of economic outcomes. Insufficiently developed policy interventions of this kind create potentially vicious cycles of mistrust in institutions and in the solutions they devise, that might prove critical for contexts that are struggling to move to superior corruption equilibria by enacting reforms. The present piece contributes to a broad topic and to an relevant dynamic that remains, to this day, understudied by the general scholarship, leaving practitioners alone in facing one of the most challenging threats to democratic policymaking.

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