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BARCELONA

Essays on Brazilian Political Economy

Francisco de Lima Cavalcanti



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1. Introduction

Countries are unequal in terms of economic development. The reasons underlying the disparity among nations have been the subject of research for several years. Recently, a line of thought has argued that institutional progress is the primary determinant of why nations fail or succeed in creating modern, well-governed states (Acemoglu and Robinson, 2013). According to this reasoning, the pathway to enhancing development would be by enforcing effective state capacity, the rule of law, and democratic accountability (Fukuyama, 2014). Thus, one of the mechanisms that could potentially ensure strong institutions would be through procedures such as free and fair multi-party elections.

Open elections, where voters periodically choose candidates or parties to represent them and their interests both locally and nationally, are widely accepted as a system that promotes governments that improve social welfare, because democracy prevents powerful groups from capturing the state to extract rent, reduces social conflict, and constrains kleptocratic dictators (Acemoglu et al., 2014). In principle, electoral competition works well for the quality of governments, just as economic competition works well for market efficiency. That is, successive elections increase the chances that good politicians will be selected to run a government. Society, in turn, would benefit from good governance, and consequently, institutions would become stronger. Although some studies note that democracy does not lead to economic growth (Alesina and Rodrik, 1994; Tavares and Wacziarg, 2001; Giavazzi and Tabellini, 2005), other studies indicate that countries with long histories of liberal democracy have produced better institutions in the long run and that they are economically advanced (Barro, 1996; Rodrik and Wacziarg, 2005).

Democratic accountability

The central mechanism by which democratic systems would ultimately benefit economic development would be through electoral accountability. Politicians would have incentives to behave according to voter preferences as voters would reward or punish incumbent politicians according to their performance (Barro, 1973; Besley and Burgess, 2002; Duch and Stevenson, 2006; Ashworth, 2012). If the incumbent government's performance is disappointing from the voters' point of view, then they

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would have the alternative to vote for opposition political parties (Ferraz and Finan, 2011; Costas-Pérez et al., 2012). In this way, the checks and balances work as a learning process for politicians to know voter preferences and, subsequently, to implement public policies that meet the demands of the electorate.

In the political economics literature, a range of theoretical models has adopted the accountability mechanism to explain the behavior of agents and its impact on the economy. Among all of these models, two classes are worth mentioning because they have been widely employed. The first class of models is the political-agency framework, which focuses on the extent to which voters can discipline elected representatives with career concerns in environments with moral hazard and adverse selection (Ferejohn, 1986; Persson et al., 1997; Besley and Burgess, 2002). The second class is the citizen-candidate framework. This class of model removes the distinction between citizens and politicians by assuming that the voters choose elected officials from those citizens who decide to become politicians in the first place and stand as candidates in an election (Besley and Coate, 1997; Osborne and Slivinski, 1996; Caselli and Morelli, 2004; Mattozzi and Merlo, 2015).

Political parties

An essential component in models of accountability is the supply of competent political candidates to manage governments. However, it is unclear whether representative democracy can provide high-ability and broadly representative political leaders (Mattozzi and Merlo, 2015; Dal Bó et al., 2017). On the one hand, voters always have the option to punish an incumbent incompetent by voting for the opposition candidates. On the other hand, the electoral accountability mechanism does not ensure that the opposition politicians are competent as well. For instance, if opposition politicians have low human capital, the electoral dynamics would not ensure an improvement in the quality of public management because public managers who are not necessarily of high quality would be elected to lead the government. Thus, voter demand for high quality public managers is limited by the supply of political candidates.

The role of political parties is of utmost importance on the supply of candidates. After all, it is the political parties that play an active role in the selection of candidates. Political economy models have exhibited challenges on political selection for the democratic systems because low-quality candidates have a comparative advantage when participating in elections because they have lower opportunity costs and free-riding incentives (Caselli and Morelli, 2004; Mattozzi and Merlo, 2015). Moreover, this adverse selection of candidates could be exacerbated by the size of the public budget (Brollo and Nannicini, 2012). For instance, it has been documented

that an unexpected shock in public revenue can worsen the functioning of political institutions because it can aggravate the agency problem and deteriorate the quality of political candidates. A recent line of literature has explored how the organization of parties works and what the criteria are for selecting candidates (Guarnieri, 2011; Avelino et al., 2012). Answering these questions would shed light on how to ensure that the best candidates run for election, thereby ensuring that governments perform efficiently.

Voters

Voters are pivotal in accountability models, but some of the underlying assumptions in these models about voters' behavior have been contested with empirical evidence. Three assumptions draw attention. First, the supposition of voters caring only about economic policies and identifying the economic signals correctly from politicians' decisions is crucial for the role played by electoral accountability to work in an ideal world. However, empirical evidence suggests that voters are sensitive to conditions other than economic policies when assessing an incumbent's performance. Voters appear to make substantial mistakes when distinguishing between economic outcomes originating from exogenous shocks and those resulting from political decisions, often blaming or rewarding politicians for good or bad luck (Achen and Bartels, 2004; Leigh, 2009; Healy and Malhotra, 2010; Cole et al., 2012; Bagues and Esteve-Volart, 2016). Voters' failure to correctly distinguish political competence from exogenous shocks beyond the control of a politician would imply improper democratic accountability. In this case, government incentives to generate policies that maximize the well-being of the citizens would be impaired.

The second empirical evidence that challenges the foundations of models of accountability comes from the fact that voters might be myopic (Healy and Malhotra, 2009). That is, citizens have a short-term memory when it comes to political issues and electoral choice, and they outweigh recent events in their assessment of government performance (Duch and Stevenson, 2006; Bechtel and Hainmueller, 2011). The political economy literature has highlighted that this characteristic of voter behavior could create incentives for the implementation of inefficient government policies (Healy and Malhotra, 2013). The timing of law enactments or public spending increases could be distorted to maximize the popularity of the incumbent (Nordhaus, 1975; Drazen and Eslava, 2010). Additionally, the design of public policies, such as the balance between relief spending with a short-term effect and preparedness spending with a long-term effect, could be affected in context of natural disasters because politicians would benefit electorally from policies that have a palliative and immediate impact on the well-being of voters (Healy and Malhotra,

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2009; Cole et al., 2012).

Other evidence that casts doubt on whether accountability models explain well the behavior of agents concerns assumptions about voter preferences. If one assumes that voters' preferences are tied to honesty, competence, and effort, then elected officials would have incentives to behave according to these attributes. An obstacle to improving the development of an economy will arise if voter preferences are grounded on norms and customs that are discordant with public policies that maximize the well-being of a society. For instance, in a context of clientelism, where citizens live in vulnerable social conditions, the preferences of voters may weigh in favor of individualistic transactions to the detriment of policies that benefit society more broadly. This could lead society into a poverty trap in which voters demand clientelism and the governments strive to deliver it (Acemoglu and Robinson, 2008). Hence, in the instance that voters provide the wrong incentives to politicians, accountability models would slide society into a slow-development course (Healy and Malhotra, 2013; Martinez-Bravo, 2014). Indeed, recent studies suggest that citizens' vulnerability is related to people participating in clientelism (Bobonis et al., 2017).

Governments

The combination of the supply of candidates and voters' evaluation of incumbents' performance have led to the formation of new governments. For their part, governments play a central role in formulating public policies. Therefore, it is crucial to scrutinize how they perform when subjected to subsequent elections. In an ideal scenario, governments are benevolent and always choose public policies that seek to maximize the well-being of the people. However, a central contribution of the public choice theory is the recognition that those individuals in power may have preferences that do not resemble those of a completely altruistic leader. Instead, it assumes that government behavior is the result of the process involving individual political actors who react to the incentives they face (Garrett and Sobel, 2003; Cohen and Werker, 2008).

At least three kinds of incentives can undermine the benevolence of a government. The first is related to the desire for re-election. As elected officials enjoy holding power, they can distort the allocation of public policies strategically to maximize their chance of remaining in office. In fact, several theoretical and empirical studies have documented that the provision of intergovernmental transfers responds to electoral incentives (Arulampalam et al., 2009; Brollo and Nannicini, 2012). Governments prioritize their constituencies and regions where there is political alignment to reap benefits in subsequent elections. This strategic behavior

happens mainly in election years, given that voters are short-sighted (Solé-Ollé and Sorribas-Navarro, 2008; Cole et al., 2012; Bracco et al., 2015).

Another behavioral distortion associated with re-election incentives is the fact that governments prioritize public policies that are more salient from voters. It has been documented that elected officials favor palliative policies rather than permanent policies (Healy and Malhotra, 2009; Neumayer et al., 2014). That is, incumbent politicians could try to remain in power by promoting clientelism. Among many perverse consequences of patronage politics, numerous studies claim that such a system exacerbates the allocative inefficiencies of the government, undermines the functioning of democratic institutions by leading to reduced political competition and the lack of provision of public goods (Besley and Burgess, 2002; Anderson et al., 2015; Bobonis et al., 2017).

The third incentive that could weaken governments in pursuit of maximizing social welfare is related to the opportunity of rent extraction (Leeson and Sobel, 2008; Burgess et al., 2012; Brollo et al., 2013). Incumbent politicians can take advantage of the deviations of resources from the public budget to fund political parties or for individual use. Although cases of government corruption are common everywhere, their levels diverge across countries, and there is robust evidence that bribery and graft are harmful to efficiency (Mauro, 1995; Treisman, 2007; Hunt, 2007; Yamamura, 2014).

Thesis structure

The above discussion leads to the conclusion that incentives matter considerably in both politics and economics. The many actors involved in democratic systems face several variables of a different nature to make decisions. These actors are subjected to all sorts of shocks that often produce conflicting incentives. The final response of the agents can sometimes be counter-intuitive and go against what would normally be expected. To formulate models that reproduce reality as closely as possible, it is necessary to empirically understand the behavior of agents when confronted with different shocks. This thesis aims to contribute to the understanding of the behavioral response of political parties, voters, and governments when affected by shocks such as corruption scandals, natural disasters, and windfall revenues.

The Brazilian political economy is an interesting context to address the goal of this thesis. The first reason is that Brazil is one of the biggest democracies in the world. In addition to the federal government, there are 26 states and 5,565 municipalities with regular elections every two years. Citizens choose the executive and the legislative branches of each jurisdiction through direct elections. The second reason is that it is a developing country with profound social and economic challenges.

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Furthermore, there is a high level of heterogeneity among its regions, which implies an attractive degree of variation to perform empirical analyses to access factors that trigger economic and social progress. The third reason is that Brazil has one of the richest and most disaggregated data collections on elections, the economy, and the environment at the municipal level.

The following chapter addresses the role of political parties and studies whether popularity shocks are crucial for electoral accountability beyond their effects on voter behavior. It specifically examines the impact of the revelation of information about a government's conduct on the types of candidates who stand for election. The empirical test focuses on the Brazilian city council elections in 2004 and 2008. The identification approach exploits the randomness of the timing of the release of audit reports on the use of federal funds by municipal governments. The study finds that when the audit reveals a high level of corruption (i.e., when it represents a negative popularity shock), the parties supporting the incumbent select more educated candidates. On the contrary, parties pick, on average, less educated candidates when the audit finds low levels of corruption (i.e., when it represents a positive popularity shock). These effects are stronger in municipalities that have easier access to local media. The evidence confirms that parties are strategic players that consider specific features of the electoral competition when making decisions and that their decisions are affected by shocks that influence the electoral race.

Chapter 3 is devoted to examining aspects of the voter preference assumptions. Citizen assessment of government performance is a cornerstone of successful democratic functioning. However, accountability is a double-edged sword. When voters misunderstand the stakes, and provide the wrong incentives to elected officials, political accountability leads to an implementation of suboptimal welfare policies. This paper reveals that an electorate can demand clientelism. To address this question, I study the behavior of voters in a context of vote-buying in Brazilian politics known as the *drought industry*. The data cover the Brazilian democratic elections from 1998 to 2012, and as empirical strategies I implement both fixed-effects models with panel data and a regression discontinuity design with heterogeneous treatment effects. I find evidence that after a drought, voters increase the vote share of local incumbent parties that are politically aligned with the central government to ensure the inflow of partisan government aid relief. Such behavior reinforces the central government's incentives to bias policies in favor of politically aligned municipalities to influence elections. Consequently, the cycle of distortion of aid relief allocation is perpetuated.

In connection with the findings indicating that the incidence of droughts and the Brazilian political economy are directly linked, Chapter 4 investigates the behavior of the local governments regarding the level of corruption. The analysis studies

whether the allocation of aid relief policies increases the level of corruption in the context of natural disasters. More specifically, the study investigates the number of federal emergency declarations against droughts, as a proxy for aid relief, and the number of irregularities in the local governments' expenditures found by auditors in Brazilian cities during a full mayor's term. The study implements an instrumental variable approach exploiting the quasi-random nature of the cycle component of a municipality's aridity relative to its trend. The findings show that an additional recognition of the state of emergency leads to an increase in corruption per capita for an entire term of a mayor.

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2. Popularity Shocks and Political Selection §

2.1. Introduction

Electoral accountability is a crucial mechanism that helps guarantee the sustainability of modern democracies. It allows sufficiently informed voters to assess the government's performance and hold politicians accountable for their actions (Barro, 1973; Mayhew, 1974). An extensive empirical literature shows that voters reduce their support of parties and officials involved in political scandals and reward politicians who are perceived to perform better (Ashworth, 2012; Ferraz and Finan, 2008; Snyder and Hirano, 2012; Chong et al., 2015; Costas-Pérez et al., 2012; Bagues and Esteve-Volart, 2016).¹

In this paper, we follow up on these findings and account for the possibility that political parties might anticipate voters' punishment, or reward, and change the composition of the pool of candidates selected to run for office accordingly. Specifically, we show that the release of information about government corruption affects the quality of candidates of the incumbent coalition. Intuitively, one might expect that if a political party (or coalition of parties) supports a government that faces a negative popularity shock, it might react by selecting more appealing and competent candidates to compensate for the expected poor results in the following election. Symmetrical intuitions could hold if there is a positive popularity shock: one might expect the party to behave strategically by reducing the share of costly, high-ability candidates given that the election will be less competitive.² These ideas are closely related to a recent strand of literature showing that political parties are strategic players that take into account specific features of the electoral competition when

[§]The paper in this chapter is coauthored with Gianmarco Daniele and Sergio Galletta.

¹Interestingly, this effect might not arise when voters already believe politicians to be malfeasant (Arias et al., 2017) or when the media market structure does not provide incentives to supply politically relevant information to their audiences (Larreguy et al., 2017).

²We follow recent studies by considering education level as a proxy for politicians' ability. See, for example, Ferraz and Finan (2009), Galasso and Nannicini (2011), Gagliarducci and Nannicini (2013), Besley et al. (2011) or Daniele and Geys (2015). In the next section, we discuss the reasons and the implications of the fact that high ability candidates are costly for political parties.

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making decisions (Galasso and Nannicini, 2011; Mattozzi and Merlo, 2015).

We address this issue in the Brazilian context by looking at the effect of popularity shocks resulting from the disclosure of reports about potential misconduct in local governments on the ability of candidates for city councilor in the 2004 and 2008 local elections. Hence, we relate the education level of candidates from the coalition supporting (or opposing) the incumbent government with the release of information about its (dis)honesty. This information was made available to voters through an anti-corruption program introduced in 2003 by the Brazilian central government that monitors how municipalities spend federal funds. The primary task is performed by auditors who examine municipalities' accounts to verify the correct use of federal transfers.

Municipalities are randomly selected into the anti-corruption program, and the timing of the release of the report is also random. These characteristics are crucial for our identification strategy, as they help to assess the multi-faceted endogeneity issue of linking government conduct and the general quality of local politicians. Another important aspect for our purposes is that the process of selecting candidates for the city council has a similar schedule in all Brazilian municipalities. This makes it possible to define when, during the term, the selection process is taking place. Further, although city councilors are elected using an open-list proportional system, parties play a central role in selecting candidates. In practice, a citizen is eligible to stand for election if he has been affiliated with the supporting party for at least 12 months before election day. Formally, the candidate list is decided during party conventions that, by law, are held in June of each election year. This list must be submitted at least 3 months before election day.

We exploit the randomness of the timing of corruption disclosures and, conditioning on the level of corruption, compare the ability of the pool of candidates in municipalities where the audits were released during the candidate selection period (i.e., from 12 to 3 months before the election) with that of municipalities in which the audits were released either before or after the selection period. While our identification strategy closely follows the one used in (Ferraz and Finan, 2008), we define treatment slightly differently to suit our distinct research question.

Our findings, which are based on 1,321 municipalities that were audited in the period 2003 – 2010, show that when reports are disclosed during the political selection period, they lead to a significant change in the average ability of the candidates running for the city council who belong to the party (or coalition) of the incumbent mayor.³ This effect crucially depends on the level of corruption reported. Indeed, the spread of information about government conduct can provide either a positive

³We reach a similar conclusion when studying mayoral candidates, though with less statistical power.

or negative shock to the expected electoral results, depending on citizens' prior beliefs about the quality of the government (Besley, 2007). On the one hand, we find that there is a decrease in the candidates' average education of nearly 4.5 months of schooling when low levels of corruption are reported (i.e., lower than the median). On the other hand, we find an increase of nearly 4 months of schooling when substantial corruption is reported (i.e., higher than the median). In other words, there is a difference of 8.5 months of schooling depending on the results of the audit, which corresponds to about 30–35% of one standard deviation. These symmetric results are not surprising in a context where trust in politics is very low, and releasing information about the absence of corruption could be more unexpected than the opposite.⁴ The effect is of a similar magnitude when considering the sample of freshmen candidates or when we look at the median level of education. Interestingly, the effect is larger when we constrain our analysis to municipalities that have easier access to information due to the existence of local radio stations. Indeed, our findings are confirmed when using a different proxy for candidates' ability, i.e. individuals' main occupation. However, we do not find any change in other observable characteristics such as age and gender. Moreover, the undesirable selection of low ability candidates in municipalities that experience a positive popularity shock fades away the closer the release of the audit report is to the election. Finally, we find that changing the composition of the candidate pool does not have a significant effect on electoral outcomes. In fact, voters still punish corrupt parties, in particular where local radio stations are available, and the elected candidates do not appear to have different characteristics compared to those elected in municipalities where the report was released outside the selection period.

To our knowledge, we are the first to estimate the effect of popularity shocks about the incumbent government on the quality of political candidates. Our findings are closely related to the recent literature that emphasizes that the characteristics of the electoral race affect candidate selection. This could be caused by a change in either individuals' incentives to enter politics or parties' incentives to select particular types of candidates (Caselli and Morelli, 2004; Galasso and Nannicini, 2011). Our interpretation of the results is more in line with studies that emphasize the role of the demand for politicians (i.e., party selection) than those that focus on the supply of politicians (i.e., self-selection of individuals). For instance, Galasso and Nannicini (2011) focus on parties' role in selecting candidates. Studying the Italian parliamentary elections, they show that better candidates (i.e., those with more years of schooling) are selected in districts where the electoral race is expected to

⁴Brazilians have very low confidence in political parties. More than 85% of the individuals interviewed in the 6th wave of the World Values Survey (2010 – 2014) responded that they have little or no confidence at all in political parties.

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be more competitive. Mattozzi and Merlo (2015) further show that the incentives to select high-ability candidates crucially depend on the electoral system. Specifically, high-ability candidates are less likely to be chosen in proportional than in majoritarian systems, as the latter are characterized by a higher level of electoral competitiveness. Likewise, Esteve-Volart and Bagues (2012), studying Senate and House elections in Spain, find that parties tend to select female candidates where they are less likely to be elected.

A second group of studies instead focuses on the supply side, showing that both monetary and non-monetary incentives might affect individuals' decision to enter politics. Kotakorpi and Poutvaara (2011), Ferraz and Finan (2008), and Gagliarducci and Nannicini (2013) show that higher salaries attract more educated individuals to politics in Finland, Brazil and Italy, respectively. Daniele and Geys (2015) and Daniele (2017) exploit shocks in the presence of criminal organizations in Italian municipalities to show that educated individuals drop out of politics when criminal groups are stronger, as they decrease the expected payoffs from politics (Dal Bó et al., 2006).⁵

Further, we complement the literature on political scandals. For instance, Ferraz and Finan (2008) focus on random audits in Brazilian municipalities to show that publicly exposed corrupt incumbents are less likely to be re-elected in the next election. In addition, Snyder and Hirano (2012) show that in US House elections, incumbents involved in scandals have a higher probability of losing their primary elections, and receive less votes in general elections compared to non-scandal incumbents. Chong et al. (2015) show that voters punish both incumbents and challengers after receiving information about the inefficient use of public funds in Mexico. Finally, Bobonis et al. (2016) find that foreseeable anti-corruption audits reduce corruption in the short-term, as they have a disciplining effect on the incumbent. In a similar vein, we find that the incumbent strategically reacts to the release of corruption news selecting different candidates.

Our results also contribute to the recent literature on the consequences of Brazil's randomized auditing policy. While Litschig and Zamboni (2013) and Avis et al. (2017) show that increasing the audit risk reduces rent extraction, Ferraz and Finan (2008), Brollo et al. (2013), and Muço (2016) confirm that releasing the audit reports indeed has an effect on electoral outcomes.⁶ Politicians seem to anticipate this potential electoral punishment by reducing malfeasance when they are eligible

⁵Finally, Baltrunaite et al. (2014) show that gender quotas increase the presence of (female) educated politicians. However, they do not discuss whether this is mostly driven by a change in the supply of or demand for educated politicians.

⁶These results are relevant to our study, as they provide additional support to the critical assumption that auditing disclosure affects voters' decisions.

for re-election (Ferraz and Finan, 2011). Interestingly, Brollo et al. (2013) hint that auditing policy has an impact on political selection: they show that the disclosure of the audit reports affects not only the mayor's likelihood of re-election but also the probability that he or she will run for reelection. Muço (2016) finds that municipal audits influence federal elections. Voters also reduce their support for the party of the incumbent mayor when voting in presidential elections. This result emphasizes that party labels matter in Brazil.⁷

The paper is organized as follows. In Section 2.2, we describe our main hypothesis. Section 2.3 describes the institutional setting. In Sections 4.4 and 2.5, we present the data and estimation strategy, respectively. The main results and additional tests are reported in Sections 2.6 and 2.7. We conclude in Section 2.8.

2.2. Hypothesis

In this section, we provide a brief explanation of our main hypothesis and clarify its underlying assumptions.

As explained above, we depart from the literature on the voting effects of popularity shocks, which concentrates on whether voters hold politicians accountable. We instead focus on the idea that parties select candidates they expect to be popular with voters. Our main hypothesis is that, as selecting high-ability candidates is costly, political parties will strategically pick them depending on their expected likelihood of winning the election. Specifically, a positive popularity shock that increases the chances of winning an election will reduce the propensity to select appealing, high-ability candidates. A negative popularity shock will have the opposite effect, which increases parties' motivation to select high-ability and competent candidates. This reasoning is based on the validity of three main assumptions.

First, it assumes that voters value candidates' ability when casting their vote. Indeed, there is a large literature showing that voters primarily care about competence when picking an elected officer (Kinder et al., 1980; Todorov et al., 2005). Importantly, there is also empirical evidence that education – which we use as a proxy for competence – is a valid predictor of electoral success (Dal Bó et al., 2017). Our findings confirm this evidence. Appendix Table A.1 shows that, after controlling for city fixed effects and other individual characteristics, educated candidates are much more likely to be elected.

Second, this hypothesis assumes that selecting high-ability candidates is costly for a political party. Several papers corroborate the idea that a lack of incentives

⁷Samuels and Zucco (2014) reach a similar conclusion, as they find that Brazilians conform their opinions on public policy to those of the parties they support.

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might limit the number of good citizens who choose to enter politics (Caselli and Morelli, 2004; Messner and Polborn, 2004). In fact, parties might benefit from selecting low-ability politicians, for example if they provide higher rents for the party or/and have a lower reservation wage (Besley, 2005; Dal Bó et al., 2006). Conversely, parties might select better candidates when an election is more competitive in order to maximize their chances of winning (Galasso and Nannicini, 2011; Mattozzi and Merlo, 2015). In line with these findings, in a recent working paper on Brazilian local politics, Colonnelli et al. (2017) show that there is a trade-off between education and party loyalty in the probability that party supporters will have a public-sector job. Finally, we find that educated candidates receive more party funds with which to finance their electoral campaigns, which suggests that selecting high-ability candidates is costlier for political parties (Appendix Table A.1).

Third, our hypothesis is valid in a political system in which political parties play a relevant role in selecting candidates, which depends on the country's institutional framework. In the next section, we provide evidence of the crucial role that Brazilian political parties play in local elections.

Our reasoning thus far has focused on an incumbent coalition that is directly hit by a shock. Yet if the effects of a shock spill over to affect the challenger's decision making, it is less clear what we should expect. First, the challenging party might expect the incumbent to react to the shock, and therefore has no incentive to change its own pool of candidates. Second, and perhaps more importantly, the type of spillover effect for the challenger is unclear. Assuming that voters have perfect information, if the incumbent is hit by a negative (positive) shock, the challenger should experience an electoral advantage (disadvantage). However, Chong et al. (2015) and Daniele (2017) show that voters may blame the entire political class for a negative popularity shock, and thus punish both the incumbent and the challenger. In this case, the negative (positive) effects of a shock would spread to the challenger as well.

2.3. Institutional Setting

2.3.1. Local Institutions and Electoral Rules

Brazil is a federation with three levels of government. In addition to the federal government, there are 26 states and 5,565 municipalities. Citizens choose the executive and the legislative branches of each jurisdiction through direct elections. Local governments have a central role in the provision of a variety of public goods (e.g., primary education, culture, health care, housing, transportation and municipi-

pal infrastructure). Transfers from upper levels of government cover a significant proportion of these expenditures.

At the municipal level, the mayor (*Prefeito*) has executive power while the city council (*Câmara de Vereadores*) exercises legislative power. The number of seats for councilors in each city council is based on population size.⁸ Mayors and city councilors are both elected for a term of 4 years, but while mayors are limited to two terms, city councilors have no term limits. The mayor plays a central role in defining the expenditure programs, and the city council is responsible for enacting municipality laws and overseeing the mayor's usage of public resources. Indeed, councilors have influence over the allocation of funds, for instance by proposing petitions, amendments and voting on the municipal budget proposal.

In municipal elections, voters are provided with a list of candidates running for mayor, at most one for each party (or coalition of parties), and a list of candidates for city councilor indicating which party they support.⁹ Voters can cast one vote for mayor and one vote for councilor. Alternatively, voters can select a party in the city council election without specifying a candidate. While mayors are elected with a majority of votes, councilor elections use an open-list proportional system, and the distribution of seats follows the d'Hondt method. This means that the number of seats assigned to a party in the city council depends on how many votes all candidates from that party received. However, for each party, only the candidates with the most votes will become city councilors.

2.3.2. Party Organization and the Candidate Selection Process

Importantly for our study, political parties play a significant role in selecting candidates to run in local elections (Mainwaring, 1999; Guarnieri, 2011; Avelino et al., 2012). Only parties registered with the Tribunal Superior Eleitoral (TSE) within one year before the elections can present candidates. Almost 30 different parties presented candidates in the 2004 and 2008 elections. However, the five biggest parties accounted for more than 65% of all mayors elected in the 2004 and 2008 elections. A similar time constraint applies to citizens who want to register to run in the mayoral or city councilor elections. They need to have their voting residency in the district they would like to represent and to be affiliated with their supporting political party for at least one year before the election. The electoral law requires parties to nominate candidates during local conventions, but it does not define how

⁸Local laws define the exact number of seats, but they have to follow federal laws that set the limit of seats according to the population in the municipality (Art. 29 of 1988 Brazilian Federal Constitution.). The number of seats ranges from a minimum of 9 in municipalities with less than fifteen thousand inhabitants, and a maximum of 55 in those with more than eight million people.

⁹Municipal elections are held in different years than federal and state elections.

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these conventions need to be organized or how candidates should be selected, delegating complete autonomy to the executive committee of each party. In the 2004 and 2008 municipal elections, parties had to choose their candidates and coalitions in a party caucus from June 10–30 of the election year.¹⁰ Final candidate lists had to be registered with the TSE before July 5.¹¹ The maximum number of candidates a coalition can put forward is twice the total number of seats it holds in the city council.¹² The parties must put forward a minimum of 30% female candidates.

Although local conventions formally control the candidate selection process, national and regional party bodies can influence it, depending on the party statute (Avelino et al., 2012; Ribeiro, 2013a,b; Guarnieri, 2011). These differ across parties depending on the number of eligible voters and on whether the system is based on elections or direct appointment. Some parties select candidates in regional or national committees, so local candidates do not have to go through an approval process. In other parties every candidate is elected by vote, and thus presumably no higher party body can modify the candidate list. Following this classification, Guarnieri (2011) labels three types of parties:¹³ monocratic, oligarchic and polyarchic. In monocratic parties, the decision-making process is in the hands of a single group that has an absolute majority of votes and absolute control over party decisions. This group includes the Partido Trabalhista Brasileiro (PTB) and the Partido Progressista (PP). In oligarchic parties, a small number of groups control a considerable proportion of votes. While no single group can control the party on its own, the small number of groups facilitates coordination, increasing the cohesion and stability of the coalition. This group includes the Partido da Social Democracia Brasileira (PSDB), the Partido Democrático Trabalhista (PDT) and the Democratas (DEM). In polyarchic parties, the organization is more decentralized. The decision making depends on constant negotiations and discussions across and within different party layers and bodies. Both the cohesion and the stability of coalitions comprised of such groups are likely to be more precarious. This last group includes the Partido dos Trabalhadores (PT) and the Partido do Movimento Democrático Brasileiro (PMDB).¹⁴

¹⁰These elections were held on October 3, 2004 and October 5, 2008.

¹¹Changes to the electoral law (Law No. 9504, of 30 September 1997) in 2015 affected some of the elements we exploit in our analysis. Beginning with the 2016 municipal elections: (1) party conventions are required to take place from July 20 to August 5 in election years; (2) candidate lists must be submitted by August 14 and (3) candidates must be affiliated with a political party for at least 6 months before election day.

¹²If a party is not in a coalition, the number of candidates cannot be more than 150% of the total number of seats it holds in the city council. Further, if not all candidates are selected during the party convention, party leaders may fill the remaining vacancies within 60 days of the election.

¹³Other scholars reach similar conclusions on classifying Brazilian parties (Ribeiro, 2013a,b).

¹⁴Other mechanisms, such as party affiliations, also influence the candidate selection process.

To conclude, the Brazilian (local) candidates selection process appears a multi-faceted one, where both local and high level party branches play a role. Importantly for our hypothesis, we face a selection process characterized by a central role of political parties.

2.3.3. The Brazilian Anti-corruption Program

In 2003, the Brazilian national government, led by Luís Inácio Lula da Silva, established an innovative anti-corruption program to improve the transparency of public spending and to tackle corruption in local governments. The Controladoria Geral da União, (CGU), a federal agency, was made responsible for auditing local spending that has been funded with federal transfers.¹⁵ Importantly for our analysis, municipalities are randomly selected to the auditing process. Lotteries are held every two or three months in the Caixa Econômica Federal, in Brasília, in the presence of the media and members of civil society. Only municipalities with more than 500,000 inhabitants are exempt from the lottery. Further, since lotteries are run independently for each state, the probability of being selected for an audit in a given year varies by state. The first lottery selected 26 municipalities. From the second lottery to the eighth, 50 municipalities were selected each time; 60 municipalities have been chosen since the ninth lottery.

For each selected municipality the CGU compiles a list of all federal transfers received since 2001. Typically, 10 to 15 auditors spend around two weeks in the municipal offices searching for potential anomalies. Once the auditors have completed the inspections, they prepare a report listing any irregularities and malpractices. The report is then sent to competent authorities for prosecution and made publicly available on the CGU website about 3 months later. As shown by Ferraz and Finan (2008), the results of the audits generally receive media attention, especially at the local level.

2.4. Data

To estimate the effect of corruption disclosure on the quality of the pool of candidates, we collect information about Brazilian municipalities from a variety of sources for the period 2001–2010, which covers two full municipal terms (i.e.,

Party leaders can deliberately withdraw existing affiliations or deny new ones (Guarnieri, 2011). Another mechanism is to replace, due to irregularities, a local committee with a provisional one directly under the control of a national/regional party committee. Indeed, there are many cases of replacements for reasons that are not clearly stated in the statute (Guarnieri, 2011).

¹⁵CGU Decree No. 247, June 20, 2003.

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2001–2004 and 2005–2008).¹⁶

Information about municipal-level corruption is taken from Brollo et al. (2013). Their data contain different measures of corruption for all 1,481 municipalities selected in the first 29 lotteries of the anti-corruption program (i.e., audits disclosed from July 2003 to March 2010). We use a broad definition of corruption that includes also irregularities that could be interpreted as government mismanagement rather than true corruption events.¹⁷

This definition includes illegal procurement practices and the diversion of funds – not justifiable payments.¹⁸ To account for the different levels of corruption across municipalities, our analysis uses the variable *Corruption*, which represents the amount of misappropriated funds as a proportion of the total amount audited.¹⁹ This information is available for only 1,422 municipalities, as it was not possible to compute the amount of resources involved in some irregularities. The level of observation is the municipality-term, meaning that we can clearly identify the term during which electoral irregularities took place. In other words, if a municipality was selected in a lottery in 2007, it will likely appear in the sample twice, as the auditors checked spending that occurred in both the 2001–2004 term and the first part of the 2005–2008 term. We present in Table 2.1 the summary statistics of *Corruption* divide by electoral term.

Table 2.1.: Corruption by electoral term

	N	Mean	Percentile			
			25%	50%	75%	99%
Term 2001-2004	1299	0.0462	0	0.001	0.049	0.532
Term 2005-2008	709	0.0561	0	0.017	0.068	0.465

Notes: This table reports information about the revealed corruption over the term 2001-2004 and 2005-2008.

We use candidates’ level of education as a proxy for quality. Specifically, we consider the minimum number of years necessary to attain a certain degree.²⁰ Alternatively, we use the share of candidates that completed *Mandatory School*. The

¹⁶Table 3.1 reports summary statistics of the variables described below.

¹⁷Therefore, in our analysis we do not distinguish between active and passive waste (Bandiera et al., 2009), as we rely on the fact that both types of misbehaviours might be salient for electoral accountability.

¹⁸The dataset and full details on how the measure is constructed are available at <https://sites.google.com/site/fernandabrollo/home/data>.

¹⁹In the original dataset this variable is called *Broad Fraction of the Amount*. Note that we do not consider a binary measure of corruption as in the Brazilian context, the presence of corruption might not be informative per se, conversely we exploit the severity of the phenomenon. See Section 2.1 and the footnote 5.

²⁰For candidates who started a degree but eventually dropped out, we assign half the number of years that would be needed to complete it.

TSE provides data on the level of education of candidates for the city council. For each municipality, we distinguish between the Average Education level of candidates in the incumbent's and the challengers' coalitions. The incumbent's coalition includes candidates from all the parties that run in the same coalition as the incumbent mayor's party and who belonged to the winning coalition in the previous municipal election.²¹ The challengers' coalition includes candidates running for all other parties. As an additional measure of quality we consider candidates' job. That is, we associate to each candidate the average national salary correspondent to her profession and we compute coalitions' *Average Income*.

Further, we compute the same variable considering new candidates (i.e., those who were not city councilors in the previous term) and re-running candidates. We also take into account the Median Education level of candidates and two other characteristics relevant for political selection – the *Share of Female* candidates and *Average Age*.²²

We compute similar measures for mayoral candidates, but focus only on candidates belonging to the same party as the incumbent. Finally, we consider the *Share of Seats Won* by candidates elected from the incumbent's coalition.

We control for local political preferences by creating a dummy variable that equals 1 if the incumbent government was led by a mayor from Partido dos Trabalhadores, and 0 otherwise. This variable also controls for whether the municipalities is ruled by the same party in power at the federal government. The 2000 Brazilian Census provides data about the Population of the municipality, monthly per capita *Income*, *Share of Population Employed*, the *Gini Coefficient* of income, the *Share of population with a Secondary Degree*, the *Share of Population in Urban* areas, the share of population working in different job sectors (*Services*, *Transport*, *Public* and *Commerce*). We also construct a dummy variable *Media*, which accounts for the presence of local radio stations in the municipal area. This information is taken from the 2006 municipality survey *Perfil dos Municípios Brasileiros: Cultura*.

To provide homogeneous results in all our estimates, we apply a sequence of restrictions to the original dataset on corruption provided by (Brollo et al., 2013). First, we remove eight municipalities, as they did not yet exist in 2000 when the population census was conducted. Second, we consider only municipalities in which at least some parties support the incumbent's coalition. Therefore, we remove 88 more municipalities. Finally, we exclude five municipalities in which there were no new candidates for city councilor (i.e., they were all incumbents). Clearly,

²¹We also show in Appendix Table A.4 our main results by defining the incumbent coalition as composed of all parties that belonged to the winning coalition in the previous municipal election.

²²We also construct the two latter variables and the *Average Education* level for the sample of elected candidates.

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by applying these restrictions the sample of municipalities we use in our analysis is no longer random. We address this issue in Section 2.7.

2.5. Estimation Strategy

The main objective of this study is to test whether information shocks that change citizens' voting behavior affect the quality of electoral candidates selected by parties. Specifically, we want to compare (1) the ability of candidates running for city council when the local government experiences an informative shock during the party selection period to (2) the ability of candidates running for city council in local governments that experience a similar informative shock at other points in time. In order to provide a reliable counterfactual analysis, we exploit the randomness of the timing of the disclosure of the audit reports to determine the group of *Treated* vs. *Control* municipalities.

To precisely determine the *selection period* (and hence the composition) of the two different groups, we identify two important dates in the electoral process. First, we account for the deadline for parties to provide their candidate lists (i.e., July 5, 2004 for the elections held in October 2004 and July 5, 2008 for the October 2008 elections). Second, we consider the cutoff date for citizens to be affiliated with a party in order to run as a candidate for that party (i.e., October 3, 2003 for the October 2004 elections and October 5, 2007 for the elections held in October 2008). Thus, a municipality is considered to be *treated* if the disclosure of the report occurred between 12 to 3 months before election day. Indeed, depending on the lottery results, the communication of the audit reports might occur during, before or after what we identify as the treatment period. We think our definition makes clear that the focus on parties' role in the selection process as the group of citizens willing to run for public office is mostly predetermined by the time the reports are disclosed.

It is worth noting that our strategy closely follows the one used in Ferraz and Finan (2008). However, as we are exploring a distinct research question, our definition of *treatment* and the way we split the sample between treated and control municipalities are rather different. Indeed, Ferraz and Finan (2008) define *treated* municipalities as those that received an auditing disclosure anytime before the election. Nevertheless, for the sake of completeness, we provide results where we use this latter definition of the treatment group. By doing so, on the one hand, we loosen the focus on parties' role in the selection process as now the group of citizens that can become eligible to run for election is not predetermined. For example, citizens can decide to enroll in a political party as a response to an audit report pub-

lished more than 12 months before the election, while this would not be taken into account with our primary definition of treatment. On the other hand, we control for the possibility that receiving a popularity shock in the three months before the election causes the withdraw from the electoral competition of previously selected candidates. For instance, political candidates from the incumbent political party that experience a very negative popularity shock might withdraw from the electoral competition so that the incumbent party may replace these candidates. Related to this point, we also report results where we exclude from the analysis municipalities that had the auditing disclosed more than 12 months before elections. We do so given that it is not straightforward to think of them as part of the control group as the informational shocks occurred before the end of the selection period.²³

Another aspect to take into account is that auditing reports reveal a misuse of public funds that could cover both terms we include in our analysis. This could be a problem if it were not possible to determine the exact timing of the misbehavior. In this were the case, citizens would be limited in their judgment as it would be less clear who was accountable for the discoveries made during the audit. Luckily, the corruption data we use categorize the misuse of public funds by term and municipality, which allows us to identify the treated and control groups for each term as reported in Figure 2.1.

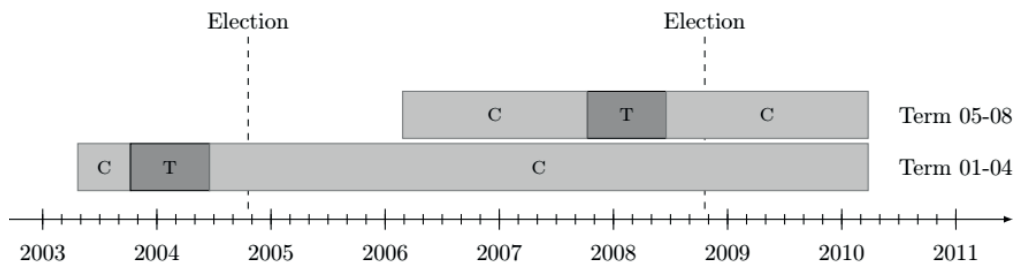


Figure 2.1.: Treated and control definition

Table 2.2 summarizes the sample composition, differentiating accordingly to the time of the disclosure of the auditing report. Over the two terms, we have 1,695 observations. Following our main definition of treatment: 327 are treated (19.3% of the sample) and 1,368 are controls (80.7% of the sample).²⁴ We have 182 treated and 960 control municipalities for the 2001 – 2004 term. Here, the treated municipalities are the ones drawn from the four lotteries disclosed between October 2003 and April 2004, while the control group includes all municipalities selected in the

²³In summary, the auditing disclosed more than 12 months before elections are used as part of the control group in Table 2.4, as part of the treated group in Table 2.6 and excluded from the sample in Appendix Table A.3.

²⁴Instead, if we consider the Ferraz and Finan (2008) definition we would have 905 treated observations (43.4% of the sample) and 790 as controls (46.6% of the sample).

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Table 2.2.: Sample details

		Term 2001	Term 2005	Total
Before elections	More than 12 months before election	68	242	310
	Between 12 months and 3 months to election	182	145	327
	From 3 months before election	93	0	93
After elections		799	166	965
Total		1142	553	1695

Notes: The table reports details on the sample of 1321 municipalities considered in our analysis. The level of observation is municipality-term.

other 25 lotteries that had audit reports released in the 2001 – 2004 term. For the 2005 – 2008 term we have 145 treated and 408 control municipalities. The treated municipalities were drawn from the three lotteries disclosed between January 2008 and June 2008, while the control group comprises all municipalities selected from the six lotteries disclosed between February 2006 and July 2007 plus the four lotteries disclosed between December 2008 and March 2010 that were audited for federal funds released in the 2005–2008 term.²⁵

Table 2.3 reports the summary statistics for the two groups of municipalities. Indeed, although our sample is a sub-sample of those selected within the randomized lotteries, it seems well balanced. Only the share of the population working in the industrial sector is different between the two groups at the 5% significance level. Note that including this variable in our specifications does not affect our findings.

Formally, we start the analysis by estimating the following ordinary least squares (OLS) model:

$$Y_{ist} = \beta T_{ist} + \delta \mathbf{X}_i + \gamma_s + \lambda_t + \epsilon_{ist}, \quad (2.1)$$

where i denotes the municipality, s the state and t the term. Y_{ist} can be the *Average Year of Schooling* of either all or freshmen candidates for city councilor, from either the incumbent or the challenger coalition. T_{ist} is a dummy taking a value of 1 for municipalities with audit reports released during the selection period (i.e., from 12 to 3 months before the election), while \mathbf{X}_i is a set of time-invariant municipal controls. Finally, γ_s are state-fixed effects, λ_t are term-fixed effects and ϵ_{is} is the error term. We use an OLS model with standard errors clustered at the municipality level.

²⁵The reports disclosed before February 2006 did not account for funds released in the 2005–2008 term. 374 municipalities appear in the sample twice, as they are part of the control group for the first term and part of either the control or treated group in the second term. Municipalities that were audited more than once appear in our sample only with reference to the first draw. In this way, we avoid the possibility that the potential long-term effects of the audit would bias our estimates (Avis et al., 2017).

Table 2.3.: Differences in the audit outcomes and observable characteristics

	Control group (1)	Treated group (2)	Difference (3)
Average education in $t - 1$ (incumbent)	9.324	9.348	-0.024
Average education in $t - 1$ (challenger)	9.262	9.165	0.097
Dummy party incumbent PT	0.215	0.202	0.013
Population	26516	23511	3004
Income	581.668	573.920	7.749
Share of pop. employed	37.720	37.313	0.408
Gini coefficient	0.560	0.558	0.002
Avg. municipal number of years of education	3.568	3.483	0.085
Share of pop. in public administration	2.109	2.196	-0.087
Share of pop. in agriculture	16.390	16.792	-0.403
Share of pop. in industry	4.038	3.592	0.446**
Share of pop. in service	6.705	6.551	0.154
Share of pop. in commerce	7.584	7.327	0.257
Share of pop. in transport	1.175	1.137	0.038
Share of pop. in service	6.705	6.551	0.154

Notes: This table displays the mean characteristics of 1321 municipalities (1695 municipality-term observations) that were audited by the *Controladoria Geral da União* (CGU) in the period 2003-2009 (i.e., from the 2nd to the 29th lottery). The control group (column 1) is composed by 994 municipalities which had disclosed the results of the auditing concerning the term 2001-2004 either before the 5th of Oct 2003 or after the 5th of Jul 2004, or had disclosed the results of the auditing concerning the term 2005-2008 after the 5th of Jul 2008 or before the 3rd of Oct 2007. Instead, the treated group (column 2) is composed by 327 municipalities which had disclosed the results of the auditing from 12 to 3 months before the elections (i.e., from one year before elections to the 5th of July 2004 or 2008). Column (3) shows the difference of the means and the level of significance. * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

Thanks to the random assignment of the auditing among municipalities, the coefficient β is the causal parameter of interest. In other words, it represents the average effect of the release of the auditing reports on candidates' education levels. State-fixed effects are included in all specifications, since the random assignment was stratified at the state level. Therefore, we ensure that our identification accounts for the heterogeneous probability of selection on the treatment faced by municipalities from different states. In addition, term-fixed effects account for other unobservable characteristics that might have changed from one term to the next. We include municipal controls in order to provide more precise estimates in case the randomization still produces a selection of treated and controlled municipalities with unbalanced characteristics.

We expect the estimates from Equation (2.1) to produce significant results if the auditing disclosure *per se* matters in the candidate selection process, regardless of the actual information provided. However, this would only be the case if the information disclosed differs from what the voters or parties expect (Ferraz and Finan, 2008) – i.e., if there is a systematic under- or over-estimation of a municipality's level of corruption.

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Therefore, we refine our baseline analysis by taking into account the results of the auditing process. We produce different estimations to test whether the effect of the disclosure on the selection of candidates depends on the kind of information reported. Specifically, we estimate the following equation:

$$Y_{ist} = \beta_1 T_{ist} \times Corruption_{ist} + \beta_2 T_{ist} + \beta_3 Corruption_{ist} + \delta \mathbf{X}_i + \gamma_s + \lambda_t + \epsilon_{ist}, \quad (2.2)$$

where $Corruption_{ist}$ is the share of corrupted resources. Moreover, to provide more flexible estimations and to account for potential non-linearity of the effects, we interact T with a variable identifying municipalities that belong to different quartiles of the distribution of corruption. In other words, we compare the effect of the disclosure of different levels of corruption on the level of education of the pool of candidates. This is a crucial point because, depending on the level of corruption revealed, the information shock could send either a positive or negative message to citizens. However, using this approach could raise important issues if our measure of corruption simply serves as a proxy for other municipal conditions. We check for this possibility in Section 2.7.

2.6. Results

2.6.1. Baseline Analysis

In this section, we report our central results. Specifically, Columns (1) and (2) of Table 2.4 show the results from the estimates of Equation (2.1), in which the dependent variable measures the level of education of all candidates in the incumbent's coalition. As expected, the disclosure of reports *per se* has no direct effect on candidates' quality. Moreover, the inclusion of municipal covariates does not seem to have a sizable impact on the main coefficient, which is in line with a balanced sample thanks to the randomization of the treatment. The remaining columns of Table 2.4 report estimates of Equation (2.2). Columns (3) to (5) report the effect of the auditing, interacted with the level of corruption, on the education of all candidates selected by the incumbent coalition's parties. Column (3) provides the first indication that reporting high corruption boosts the quality of candidates put forward by the incumbent's party. The interaction term has a positive sign, but it is not statistically significantly different from 0. Column (4) reports the estimate where the treatment status variable interacts with a dummy that identifies whether

a municipality is in the top 50th percentile of the distribution of our measure of corruption.²⁶ This result, coherently with Column (3), suggests that when the audit report is disclosed during the selection period, in municipalities with low levels of corruption candidates from the incumbent's coalition have around 4.5 months less of schooling (coeff. -0.378). However, if high levels of corruption are exposed, we observe an increase in the average education of slightly more than 4 months of schooling (coeff. $0.724 - 0.378 = 0.346$). Both coefficients are statistically significant. Therefore, there are more than 8.5 months of education difference depending on the signal provided by the audit. This difference corresponds to about 30–35% of one standard deviation.²⁷ The coefficients in Column (5) indicate the quartile of corruption and emphasize that the effect is stronger with higher levels of reported corruption.²⁸ The last three columns suggest similar results once the focus is only on new candidates (i.e., those who were not previously on the city council). This is a crucial finding, as using new candidates rules out the possibility that the estimates rely on a mechanical effect coming from the direct consequences of the audit (e.g., if the audit led to the incarceration of involved councilors who were mostly low-ability individuals).²⁹ Table 2.5 shows that the challenging coalition, which is not directly accountable for the outcome of the audit, does not change the educa-

²⁶The level of corruption in the median municipality was 0.5%. Note that our results are confirmed when considering as low level of corruption only cities without any corruption event.

²⁷Note that the magnitude of our findings is comparable with previous papers on political selection. For instance, Daniele and Geys (2015) and Daniele (2017) estimate a change in politicians' education due to mafia presence of about 35% of a standard deviation. Kotakorpi and Poutvaara (2011) find that a 35% increase in salary leads to a 5-percentage-point increase in candidates with high education. Baltrunaite et al. (2014) report a modest increase of 2-3 months in politicians' education after the introduction of gender quotas.

²⁸Appendix Table A.3 show that the results are similar if we only use as control those municipalities that have the auditing disclosure after the selection period. Appendix Table A.4 provides the results of changing the definition of the incumbent's coalition to include all parties that supported the mayor in the previous election, regardless of whether they do so in the following election. The results are similar, though the positive effect on the education of candidates running for office in highly corrupt municipalities is smaller than the one found with our standard definition of incumbent's coalition. This does not change if we consider the extended sample which includes all the 1396 municipalities where there is at least one candidate representing the old incumbent's coalition. Additionally, Appendix Table A.5 includes an alternative measure of schooling as a dependent variable – the share of candidates that completed *Mandatory School*. Our findings are confirmed. Specifically, the share of candidates that completed mandatory school decreases by 4.3 percentage points where low levels of corruption are found, and increases by about 3.5 percentage points with levels of corruption above the median. Further, in Appendix Table A.6, we show that our results are mainly driven by the candidates' selection for the 2004 elections. Finally, note that our main findings do not vary depending on the level of electoral competitiveness. Specifically, the change in candidates' ability is not significantly higher in close elections and neither in case the mayor is re-running for office or is facing term limits (results available upon request).

²⁹Appendix Table A.7 provides results on re-running candidates. While the magnitude of the effect is similar to the one estimated so far, this is barely significant.

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tion levels of its pool of candidates: the coefficients of interest are not statistically significant in any of the regressions.³⁰ As suggested in Section 2.2, the effects of a popularity shock hitting the incumbent might spill to or have the opposite effect on the challenger. Therefore, these (lack of) results can be read in the light of the unclear nature of the shock received by the challenger.

Table 2.4.: Audit releases and the quality of candidates - incumbent coalition

	All candidates					New candidates		
	(1)	(2)	Linear (3)	Median (4)	Quartile (5)	Linear (6)	Median (7)	Quartile (8)
Pre-selection auditing	0.016 (0.134)	0.041 (0.115)	-0.073 (0.131)	-0.378** (0.172)	-0.429** (0.194)	-0.160 (0.141)	-0.448** (0.191)	-0.531** (0.212)
Pre-selection auditing × corruption			1.567 (0.993)			1.796* (1.080)		
Pre-selection auditing × above median corruption				0.724*** (0.235)			0.723*** (0.258)	
Pre-selection auditing × second quartile of corruption					0.281 (0.393)			0.448 (0.459)
Pre-selection auditing × third quartile of corruption					0.629** (0.315)			0.691** (0.333)
Pre-selection auditing × fourth quartile of corruption					0.899*** (0.280)			0.906*** (0.314)
Corruption			0.297 (0.452)			0.377 (0.496)		
Above median corruption				-0.017 (0.097)			-0.004 (0.109)	
Second quartile					-0.005 (0.202)			0.139 (0.207)
Third quartile					0.002 (0.115)			0.040 (0.132)
Fourth quartile					-0.036 (0.126)			0.005 (0.143)
F-test inter			0.181	0.012	0.078	0.142	0.025	0.107
R ²	0.117	0.374	0.376	0.379	0.379	0.319	0.320	0.322
N observations	1695	1695	1695	1695	1695	1695	1695	1695
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is *Average years of education of candidates - as a city councilor - from the incumbent coalition*. *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT*, *ln(population)*, *income*, *gini coefficient*, *share of population with a secondary degree*, *share of population employed*, *share of population working in agriculture*, *share of population working in industry*, *share of population working in commerce*, *share of population working in transport*, *share of population working in service* and *share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * p < 0.1, ** p < 0.05 and *** p < 0.01.

So far we considered a municipality to be treated if the disclosure of the report occurred during the selection period that we define taking places between 12 to 3 months before election day. However, one can still think of a more general definition of treatment period by considering as treated all municipalities that experience the disclosure of the report before the election. Therefore, following Ferraz and Fi-

³⁰Our results are unchanged when considering the reaction of the most voted challenging coalition, either in the previous or in the present elections.

Table 2.5.: Audit releases and the quality of candidates - challenger coalitions

	All candidates					New candidates		
	(1)	(2)	Linear (3)	Median (4)	Quartile (5)	Linear (6)	Median (7)	Quartile (8)
Pre-selection auditing	-0.043 (0.095)	-0.012 (0.075)	0.014 (0.088)	-0.034 (0.112)	-0.048 (0.125)	-0.026 (0.090)	-0.091 (0.118)	-0.114 (0.133)
Pre-selection auditing × corruption			-0.452 (0.886)			-0.602 (0.845)		
Pre-selection auditing × above median corruption				0.038 (0.150)			0.052 (0.156)	
Pre-selection auditing × second quartile of corruption					0.075 (0.266)			0.138 (0.277)
Pre-selection auditing × third quartile of corruption					0.174 (0.191)			0.220 (0.200)
Pre-selection auditing × fourth quartile of corruption					-0.058 (0.185)			-0.054 (0.193)
Corruption			0.246 (0.283)			0.309 (0.297)		
Above median corruption				0.010 (0.068)			0.013 (0.069)	
Second quartile					0.077 (0.127)			0.005 (0.125)
Third quartile					-0.017 (0.084)			-0.036 (0.086)
Fourth quartile					0.067 (0.086)			0.069 (0.088)
F-test inter			0.839	0.983	0.932	0.618	0.846	0.857
R ²	0.172	0.515	0.515	0.515	0.515	0.499	0.499	0.500
N observations	1695	1695	1695	1695	1695	1695	1695	1695
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is *Average years of education of candidates - as a city councilor - from the challenger coalitions*. *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT*, $\ln(\text{population})$, *income*, *Gini coefficient*, *share of population with a secondary degree*, *share of population employed*, *share of population working in agriculture*, *share of population working in industry*, *share of population working in commerce*, *share of population working in transport*, *share of population working in service* and *share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

nan (2008), we report in Table 2.6 the results from regressions where now the treatment includes the entire pre-electoral period.³¹ From Column 1 (all candidates) and Column 3 (freshmen), we still observe that audits reporting a low level of corruption lead to a worse political selection, while candidates' ability increases in cities reporting above median levels of corruption. However, in the latter case, the magnitude of the estimated effect is lower than the one in Table 2.4. In Columns 2 and 4, instead, we separate the pre-electoral period in three parts: i) at least 12 months before the elections; ii) between 3 and 12 months before the elections; iii) between 0 and 3 months before the elections. It seems our results are driven uniquely by the period in between 3 and 12 months. Conversely, it appears that political selection does not change when the audit is released far from the elections (more than 12

³¹Brollo et al. (2013) apply also a very similar identification strategy.

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months). Also, audits released just before the elections (less than 3 months) do not affect candidates' selection. These results add credibility to our main definition of treatment.

Table 2.6.: Audit releases and the quality of candidates - before election

	All candidates		New candidates	
	(1)	(2)	(3)	(4)
Pre-election auditing	-0.311** (0.144)		-0.446** (0.157)	
Pre-election auditing × above median corruption	0.432** (0.184)		0.540** (0.201)	
Pre-election auditing (at least 12 months before)		-0.039 (0.145)		-0.206 (0.160)
Pre-election auditing (between 3 and 12 months before)		-0.393** (0.184)		-0.531*** (0.205)
Pre-election auditing (between 0 and 3 months before)		0.104 (0.318)		0.028 (0.332)
Pre-election auditing (at least 12 months before) × above median corruption		-0.248 (0.199)		0.025 (0.223)
Pre-election auditing (between 3 and 12 months before) × above median corruption		0.632** (0.252)		0.739*** (0.280)
Pre-election auditing (between 0 and 3 months before) × above median corruption		-0.150 (0.420)		0.006 (0.438)
Above median corruption	-0.044 (0.112)	0.072 (0.130)	-0.076 (0.126)	-0.026 (0.152)
F-test inter	0.052	0.041	0.014	0.105
R ²	0.377	0.380	0.321	0.322
N observations	1695	1695	1695	1695
State FE	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes

Notes: The dependent variable is *Average years of education of candidates - as a city councilor - from the incumbent coalition*. Corruption is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT*, $\ln(\text{population})$, *income*, *gini coefficient*, *share of population with a secondary degree*, *share of population employed*, *share of population working in agriculture*, *share of population working in industry*, *share of population working in commerce*, *share of population working in transport*, *share of population working in service* and *share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * p < 0.1, ** p < 0.05 and *** p < 0.01.

Overall, our main result points to the effect of information on shaping the selection of political candidates. Given the institutional background of the case at study, our preferred interpretation of the empirical evidence is that political parties, rather than politicians, do react to the expected electoral shocks. On the one hand, they increase the quality of candidates in municipalities where elections become more difficult (i.e., where a severe report has been released) and they need to increase their popularity. This is in line with previous research showing that parties select better candidates when they need them, namely during more competitive elections (Galasso and Nannicini, 2011; Mattozzi and Merlo, 2015). Importantly, this intuition is supported by the suggestive results of Table A.1 and other more accurate studies showing that educated individuals are more likely to be elected (Dal Bó

et al., 2017). On the other hand, elections become less risky when the local government experiences a positive shock, such as reporting no or little corruption. In this case, parties might decide to reduce the number of high-ability individuals if they are *costly*. For example, this is possible if the party shares rents with selected candidates, and this rent is higher the lower the public motivation (Besley, 2007), or if the party has to supplement candidates' salaries, as high-ability individuals have a higher reservation wage (Mattozzi and Merlo, 2015). In our context, as highlighted in Table A.1, more educated candidates tend to receive a higher direct economical support from political parties.

Below, we propose additional analyses that complement the previous findings. We only report results that focus on parties that support the incumbent, as we have already shown the absence of effects for the challengers.

2.6.2. Additional Analysis

A Test on the Role of Political Parties

Although we believe our results can likely be explained by parties' strategic behaviors, we cannot rule out the possibility that they are also influenced by changes in the supply of politicians (i.e., the pool of individuals willing to run for office). This potential effect is already partially reduced because our treated municipalities cannot select individuals who are external to the party. In other words, the pool of potential candidates is predetermined with respect to the treatment. Still, even within this sample of individuals, an effect could be expected. However, in principle, citizen–candidate models would predict results that are opposite to our findings (Caselli and Morelli, 2004; Osborne and Slivinski, 1996). For instance, one might expect high-ability individuals to have even lower incentives to enter politics after a political scandal. Similarly, it is hard to explain why high-ability candidates would refuse to stand for election in a municipality that appears to have a functioning administration, where it would be easier to be elected. Therefore, the effect of the disclosure of the audit report on individuals' willingness to enter politics will, if anything, adjust the size of our coefficients downward.

To provide additional support to our interpretation, we test the heterogeneity of the estimated effect with respect to the power structure of the mayor's party. As explained in Section 2.3.2, in centralized parties top and mid-level party leaders are the decision-makers, while ordinary members have a greater voice in decentralized parties. Therefore, we expect that changes in the supply side of politicians would affect our results mostly when the popularity shock hits a decentralized party. In other words, it is easier for individuals who are willing to run for office to enter the

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pool of candidates of a decentralized rather than a centralized party.

Table 2.7.: Audit releases and the quality of candidates - party organization

	All candidates		New candidates	
	Centralized (1)	Decentralized (2)	Centralized (3)	Decentralized (4)
Pre-selection auditing	-0.374 (0.246)	-0.260 (0.273)	-0.574*** (0.275)	-0.063 (0.323)
Pre-selection auditing × above median corruption	0.858** (0.373)	0.633 (0.430)	0.962*** (0.401)	0.240 (0.505)
Above median corruption	-0.123 (0.147)	-0.169 (0.190)	-0.110 (0.170)	-0.151 (0.200)
F-test inter	0.151	0.513	0.102	0.887
R ²	0.334	0.529	0.292	0.489
N observations	758	437	758	437
State FE	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes

Notes: The dependent variable is *Average years of education of candidates - as a city councilor - from the incumbent coalition*. Corruption is the share of the amount of the audited budget involved in general violations. Municipality controls include: $\ln(\text{population})$, *income*, *gini coefficient*, *share of population with a secondary degree*, *share of population employed*, *share of population working in agriculture*, *share of population working in industry*, *share of population working in commerce*, *share of population working in transport*, *share of population working in service* and *share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

We operationalize such a measure of party organization by following Guarnieri (2011)'s definition and consider parties to be decentralized if they are defined as polyarchic, and centralized if they are classified as either oligarchic or monocratic. We investigate the potential presence of a heterogeneous effect by splitting the sample into two groups depending on the incumbent mayor's party of affiliation and running separate regressions.³² Table 2.7 presents the results from these regressions. In Columns (1) and (3) we consider municipalities governed by centralized parties (i.e., PSDB, PDT, DEM, PTB and PP), while in Columns (2) and (4) we use the sample of municipalities ruled by decentralized parties (i.e., PT and PMDB). The results tend to confirm our intuition. Indeed, we find a significant change in the education of the selected candidates only when the incumbent hit by the popularity shock was affiliated with a centralized party. Column (1) suggests that the presence of a positive shock decreases the quality of the pool of candidates by nearly 4 months of education (coeff. -0.374), though the coefficient is not significant. A negative shock significantly increases the level of education by about 6 months of schooling (coeff. $0.858 - 0.374 = 0.484$). Column (3) reports similar results for freshman candidates. In this case, a significant, though negative, effect on candi-

³²It is worth highlighting that the mayor's party of affiliation in a given municipality is not randomly assigned, thus the following results are not intended to establish causal relationships.

date quality is also observed for positive popularity shocks. In Columns (2) and (4), in which we limit the analysis to decentralized parties, none of the coefficient estimates is significantly different from 0. The signs are coherent with previous findings, but the coefficients are smaller in size.

Alternative Dependent Variables

We first repeat our main analysis on a different measure of candidates quality. That is, instead of using candidates' level of education we focus on candidates' job (Besley et al., 2017). We estimated each candidate's salary based on the average national salary of her profession, which we used to compute coalitions' *average salary*. We used the logarithm of the average income of the incumbent coalition as the dependent variable. We report these new results in Columns (1) and (6) of Table 2.8. Both columns report very similar results: a negative popularity shock significantly increases the average income of the pool of candidates. The average income increase by 7.6% (coeff. $0.117 - 0.041 = 0.076$) among all candidates and 9.6% (coeff. $0.157 - 0.061 = 0.096$) in the sample of freshman. Hence our results are confirmed by this alternative measure of ability for the case of a negative shock. However, using this measure, we cannot confirm that a positive popularity shock affects candidates' selection.

Second, we check whether the effect of the disclosure on candidates' average level of education was driven by a general increase (or decrease) in the quality of the pool of candidates or whether it came from the selection of a few very good (or bad) candidates. Therefore, in Columns (2) and (7) of Table 2.8 we estimate Equation (2.2) by using the median level of education of candidates for city councilor as a dependent variable. The estimates reveal that the median level of education is also significantly affected, in the same direction as in the main analysis. In particular, the disclosure of a positive report decreases the level of median education by 5 months (coeff -0.431), while it increases by 3 months (coeff. $0.690 - 0.431 = 0.259$) when the report is negative. The point estimates are similar when looking at freshmen candidates. This is consistent with a general change in the composition of the pool of candidates.

Third, we replicate the principal analysis looking at the education of mayoral candidates. Therefore, the regression reported in Column (3) of Table 2.8 considers only municipalities in which the party of the incumbent mayor decided to present a candidate (either the incumbent mayor or a new candidate) in the next election. In Column (8) we focus on the sub-sample of new candidates. The main coefficients are not statistically significantly different from 0, but their direction is consistent

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Table 2.8.: Audit releases and candidates' characteristics

	All candidates					New candidates				
	Income (1)	Median edu. (2)	Mayor edu. (3)	Female (4)	Age (5)	Income (6)	Median edu. (7)	Mayor edu. (8)	Female (9)	Age (10)
Pre-selection auditing	-0.041 (0.049)	-0.431* (0.225)	-0.415 (0.466)	-0.012 (0.013)	-0.239 (0.377)	-0.061 (0.057)	-0.474* (0.253)	-0.615 (0.687)	-0.021 (0.015)	-0.250 (0.426)
Pre-selection auditing × above median corruption	0.117** (0.060)	0.690** (0.316)	0.309 (0.594)	0.010 (0.017)	0.449 (0.508)	0.157** (0.069)	0.600* (0.346)	0.434 (0.852)	0.010 (0.020)	0.558 (0.566)
Above median corruption	0.004 (0.026)	-0.018 (0.137)	0.177 (0.256)	-0.008 (0.007)	0.035 (0.218)	0.019 (0.031)	0.058 (0.151)	-0.023 (0.339)	-0.009 (0.009)	0.117 (0.252)
F-test inter	0.077	0.125	0.646	0.578	0.765	0.012	0.173	0.821	0.309	0.563
R ²	0.223	0.301	0.138	0.065	0.143	0.202	0.267	0.151	0.053	0.139
N observations	1665	1695	1195	1695	1694	1657	1695	663	1695	1694
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is $\ln(\text{average candidates income})$ in Columns (1) and (6), $\text{median years of education}$ in Columns (2) and (7), $\text{mayor cand. avg. number of years of education}$ in Columns (3) and (8), Share of female in columns (4) and (9) and average age in columns (5) and (10). *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT, population, income, Gini coefficient, share of population with a secondary degree, share of population employed, share of population working in agriculture, share of population working in industry, share of population working in commerce, share of population working in transport, share of population working in service and share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

with the results for candidates for city councilors.³³ The insignificant effect could be driven by two characteristics of the mayoral race that make the statistical test weaker. First, there is low variability in the pool of candidates from one term to the next, as many mayors can run for re-election. This is not an issue if we look

³³Recall that for mayor, instead of looking at the coalition, we focus on party affiliation.

at the results in Column (8), which pertain to new candidates. Indeed, the effects are larger than those reported in Column (3) for incumbent mayors. The second characteristic that could be driving the insignificant effect is the limited variability in the level of education of mayoral candidates: they are usually highly educated, particularly compared to the general population and city councilors (see Table 3.1).

Finally, we explore the possibility that the disclosure might also affect other candidate characteristics – namely age and gender, which have been analyzed in previous studies on political selection (Esteve-Volart and Bagues, 2012; Daniele and Geys, 2015; De Paola et al., 2010). We consider the average age of candidates and the share of female candidates as dependent variables. Our findings, reported in Columns (4 and 9) and (5 and 10) of Table 2.8, do not highlight any substantial change concerning these characteristics.³⁴

Timing of the Disclosure

In this section, we test the heterogeneity of the effect of the disclosure depending on when it occurred to determine whether the effect was mainly due to municipalities in which the audit report was released close to the end of the selection process. However, it could also be that audits released earlier have the strongest effect because the incumbent would have longer to change the selection of candidates. For this analysis we estimate the following regression separately for the sub-samples of municipalities with high and low corruption:

$$Y_{ist} = \sum_{\tau=1}^{\tau=3} \beta_{\tau} T_{ist} \times 1(t = \tau) + \delta \mathbf{X}_i + \gamma_s + \lambda_t + \epsilon_{ist}, \quad (2.3)$$

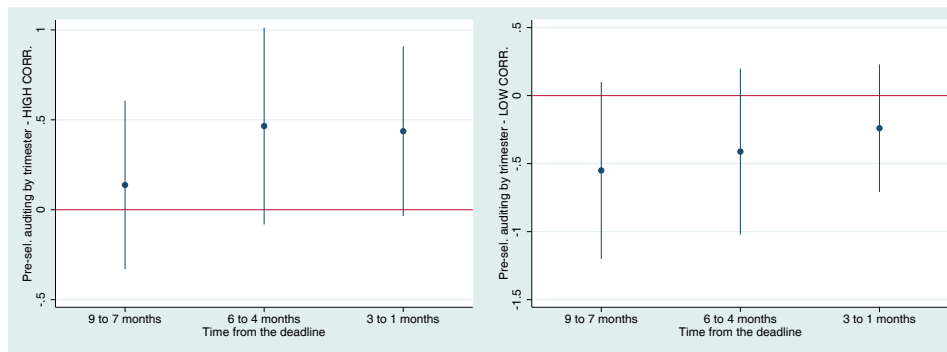
where T_{ist} interacts with a set of dummies for each trimester of the treatment period. We report the results of these estimates in Figure 2.2. In the left panel we display point estimates and (95%) confidence intervals from a regression limited to the sample of municipalities in which audit reports revealed high levels of corruption, while in the right panel the analysis is constrained to municipalities with low levels of corruption reported in their audit.

While all coefficients are borderline insignificant, we can draw two interesting implications. First, the difference between the point estimates from the two groups of municipalities remains relatively stable over time.

Second, the level of education of the selected candidates is higher the closer to the election the report is disclosed. This is true regardless of the report's severity. Thus, for instance, auditing disclosures that took place from 3 months to 1 month

³⁴We also tested whether the disclosure affects the share of freshmen elected candidates. Also in this case, we did not find any significant effect (results available upon request).

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(a) High corruption

(b) Low corruption

Note: The y-axis is heterogeneity of the effect of the disclosure and the x-axis represents when the disclosure occurred.

Figure 2.2.: Timing of the release - high and low corruption sub samples

before the candidate list deadline had no effect on education when they revealed low levels of corruption, but had a large positive effect in municipalities with high levels of corruption. Hence, for a policy maker that aims at improving the overall candidates' quality would be optimal to concentrate the auditing disclosure closer to the election. By doing so, it should avoid the unintended negative effect on candidates' quality coming from the disclosure of positive reports occurred far from the election day.

Electoral Results and Local Media

The mass media are the main channel through which citizens are informed about politicians' behavior (Enikolopov et al., 2011). Indeed, the effect of a popularity shock on the electoral results varies depending on the availability and accessibility of sources of information (Ferraz and Finan, 2008; Costas-Pérez et al., 2012). If the results shown so far come from parties that act in anticipation of the potential impact of the audit report on the electoral results, we should also find that parties' reactions depend on the presence of local media. Therefore, we expect audit reports to have a greater effect on the quality of candidates where citizens have easier access to information. To test this hypothesis, we follow Ferraz and Finan (2008) and account for the presence of local radio stations to characterize the different degrees of media penetration across municipalities. Therefore, we provide estimates by separately considering (1) municipalities that have at least one local AM/FM radio station and (2) municipalities with no local radio stations.³⁵

Initially, we look at the impact of the audit reports on electoral outcomes. It is

³⁵Similarly to what already discuss in section 2.6.2, media presence in a given municipality is not randomly assigned, which implies the following results are not intended to establish causal links.

important to emphasize that this analysis adds to previous findings about the effect of the Brazilian auditing policy on electoral results since, to our knowledge, we are the first to examine how the disclosure of corruption might also effect city council elections.³⁶ Hence, we replicate the baseline model using the *Share of Seats* won by the incumbent's coalition as a dependent variable. We first study the whole sample and then split it depending on whether municipalities have local radio stations. The results are reported in Columns (1), (2) and (3) of Table 2.9. The coefficients of interest are only significant when looking at municipalities where citizens have greater access to information. On the one hand, Column (2) shows that in municipalities where the disclosure of a positive report occurs during the selection period, the parties supporting the incumbent mayor win 5% more seats, while if the report is negative they win 5% fewer seats.³⁷ On the other hand, Columns (1) and (3) suggest that, on average (and in the absence of a media presence) audits have very little effect on the electoral results. We can draw three important implications from these results. First, we can confirm that local media play an essential role in the accountability process. Second, the publication of the audit reports has a real effect on the election. This is a crucial element as in order for parties to react to the results of the audit report, they have to expect that voters care enough about the contents of the report to change how they vote. Third, voters still punish corrupt parties in elections even if they could select better candidates. Indeed, the electoral reward of a positive report is not affected by a potential decrease in the quality of candidates.

We then apply the same procedure to our baseline estimation. In Column (4) we report the same results as Column (4) from Table 2.4, while Columns (5) and (6) report the results for the samples of municipalities with and without local radio, respectively. In municipalities with radio stations, when the disclosure of the report provides a positive signal (i.e., low corruption) there is a decrease in the average years of schooling of all candidates of a bit less than 7 months (coeff. -0.582), while there is an increase in education of 3 months when the report provides a negative signal (coeff. $0.845 - 0.582 = 0.263$). The coefficients are of a similar size when we consider either all candidates or only new candidates (Column 2). When we focus on municipalities with no local radio stations, Columns (3) and (4), the effect is either not significant or borderline significant and the reported coefficients are also smaller.

³⁶Ferraz and Finan (2008) show that corrupt mayors have a lower probability of being re-elected, while Muço (2016) finds that voters also reduce their support of a corrupt incumbent mayor's party in presidential elections.

³⁷If we replicate this analysis and add to our treated municipalities those in which the report is disclosed between the end of the selection period and election day, we have slightly different results. While the punishment for having a negative report is significant and of a similar magnitude, there is no electoral reward associated with receiving a positive report.

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Table 2.9.: Audit releases, electoral results and the quality of candidates by media presence

	Share of seats won			Education of candidates		
	All (1)	Local Radio (2)	No Local Radio (3)	All (4)	Local Radio (5)	No Local Radio (6)
Pre-selection auditing	0.011 (0.018)	0.053** (0.026)	-0.019 (0.025)	-0.378** (0.172)	-0.582*** (0.182)	-0.275 (0.266)
Pre-selection auditing × above median corruption	-0.031 (0.024)	-0.110*** (0.034)	0.024 (0.033)	0.724*** (0.235)	0.845*** (0.304)	0.578* (0.340)
Above median corruption	0.008 (0.010)	0.013 (0.016)	0.008 (0.014)	-0.017 (0.097)	-0.055 (0.134)	0.029 (0.141)
F-test inter	0.520	0.006	0.707	0.012	0.006	0.240
R ²	0.146	0.188	0.134	0.379	0.427	0.289
N observations	1695	730	965	1695	730	965
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is *Share of seats won by the incumbent coalition* in columns (1-3) and *Average years of education of candidates - as a city councilor - from the incumbent coalition* in columns (4-6). *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT, population, income, Gini coefficient, share of population with a secondary degree, share of population employed, share of population working in agriculture, share of population working in industry, share of population working in commerce, share of population working in transport, share of population working in service and share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * p < 0.1, ** p < 0.05 and *** p < 0.01.

Overall, the results seem to be coherent with our intuition, as the effect of the audit report appears to be larger, and statistically significantly different from 0 at the 1% level, only in municipalities where citizens are likely to be more exposed to the media (i.e., those with local radio stations).³⁸ Interestingly, the substantial change in the quality of the pool of candidates does not seem to be enough to significantly change the electoral results.³⁹ These results are consistent with recent studies providing evidence that voters do not seem to be affected by ex-ante justifiable parties' reactions to popularity shocks (Adams, 2012).

2.7. Identification Checks

2.7.1. Sample Selection

In our analysis we always constrain the sample to municipalities in which the incumbent's coalition decides to run for election. In other words, our sample is not random. This might produce a self-selection bias if the disclosure of the audit re-

³⁸We apply the same strategy by replicating the estimates in Table 2.8. The main findings do not seem to be dependent on the presence of the media, except if the dependent variable is the median level of education; in that case, similarly to the results of this section, the effect appears to be more pronounced where local radio stations are available.

³⁹In Appendix B we also provide results indicating the absence of an effect on the average quality of the elected candidates.

2.7. Identification Checks

port affects the stability of the coalition and hence the decision to run for office. To account for this potential issue, we test whether there is any difference in the probability of being part of our sample between municipalities in which the auditing report was disclosed during the selection period (i.e., the treated group) or at other times (i.e., the control group). Therefore, we take the sample provided by Brollo et al. (2013) which includes all municipalities selected in the first 29 lotteries. From this larger sample we keep the municipalities used so far in the paper as well as those that were only excluded from the analysis because the incumbent party did not present any candidates. We then create a dummy variable equal to 1 if the municipality was part of our sample, and 0 otherwise. We replicate the same procedure for the sample used in the mayoral election analysis. The whole sample would be composed of 1,858 municipality-term observations, which excludes only 163 observations from the analysis. We report in Table 2.10 the formal test of the potential presence of self-selection bias, including a number of t-tests showing that being treated does not affect the average probability of being part of our sample. This is also true when we look separately at the samples with high and low levels corruption. In conclusion, this analysis suggests that a coalition's decision to run for re-election is uncorrelated with the treatment, and that this is true for different levels of corruption.

Table 2.10.: Sample selection

	Control group (1)	Treated group (2)	Difference (3)
<i>All</i>			
Re-run city council (inc)	0.911	0.919	-0.008
Re-run mayor all (inc)	0.648	0.621	0.028
Re-run mayor new (inc)	0.362	0.334	0.028
<i>High corruption</i>			
Re-run city council (inc)	0.890	0.913	-0.023
Re-run mayor all (inc)	0.670	0.649	0.021
Re-run mayor new (inc)	0.367	0.351	0.016
<i>Low corruption</i>			
Re-run city council (inc)	0.930	0.919	0.004
Re-run mayor all (inc)	0.626	0.601	0.025
Re-run mayor new (inc)	0.356	0.322	0.034

Notes: This table reports differences between the control (1502 observations) and the treated (356 observations) group about the mean probability of being part of the sample of municipalities we use in the different sections of the paper. * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

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2.7.2. Robustness of the Corruption Measure

Another potential concern about our identification is that our corruption measure may be serving as a proxy for other municipal features. In fact, while municipalities are randomly assigned to either a treated or control group, the level of corruption is not random and might depend on specific municipal conditions. For instance, corruption is potentially higher where there is extensive use of public investments, which usually occurs in bigger and richer cities. If this is true, we may be estimating how the release of an audit (regardless of its severity) has a differential effect on candidates' education, for example in small vs. big or poor vs. rich cities. To help assess this potential issue, we replicate our baseline estimations and include additional interaction terms, where we multiply the treatment status dummy by a set of covariates that could be expected to be correlated with the level of corruption: *Population, Income, Education and Share of Pop. in Public Administration*. The results, presented in Table 2.11, reduce our concerns: in all the estimates, the interaction between the treatment status variable and the level of corruption is always significantly different from 0 and the coefficient is relatively stable across the different specifications. Moreover, the interaction terms that include the covariates never reach the conventional level of significance, whether analyzed in separate estimations (Columns 1 to 4) or jointly (Column 5).⁴⁰ Overall, this suggests that our measure of corruption is unlikely to be proxying for other municipal characteristics.⁴¹

2.8. Conclusion

This paper provides some of the first evidence on the effect of information about government behavior on the selection of political candidates. Using city council election data from Brazil, we find that an unexpected positive shock regarding the government's honesty has a detrimental effect on the quality of candidates put forward by its coalition in the next election. By contrast, it selects better candidates when there is a negative shock. Indeed, we show that these effects are present whether we use the average or median years of candidates' schooling. Importantly, the results of our analysis are of similar size when focusing only on freshmen candidates. Our findings also show that the accessibility of information plays a role,

⁴⁰We find similar results when the interaction term instead uses a dummy for the top 50% percentile or dummies for the different quartiles.

⁴¹Note that in this table, while the interpretation of the interaction term "Pre-selection auditing X above median corruption" does not change, the uninteracted coefficient "Pre-selection auditing" cannot be interpreted as in the previous tables, as in this case, we interact it with more than one variable in the same regression.

Table 2.11.: Robustness of the corruption measure

	(1)	(2)	(3)	(4)	(5)
Pre-selection auditing	-0.227 (0.870)	-0.686** (0.291)	-0.969* (0.499)	-0.318 (0.265)	0.128 (1.014)
Pre-selection auditing × above median corruption	0.725*** (0.234)	0.781*** (0.239)	0.798*** (0.248)	0.716*** (0.236)	0.806*** (0.246)
Pre-selection auditing × ln(population)	-0.016 (0.087)				-0.108 (0.100)
Pre-selection auditing × income		0.000 (0.000)			0.000 (0.000)
Pre-selection auditing × education			0.157 (0.110)		0.130 (0.161)
Pre-selection auditing × pop. in pub. administration				-0.025 (0.077)	-0.081 (0.083)
Above median corruption	-0.017 (0.097)	-0.025 (0.097)	-0.026 (0.097)	-0.016 (0.097)	-0.029 (0.097)
F-test inter	0.008	0.007	0.008	0.012	0.002
R ²	0.379	0.379	0.379	0.379	0.380
N observations	1695	1695	1695	1695	1695
State FE	Yes	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the *average years of education of candidates - as a city councilor - from the incumbent coalitions*. Municipality controls include: *dummy party incumbent PT, ln(population), income, Gini coefficient, share of population with a secondary degree, share of population employed, share of population working in agriculture, share of population working in industry share of population working in commerce, share of population working in transport, share of population working in service and share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * p < 0.1, ** p < 0.05 and *** p < 0.01.

as our results are clearer in municipalities that have radio stations. However, other candidate characteristics, such as the share of female candidates and the average age of the pool of candidates, are not affected. We also find that, despite the changes in the quality of candidates, neither the electoral results nor the types of elected representatives seem to be significantly affected.

Overall, our analysis first provides one of the few causal estimates supporting the predictions of recent studies showing that political parties react to specific characteristics of electoral competition (Galasso and Nannicini, 2011; Mattozzi and Merlo, 2015). Second, we show that information releases might have indirect effects on electoral outcomes. In light of our results, it could be plausible that studies showing a change in support for the incumbent after a popularity shock might underestimate the shock's *pure* effect on voters' preferences, as their voting decisions might also have been affected by changes to the quality of the pool of candidates. Finally, we find that the Brazilian policy analyzed in this study does not help improve the quality of elected politicians. On the contrary, in the absence of corruption we observe that the incumbent party selects lower-ability candidates. In other words, parties select

2. Popularity Shocks and Political Selection

lower-ability candidates when it is easier to win the election. However, our results also suggest that a way to account for this unintended effect of the anti-corruption measures on the dynamics of political accountability would be to disseminate all reports closer to end of the selection period. Indeed, only in this case the disclosure of positive audit reports has no consequences on candidates' quality.

Appendix A Appendix - Additional Tables

Table A.1.: Education, voting and party contribution

	Electoral success	Party contributions
	(1)	(2)
Low level of education	0.027 (0.020)	0.225* (0.026)
Medium level of education	0.070*** (0.020)	0.422** (0.023)
High level of education	0.147*** (0.021)	0.759*** (0.010)
Age	0.001*** (0.000)	0.002 (0.001)
Female	-0.049*** (0.004)	-0.668** (0.014)
R ²	0.578	0.395
N observations	243,058	243,058
Municipality FE	Yes	Yes
Term FE	Yes	Yes

Notes: These results come from a sample of all candidates as city councilor for which we have information on the level of education and that received party's contributions. The dependent variable is, in column (1), $\ln(\text{number of votes})$ received by a candidate running for a seat in the city council, while in column (2) is $\ln(\text{amount of party's contribution})$ to a candidate campaign. The coefficients on education have as a reference group the candidates with *no education*. *Low level of education* is a dummy indicating whether a candidate maximum achievement in school was to start or complete the "ensino fundamental" (i.e., from 1 to 9 years of schooling). *Median level of education* indicates whether a candidate maximum achievement in school was to start or complete the "ensino medio" (i.e., from 10 to 12 years of schooling), while *high level of education* indicates whether a candidate maximum achievement in school was to start or complete the "ensino superior" (i.e., from 13 to 17 years of schooling). Standard errors clustered at the municipality level in parenthesis * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

2. Popularity Shocks and Political Selection

Table A.2.: Summary statistics

Variable	Mean	Std. Dev.	Min	Max	N
<i>Municipalities sample</i>					
Average education - n. of years (incumbent)	9.938	2.186	1	17	1,695
Average education - n. of years (challenger)	9.805	1.675	3.571	14.258	1,695
Average education of freshmen - n. of years (incumbent)	9.919	2.331	1	17	1,695
Average education of freshmen - n. of years (challenger)	9.734	1.687	3	14.3	1,695
Average education of re-running - n. of years (incumbent)	10.137	3.298	1	17	1,466
Average income (incumbent)	1,155.935	455.112	88.745	4,207.949	1,665
Average income of freshmen (incumbent)	1,132.868	499.168	88.745	4,207.949	1,657
Mandatory school (incumbent)	0.678	0.231	0	1	1,695
Mandatory school of freshmen (incumbent)	0.679	0.247	0	1	1,695
Median education - n. of years (incumbent)	9.882	2.891	1	17	1,695
Average age (incumbent)	43.542	4.092	23.5	59	1,694
Share of female (incumbent)	0.211	0.133	0	1	1,695
Median education of freshmen - n. of years (incumbent)	9.822	3.058	1	17	1,695
Average age of freshmen (incumbent)	42.871	4.662	22	67	1,694
Share of female of freshmen (incumbent)	0.237	0.166	0	1	1,695
Average education elected - n. of years (incumbent)	10.472	3.007	1	17	1,540
Average age elected (incumbent)	42.964	6.348	22	69	1,540
Share of female elected (incumbent)	0.132	0.212	0	1	1,540
Mayor average education - n. of years (incumbent)	13.257	4.013	1	17	1,195
Mayor average education freshmen - n. of years (incumbent)	13.206	4.015	1	17	663
Decentralized party	0.305	0.461	0	1	1431
Share of seats won (incumbent)	0.330	0.196	0	1	1,695
Corruption	0.048	0.1	0	0.905	1,695
Media	0.431	0.495	0	1	1,695
Dummy party incumbent PT	0.212	0.409	0	1	1,695
Population	25,936.923	48,667.258	795	461,534	1,695
Income	580.174	317.944	80.967	3,062.481	1,695
Share of pop. employed	37.641	8.202	11.499	75.59	1,695
Gini coefficient	0.560	0.068	0.344	0.796	1,695
Municipality average education	3.552	1.088	0.746	7.711	1,695
Share of pop. in public administration	2.126	1.202	0.122	9.147	1,695
Share of pop. in agriculture	16.467	9.856	0.041	64.043	1,695
Share of pop. in industry	3.952	3.69	0	34.637	1,695
Share of pop. in service	6.675	2.763	0.257	18.756	1,695
Share of pop. in commerce	7.535	3.848	0.26	27.764	1,695
Share of pop. in transport	1.167	0.695	0	5.593	1,695
Share of pop. in service	6.675	2.763	0.257	18.756	1,695
Rerun (coalition)	0.917	0.275	0	1	1,858
Rerun (mayor/ first term)	0.607	0.489	0	1	1,256
<i>Individuals sample</i>					
No education	0.025	0.157	0	1	243,058
Low level education	0.375	0.484	0	1	243,058
Medium level education	0.364	0.481	0	1	243,058
High level education	0.235	0.424	0	1	243,058
Party's contributions	989.35	6,444.851	0.01	1,081,211.875	243,058
Number of votes	305.994	1,110.917	1	165,880	243,058

A. Appendix - Additional Tables

Table A.3.: Audit releases and the quality of candidates - restricted sample

	All candidates					New candidates		
	(1)	(2)	Linear (3)	Median (4)	Quartile (5)	Linear (6)	Median (7)	Quartile (8)
Pre-selection auditing	-0.092 (0.140)	-0.026 (0.120)	-0.144 (0.137)	-0.451** (0.182)	-0.496** (0.202)	-0.257* (0.149)	-0.558*** (0.202)	-0.634*** (0.222)
Pre-selection auditing × corruption			1.608 (1.030)			2.146* (1.119)		
Pre-selection auditing × above median corruption				0.749*** (0.242)			0.797*** (0.268)	
Pre-selection auditing × second quartile of corruption					0.229 (0.402)			0.430 (0.470)
Pre-selection auditing × third quartile of corruption					0.713** (0.322)			0.805** (0.342)
Pre-selection auditing × fourth quartile of corruption					0.845*** (0.288)			0.924*** (0.326)
Corruption			0.331 (0.510)			0.114 (0.552)		
Above median corruption				-0.048 (0.113)			-0.081 (0.127)	
Second quartile					-0.022 (0.224)			0.080 (0.231)
Third quartile					-0.115 (0.135)			-0.106 (0.158)
Fourth quartile					0.012 (0.146)			-0.016 (0.167)
F-test inter			0.173	0.012	0.067	0.115	0.017	0.099
R ²	0.123	0.375	0.378	0.381	0.382	0.315	0.317	0.319
N observations	1385	1385	1385	1385	1385	1385	1385	1385
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: These results come from a sample where we exclude municipalities for which the audit report was published more than 12 months before the election. The dependent variable is *Average years of education of candidates - as a city councilor - from the incumbent coalition*. *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT*, $\ln(\text{population})$, *income*, *gini coefficient*, *share of population with a secondary degree*, *share of population employed*, *share of population working in agriculture*, *share of population working in industry* *share of population working in commerce*, *share of population working in transport*, *share of population working in service* and *share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * p < 0.1, ** p < 0.05 and *** p < 0.01.

2. Popularity Shocks and Political Selection

Table A.4.: Audit releases and the quality of candidates - old incumbent coalition

	All candidates					New candidates		
	(1)	(2)	Linear (3)	Median (4)	Quartile (5)	Linear (6)	Median (7)	Quartile (8)
Pre-selection auditing	-0.102 (0.112)	-0.083 (0.093)	-0.169 (0.105)	-0.323** (0.137)	-0.358** (0.154)	-0.182* (0.106)	-0.370*** (0.139)	-0.402*** (0.155)
Pre-selection auditing × corruption			1.224 (0.878)			1.311 (0.905)		
Pre-selection auditing × above median corruption				0.414** (0.187)			0.482** (0.189)	
Pre-selection auditing × second quartile of corruption					0.179 (0.320)			0.164 (0.334)
Pre-selection auditing × third quartile of corruption					0.253 (0.245)			0.319 (0.247)
Pre-selection auditing × fourth quartile of corruption					0.622*** (0.229)			0.686*** (0.231)
Corruption			0.097 (0.409)			0.009 (0.416)		
Above median corruption				-0.006 (0.086)			-0.028 (0.086)	
Second quartile					0.092 (0.171)			0.080 (0.172)
Third quartile					0.058 (0.104)			0.038 (0.103)
Fourth quartile					-0.040 (0.110)			-0.066 (0.111)
F-test inter			0.279	0.073	0.190	0.278	0.037	0.124
R ²	0.126	0.419	0.420	0.421	0.422	0.417	0.418	0.419
N observations	1695	1695	1695	1695	1695	1695	1695	1695
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is *Average years of education of candidates - as a city councilor - from the old incumbent coalition*. *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT*, $\ln(\text{population})$, *income*, *gini coefficient*, *share of population with a secondary degree*, *share of population employed*, *share of population working in agriculture*, *share of population working in industry*, *share of population working in commerce*, *share of population working in transport*, *share of population working in service* and *share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * p < 0.1, ** p < 0.05 and *** p < 0.01.

A. Appendix - Additional Tables

Table A.5.: Audit releases and the quality of candidates - mandatory school

	All candidates					New candidates		
	(1)	(2)	Linear (3)	Median (4)	Quartile (5)	Linear (6)	Median (7)	Quartile (8)
Pre-selection auditing	0.000 (0.014)	0.002 (0.013)	-0.007 (0.015)	-0.043** (0.021)	-0.053** (0.024)	-0.018 (0.016)	-0.054** (0.023)	-0.060** (0.026)
Pre-selection auditing × corruption			0.124 (0.107)			0.189 (0.119)		
Pre-selection auditing × above median corruption				0.078*** (0.027)			0.086*** (0.029)	
Pre-selection auditing × second quartile of corruption					0.056 (0.045)			0.035 (0.053)
Pre-selection auditing × third quartile of corruption					0.071** (0.034)			0.080** (0.035)
Pre-selection auditing × fourth quartile of corruption					0.102*** (0.034)			0.102*** (0.037)
Corruption			0.036 (0.052)			0.048 (0.057)		
Above median corruption				-0.003 (0.011)			0.000 (0.012)	
Second quartile					-0.015 (0.021)			0.005 (0.022)
Third quartile					-0.001 (0.013)			0.003 (0.015)
Fourth quartile					-0.010 (0.015)			-0.000 (0.016)
F-test inter			0.320	0.023	0.146	0.123	0.016	0.155
R ²	0.111	0.286	0.288	0.291	0.292	0.251	0.253	0.254
N observations	1695	1695	1695	1695	1695	1695	1695	1695
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is *Share of candidates which finished mandatory school - as a city councillor - from the incumbent coalition*. *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT*, $\ln(\text{population})$, *income*, *gini coefficient*, *share of population with a secondary degree*, *share of population employed*, *share of population working in agriculture*, *share of population working in industry*, *share of population working in commerce*, *share of population working in transport*, *share of population working in service* and *share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * p < 0.1, ** p < 0.05 and *** p < 0.01.

2. Popularity Shocks and Political Selection

Table A.6.: Audit releases and the quality of candidates by electoral term

	Term 2001-2004				Term 2005-2008			
	Incumbent		Challenger		Incumbent		Challenger	
	All (1)	New (2)	All (3)	New (4)	All (5)	New (6)	All (7)	New (8)
Pre-selection auditing	-0.612*** (0.227)	-0.711*** (0.256)	-0.088 (0.152)	-0.148 (0.162)	0.157 (0.254)	0.112 (0.275)	0.052 (0.148)	-0.007 (0.153)
Pre-selection auditing × above median corruption	0.783** (0.304)	0.819** (0.339)	0.153 (0.204)	0.175 (0.212)	0.331 (0.368)	0.280 (0.405)	-0.197 (0.213)	-0.202 (0.219)
Above median corruption	0.007 (0.115)	0.030 (0.127)	-0.040 (0.081)	-0.074 (0.083)	-0.213 (0.182)	-0.164 (0.209)	0.111 (0.119)	0.183 (0.120)
F-test inter	0.032	0.028	0.891	0.721	0.237	0.546	0.718	0.337
R ²	0.389	0.332	0.518	0.496	0.375	0.321	0.515	0.508
N observations	1142	1142	1142	1142	553	553	553	553
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is *Average years of education of candidates as a city councilor*. *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT, population, income, Gini coefficient, share of population with a secondary degree, share of population employed, share of population working in agriculture, share of population working in industry, share of population working in commerce, share of population working in transport, share of population working in service and share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * p < 0.1, ** p < 0.05 and *** p < 0.01.

Table A.7.: Audit releases and the quality of candidates - re-running candidates

	Re-running candidates		
	Linear (1)	Median (2)	Quartile (3)
Pre-selection auditing	0.183 (0.226)	-0.185 (0.289)	-0.001 (0.314)
Pre-selection auditing × corruption	0.809 (1.338)		
Pre-selection auditing × above median corruption		0.732* (0.400)	
Pre-selection auditing × second quartile of corruption			-0.860 (0.764)
Pre-selection auditing × third quartile of corruption			0.177 (0.546)
Pre-selection auditing × fourth quartile of corruption			0.841* (0.471)
Corruption	-0.521 (0.816)		
Above median corruption		-0.146 (0.181)	
Second quartile			-0.324 (0.333)
Third quartile			-0.124 (0.225)
Fourth quartile			-0.303 (0.234)
F-test inter	0.646	0.265	0.245
R ²	0.229	0.231	0.234
N observations	1466	1466	1466
State FE	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes

Notes: The dependent variable is *Average years of education of candidates - as a city councilor - from the incumbent coalition*. *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT*, $\ln(\text{population})$, *income*, *gini coefficient*, *share of population with a secondary degree*, *share of population employed*, *share of population working in agriculture*, *share of population working in industry*, *share of population working in commerce*, *share of population working in transport*, *share of population working in service* and *share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

Appendix B Appendix - Elected Candidates

As outlined above, city councilors are elected using an open-list proportional system, and citizens still have a say over who will eventually be chosen. Here we analyze whether the disclosure of the report, affecting candidates' selection, has also an effect on the elected candidates. This may happen because selecting more- (or less-) educated candidates could lead to more- (or less-) educated elected politicians. To do so, we use the usual specification and consider the average years of schooling of candidates who were elected from the incumbent coalition as a dependent variable. We also provide alternative results that examine the share of females and the average age of the elected candidates. We report the estimated coefficients in Table B.1. The main coefficients in all three columns are not significantly different from 0, though the signs of the first column are in line with the once from the main analysis.

Table B.1.: Audit releases and elected candidates

	Education (1)	Age (2)	Female
Pre-selection auditing	-0.102 (0.263)	-1.031* (0.609)	0.018 (0.020)
Pre-selection auditing × above median corruption	0.064 (0.351)	0.866 (0.818)	-0.030 (0.028)
Above median corruption	0.033 (0.160)	0.082 (0.367)	0.010 (0.013)
F-test inter	0.965	0.342	0.696
R ²	0.256	0.086	0.047
N observations	1540	1540	1540
State FE	Yes	Yes	Yes
Municipality control variables	Yes	Yes	Yes

Notes: The dependent variable is *average years of education of elected candidates* in column (1), *average age of elected candidates* in column (2) and *share of female of the elected candidates* in column (3). *Corruption* is the share of the amount of the audited budget involved in general violations. Municipality controls include: *dummy party incumbent PT, population, income, Gini coefficient, share of population with a secondary degree, share of population employed, share of population working in agriculture, share of population working in industry share of population working in commerce, share of population working in transport, share of population working in service and share of population working in the public administration*. Standard errors clustered at the municipality level in parenthesis * p < 0.1, ** p < 0.05 and *** p < 0.01.

However, it is difficult to assess whether voters are responding to the strategic change in the composition of candidates a party chooses for its ticket/party list, as

B. Appendix - Elected Candidates

it is not possible to have a proper counter-factual. This would require comparing municipalities that experienced a shock in which (by accident) the composition of candidates did not change with municipalities hit by a shock where the composition did change. Without such a comparison, it is difficult to draw conclusions about whether voters are affected by parties' reactions to popularity shocks.

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3. Voters Sometimes Provide the Wrong Incentives [§]

3.1. Introduction

Democratic systems based on free elections are commonly accepted as one of the most efficient means to form welfare-enhancing governments. A wide range of models (Ferejohn, 1986; Persson et al., 1997; Besley and Burgess, 2002) suggest that public officers implement optimal policies when they are selected and sanctioned through universal suffrage. In this process, voters retrospectively evaluate the performances of incumbents and punish or reward them in subsequent elections. If one assumes that voters' preferences are tied to honesty, competence, and effort and that voters hold politicians accountable for those attributes, politicians have the incentive to behave accordingly. Democracy can produce inefficient policies, however, if voters' choices provide misleading incentives for elected officials. In this case, accountability plays a detrimental role and weakens the motives to implement optimal welfare policies. For instance, in a context where a narrow elite controls economic institutions and persists in power through patronage by distorting the allocation of public resources, retrospective voting could slide society into a slow-developing course (Acemoglu and Robinson, 2008). Understanding the circle that connects retrospective voting behavior with policy outcomes is a major line of research (Healy and Malhotra, 2013).

This study addresses this research topic by showing that voters could demand clientelism.¹ I find empirical evidence that an electorate rewards partisan alignment between different tiers of the government in order to ensure the inflow of public funds. A partisan alignment between the central government and local politicians is a strong determinant of intergovernmental transfers. Voters may decide to settle this connection to take advantage of the short-run benefits of patronage politics. However, this relationship comes at the cost of encouraging the government to bias the distribution of public resources toward party labels. Such behavior could be

[§]The paper in this chapter is single-authored.

¹Defined as large-scale exchanges of targeted goods and favors between patrons and client with a hierarchy of intermediaries (Fukuyama, 2014).

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translated as a failure of government efficiency and might lead to a long-run sub-optimal equilibrium (Healy and Malhotra, 2009). In a desirable situation, voters in such environments should condemn individualistic transactions that are more costly than socially beneficial (Hicken and Simmons, 2008; Shin, 2015). However, if voters demand it, elections could fall into a vicious circle of clientelism in which voters from different localities do not cooperate and in which everyone ends up worse off, similar to a Bertrand trap (Cabral and Villas-Boas, 2005).

The paper demonstrates such a relation by empirically studying voters' reaction to droughts, the government's accountability when assigning drought mitigation policies, and voters' responses to this (perverse) accountability in a vote-buying context known in Brazil as the *drought industry*.² Investigating voters' choices that undermine social welfare is suitable in contexts where there are frequent natural catastrophes because these events could lead to a situation where voters must make a difficult decision. More specifically, considering a scenario in which political alignment between tiers of government is already in place, voters face the following dilemma after a shock of drought. Although getting rid of incumbent politicians in elections should discipline governments into good performance, thereby blaming the incumbent for a lack of policies that have resulted in permanent solutions, voting for these incumbents may guarantee partisanship in the provision of palliative policies from other governmental levels. If citizens in vulnerable conditions who have their standards of living diminished by natural disasters still show increased support for politicians in power because of political alliances, then this finding would indicate a decision in favor of patronage politics rather than disciplining welfare-enhancing governments. This dynamic is exactly what the paper shows; droughts drive voters to plead for even more political alignment.

The dilemma faced by voters is based on three premises: 1) whether voters punish incumbents because of natural disasters, 2) whether voters reward governments because of aid relief, and 3) whether governments bias the allocation of aid relief. More precisely, the first hypothesis is that droughts alter political preferences as voters blame politicians for worsening living conditions (Gasper and Reeves, 2011; Bruckner and Ciccone, 2011). This willingness to blame the political class may arise either because governments have not implemented adaptation policies to mitigate the effects of droughts or because governments are responsible for some of the vulnerable social conditions exacerbated by this kind of natural disaster (Quiroz Flores and Smith, 2013; Heersink et al., 2017). I analyze two types of

²The *drought industry*, or *indústria da seca*, is an expression used to refer to the clientelism relationship between the elite (*coronéis*) and the peasants (*flagelados*) who exploit the humanitarian collapse driven by droughts in Brazil to obtain electoral influence in exchange for votes for water distribution. This term was first mentioned by Callado (1960). More details in Appendix A.

3.1. Introduction

Brazilian elections at the municipality level: presidential (1998, 2002, 2006, 2010) and mayoral (2000, 2004, 2008, 2012). Implementing a fixed effects panel data analysis, I find that voters indeed punish the incumbent party for droughts in elections. On average, the president's party vote decreases by five percentage points, and the mayor's party vote decreases by seven percentage points.

The second hypothesis is that central governments are electorally rewarded for distributing mitigation policies that aim to alleviate the misery triggered by water scarcity. An increase in the public supply of water and food and infrastructure investment in the affected areas may reduce economic losses and relieve the pain of the population. Consequently, voter's anger against the political class could be attenuated or even converted into popularity (Healy and Malhotra, 2010; Bechtel and Hainmueller, 2011). In the same line, the paper shows that the adverse impact of drought is, on average, completely compensated by the provision of palliative and prevention policies in presidential elections but not in mayoral elections. The results also indicate that voters are keenly aware that it is the central government that grants the droughts aid relief and that voters react more positively to palliative rather than preventive policies.

The third hypothesis is that governments tend to allocate intergovernmental transfers strategically. If the central government has strong opportunistic electoral objectives, the government may distort the provision of aid relief in order to maximize its chances of re-election (Arulampalam et al., 2009). Indeed, the nature of disaster relief as an emergency expenditure creates room for discretionary allocations, and disaster relief is frequently associated with misuse of public spending (Garrett and Sobel, 2003; Cole et al., 2012). The capture of drought mitigation policies by the government as a tool to influence electoral outcomes is at the heart of inefficient resource allocation. Applying a flexible version of the regression discontinuity design (RDD) with heterogeneous effects proposed by Becker et al. (2013), I show that before presidential elections, any aligned municipality is more likely to receive palliative policy than the non-aligned ones. The effect is enormous and reaches almost 74 percentage points for droughts categorized as extreme. I do not find similar opportunistic behavior with the provision of preventive policies or when the target elections are for mayors.

The dynamics between the negative shock of droughts on elections and the political use of mitigation policy converges to a predicament for the electorate, which in turn allows for the assessment of voters' choices. In one regard, disciplining government performance implies punishing politicians in power after a serious drought. However, in the circumstance where there is already party symmetry between local politicians and central government, punishing incumbents would extinguish the political alignment. In this case, changing the incumbent party for the opposition

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parties would result in fewer chances for intergovernmental transfers coming from the central government. If voters acknowledge the political alignment advantage to the extent that they reward it or are even aware that there is a bias, incumbents have no incentive to change the way they operate, which will perpetuate the problem. As a result, disaster relief could become an institutionalized form of clientelism between governments and citizens, which in turn prevents the implementation of more sustainable adaptation policies against droughts, as denounced in Nelson and Finan (2009).

The main analysis adopts as empirical strategies both the fixed effects panel data model and also the RDD with heterogeneous treatment effects. The study reveals that voters reward party alignment in mayoral elections but that there is no evidence of strategic voting during presidential elections. The reward for the partisan alliance rises after the shock of a drought. I find that droughts increase the effect of political alignment by approximately 8 percentage points in the vote share of the mayor's party. In presidential elections, however, voters do not seek alignment, and after a severe drought, the vote share of the president's party decreases by 12 percentage points in aligned municipalities. This behavior may be explained by the fact that voters care about their mayor being aligned with the president before presidential elections (when this may be beneficial by enhancing the chances of obtaining a palliative policy), but voters do not seem to care about the president being aligned with the mayor in the period before mayoral elections (when the central government seems to act less opportunistically).

The paper contributes by showing that voters can provide the wrong incentives. The findings complement the existing literature on patronage politics (Shin, 2015; Anderson et al., 2015). In particular, in a randomized control trial, Bobonis et al. (2017) show that citizens granted a residential water cistern in drought-prone areas of Northeast Brazil are less likely to ask politicians for private benefits. This paper supports their most important conclusion but differs in at least two important aspects. First, while the above experiment is based on an intervention from an international development agency, which means a third party that is independent from the government-voter relationship, my analysis takes into account policies financed by Brazilian governments. This difference is crucial because politicians claim credit, which is a relevant intrinsic feature of the political economy of the *drought industry*. Otherwise, there would be no incentive for discretionary resource allocation, and government inefficiency would not exist. Distinct from Bobonis et al. (2017) who, unsurprisingly, do not find that the delivery of cisterns directly benefits politicians and conclude that political alignment plays no role, my findings show that voters reward central governments for the provision of drought policy and that the political alignment advantage is considerable in mayoral elections. In this

sense, the findings of this paper are closer to those of Martinez-Bravo (2014), who shows that villages in Indonesia with a village head who is prone to clientelism experience a stronger electoral alignment with the district-level government. That is, those village heads are more likely to vote for the party that is politically aligned. Second, this paper provides a notion of long-run patronage equilibrium. Instead of analyzing a single electoral period for mayor, my data extend over a period that captures four national elections and four municipal elections.

The paper is related and contributes to several branches of the literature. First, I use an innovative measure of drought, the *aridity index*, which performs better than the measures used in previous studies in Brazil (Rocha and Soares, 2015; Larreguy and Monteiro, 2014). This measure is already commonly used in the hydrology literature (Wolfe, 1997; Arora, 2002). Second, this work contributes to the voter responsiveness literature by adding evidence that voters punish incumbents because of natural disasters (Barnhart, 1925; Achen and Bartels, 2004; Gasper and Reeves, 2011; Cole et al., 2012; Fair et al., 2015) and reward them because of aid relief (Healy and Malhotra, 2010; Fuchs and Rodriguez-Chamussy, 2014). However, this paper opposes the previous work showing that the effect of aid relief is not strong enough for a net increase in popularity (Bechtel and Hainmueller, 2011; Gasper and Reeves, 2011; Lazarev et al., 2014). Third, this paper extends the literature that studies the strategic allocation of resources by central governments that intend to maximize their electoral support (Downton and Pielke Jr, 2001; Solé-Ollé and Sorribas-Navarro, 2008; Cohen and Werker, 2008; Brollo and Nannicini, 2012; Larreguy and Monteiro, 2014). Furthermore, this work provides evidence that the type of election matters for discretionary bias. Lastly, the paper contributes to the literature that explores how retrospective voting influences policy outcomes (Healy and Malhotra, 2013).

The remainder of the paper proceeds as follows. Section 3.2 gives a conceptual framework. Section 3.3 presents the data. Sections 3.4, 3.5, and 3.6 show the results of the empirical analysis. Section 3.7 concludes. Appendix A gives an overview of the institutional setting and explains the roots of the political anecdote *drought industry* and its transition to the current situation. Appendix B shows the robustness checks of the main analysis. Appendix C shows the RDD validation graphs. Appendix D provides the data sources.

3.2. Framework

Consider a setting in which there are three agents: voters, the mayor, and the president. Voters are subject to two types of time-separated elections: mayoral contests

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and presidential contests. In every election year, voters observe the past and make a judgment related to government performance (retrospective voting) (Nordhaus, 1975), build expectations on what could maximize future benefits (forward-looking voting) (Drazen and Eslava, 2010), and then decide to vote against or in favor of the incumbent party. Retrospective voting depends on two observed events: drought and mitigation policies against droughts. Politicians, in turn, study voter decisions and adjust their objective function (Persson and Tabellini, 2002).

Voters blame incumbents for droughts

An extended period of water scarcity in a region affects local communities through economic and health mechanisms (Girma Kebede, 1988; Felbermayr and Gröschl, 2014; Rocha and Soares, 2015). In these communities, voters, who see themselves in a worsened condition, are more likely to infer a low effort from incumbents and blame them for the increase in population vulnerability.

One can argue that drought is an event that is beyond the influence of politicians and that it is illogical to expect a voter to punish incumbents for reasons other than government performance. However, the willingness to blame incumbents for natural disasters is valid under both of the following assumptions of voter information about drought as an exogenous event from political decisions: fully aware and insufficiently aware. In the case in which voters are fully aware that politicians in power do not influence the incidence of droughts, voters may still blame incumbents for the lack of preparation for negative shocks. In the case in which voters are insufficiently aware, voters may vote against the politician in office in subsequent elections as the result of linking their lower standard of living to the performance of the incumbent.

The economic literature suggests that there are at least two reasons, not mutually exclusive, why voters may not disentangle economic outcomes originating from exogenous shocks, such as droughts, from those resulting from political decisions. The first reason entails a context with asymmetric information, where voters observe the economic condition and mistakenly infer politicians' quality and effort. The second reason is *attribution errors*, where voters might be wrong about the causes of economic conditions (Nordhaus, 1975; Wolfers, 2011; Bagues and Esteve-Volart, 2016). Along the same line of reasoning, one arrives at the following hypothesis to be tested:

- Hypothesis 1: droughts have a negative impact on incumbents in elections.

The economic literature on natural disasters has previously addressed this issue. For instance, by analyzing extreme weather events in India to examine the hypothe-

sis that voters respond to events that are beyond a government's control, Cole et al. (2012) show that on average, incumbent parties that run for re-election lose approximately three percent of the vote per one-standard deviation change in district-level rainfall from the optimum level. Similar evidence of adverse effects on incumbent re-elections due to droughts can be found for the U.S. (Barnhart, 1925; Achen and Bartels, 2004) and for Sub-Saharan African countries (Bruckner and Ciccone, 2011). Evidence for such adverse effects can also be found for other types of natural disasters, such as floods (Arceneaux and Stein, 2006; Fair et al., 2015), tornadoes (Healy and Malhotra, 2010), hurricane (Abney and Hill, 1966), and others (Ahlerup, 2013; Quiroz Flores and Smith, 2013; Lazarev et al., 2014).

Voters reward incumbents for aid relief

In contrast to droughts, the aid relief from governments may ease public dissatisfaction. Households that experience thirst and hunger have their living conditions ameliorated when external agents send supplies of water and food. Consequentially, voters might reward politicians in power (or just not punish them) because of the responsiveness of politicians after natural catastrophes. Even if the mitigation policy does not come directly from a policy sphere that is controlled by the politician in office, voters may infer that good politicians are better able to capture extra resources from distinct government tiers to reduce drought consequences. Thus, aid relief from a specific level of government may indirectly affect elected officials at other levels of government. In the same logical line, the next hypothesis to be tested is as follows:

- Hypothesis 2: mitigation policies have a positive impact on incumbents in elections.

Earlier works have addressed the reward that voters grant politicians in elections for responsiveness to natural disasters. For instance, Healy and Malhotra (2010) study the effect of tornadoes on U.S. elections when disaster declarations are made and show that the incumbent party's presidential vote share receives an almost four-percentage point increase. Notably, this study suggests that the benefit of such declarations mostly outweighs the electoral cost of severe weather. Other studies that show that aid relief converts a negative shock of natural disasters into popularity can be found for droughts (Gasper and Reeves, 2011; Fuchs and Rodriguez-Chamussy, 2014), floods (Bechtel and Hainmueller, 2011), and fires under a non-democratic regime (Lazarev et al., 2014). I do not find studies showing that governments fail to convert the negative shock of a natural disaster into a net increase in popularity with disaster response.

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Aid relief bias

In the Brazilian context, mayors have limited available resources, and most of the local public revenues come from transfers from higher levels of government. The president controls the greatest share of public resources and has more decision-making power to allocate them.³ When there is a drought shock, mayors alone are incapable of properly responding to citizen demands. Mayors are constrained by the small level of fiscal capacity to deal with unexpected events. Thus, mayors have to beg the central government for help. In turn, the central government takes into account two elements when deciding where to send resources for mitigation policies. The first is the severity of the drought. High levels of water scarcity lead to a high probability of federal intervention. The second is political alignments, meaning the mayors in the same party as the president have higher chances of receiving aid interventions than mayors in the opposition party.

There could be two types of political bias experienced by the central government in sending aid relief, namely, opportunistic and partisan bias (Arulampalam et al., 2009). Assuming that mitigation policies affect voters' perceptions of politician quality, this distortion could be used as an instrument to influence electoral outcomes. An upcoming presidential election could increase the incentives for the central government to prioritize the allocation of public resources when there is a political alliance with local politicians in office. In this case, voters might easily assimilate central government action and reward the president's party directly in the voting booth. There will be a political bias, then, if the central government has a sufficiently opportunistic electoral motivation. For instance, when studying the U.S. presidential discretion in disaster declarations involving floods, Downton and Pielke Jr (2001) find that presidents tend to issue disaster declarations more generously in years when they are facing re-election.

If the central government has a partisan performance motivation, then one should observe similar behavior before local elections. Furthering the allocation of mitigation policies to aligned municipalities just before the mayoral elections would boost the re-election performance of local politicians. On the one hand, the mitigation policies could help maintain the political alliance with municipalities; however, the central government has low autonomy in the extraction of rent from municipal governments. In this sense, for instance, if the central government is relatively more opportunistic than partisan, one would expect a greater impetus from the party of the president to favor aligned municipalities just before the presidential elections (Arulampalam et al., 2009). Hence, under municipal election circumstances, the

³The most important source of municipal revenues is represented by federal transfers, which account for 65% of the municipal budget on average (Brollo and Nannicini, 2012).

central government might exhibit comparatively less biased behavior.

Government efficiency is guaranteed if the central government is fully benevolent. In this desirable situation, elected officials pursue neither opportunistic nor partisan electoral objectives. Thus, aid delivery policies are orthogonal to party labels, and no bias occurs. Otherwise, the discretionary of allocation of public funds for the purpose of influencing incumbent electoral performance rather than enhancing general public welfare would be a failure of efficiency in the provision of the public policy (Healy and Malhotra, 2009). These discussions call for testing the following hypothesis:

- Hypothesis 3: the central government biases the allocation of mitigation policies in favor of aligned municipalities.

The determinants of intergovernmental transfers have been exhaustively studied, and there is a convincing set of results indicating that party alignment plays a significant role in the allocation of public resources in various institutional contexts. Such evidence can be found for the U.S (Downton and Pielke Jr, 2001; Garrett and Sobel, 2003), Spain (Solé-Ollé and Sorribas-Navarro, 2008), India (Arulampalam et al., 2009), and Brazil (Brollo and Nannicini, 2012). In the closest work that addresses droughts and declarations of states of emergency in Brazil, Larreguy and Monteiro (2014) find that non-aligned municipalities are 5 percentage points less likely to receive drought relief from the central government. The characteristics of the mitigation policy might also impact the government's incentive to bias its allocation. For instance, the temporal effect of the policy as palliative or preventive, the degree of voter awareness of each policy, and the bureaucratic rigidity of policy implementation might enhance or decrease government strategic behavior (Cohen and Werker, 2008).

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Political alignment advantage

Prolonged droughts can also affect strategic voting. Citizens affected by water shortages could adjust their expectations regarding the persistence and recurrence of these events.⁴ An increase in vulnerability increases the marginal utility of consumption such that poverty-stricken citizens may also shift preferences toward private target goods and away from the provision of public goods (Bardhan and Mookherjee, 2013; Bobonis et al., 2017). In aggregate effect, the voters may demand more targeted mitigation policies for droughts. Considering that households acknowledge that mitigation policies are also a function of political alignment (Larreguy and Monteiro, 2014), as in a long-run patronage system equilibrium (Nelson and Finan, 2009), the electorate might ascribe even more weight to the alignment between the incumbent parties at the national and municipal levels when deciding how to vote. Thus, voters could be induced to vote for an incumbent who is politically aligned to increase their chance of receiving future government assistance.

The impact of drought-based voter incentives on the inference of the quality of incumbents and the shift in preferences toward mitigation policies is unclear. Although sanctioning politicians in power after a drought could discipline the government's accountability by signaling for more sustainable adaptation policies, the ousting of a politically aligned incumbent after a serious drought will extinguish political alignment. The final behavior of the voters will determine what they find to be most relevant, and in this way, will influence the actions of policy makers. If voters are perishing from a drought and deliberately support the incumbent because of a political alliance, this behavior would be characterized as a demonstration of support for patronage among the levels of government. This is the last hypothesis to test.

- Hypothesis 4: droughts have a positive impact on the re-election of incumbents if they are politically aligned with other levels of government.

The exact answer to the above hypothesis suggests that the effect of droughts on the vote share of an incumbent who is politically aligned would be distinct for each type of election depending on whether the central government is essentially opportunistic, partisan, or benevolent. For instance, if the central government is opportunistic, political alignment is more relevant for the allocation of mitigation policies when presidential elections are approaching. Then, one would expect that considering a municipal election, voters ascribe more weight to party alignment in

⁴Lichand and Mani (2016), when studying 47 municipalities in Ceará, Northeast Brazil, show that uncertainty about future rainfall affects farmers' decision-making through cognitive load leading to a variety of behavioral biases.

their voting decision. This behavior would be the case since in the years following mayoral elections, the central government is more prone to send government aid to the aligned municipalities to increase their electoral performance in the forthcoming presidential election. In any case, if voters value the chance of receiving targeted mitigation policies to the extent that the party alignment effect is positive, then the impact of a drought will further exacerbate this advantage if voters demand clientelism.

As voters in elections hold politicians accountable by selecting and sanctioning politicians according to voter preferences, politicians are induced to behave accordingly. If voters signal that they want aid relief with partisan bias, politicians strive to deliver it. This behavior connects the dots of a clientelism circle. The final outcome is to some extent similar to a Bertrand supertrap (Cabral and Villas-Boas, 2005). If all municipalities were to cooperate, the welfare-maximizing strategy would be to cease the patronage politics that cause an inefficient redistribution of resources and to demand unbiased policies with an emphasis on long-term prevention. However, rejecting patronage politics implies bearing an immediate cost to citizens that materializes in the form of less aid for droughts. This dynamic could mean that the rational response of individual municipalities may be to favor rather than oppose patronage.

The voter rationality assumption is what connects all the hypotheses above and is the most significant shortcoming in the democratic accountability dynamic. The behavioral response that allows assessing voter preferences requires that they have a sufficient cognitive capacity to evaluate government performance and form an expectation of future actions. Thus, if this assumption does not hold, the theory that justifies the central hypothesis of the paper might generate different predictions.

3.3. Data

The empirical analysis exploits Brazilian data on mayoral elections, presidential elections, municipality characteristics, the incidence of droughts, and mitigation policies against droughts. In the following subsections, I show the sources and explain how I constructed the variables used.

3.3.1. Elections

The 1988 Brazilian constitution enabled free and universal elections after more than 20 years of military dictatorship. The contemporaneously constituted democracy follows elections with a majority rule in which voters elect the candidate with the

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most votes for each of the three levels of government executive power: the central government (president), state government (governor) and municipal government (mayor). Elected officials have a four-year term in office. However, election years are different for mayors and the governor/president. The central government and state governments are decided together in elections in the same year, but municipal governments have a specific election, and the timing difference between these two elections is always two years, as described in Figure 3.1. I extracted data on mayoral elections (years 2000, 2004, 2008, and 2012) and presidential elections (years 1998, 2002, 2006, and 2010) from *Tribunal Superior Eleitoral*.

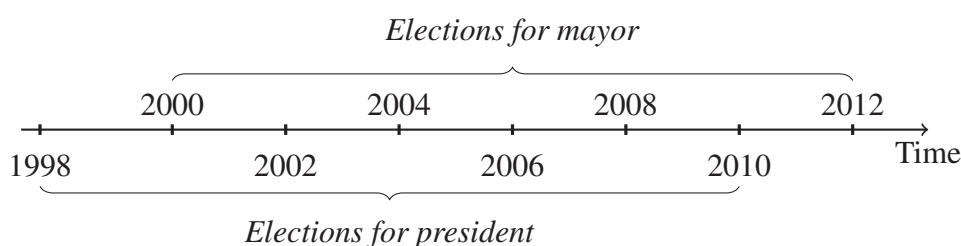


Figure 3.1.: Timing of elections

Two key variables are used in this analysis. The first variable is the vote share of the incumbent party running for re-election. I construct this variable for the mayor's party and the president's party at the municipality level for both cases. These variables are implemented as dependent variables throughout the investigation.⁵

The second variable is the previous margin of victory for the mayor's candidate who is affiliated with the same party as the Brazilian president if the candidate is one of the two candidates who receive the most votes. For this variable, I also need to extract information related to the 1996 mayoral elections. The previous margin of victory is used as a running variable for regression discontinuity design (Lee and Lemieux, 2010). Margins of victory above zero determine whether politically aligned candidates with the incumbent president were elected. The previous margin of victory, which is used as a forcing variable, changes with the type of election analyzed. For municipal elections, I use the margin of victory in the last municipal election, which is exactly four years before. For federal elections, the margin of victory used is from the municipal election two years earlier. The difference stems

⁵It is worth mentioning that for the executive branch in Brazil, re-election has been permitted only since 1997 (*Emenda Constitucional* n. 16, 4th of June) but is still limited to two terms. This restriction means that once a politician with executive power is re-elected, he becomes a lame duck and cannot be eligible for re-election again. Thus, the vote share of the incumbent politician seeking re-election is truncated, and using this value in the analysis means losing observations. Using the vote share of the incumbent party appears to be a better option since these parties can seek re-election as many times as they try.

from the fact that for the outcomes of both the 2002 federal elections and the 2004 municipal elections, the previous margins of victory for candidates in the same party as the incumbent president are drawn from the 2000 mayoral elections. However, the incumbent presidential party in the federal election in 2002 was PSDB, and the incumbent party in the mayoral election in 2004 was PT.

During the period for which I test the presence of the *drought industry*, two parties with different political preferences ruled the Brazilian central government. Fernando Henrique Cardoso from PSDB, a right-wing party, won the election in 1994. He was re-elected in 1998 and stayed in office until December of 2002. The presidential election of 2002 was won by Luiz Inácio Lula da Silva from PT, a left-wing party. Lula started his term in January of 2003, was re-elected in 2006, and stayed in office until December of 2010. Dilma Rousseff, who is also affiliated with PT, replaced Lula in January of 2011 after winning the presidential election in October of 2010.

3.3.2. Droughts

Droughts in the Brazilian territory are extreme cases of water shortages that provoke negative consequences that go beyond of merely harvest losses. The effects of drought have historically been associated with the disappearance of lakes, streams, and vegetation, leading to the death of cattle, goats, and other livestock, which in turn increases malnutrition and thirst and often leads to humanitarian crisis (Guilhoto et al., 2011). Thus, these extreme events are better explained as a matter of water balance than simply the level of rainfall. The economic literature that addresses droughts across large areas in Brazil using only episodes with a lack of rainfall is vulnerable to misidentifying extreme water scarcity in locations that have an abundance of groundwater as they ignore geographical, vegetation, and soil characteristics. As a matter of fact, it is very unlikely that a period of little rain in the Amazon rainforest would trigger similar consequences as a similar period in areas that are susceptible to desertification (PAN-BRASIL, 2004). Thus, using variations in rainfall could incorrectly suggest that there is water scarcity in highly humid areas.

In order to overcome the above caveat, I propose an improved measure of the severity of droughts in relation to those used in Larreguy and Monteiro (2014), Rocha and Soares (2015), and Bobonis et al. (2017).⁶ Instead of only using levels

⁶The measure of drought in the studies of Larreguy and Monteiro (2014) and Bobonis et al. (2017) is the municipal rainfall z-score, i.e., the deviation from the historical mean normalized by the historical standard deviation. The measure of drought in the study of Rocha and Soares (2015) is a dummy variable that indicates that rainfall over 12 months was more than one standard deviation below the historical average.

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of rainfall, I propose using the ratio between evaporation and rainfall. This procedure of measurement is similar to those used in studies focused specifically on hydrology (Wolfe, 1997; Arora, 2002). I define the *aridity index* as the ratio between the accumulated months of evaporation and the accumulated months of rainfall, as follows:

$$AI_{t,m} = \frac{\sum_1^t Evaporation_{t,m}}{\sum_1^t Precipitation_{t,m}}. \quad (3.1)$$

The term *AI* represents the *aridity index*. The subscript t represents the month of the observation for each variable, and the subscript m represents the municipality. Throughout the paper, I will use a time window of two years, i.e., $t = 24$.⁷ This index gives an idea of how much water is stored in a municipality during the time window adopted. Higher values of this index indicate that the environment is arid.

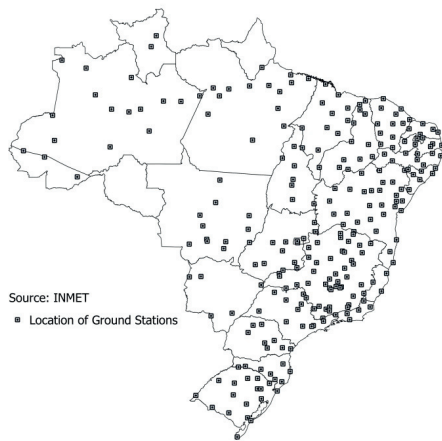
I extract monthly precipitation and evaporation data from ground weather stations from INMET, the Brazilian Institute of Meteorology.⁸ Figure 3.2a depicts the location of the 180 ground stations. The monthly level of precipitation and evaporation for every Brazilian municipality is extrapolated as follows. I first locate the closest ground meteorological station in each quadrant to the north-east, north-west, south-east, and south-west of every municipality. Then, I compute the distance between the municipality's centroid and the ground station. The inverse square of this distance is used as a weight to proxy weather variables for each municipality.

Since elections regularly occur in October, I account for the level of rainfall and evaporation prior to this month. I sum the weather data for the 24 months preceding a specific election. For example, the *aridity index* of the 2010 presidential election takes into account the sum of the monthly levels of evaporation and precipitation from November 2008 (exactly one month after the mayoral election in 2008) through October 2010 (the month of the presidential election).

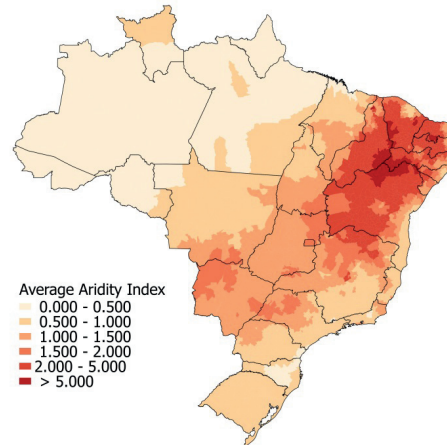
There are three significant advantages of this measure compared with the standard variables used for drought in the literature. First, the measure relies on two indicators of water availability, namely, how much it rains and how much it evaporates. These features are important because they depict the water storage capacity. Using levels of precipitation alone provides incomplete information about the soil moisture balance. Second, the measure gives an intensity indicator that captures how arid a municipality is during a certain period and distinguishes high humidity areas from desertification areas. Third, the measure is constructed using ground station data, which provide objective weather information. Rainfall levels are recorded

⁷Since the timing difference between mayoral elections and presidential elections is always two years, the adoption of this time window of 24 months is convenient.

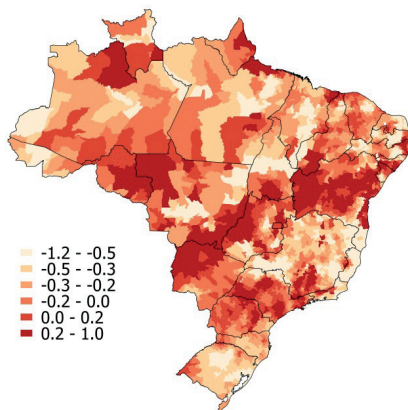
⁸This dataset contains an extensive list of weather variables in the period between 1963 to 2013.



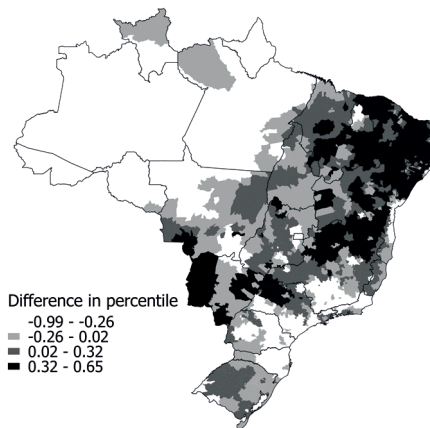
(a) Location of ground stations



(b) Average of Aridity Index



(c) Average of Z-score of rainfall



(d) Difference in percentile

Note: maps of Brazil divided by states. Figure 3.2a depicts the location of the INMET ground stations. Figure 3.2b represents the average *Aridity Index* in the period of analysis. Figure 3.2c depicts the average Z-score of rainfall during the period of analysis. Figure 3.2d illustrates the difference of percentile of average Z-score of rainfall and percentile of average of *Aridity Index* in the period of study. Municipalities in color white represent areas where the Z-score of rainfall infer higher propensity of drought compared with the *Aridity Index*, and cities in black the other way around.

Figure 3.2.: Maps of Brazil

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with a pluviograph, and levels of evaporation are recorded from a Piche evaporimeter. These data collection instruments provide meteorological observations that are superior to widely used satellite data because satellites do not directly measure precipitation and evaporation but rather make inferences (Dell et al., 2014).⁹

Figure 3.2b depicts the average *aridity index* from 1998-2012 for the Brazilian territory. The map confirms that the proposed measure of drought overcomes the problem of misidentifying water scarcity for areas where such scarcity is unlikely to occur. In fact, a significant part of the northern region, which is mostly covered by the Amazon rainforest, has an average *aridity index* below 0.5. However, the inland regions of the northeast, an area that is commonly associated with humanitarian crisis because of droughts, has the highest index in the territory. The north of the southeast region and the south of the center-west region exhibit a high average aridity. For comparison, Figure 3.2c shows the average Z-score of rainfall during the period of analysis, similar to Larreguy and Monteiro (2014) and Bobonis et al. (2017).¹⁰ The map indicates the dispersion of drought propensity throughout the territory, including that in highly humid areas such as the Amazon rainforest. Figure 3.2d compares the difference in the percentiles of average *aridity index* and average rainfall Z-score for municipalities during the study period. White areas show the municipalities where the Z-score of rainfall suggests a higher incidence of drought in comparison to the measure of the *aridity index*. Black areas indicate otherwise. In summary, while the *aridity index* identifies more drought incidence in the Brazilian region known as semi-arid, the Z-score of precipitation identifies more drought incidence in extremely humid areas.¹¹

Figure 3.3 shows how levels of aridity have evolved in time. The positive trend raises concern about the growth of drought incidence and the spread to areas that never suffered from this natural disaster before.

For the empirical analysis, I compute the incidence of drought following the generalized climate classification scheme for values of the global aridity index proposed by Middleton et al. (1997).¹² In the simplest definition, I assume that the variable *drought* takes a value of one if during the two years preceding each election the

⁹Indeed, the *aridity index* outperformed the prediction of states of emergency because of drought compared with measures of drought using satellite datasets such as NOAA's PRECipitation RE-Construction Dataset (PREC) (Chen et al., 2002) and Terrestrial Precipitation - 1900-2010 Gridded Monthly Time Series (Matsuura and Willmott, 2013). These results are available upon request.

¹⁰High values indicate low precipitation.

¹¹Note that using the Z-score of the *aridity index* would encounter the same problem described if one uses the Z-score of rainfall only. That is, the measure could infer that there is a high incidence of drought in highly humid areas in Brazil.

¹²Note that my index is equivalent to the inverse of the index of aridity in Middleton et al. (1997). The reason for this transformation is the ease of interpreting of results. The idea is to make the measurement positively monotonic with drought.

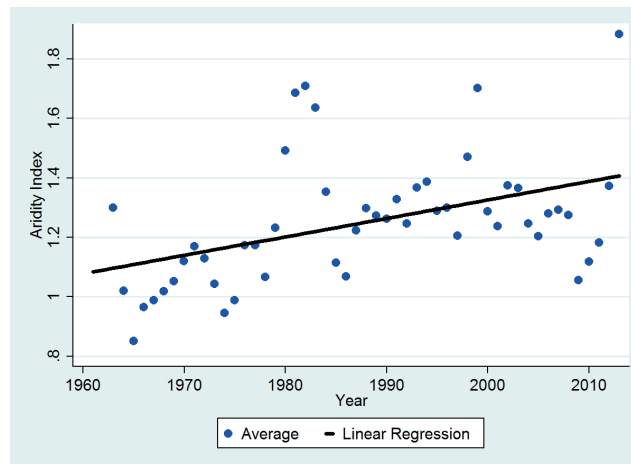


Figure 3.3.: Historical evolution of average of *aridity index* in Brazil

aridity index is equal to or higher than 2.¹³ That is, evaporation was at least twice as large as rainfall. In order to account for nonlinear effects among different levels of droughts and ensure that a few extreme cases do not drive the primary results, I split the drought variable into categories. The category *extreme* takes a value of one if the index is equal to or higher than 5 and is zero otherwise. The category *severe* takes a value of one if the index is equal to or greater than 2 and lower than 5 and is zero otherwise. Finally, the category *moderate* takes a value of one if the index is equal to or higher than 1.59 and lower than 2 and is zero otherwise. In this sense, the measure of water scarcity informs gradual levels of droughts. Figure 3.4 shows the kernel density of the *aridity index* and presents the drought classification scheme.

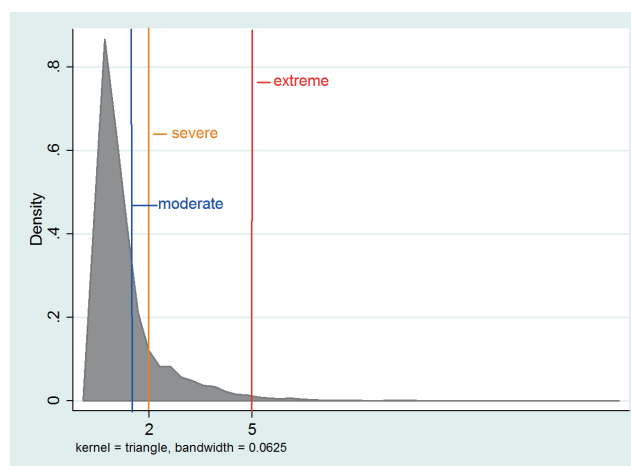


Figure 3.4.: Kernel density of *aridity index*

¹³The purpose is to make the empirical analysis as readable as possible. The interpretation and statistical significance of the results are the same if one uses levels of the *aridity index*.

3.3.3. Drought Mitigation Policies

The government's methods of dealing with the consequences of drought can be assigned into the following two groups: palliative policies and preventive policies (Cohen and Werker, 2008; Neumayer et al., 2014). Essentially, palliative policies are designed to serve citizens after an incidence of drought and have an immediate but short-term effect. In contrast, prevention policies are intended to mitigate the impact of the future conditions and usually have a long-term benefit.

In the context of this study, palliative and preventive policies also have different bureaucratic rigidities. Palliative policies are more bureaucratically loose to carry out, and therefore more easy to bias, than prevention policies. Additionally, palliative policies are more easily manageable to promote vote buying. For instance, in addition to instantly diminishing the suffering of the poor, politicians can also use these palliative grants to buy voter support by sending water trucks to the appropriate places and by making food distribution conditional on voting for the national incumbent or at least on turning out on the day of the election. Infrastructure investments, in turn, are more immobile in space and are thus less flexible for discriminatory purposes.

Concerning electoral results, both methods may have distinct implications. Palliative policies may be more salient for voters than prevention policies since the absence of the latter diminishes voters' counterfactual perceptions of their situation. From a political economy perspective, these implications could distort the optimal balance between these policies. For instance, studying natural disasters in the U.S., Healy and Malhotra (2009) show that voters reward ruling parties in elections for spending on disaster relief but not for investing in disaster prevention, leading governments to spend less on the preparation for disasters, thus causing substantial public welfare losses.

Although the objective of this study is not to determine the best balance between prevention and palliative policies, I aim to identify how these policies are allocated among municipalities and how their effects are associated with elections given their different properties. Below, the construction of proxies for both policies is detailed.

Palliative policy

When a municipality suffers from an extended period of water scarcity that damages the economy of the locality, the mayor may request emergency aid from higher levels of public administration (i.e., the state and central governments). To be accepted by the central government, this request has to pass through a public administrative bureaucracy. The mayors of the affected municipalities need to declare a

state of emergency because of drought and send documents and reports to the Ministry of National Integration of Brazil.¹⁴ The National Secretariat of Civil Defence is the agency within the ministry that has the designation of analyzing the gravity of the drought. This agency comprises a commission that decides whether the mayor's request for aid relief is reasonable. All these institutions are politically subordinate to the president. If these institutions agree that providing aid to the municipality is necessary, the central government publishes an ordinance recognizing the declaration of a state of emergency or public calamity.¹⁵

From this moment, the municipalities for which the state of emergency was recognized become eligible to receive several types of financial support and bureaucratic relief. In cases of drought, federal aid relief may arrive in the form of emergency funding through so-called mandatory transfers, the supply of water trucks, food distribution, the waiver of legal bureaucratic procedures for public spending, the renegotiation of agriculture debts, and permission for citizens withdraw money from labor insurance (FGTS).

I extract from *Sistema Integrado de Informações sobre Desastres Naturais* (S2ID) the central government ordinances recognizing the declaration of the state of emergency by drought for the period 1993 - 2013. Unfortunately, there is no information available on the monetary amount disbursed for each instance of emergency aid.

I use in my analyses a dummy variable that is a proxy for palliative policy. This variable takes a value of 1 if there was a recognition of the state of emergency because of drought for up to two years before the specific election in question (mayoral election or presidential election) and is 0 otherwise.

Prevention policy

One of the principal forms of transferring resources for infrastructure investments between the central government and municipal governments is through the so-called *Convênios*. These plans function as agreements between different levels of public administration and usually serve as a means of promoting the decentralization of public expenditure on projects related to physical facilities and structures. This modality of intergovernmental transfer is the most commonly adopted to implement long-lasting public policies to fight the effects of droughts.

I gather detailed data from 461,958 *Convênios* projects that were implemented

¹⁴For example, some of these documents are preliminary notifications of disaster, detailed damage reports, and action plans.

¹⁵A state of emergency because of drought was first proposed in 1962 by Celso Furtado, at the time the chair of SUDENE (Superintendência do Desenvolvimento do Nordeste), in Resolution No: 453.

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between the years 1995 and 2014. Among these projects, I identified 2,023 that have included in their project scopes the construction or maintenance of water dams, barrages for lakes, cisterns, and water distribution systems. These types of public works have minimal effects in situations where drought is already present but are intended to relieve the aftermath of future droughts.

The average execution time on these kinds of projects is one year and six months, and the average amount of money spent is US 120,000.00, an increase of approximately 9% in the annual budget of a medium-sized municipality.¹⁶

I use in my analyses a dummy variable that is a proxy for prevention policy. This variable takes a value of 1 if one such project was implemented up to two years before the specific election in question and is 0 otherwise. It is reasonable to suppose that prevention policies are likely to provide spillover effects on neighboring municipalities. After all, in times of crisis, people can commute a short distance to the nearest source of water. To take into account this possibility, I also expand my definition of municipalities that have benefited from drought prevention policies to include those localities that are located at a maximum distance of 30 km from the centroid of the municipality in which the project was originally addressed.¹⁷

3.3.4. Municipality Characteristics

To ensure that the effect of droughts does not incorporate time-varying characteristics of municipalities, I gather information from the 1991, 2000 and 2010 population censuses conducted by the Brazilian Bureau of Statistics (IBGE), and then, I extrapolate these data to election years. More specifically, I control for production structure by introducing variables such as the share of workers in the agricultural sector and the industrial sector. The average income controls the welfare of a municipality. The GINI coefficient controls the inequality. The proportion of graduates controls the level of human capital.

Additionally, I also gather information on the size of the municipal population from *Tesouro Nacional FINBRA* and the share of voters below 18 years old from *Tribunal Superior Eleitoral*.¹⁸ The average temperature is collected from INMET. For these variables, extrapolations are not needed. In the empirical analyses, the control variables used are lagged for two years (the initial condition before drought shock) and log transformed. Because state capital cities have special political designations, these cities are dropped from the database. Table 3.1 depicts the summary

¹⁶Prices deflated by the IPCA (IBGE) with the base year 2000, which represents approximately R\$ 380,000.00.

¹⁷The results do not change when using a maximum distance of either 10 km or 50 km.

¹⁸Voting is optional for Brazilians between 16 and 18 years.

3.4. Droughts, Mitigation Policies, and Electoral Results

statistics.

Table 3.1.: Summary statistics

Dependent variable:	Mean	Std. Dev.	Min	Max	N
Vote share of mayor's party	0.268	0.271	0.000	1.000	22,099
Vote share of president's party	0.500	0.192	0.022	0.950	21,712
Palliative policy before presidential elections	0.189	0.391	0	1	50,436
Palliative policy before mayoral elections	0.264	0.441	0	1	50,436
Prevention policy before presidential elections	0.104	0.306	0	1	50,456
Prevention policy before mayoral elections	0.144	0.351	0	1	50,446
Weather variables:	Mean	Std. Dev.	Min	Max	N
Aridity Index (AI)	1.305	1.043	0.133	15.625	50,436
Drought	0.153	0.360	0	1	50,436
Categories:					
Extreme drought	0.013	0.113	0	1	50,436
Severe drought	0.140	0.347	0	1	50,436
Moderate drought	0.083	0.276	0	1	50,436
Temperature	23.164	3.055	13.299	29.737	50,436
Forcing variables:	Mean	Std. Dev.	Min	Max	N
Margin of victory before presidential elections	0.003	0.247	-0.988	0.938	2,549
Margin of victory before mayoral elections	-0.029	0.257	-0.988	0.938	2,018
Municipalities characteristics	Mean	Std. Dev.	Min	Max	N
Share of workers in agriculture sector	16.227%	9.355	0%	72.294%	44,314
Share of workers in industry sector	4.764%	4.580	0%	40.880%	44,314
Population	24,693	56,820	163	1,251,831	47,536
Average income	R\$ 660.01	R\$ 358.37	R\$ 29.84	R\$ 7,726.86	44,314
Share of graduated citizens	2.228%	1.944	0%	24.212%	44,314
GINI coefficient	0.528	0.066	0.261	0.880	47,285
Share of voters below 18 years old	3.337%	1.455	0.059%	11.355%	49,645

3.4. Droughts, Mitigation Policies, and Electoral Results

To demonstrate why politicians should be concerned about the incidence of natural disasters and how voters react to the actions of central government, I investigate in this section the relationship between droughts, mitigation policies, and electoral outcomes in presidential and mayoral elections in Brazil, both at the municipality level. More specifically, I test the hypotheses that severe droughts adversely affect the parties in office in subsequent elections and that mitigation policies, conditioned on drought, have a positive influence on the re-election performance of incumbents.

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Methodology

To adequately address the first hypothesis, I propose the following econometric specification:

$$SV_{t,m} = \alpha D_{t,m} + \gamma X_{t-2,m} + f_m + f_t + \epsilon_{t,m}, \quad (3.2)$$

where SV_{tm} represents the share of the vote for the incumbent party in election t and in municipality m . The term D indicates the set of variables that reflect water scarcity during the two years prior to the election. X represents a number of control variables lagged two years before the corresponding election. The terms f_m and f_t are a municipality fixed effect and year fixed effect, respectively. The term ϵ is the error.

Such specification captures the effect of droughts using within-municipality variation. The underlying assumption here is that weather conditions over two years preceding an election are exogenous to the political dispute. This assumption is reasonable because it is hard to argue that there is an omitted factor that could drive the vote share of the incumbent party and the levels of rainfall and evaporation simultaneously. Indeed, several studies relied on a similar assumption to identify the causal effect of natural disasters on economic outcomes (Kahn, 2005; Strömberg, 2007; Yamamura, 2014; Felbermayr and Gröschl, 2014; Neumayer et al., 2014).

Subsequently, I examine the hypothesis that the promulgation of public policies to fight droughts would reduce the negative impact these natural events may have on re-election. If voters indeed punish incumbent political parties because of droughts, it is to be expected that this effect will be exacerbated in places that have not received government assistance. Therefore, I introduce interaction variables in the previous econometric model as follows:

$$SV_{t,m} = \alpha D_{t,m} + \delta P_{t,m} + \beta(D_{t,m} * P_{t,m}) + \gamma X_{t-2,m} + f_m + f_t + \epsilon_{t,m}, \quad (3.3)$$

where $P_{t,m}$ expresses whether a municipality has received a drought mitigation policy. I study the palliative policies and prevention policies separately. β is the coefficient of interest that expresses the heterogeneous correlation of the impact of drought on the municipalities benefiting from each type of public policy.¹⁹

¹⁹One could be concerned as to whether drought and palliative policies suffer from multicollinearity. However, this is not the case because there is no particular rule for the recognition of a state of emergency, and the conditions for this declaration turn out to be subjective. The pairwise correlation between palliative policies and the aridity index is only 0.36, so estimations should not suffer from multicollinearity. Still, my measure of drought has the highest correlation between

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This coefficient must be interpreted with care. Although the estimation extracts the coefficient from the within-municipality variation and controls for the common yearly shocks as well as a set of covariates, it is still unable to rule out the hypothesis that there is an unobservable time-variant characteristic that drives the allocation of these public policies. The results should be taken only as suggestive rather than causal. For instance, it is well documented that political alignment is a determinant of the allocation of public policy, and this dynamic may also influence election results, which in turn may bias the coefficient (Solé-Ollé and Sorribas-Navarro, 2008; Brollo and Nannicini, 2012). I will address this issue explicitly later in the paper. However, it would be informative to construct a horse race for the effects of policy design on election results and make a comparative analysis.

Results

There are four takeaways from the analyses. First, the results confirm that droughts have an adverse impact on incumbent parties in elections. Moreover, the findings suggest that voters are likely to blame the local politicians and the central government alike for water shortages. In a way, this result could enigmatically stem from a voter rationality perspective since mayors have less decision-making power than presidents to implement preparedness policies and immediate aid relief in the aftermath of a drought. Nevertheless, this evidence could be explained by the citizen's perceptions of their proximity to public agents. An uprising against local politicians could be more effective since they usually live in the same city and are more tangible, whereas bureaucrats in higher levels of public administration usually live in capital cities that are generally distant.

Second, voters reward the incumbent party for the delivery of mitigation policies. Such a reward seems to be more sensitive to short-term relief spending than investments in drought preparedness spending. This finding suggests that if the central government has opportunistic and partisan motivations and selects policies so as to maximize electoral outcomes, it would be wise to focus more on palliative policies when allocating strategically among municipalities.

Third, the effectiveness of the use of drought mitigation policies as a strategic tool to reduce voter dissatisfaction in certain municipalities appears only for presidential elections, whereas this effect is absent in municipal elections. One could expect that the mayor's party might reap some electoral benefit from an additional expenditure financed by higher levels of public administration if voters are unsure

palliative policies compared to other indexes of droughts. For instance, the pairwise correlation with the Z-Score of rainfall is 0.08, and the correlation with rainfall that is one standard deviation below the mean precipitation is 0.02.

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about the source of the government spending. As this is not the case, an alternative explanation may be that citizens are sufficiently able to disentangle the provision of public goods by the central government from that by the municipal government. Reconciling these findings with the assumption that voters are irrational is difficult.

Fourth, the findings demonstrate that the effect of aid relief is not high enough for a net increase in popularity. Previous works have shown that the government's responsiveness implies a gain in voter gratitude to the extent that negative shocks from natural disasters disappear completely until these events become beneficial for the incumbent (Bechtel and Hainmueller, 2011; Gasper and Reeves, 2011; Lazarev et al., 2014). This dynamic does not apply for droughts in Brazil. The provision of palliative and prevention policy compensates for the negative shock of droughts, but this compensation is not enough to result in an increase in the vote share.²⁰

Interpreting the results in detail, Table 3.2 depicts the findings using the vote share of the incumbent party in presidential elections as the dependent variable. Column (1) shows that a drought decreases the vote share of the incumbent party by an average of five percentage points. Column (2) suggests that such an effect is nonlinear regarding the intensity of water scarcity. While extreme droughts decrease the vote share of the presidential party by an average of twenty percentage points, severe droughts and moderate droughts result in a six percentage points and one percentage point decrease, respectively. Column (3) confirms that the nonlinear effect holds even when including time-varying covariates. Column (4) includes interactions between the incidence of drought and policies aimed at mitigating the consequences of drought. The results show that such policies indeed, on average, counteract the adverse impact that an incumbent party in central government would face if voters suffered from water shortages. Column (5) shows that even though voters experiencing an extreme drought react positively to the delivery of palliative and prevention policies by eleven and thirteen percentage points, respectively, the overall impact is still negative. When voters are subjected to a severe drought, only palliative policies appear to be relevant to the electoral outcome. In the case of moderate droughts, mitigation policies do not appear to be relevant at all. These findings suggest that from the perspective of the central government, the allocation of palliative policy against drought is potentially more rewarding with respect to electoral outcomes than preventive policy, which is in line with the results in Healy and Malhotra (2009).

Regarding municipal elections, Table 3.3 shows the results using the vote share of the mayor's party as the dependent variable. Column (1) confirms that drought

²⁰This conclusion holds even for municipalities that have received the palliative and preventive policies simultaneously. The triple interaction between drought and the two type of policies is statistically insignificant. The results are available upon request.

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Table 3.2.: Drought and mitigation policies on presidential elections

<i>Dependent variable:</i>	vote share of president's party				
	(1)	(2)	(3)	(4)	(5)
Drought	-0.052*** (0.007)			-0.049*** (0.009)	
<i>Categories of drought:</i>					
Extreme		-0.209*** (0.014)	-0.131*** (0.015)		-0.251*** (0.021)
Severe		-0.059*** (0.008)	-0.012 (0.008)		-0.036*** (0.009)
Moderate		-0.011** (0.005)	0.011** (0.005)		0.013** (0.005)
<i>Interactions between policies and drought:</i>					
Palliative x Drought				0.065*** (0.008)	
Prevention x Drought				0.021** (0.010)	
<i>Interactions between policies and categories of drought:</i>					
Palliative x Extreme					0.114*** (0.019)
Palliative x Severe					0.063*** (0.009)
Palliative x Moderate					0.008 (0.010)
Prevention x Extreme					0.133*** (0.020)
Prevention x Severe					0.008 (0.011)
Prevention x Moderate					-0.016 (0.012)
Observations	21,712	21,712	19,343	19,343	19,343
R-squared	0.447	0.455	0.503	0.504	0.511
Controls	No	No	Yes	Yes	Yes

Note: the analyses use data for presidential elections of 1998, 2002, 2006, and 2010. Municipalities and years fixed effects included in all specifications. Controls variables are lagged two years before each election and include population, average income, GINI coefficient, the share of graduated citizens, the share of voters below 18 years old, the proportion of workers in agriculture and industry sector, palliative policy, and prevention policy. Standard error clustered at municipality level in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

is harmful to the re-election efforts of the party of the mayor. The incidence of drought decreases, on average, the vote share of the mayor's party by seven percentage points. Although this negative shock seems to grow according to drought levels, the hypothesis that the effect is linear cannot be ruled out, as observed in column (2). Column (3) reaches a similar conclusion once time-varying covariates are included. Concerning the allocation of mitigation policies, column (4) provides

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Table 3.3.: Drought and mitigation policies on mayoral elections

<i>Dependent variable:</i>	vote share of mayor's party				
	(1)	(2)	(3)	(4)	(5)
Drought	-0.070*** (0.015)			-0.065*** (0.018)	
<i>Categories of drought:</i>					
Extreme		-0.131*** (0.032)	-0.100*** (0.033)		-0.039 (0.072)
Severe		-0.080*** (0.017)	-0.070*** (0.018)		-0.071*** (0.021)
Moderate		-0.011 (0.010)	-0.007 (0.011)		-0.008 (0.012)
<i>Interactions between policies and drought:</i>					
Palliative x Drought				0.001 (0.015)	
Permanent x Drought				0.012 (0.018)	
<i>Interactions between policies and categories of drought:</i>					
Palliative x Extreme					-0.008 (0.071)
Palliative x Severe					0.008 (0.016)
Palliative x Moderate					0.008 (0.016)
Prevention x Extreme					-0.143*** (0.053)
Prevention x Severe					0.019 (0.020)
Prevention x Moderate					-0.015 (0.025)
Observations	22,099	22,099	19,585	19,585	19,585
R-squared	0.020	0.020	0.019	0.020	0.021
Controls	No	No	Yes	Yes	Yes

Note: the analyses use data for mayoral elections of 2000, 2004, 2008, and 2012. Municipalities and years fixed effects included in all specifications. Controls variables are lagged two years before each election and include population, average income, GINI coefficient, the share of graduated citizens, the share of voters below 18 years old, the proportion of workers in agriculture and industry sector, palliative policy, and prevention policy. Standard error clustered at municipality level in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

different evidence from what was found in presidential elections. Neither palliative policies nor prevention policies appear to deter voter anger in local elections. Surprisingly, column (5) exhibits a negative correlation between the provision of prevention policies and the vote share of the incumbent in municipalities that suffer from extreme drought. This result could be explained by the electorate believing that the prevention policies implemented were useless given the scale of the environmental catastrophe.

3.5. Allocation of Drought Mitigation Policies

In sum, these findings are in line with those in the economics and political science literature. The results of this work reinforce and extend the validity of previous work by showing that there is gratitude from the electorate for the government's response following natural disasters, and by showing that the reaction of voters is nonlinear in the severity of natural disasters.

3.5. Allocation of Drought Mitigation Policies

In the structure of Brazilian federalism, the central government has the greatest decision-making authority to intervene in areas devastated by drought. Given that high levels of water scarcity are also bad news for the electoral future of politicians in office, the central government could bias particular policies towards aligned municipalities. There may be different impetuses in mayoral compared to presidential elections. The discretionary allocation of public resources can be accomplished through the recognition of a state of emergency or an agreement through *Convênios* for investment in infrastructure. The distortion in these policies in favor of specific political parties penalizes the efficient provision of public goods. To rigorously investigate the veracity of the aforementioned assertion, I examine the effect of political alignment on the provision of drought mitigation policies.

Methodology

The methodological challenge comes from the fact that political preferences could be associated with several confounding factors that would bias the analysis. Thus, the most appropriate way to extract the causal impact of political alignment is to analyze the municipalities that met the following condition in the previous municipal elections: the two candidates with the most votes are from the party of the president and a party in the opposition coalition, as in Brollo and Nannicini (2012).²¹ The underlying assumption is that in elections where the margin of victory is very small, the only difference between the treatment groups and control groups is party alignment with the central government, and all the confounding factors are randomly assigned around the threshold of winning an election.²² More specifically, I implement the following regression discontinuity design (RDD):

²¹An alternative condition is a restriction of the sample to municipalities in which only two candidates ran in the previous municipal election, one of whom is in the same party as the incumbent president. The results are qualitatively and statistically the same, and they are available upon request.

²²The validity of such an assumption could be tested using municipality characteristics as dependent variables. Appendix C shows the graphs of this test.

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$$P_m = \beta + \gamma \sum_{p=1}^p (MV_m)^p + T_m * [\alpha + \lambda \sum_{p=1}^p (MV_m)^p] + \epsilon_m, \quad (3.4)$$

where P_m represents whether a municipality m received palliative or prevention policies during the two years before an election. The term MV is the forcing variable that represents the margin of victory of the candidate for mayor of the president's party in the previous mayoral election. The term T accounts for the treatment effect to be aligned with the central government. Naturally, T assumes a value of one if and only if $MV > 0$. Such specification allows for local or global polynomial order (p) in the forcing variable on both sides of the threshold 0. The coefficient α gives the local average treatment effect (LATE) of political alignment on receiving federal drought relief (Lee and Lemieux, 2010).

I implement two criteria to deal with the traditional trade-off between the size of bandwidth around the threshold and the polynomial order of the forcing variable when executing the RDD. First, I select arbitrarily elections in which the margin of victory is between -50% and 50% (excluding cases where the margin of victory is extremely high). Then, I regress for different degrees of polynomial order of the forcing variable, allowing different shapes on both sides of the threshold. I use first-, second-, third-, and fourth-order polynomials and select the one that minimizes the AIC criteria (Akaike, 1974). Second, I use the bandwidth selectors proposed by Imbens and Kalyanaraman (2012) and Calonico et al. (2014) and use the local polynomial with a rectangular kernel.

Additionally, I investigate whether politically-motivated allocation is heterogeneous across levels of water scarcity. Depending on the type of election that is approaching, the drought may exacerbate or reduce the central government bias. Put another way, the effect of political alignment could be directed towards a particular set of municipalities depending on the magnitude of the drought. I implement an RDD with heterogeneous treatment effects that provide a heterogeneous local average treatment effect (HLATE) but with a less conservative specification proposed by Becker et al. (2013). Instead of just allowing different shapes on both side of the threshold of the forcing variable regarding the treatment effect, I also allow different shapes for the heterogeneous treatment effect. More specifically, I implement the following econometric specification:

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$$P_m = \beta + \sum_{p=1}^p (\gamma_p + \eta_p D_m) * (MV_m)^p + \vartheta D_m + T_m * [\alpha + \theta D_m + \sum_{p=1}^p (\lambda_p + \delta_p D_m) * (MV_m)^p] + \epsilon_m, \quad (3.5)$$

where the coefficient ϑ indicates the local probability of receiving mitigation policy if a municipality suffers a certain degree of drought. The coefficient θ represents the heterogeneous local average treatment effect (HLATE) of party alignment when a municipality experiences a certain degree of drought. The linear combination of coefficients ϑ , α , and θ informs the local probability of receiving drought mitigation policy when a municipality suffers a certain degree of drought, and the incumbent mayor is in the same party of the current president of Brazil.²³

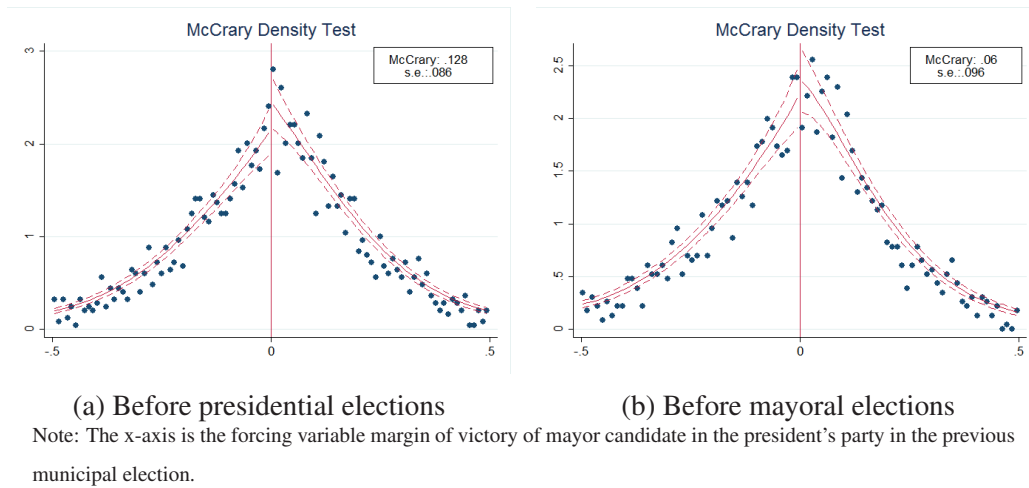


Figure 3.5.: McCrary tests

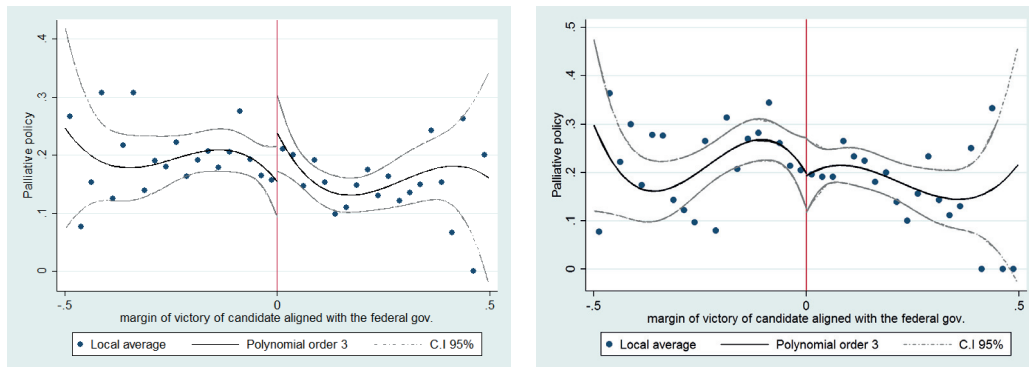
There are three important underlying assumptions required to implement this identification strategy. First, the covariates are continuous, and there is no jump at the threshold. Second, there is continuity in the probability of the degree of droughts at the threshold to capture genuine variation in the interaction with party alignment. Third, the interactions between the degree of drought and party alignment are not correlated with the error term, conditional on the margin of victory (Becker et al., 2013). The first two assumptions are tested in Appendix C. The latter assumption relies on the exogenous characteristic of weather variables and the fact

²³The results are equivalent in terms of sign and have more statistical power if we strictly follow the econometric specification proposed in Becker et al. (2013). However, their specification is rigid in the sense that they do not admit the margin of vote share to have a distinct prediction for municipalities that suffered from droughts and municipalities that did not.

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that the electoral victory of aligned parties is controlled by the forcing variable margin of victory. The McCrary (2008) test of the forcing variables depicted in Figure 3.5. It can be seen that for both forcing variables, there is a smooth distribution around the threshold of zero, which rules out sorting concerns.

Results

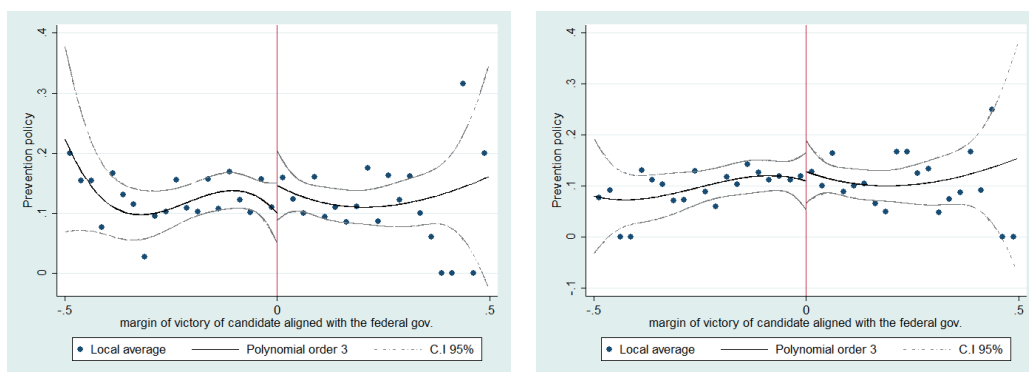


(a) Before presidential elections

(b) Before mayoral elections

Note: The x-axis is the margin of victory of mayor candidate in the president's party in the previous municipal election. The y-axis of Figure 3.6a is the probability of receiving palliative policy before presidential elections. The y-axis of Figure 3.6b is the probability of receiving palliative policy before mayoral elections.

Figure 3.6.: Graphical analysis of RDD: Alignment on palliative policy



(a) Before presidential elections

(b) Before mayoral elections

Note: The x-axis is the margin of victory of mayor candidate in the president's party in the previous municipal election. The y-axis of Figure 3.7a is the probability of receiving prevention policy before presidential elections. The y-axis of Figure 3.7b is the probability of receiving prevention policy before mayoral elections.

Figure 3.7.: Graphical analysis of RDD: Alignment on prevention policy

The analyses prove that there is a tremendous advantage for voters that experience a water shortage in having an incumbent mayor who is aligned with the central

3.5. Allocation of Drought Mitigation Policies

government. Such a feature is strikingly important for receiving palliative policies. However, the same is not applicable for policies that have long-run effects and is less immediately noticeable for the poor. In short, when water scarcity reaches a critical level in which thirst and famine forces citizens into extreme attitudes, such as blocking roads to plunder necessary supplies, the marginal effect of government assistance makes a big difference. Hence, a political alliance between the levels of government becomes a strong factor. The patronage relationship in the provision of drought palliative policies seems to persist.

Table 3.4.: Palliative policies before presidential elections

Dependent variable:	palliative policy before election for president					
	(1)	(2)	(3)	(4)	(5)	(6)
Party alignment	-0.032 (0.025)	0.013 (0.042)	0.016 (0.043)	-0.013 (0.035)	-0.010 (0.044)	-0.007 (0.044)
Party alignment x Drought	0.181** (0.087)	0.343** (0.145)	0.358** (0.147)			
Drought	0.196*** (0.060)	0.109 (0.103)	0.083 (0.103)			
<i>Categories of drought:</i>						
Party alignment x Extreme				0.519** (0.259)	0.738*** (0.273)	0.736*** (0.273)
Party alignment x Severe				0.236* (0.135)	0.287* (0.163)	0.300* (0.165)
Party alignment x Moderate				0.192 (0.134)	0.239 (0.155)	0.237 (0.155)
Extreme				0.162 (0.200)	-0.014 (0.209)	-0.012 (0.209)
Severe				0.137 (0.093)	0.112 (0.112)	0.085 (0.114)
Moderate				-0.071 (0.082)	-0.077 (0.088)	-0.075 (0.088)
Observations	2,395	1,165	1,152	2,395	1,165	1,152
R-squared	0.104	0.089	0.093	0.121	0.097	0.101
Polynomial order	1	1	1	2	1	1
Bandwidth	0.500	0.120	0.118	0.500	0.120	0.118
Procedure		CTT	IK		CTT	IK
AIC	1943.09			1920.045		

Note: forcing variable is the vote margin of victory of the candidate for mayor in the president's party in the previous municipal election. In columns (1) and (4) the bandwidth is chosen arbitrarily and the polynomial order of forcing variable is chosen by AIC criteria. The columns (2) and (5) use bandwidth selector proposed by Calonico et al. (2014) (*CTT*). The columns (3) and (6) use the bandwidth selector proposed by Imbens and Kalyanaraman (2012) (*IK*). Robust standard error in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

Demonstrating the interpretation of the results, Figure 3.6 provides a graphical analysis of the effect of party alignment on the provision of palliative policy against droughts. Although the coefficient for party alignment is larger when the election following the delivery of palliative policy is the presidential election, the effect for both types of electoral contest is not statistically significant at usual standards. Figure 3.7 shows similar results for the case of prevention policy.

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Table 3.5.: Palliative policies before mayoral elections

Dependent variable:	palliative policy before election for mayor					
	(1)	(2)	(3)	(4)	(5)	(6)
Party alignment	-0.031 (0.040)	-0.009 (0.039)	0.018 (0.052)	-0.017 (0.040)	0.005 (0.039)	0.025 (0.052)
Party alignment x Drought	0.100 (0.151)	-0.018 (0.143)	0.139 (0.189)			
Drought	0.316*** (0.095)	0.390*** (0.093)	0.432*** (0.127)			
<i>Categories of drought:</i>						
Party alignment x Extreme				-0.516 (0.640)	-0.633 (0.461)	-0.707 (0.674)
Party alignment x Severe				0.108 (0.156)	0.011 (0.148)	0.161 (0.193)
Party alignment x Moderate				-0.102 (0.154)	-0.141 (0.150)	-0.024 (0.202)
Extreme				0.690 (0.463)	0.544* (0.328)	0.360 (0.527)
Severe				0.328*** (0.097)	0.405*** (0.096)	0.450*** (0.130)
Moderate				0.155 (0.104)	0.159 (0.105)	0.145 (0.131)
Observations	1,868	1,327	838	1,868	1,327	838
R-squared	0.160	0.158	0.123	0.188	0.185	0.141
Polynomial order	1	1	1	1	1	1
Bandwidth	0.500	0.197	0.104	0.5	0.197	0.104
Procedure		CTT	IK		CTT	IK
AIC	1673.608			1636.946		

Note: forcing variable is the vote margin of victory of the candidate for mayor in the president's party in the previous municipal election. In columns (1) and (4) the bandwidth is chosen arbitrarily and the polynomial order of forcing variable is chosen by AIC criteria. The columns (2) and (5) use bandwidth selector proposed by Calonico et al. (2014) (*CTT*). The columns (3) and (6) use the bandwidth selector proposed by Imbens and Kalyanaraman (2012) (*IK*). Robust standard error in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

In order to dig a little more into whether there is a political bias towards a set of areas depending on the level of water scarcity, Tables 3.4 and 3.5 show the results of the heterogeneous treatment effects of political alignment. Table 3.4 depicts the results for palliative policy before presidential elections. Columns (1), (2), and (3) use the three proposed criteria for the simplest definition of drought. The results indicate that the central government distributes palliative policy opportunistically following a pattern. The president prioritizes aligned municipalities in the event that the environmental condition is acute. Given a drought, an aligned city has an approximately 18 - 36 percentage points greater chance of receiving assistance from the central government. This effect is more than 70 percent greater than the average probability of receiving a palliative policy. Columns (4), (5), and (6) show the results once the drought variable is decomposed into categories. The sizes of the effects are astonishing. Aligned municipalities experiencing an extreme case of drought have an approximately 51 - 73 percentage points greater chance of receiving immediate relief compared to municipalities where the incumbent mayor is in the

3.5. Allocation of Drought Mitigation Policies

opposition coalition. In the case of severe drought, the chances are approximately 23 - 30 percentage points higher. Moderate drought has similar coefficients, but these are not statistically significant.

Table 3.5 shows the results for palliative policy before mayoral elections. In this case, party alignment seems to not play a role in the allocation of instantaneous aid relief. The linear combination of the party alignment variable and its interaction with drought has no statistical significance in any specification. In fact, the drought variable alone seems to be responsible for a large part of the predictive power in the econometric model, which suggests that the behavior of the central government is responsible, as seen in columns (1), (2) and (3). The same conclusion is reached when the results are observed in the categories of droughts, in columns (4), (5) and (6).

Table 3.6.: Prevention policy before presidential elections

Dependent variable:	prevention policy before election for president					
	(1)	(2)	(3)	(4)	(5)	(6)
Party alignment	0.019 (0.018)	0.036 (0.028)	0.036 (0.030)	0.025 (0.017)	0.034 (0.025)	0.038 (0.026)
Party alignment x Drought	-0.052 (0.086)	-0.075 (0.136)	0.017 (0.146)			
Drought	0.397*** (0.060)	0.428*** (0.097)	0.382*** (0.105)			
<i>Categories of drought:</i>						
Party alignment x Extreme				0.229 (0.199)	0.028 (0.291)	0.299 (0.307)
Party alignment x Severe				-0.107 (0.094)	-0.080 (0.150)	-0.030 (0.160)
Party alignment x Moderate				-0.040 (0.098)	0.022 (0.164)	-0.023 (0.181)
Extreme				0.131 (0.139)	0.206 (0.195)	0.081 (0.210)
Severe				0.464*** (0.065)	0.494*** (0.106)	0.468*** (0.113)
Moderate				0.203*** (0.072)	0.259** (0.121)	0.296** (0.131)
Observations	2,395	1,393	1,240	2,395	1,393	1,240
R-squared	0.172	0.164	0.165	0.195	0.189	0.192
Polynomial order	1	1	1	1	1	1
Bandwidth	0.500	0.151	0.129	0.500	0.151	0.129
Procedure		CTT	IK		CTT	IK
AIC	1041.097			1002.844		

Note: forcing variable is the vote margin of victory of the candidate for mayor in the president's party in the previous municipal election. In columns (1) and (4) the bandwidth is chosen arbitrarily and the polynomial order of forcing variable is chosen by AIC criteria. The columns (2) and (5) use bandwidth selector proposed by Calonico et al. (2014) (*CTT*). The columns (3) and (6) use the bandwidth selector proposed by Imbens and Kalyanaraman (2012) (*IK*). Robust standard error in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

The results of party alignment heterogeneous effects for droughts on prevention policy are depicted in the Tables 3.6 and 3.7. For this kind of policy, the central government does not seem to have any partisan or opportunistic conduct. In both

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Table 3.7.: Prevention policies before mayoral elections

Dependent variable:	prevention policy before election for mayor					
	(1)	(2)	(3)	(4)	(5)	(6)
Party alignment	0.016 (0.020)	0.033 (0.026)	0.036 (0.031)	0.025 (0.019)	0.032 (0.024)	0.038 (0.028)
Party alignment x Drought	-0.121 (0.104)	-0.043 (0.142)	-0.021 (0.166)			
Drought	0.419*** (0.065)	0.450*** (0.090)	0.451*** (0.107)			
<i>Categories of drought:</i>						
Party alignment x Extreme				-0.024 (0.394)	-0.028 (0.467)	-1.286** (0.628)
Party alignment x Severe				-0.122 (0.108)	-0.045 (0.148)	-0.017 (0.171)
Party alignment x Moderate				-0.060 (0.088)	0.018 (0.119)	-0.003 (0.144)
Extreme				-0.119 (0.282)	0.333 (0.310)	0.524 (0.525)
Severe				0.463*** (0.066)	0.481*** (0.093)	0.480*** (0.110)
Moderate				0.204*** (0.061)	0.139* (0.080)	0.117 (0.090)
Observations	1,868	1,361	1,064	1,868	1,361	1,064
R-squared	0.126	0.138	0.141	0.152	0.165	0.171
Polynomial order	1	1	1	1	1	1
Bandwidth	0.500	0.207	0.143	0.500	0.207	0.143
Procedure		CTT	IK		CTT	IK
AIC	718.4768			688.3544		

Note: forcing variable is the vote margin of victory of the candidate for mayor in the president's party in the previous municipal election. In columns (1) and (4) the bandwidth is chosen arbitrarily and the polynomial order of forcing variable is chosen by AIC criteria. The columns (2) and (5) use bandwidth selector proposed by Calonico et al. (2014) (*CTT*). The columns (3) and (6) use the bandwidth selector proposed by Imbens and Kalyanaraman (2012) (*IK*). Robust standard error in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

types of electoral contest, neither the party alignment variable nor the linear combination with its interaction with droughts have sizable or statistically significant impacts. The only exception is the interaction between extreme droughts and party alignment, which has an unstable coefficient and is likely to be dictated by a just few observations.

These findings corroborate the results of previous academic works supporting that political alignment is an important determinant in the allocation of targeted public good (Downton and Pielke Jr, 2001; Solé-Ollé and Sorribas-Navarro, 2008; Cohen and Werker, 2008; Brollo and Nannicini, 2012; Larreguy and Monteiro, 2014). What can also be learned from this exercise is that the government seems to be more opportunistic than partisan, and therefore, the type of election matters. In addition, governments are more likely to bias the distribution of more immediate and less bureaucratic spending than long-term and bureaucratically rigid spending.

3.6. Voters Sometimes Demand Clientelism

The two last sections have demonstrated that although high levels of water scarcity have adverse effects on incumbents, citizens living in areas in which the mayor and the president are aligned have a significant gain in terms of the probability of receiving immediate aid from the central government in times of drought. From the voters' point of view, this dynamic creates a situation that requires a decision between opposing alternatives. The failure to implement adaptive policies to mitigate the effects of droughts would be a motivation to punish the incumbent in the subsequent election. However, in a scenario in which there is already a party alignment between the two levels of government, punishing the incumbent, and thereby changing the party in power, would result in fewer chances to obtain palliative policies. I address the outcome of these two antagonistic motivations in this section. More specifically, the two key questions are as follows: do voters comprehend the benefit of political alignment and vote in favor of this alignment in elections? Given a voter's perception of the political alignment advantage, what is the effect of actual drought on this perception? In the next two subsections, I address these points.

3.6.1. Voter Assessment of Political Alignment

Voters can appraise two kinds of political alignment. In municipal elections, voters evaluate whether to vote for the incumbent mayor who is aligned with the central government taking into account the expected benefit from such an action. Meanwhile, in presidential elections, voters evaluate whether the maintenance of the incumbent president's party is relevant given that their mayor is in the same party as the central government. The importance of party alliance may be different in each situation. In one case, the voter decides the party at the top of the state, and in the other circumstance, the voter chooses the party lower in the hierarchy. In this subsection, I first analyze whether party alignment per se is relevant to voter behavior.

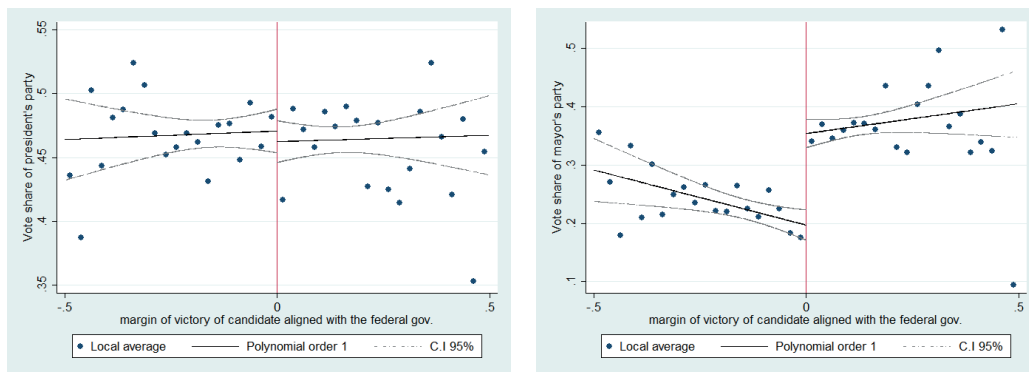
Methodology

Similar to the previous section, I implement an RDD, as in equation 3.4. Nevertheless, the dependent variable in this empirical strategy is SV_m , which assumes the share of the vote for the mayor's party when analyzing municipal elections and the share of the vote for the president's party when analyzing federal elections. The rest of the approach remains the same.

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Results

It could be inferred that voters assess political alignment positively in municipal elections but do not mind such alignment in presidential elections. Figure 3.8a gives a graphical analysis of the political alignment advantage for presidential elections. It can be seen that in this kind of election, party alignment does not seem to play any role. The predicted vote share of the president's party is flat across the threshold of the forcing variable. Voters are indifferent whether the incumbent president is in the same party as the mayor or not. A different conclusion is reached when I analyze the party alignment advantage for mayoral elections. The results can be examined graphically in Figure 3.8b. The upward jump in predicted vote share around the threshold suggests a large party alignment advantage. In this case, alignment between the local and federal parties increases the vote share of the incumbent party in the municipal contest by approximately 17 percentage points.



(a) Presidential elections

(b) Mayoral elections

Note: The x-axis is the margin of victory of mayor candidate in the president's party in the previous municipal election. The y-axis of Figure 3.8a is the vote share of president's party in federal elections. The y-axis of Figure 3.8b is the vote share of mayor's party in municipal elections.

Figure 3.8.: Graphical analysis of RDD: Alignment on election

There are at least two possible explanations for the results. The first reason is related to the federative structure of Brazilian public administration. Since the municipal budget is mainly formed by transfers from the central government, mayors are dependent on higher levels of executive power. Therefore, the alliance with the central government in municipal elections is expected to have a significant advantage. This expectation is consistent with the system of patronage between different levels of public administration in order to ensure the flow of federal revenues (Brollo and Nannicini, 2012). The second possible explanation is related to group rule-utilitarian theory (Coate and Conlin, 2004), which suggests that in municipal elections, individual voters can more easily identify themselves as members of a

group of citizens, organize themselves strategically, and then do their part to help their group win. A strategic vote is less likely to happen in national elections because the heterogeneity of individuals is sufficiently large to prevent a voter bloc from forming in larger contests. Thus, voters might behave more strategically in municipal elections than in national elections, and the importance of political alignment for a group of citizens is taken into account in their voting decisions.

3.6.2. Droughts Impact on Voter Assessment of Political Alignment

Bearing in mind the assessment of party alignment, what would be the influence of the water shortage on the voting strategy of citizens and subsequently on the chances of re-electing the incumbent? As droughts expand economic vulnerability, voters might increase their demand for individualistic transactions and place even more value in the party alliance advantage. One possible way to test this hypothesis is to examine whether party alignment has a heterogeneous effect among municipalities regarding incidents of drought.

Methodology

Two feasible methodologies could provide both informative and distinct answers. The first is a fixed effects panel data model, similar to equation 3.2. The difference in this case is that the interactions are between levels of water scarcity and party alignment. Remember, the coefficients of such a specification should be interpreted as a global average correlation. Because political preferences are not randomly assigned, causal interpretation has to be avoided. Second, in order to employ political alignment as if this effect were exogenous, I implement a regression discontinuity design analysis with heterogeneous treatment effects, as in equation 3.5. The coefficients express the causal average local effect. However, since in this analysis, the impact extracted applies to a particular type of municipality that has the largest share of swing voters, the results raise external validity concerns.

Results

There are three important conclusions from the findings. First, the results confirm that the effect of drought on the vote share of the politically aligned incumbent party would be different in each kind of election. An aligned mayor's party in a municipal election is rewarded, and an aligned president's party in a federal election

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is not. Second, voters act more strategically in mayoral elections than in presidential elections. Voters perceive that is important to maintain mayors who are aligned with the president to maximize their chances of being assisted with drought relief. Third, although both applied methodologies are based on completely different assumptions and provide a distinct interpretation of results, the global average effects, and the causal local average treatment effects are very similar, suggesting that both causal inference and external validity can be applied with some reasonableness. So, when there is an extended period of water shortage, voters might ponder the value of immediate aid relief, rewarding, even more, political alignment in the municipal contest. Thus, voters boost patronage relations by rewarding political linkages between levels of the state's hierarchy. In presidential elections, however, there is no clear incentive for voters to act strategically.

To be more precise, Table 3.8 shows the results for presidential elections. Columns (1) and (2) depict the results of the interaction between party alignment and drought extracted from the fixed effects panel data model with and without controls, respectively. As a global average, the correlation of party alignment in municipalities affected by drought decreases the vote share of the incumbent party by approximately 12 percentage points. Columns (5)-(7) depict the coefficients extracted from the regression discontinuity design analysis with heterogeneous treatment effects. The findings are very similar. The local causal impact of party alignment when there is a water shortage decreases the incumbent party vote share by approximately 9 to 12 percentage points. Splitting the drought variable into categories provides a coherent interpretation of what was found. Columns (3) and (4) suggest no differences in effects among extreme and severe droughts. The correlation accounts for approximately 11 to 13 percentage points of the incumbent party vote share. The moderate category has a tiny and insignificant effect. The effects are larger in columns (8)-(10), which depict the results from RDD. In the cities near the cut-off point of the previous margin of victory, the causal effect of drought combined with party alignment is negatively strong, approximately 30 and 32 percentage points for extreme droughts. However, the hypothesis of linearity between the categories cannot be rejected.

The results are inverted when I study the elections for mayor. Table 3.9 shows the results. Instead of punishing the incumbent party that is aligned with the president for the impacts of a drought, voters switch to voting for the re-election of this party. In other words, voters reward political alignment when they experience a drought. In the columns (1) and (2), the global average correlation of party alignment and drought increases the vote share of the mayor's party by approximately 7 percentage points. Columns (5)-(7) present the results of the RDD that deal with causality issues. The effects are slightly larger, 9 to 12 percentage points. When I study the

Table 3.8.: Political alignment and drought on presidential election

	Dependent variable: vote share of president's party									
	Fixed-effects model					RDD for HLATE				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Party alignment	0.038*** (0.004)	0.036*** (0.004)	0.039*** (0.004)	0.037*** (0.004)	-0.046** (0.022)	-0.030 (0.021)	-0.023 (0.022)	-0.027 (0.023)	-0.015 (0.022)	-0.009 (0.023)
Party alignment x Drought	-0.128*** (0.013)	-0.121*** (0.013)			-0.096 (0.073)	-0.109 (0.071)	-0.121* (0.074)			
Drought	-0.034*** (0.007)	-0.003 (0.007)			0.001 (0.050)	0.023 (0.050)	0.032 (0.052)			
<i>Categories of drought:</i>										
Party alignment x Extreme			-0.112*** (0.028)	-0.129*** (0.027)				-0.322*** (0.152)	-0.300*** (0.129)	-0.296** (0.147)
Party alignment x Severe			-0.124*** (0.014)	-0.115*** (0.014)				-0.093 (0.081)	-0.094 (0.079)	-0.109 (0.082)
Party alignment x Moderate			-0.017 (0.012)	-0.015 (0.012)				-0.203*** (0.079)	-0.161*** (0.077)	-0.145* (0.081)
Extreme			-0.187*** (0.014)	-0.109*** (0.015)				0.122 (0.077)	0.102* (0.061)	0.100 (0.061)
Severe			-0.042*** (0.008)	0.003 (0.008)				0.012 (0.057)	0.031 (0.057)	0.035 (0.060)
Moderate			-0.008 (0.005)	0.013** (0.005)				0.126** (0.057)	0.125** (0.057)	0.111* (0.059)
Observations	21,551	19,224	21,551	19,224	2,391	1,119	1,061	2,391	1,119	1,061
R-squared	0.455	0.504	0.463	0.509	0.017	0.011	0.009	0.026	0.025	0.021
Controls	No	Yes	No	Yes						
Polynomial order					1	1	1	1	1	1
Bandwidth					0.500	0.114	0.108	0.500	0.114	0.108
Procedure					CTT	CTT	IK	CTT	CTT	IK
AIC					-1389.1			-1377.86		

Note: fixed-effects model analyses in columns (1) - (4). The municipalities and years fixed effects included. Standard error clustered at municipality level in parentheses. Controls variables are lagged two years before each election and include population, average income, GINI coefficient, the share of graduated citizens, the share of voters below 18 years old, the proportion of workers in agriculture and industry sector, palliative policy, and prevention policy. RDD analyses in columns (5) - (10). Forcing variable is the vote margin of victory of the candidate for mayor in the president's party in the previous municipal election. In columns (5) and (8) the bandwidth is chosen arbitrarily and the polynomial order of forcing variable is chosen by AIC criteria. The columns (6) and (9) use bandwidth selector proposed by Calonico et al. (2014) (CTT). The columns (7) and (10) use the bandwidth selector proposed by Imbens and Kalyanaraman (2012) (IK). Robust standard error in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

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Table 3.9.: Political alignment and drought on mayoral election

Dependent variable: vote share of mayor's party	RDD for HLATE									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Party alignment	0.081*** (0.007)	0.089*** (0.008)	0.087*** (0.008)	0.097*** (0.009)	0.143*** (0.019)	0.153*** (0.027)	0.157*** (0.031)	0.131*** (0.021)	0.131*** (0.030)	0.129*** (0.033)
Party alignment x Drought	0.078*** (0.021)	0.075*** (0.022)	0.075*** (0.022)	0.075*** (0.022)	0.124** (0.053)	0.099 (0.072)	0.125 (0.080)			
Drought	-0.079*** (0.015)	-0.071*** (0.016)	-0.071*** (0.016)	-0.071*** (0.016)	0.007 (0.038)	-0.017 (0.053)	-0.047 (0.059)			
<i>Categories of drought:</i>										
Party alignment x Extreme			0.126** (0.056)	0.099* (0.056)				0.084 (0.196)	-0.119 (0.264)	0.185 (0.324)
Party alignment x Severe			0.068*** (0.022)	0.065*** (0.023)				0.147*** (0.057)	0.127* (0.075)	0.154* (0.083)
Party alignment x Moderate			-0.045* (0.023)	-0.055** (0.025)				0.089 (0.055)	0.177** (0.076)	0.214** (0.083)
Extreme			-0.155*** (0.033)	-0.120*** (0.034)				0.084 (0.190)	0.154 (0.261)	-0.148 (0.321)
Severe			-0.090*** (0.017)	-0.078*** (0.018)				-0.003 (0.039)	-0.039 (0.054)	-0.063 (0.060)
Moderate			-0.010 (0.011)	-0.004 (0.011)				-0.047 (0.039)	-0.087* (0.051)	-0.099* (0.053)
Observations	21,888	19,466	21,888	19,466	1,864	1,191	973	1,864	1,191	973
R-squared	0.032	0.031	0.033	0.032	0.088	0.100	0.105	0.097	0.104	0.111
Controls	No	Yes	No	Yes	I	I	I	I	I	I
Polynomial order					0.500	0.168	0.124	0.5	0.168	0.124
Bandwidth					IK	CTT	IK	0.5	CTT	IK
Procedure										
AIC					160.0612			167.6262		

Note: fixed-effects model analyses in columns (1) - (4). The municipalities and years fixed effects included. Standard error clustered at municipality level in parentheses. Controls variables are lagged two years before each election and include population, average income, GINI coefficient, the share of graduated citizens, the share of voters below 18 years old, the proportion of workers in agriculture and industry sector, palliative policy, and prevention policy. RDD analyses in columns (5) - (10). forcing variable is the vote margin of victory of the candidate for mayor in the president's party in the previous municipal election. In columns (5) and (8) the bandwidth is chosen arbitrarily and the polynomial order of forcing variable is chosen by AIC criteria. The columns (6) and (9) use bandwidth selector proposed by Calonico et al. (2014) (CTT). The columns (7) and (10) use the bandwidth selector proposed by Imbens and Kalyanaraman (2012) (IK). Robust standard error in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

categories of drought, the analyses reach very similar conclusions. Columns (3) and (4) show that extreme droughts have a positive average correlation of 10 to 12 percentage points, severe droughts have a correlation of approximately 6 percent-

age points, and moderate droughts have a negative correlation. Columns (8)-(12) show the heterogeneous local causal effect of each category of drought. Although the extreme category yields unstable coefficients, in general, the findings are very similar in the sense that it seems that the causal effect is positive and linear.

3.7. Concluding Remarks

In Brazil, drought is the most common natural disaster and is a major cause of hunger, thirst, death, and forced migration. However, Nelson and Finan (2009) indicate that some people living in regions that have historically been devastated by these events in fact prefer periods of extreme water scarcity. In particular, they report that an elderly female goes so far as to pray for a drought. The suggested reason for someone to have such a desire is that the poor have become entirely dependent on government aid. As the arrival of the drought triggers the government aid relief mechanism, locals come to wish for it.

The above paper departed from this long debate suggesting that the high incidence of droughts in Brazil established the foundation for a long-run model of clientelism. For centuries, the rural households have traded their servility with large landowners by means of subsistence. In periods of severe water scarcity, people in extreme poverty face even higher welfare deterioration. Landlords, in turn, afforded them supplies food and water, among other necessities, but not without imposing rent extraction and patronage. This enforced social interaction becomes known as the *drought industry*.

A force that could detach the historical ties of rural households with their patrons would be the insertion of a welfare-maximizing state, which would promote long-run investment in infrastructure to improve natural disasters preparedness and assist the most deprived. Instead, what is found is that the Brazilian government opportunistically captures the historical aid dependency of peasants in order to maximize re-election chances.

The high political turnover due to drought creates a unique scenario that encourages the central government to allocate aid relief strategically in order to assist politically aligned municipalities to the detriment of those governed by opposition parties. On the one hand, sending aid relief to aligned municipalities is a manner of forestalling the advance of opposition parties and maintaining incumbent party alignment between municipal and central governments. On the other hand, neglecting aid relief for non-aligned municipalities could guarantee municipal political turnover and make room for candidates allies.

This paper finds evidence suggesting that voters anticipate the opportunistic be-

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havior by governments and therefore act strategically to maximize their future benefits. As political alignment plays a significant role in public resource allocation, citizens foster this alignment to increase their chances of being assisted in times of water crisis. Such voter behavior is boosted when voters are experiencing a drought, which confirms that increased vulnerability leads citizens to demand patronage politics in line with Bobonis et al. (2017). Politicians, for their part, assimilate the demand of voters and strive to give them what they want. This dynamic connects results in a long-run patronage equilibrium that leads to the sub-optimal provision of public goods and prevents the development of a region.

In conclusion, the continued absence of investments in drought preparedness infrastructure combined with the lack of responsibility in the allocation of immediate relief verifies the government's low willingness to promote the general welfare. In fact, the intrinsic motivation of landlords to take advantage of such degrading situations remains present in governmental actions. Therefore, the old clientelism system between landlords and peasants described as the *drought industry* did not vanish. Instead, this system was captured by the central government. A relevant follow-up research agenda would be to examine how to break such a clientelism cycle to ensure government efficiency in implementing welfare-enhancing public policies and in promoting resilience against droughts.

Appendix A Institutional Setting

Drought is one of the most common natural disasters in the world, and this phenomenon is the most frequent disaster in Brazil and is both economically and environmentally destructive (Dai, 2011). In this scenario, the expression *drought industry* was invented from a singular observance of the long-term socioeconomic consequences that this natural event triggers (Callado, 1960). This catchphrase is regularly used in novels, song lyrics, and political debates (de Castro, 1967; Oliveira, 1977; Ribeiro, 1995; Silva, 2006; Matos, 2013) and refers to a complex process of distortions in government transfers that initially aimed to alleviate the drought's adverse effects but are instead used for political purposes. Although the *drought industry* has never been empirically tested, it is sustained by an informal rhetoric suggesting that the incidence of droughts and the Brazilian political economy are directly linked. In the following subsections, I first detail the background of the political anecdote, and I then describe the contemporary situation.

A.1 The Origins of the *Drought Industry*

The roots of the *drought industry* in Brazil originate in the Portuguese colonial period (1500 – 1815). To populate the Brazilian territory and extract its productive value, the Portuguese Crown granted vast areas of land to nobles, military officers, investors, and others loyal to Lisbon. This practice resulted in land being distributed to just a few people who had some prestige with the Crown. Because of this uneven distribution of land, large landholdings remained, through time, in the hands of a small group of privileged citizens, who we will call the elite.

Because the environment in drought-prone regions is characterized by very precarious conditions, agricultural workers and their families lived permanently on the edge of survivability (Guilhoto et al., 2011). In addition to facing extreme poverty and all social illnesses intrinsic to this condition, during periods of severe water shortages, rural households are fated to fight every day to find means to drink and to eat. Hunger and thirst reached their most drastic levels and often led to death. Rural families become dependent on external aid to stay alive.

In exchange for protection during periods of thirst and starvation, landlords extended to rural households a condition of servility, exploitation, and dependency. The characteristics of such social contract, which has close ties with the uneven land ownership regime, are usually associated with the “aristocratic mentality” that is characterized by values of obedience and loyalty and have built a set of social relations marked by despotism and violence (Matos, 2013).

The panorama of elite citizens running their landholdings as fiefdoms persisted

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for centuries throughout Brazilian history and underpinned a platform of unequal power of exchange between landlords and peasants. Such a relationship with landlords providing the means of survival and demanding loyalty in return become known as *coronelismo*.

A.2 The Contemporary Drought Industry

At the end of the nineteenth century, droughts began to receive public attention, and the Brazilian central government began to promote aid relief policies to assist rural households suffering from this kind of natural disaster. Indeed, these governmental interventions reduced drought-related mortality, attenuated migration movements, and reduced the vulnerability of rain-fed farmers but did so at the expense of resilience to the weather perturbation. Families living in drought-prone regions remained dependent on external aid to deal with a crisis because aid relief policies are rarely designed for drought preparedness measures. Instead, these policies are mostly formulated in real time to attenuate the ongoing consequences of drought. This approach to humanitarian aid programs led peasants to long-run adaptations to weather shocks and has precluded these people from developing alternative adaptive mechanisms for survival. Thus, in the absence of landlord protection, rural households continued to rely solely on government actions for access to food, water, and necessary supplements during periods of drought.

In this new context of governmental intervention, the role played by landlords in protection for peasants did not vanish. Rather, quite the opposite occurred. Oligarchs captured the agencies responsible for the aid relief programs, and the access to public resources from the central government, in some way, was controlled by landlords (Silva, 2006). For instance, the locations chosen for the construction of water reservoirs with public funds were on the landlords' properties so they could charge exorbitant prices for water access. In addition, funding for public works was put directly into the hands of landlords, and thus, they had a stronger influence over the hiring of rural workers, thus expanding their patriarchal authority. In short, drought had become a large and prosperous business for this *sui generis* social class, the *industriais da seca* (industrials of drought) (Callado, 1960).

Farmworkers in drought-prone regions, regardless of whether they had been servile to landlords, are dependent on the political apparatus of the government, and thus to the local elite, during times of crisis. Since rural households face livelihood insecurity almost every year, there are some anecdotes suggesting that beneficiaries, who are not sponsored by landlords, actually prefer years of drought because government intervention brings a sense of security (Nelson and Finan, 2009). In the period before public assistance is mobilized, communities commonly invade local

A. Institutional Setting

stores in search of food and block federal highways, stopping commercial food trucks to plunder their contents. This activity indicates that citizens have become accustomed to the dynamics between drought and governmental policies and have built expectations around future actions by government agencies.

In some sense, the direct patronage role of landlords was replaced by government actions, which is still an indirect patronage relationship between a political apparatus (landlords) and voters (peasants). Mitigation policies for drought started to be a political commodity between lower levels and higher levels of public administration. In order to guarantee the flow of public revenues, a coalition between politicians at the municipality level and the federal level began to be crucial. Municipalities where mayors were members of the opposition political party of the central government suffer from delays in emergency funds or even the denial of funds (Larreguy and Monteiro, 2014). Thus, the patronage system (*coronelismo*) also dominated the relationships between levels of government itself (Nelson and Finan, 2009).

Appendix B Robustness Checks of the Main Analysis

The main findings of the study are corroborated by two empirical strategies that are grounded in different assumptions: a fixed-effects model and a regression discontinuity design. However, there is still room to question potential flaws in the methodology. In order to address some possible flaws, this section proposes robustness checks that complement the main findings. Specifically, I reanalyze the two empirical strategies by 1) using a different technique for clustering the standard errors, 2) restricting the sample to municipalities with a higher propensity for drought, 3) decomposing droughts into cyclical and trend components, and 4) testing an alternative measure of aridity shocks. I report the results for mayoral elections given that this is the type of election where I find that voters rewarded political alignment and given that voters do not care about party alignment in the presidential election. Table B.1 shows the results.

One concern might be how the standard errors are grouped. Although the technical procedure for clustering the standard errors does not affect the estimations, this procedure affects the statistical significance. For instance, it may be that the variance of the incidence of drought is correlated within municipalities that are geographically close to one another, that is, cities located in the same Brazilian federal state. To consider this potential flaw in the analyses, I cluster the standard errors at the state level instead of at the municipality level in Panel A. As it can be seen in columns (1) to (5), how standard errors are clustered is not of concern since the results maintain their statistical significance at the usual levels.

Given the extensive heterogeneity of Brazilian municipalities, one may be concerned about which cities are driving the results. In the fixed-effects models, the estimates are provided by the variation of droughts within the municipalities. However, some cities did not experience any droughts during the analysis period, and therefore, these municipalities are not adding information on the impact of droughts to the outcome variable apart from improving the accuracy of the prediction of the control variables. In the same vein, municipalities that have never experienced droughts may not be reasonable counterfactuals for the regression discontinuity design approach. To ensure that the effects found are not due to the presence of municipalities that have a very low likelihood of experiencing droughts, I restrict the sample to cities that have experienced at least one drought. The results are depicted in Panel B. The coefficient sizes remain similar to those in the primary analysis; however, for RDD, I probably lose statistical power due to the drastic reduction in sample size in columns (8) to (10).

B. Robustness Checks of the Main Analysis

Table B.1.: Robustness checks

Panel A					
	Fixed-effects model		RDD for HLATE		
	(1)	(2)	(3)	(4)	(5)
Party alignment	0.081*** (0.013)	0.089*** (0.013)	0.143*** (0.024)	0.153*** (0.030)	0.157*** (0.035)
Party alignment x Drought	0.078** (0.033)	0.075** (0.036)	0.124** (0.057)	0.099 (0.112)	0.125 (0.093)
Drought	-0.079*** (0.020)	-0.071*** (0.023)	0.007 (0.042)	-0.017 (0.073)	-0.047 (0.066)
Observations	21,888	19,466	1,864	1,191	973
R-squared	0.032	0.031	0.088	0.100	0.105
Panel B					
	Fixed-effects model		RDD for HLATE		
	(6)	(7)	(8)	(9)	(10)
Party alignment	0.043* (0.022)	0.050** (0.024)	0.185* (0.111)	0.183* (0.095)	0.132 (0.095)
Party alignment x Drought	0.101*** (0.030)	0.106*** (0.032)	0.071 (0.139)	0.071 (0.118)	0.122 (0.119)
Drought	-0.072*** (0.015)	-0.066*** (0.016)	-0.091 (0.100)	-0.047 (0.090)	-0.099 (0.090)
Observations	5,631	5,198	446	260	252
R-squared	0.071	0.065	0.136	0.156	0.171
Panel C					
	Fixed-effects model		RDD for HLATE		
	(11)	(12)	(13)	(14)	(15)
Party alignment	0.074*** (0.011)	0.083*** (0.011)	0.101* (0.056)	0.093** (0.043)	0.085* (0.049)
Party alignment x Cyclical component (AI)	0.070*** (0.023)	0.056** (0.023)	0.300** (0.125)	0.219** (0.096)	0.247** (0.106)
Party alignment x Trend component (AI)	0.016** (0.007)	0.014** (0.007)	0.079** (0.036)	0.066** (0.028)	0.079** (0.032)
Cyclical component (AI)	-0.066*** (0.010)	-0.057*** (0.010)	-0.241*** (0.071)	-0.119** (0.060)	-0.169** (0.068)
Trend component (AI)	-0.019*** (0.007)	-0.015** (0.007)	-0.056*** (0.021)	-0.029* (0.017)	-0.051** (0.020)
Observations	21,888	19,466	1,864	1,191	973
R-squared	0.033	0.032	0.093	0.101	0.111
Panel D					
	Fixed-effects model		RDD for HLATE		
	(16)	(17)	(18)	(19)	(20)
Party alignment	0.090*** (0.007)	0.098*** (0.008)	0.181*** (0.032)	0.157*** (0.026)	0.166*** (0.029)
Party alignment x Zscore AI	0.009 (0.008)	0.010 (0.009)	0.086** (0.038)	0.086*** (0.029)	0.075** (0.032)
Zscore AI	-0.021*** (0.003)	-0.021*** (0.003)	-0.049** (0.023)	-0.046** (0.019)	-0.052*** (0.020)
Observations	21,888	19,466	1,864	1,191	973
R-squared	0.033	0.032	0.086	0.098	0.103

Note: no controls included in columns (1), (6), (11), and (16). Controls included in columns (2), (7), (12), and (17). In columns (3), (8), (13), and (18) the bandwidth are chosen arbitrarily and the polynomial order of forcing variable are chosen by AIC criteria. Columns (4), (9), (14), and (19) use bandwidth selector proposed by Calonico et al. (2014) (*CIT*). Columns (5), (10), (15), and (20) use bandwidth selector proposed by Imbens and Kalyanaraman (2012) (*IK*). Robust standard error in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

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Additionally, I deal with two crucial issues that can be raised from the analyses. First, an open question in the literature regarding the economic impacts of natural disasters is that what matters most is not clear: the intensity of a shock or the change in the level of a shock (Healy and Malhotra, 2013). It may be that voters living in a region that regularly experiences extreme droughts act differently from voters residing in an area that rarely faces these kinds of events and that between-municipality variation characteristics do not accurately capture these behavioral differences. Second, since voters' previous expectations are crucial in deciding who to vote for, and because both the level and level change shocks may have different implications for voter expectations, the electoral consequences may also be different. One way to address both of the issues referred to above is to decompose the drought measure into the following two components: the trend and the cycle. Municipalities that have A path dependence on droughts would exhibit high values of the trend variable, ensuring control of the difference in characteristics between drought-prone cities, and the cyclical component would capture an unexpected change in the level of aridity. Since I have time series data on aridity from 1963 to 2013, I decompose the trend and cyclical components following the procedure suggested by Hamilton (2017). This procedure calculates the trend and cycle components for each period as a simple prediction of linear regression taking into account only past observations. This approach is fundamental because a priori, voters have no information on future weather conditions and base all their beliefs solely on the past. Note that the trend component smoothly tracks the actual aridity index, while the cyclical component follows a random walk around zero and is arguably exogenous. Panel C depicts the results. The cyclical component seems to be the most determinant for the positive effect of drought and political alignment on the vote share of mayor's party. That is, both impacts on the level and level changes are important for voter behavior. However, the change in level has a stronger consequence. This finding may be because an unexpected change leads to a reordering of the beliefs of voters. That is to say, voters alter their predictions of the trajectory of the level of aridity drastically, and thus, they react more forcefully in their voting strategies.

Finally, to ensure meaningful comparisons across municipalities with different water scarcity conditions, aridity shocks are measured as the Z-score of the aridity index. More specifically, I calculate the difference between the current period's aridity and the historical mean of aridity in the municipality during the same two years divided by the municipality's historical standard deviation. Such a measurement is regularly used in studies that analyze droughts only taking into account precipitations (Rocha and Soares, 2015; Bobonis et al., 2017). The results presented in Panel C are robust to this aridity shock measurement choice and confirm that the party alignment effect increases when municipalities suffer a more significant water

B. Robustness Checks of the Main Analysis

shortage than their historical average.

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Appendix C RDD Validity

In this appendix, I graphically show the validity of the Regression Discontinuity Design (RDD) using covariates as dependent variables.

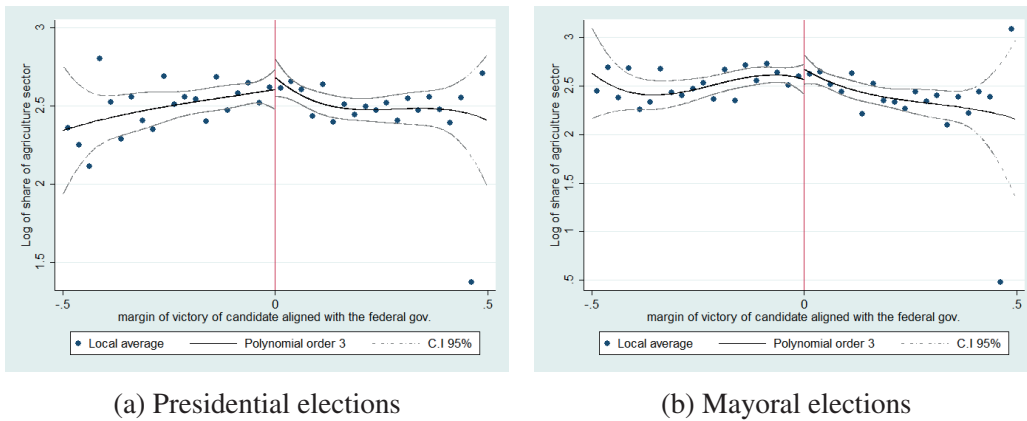


Figure C.1.: Share of workers in agriculture sector

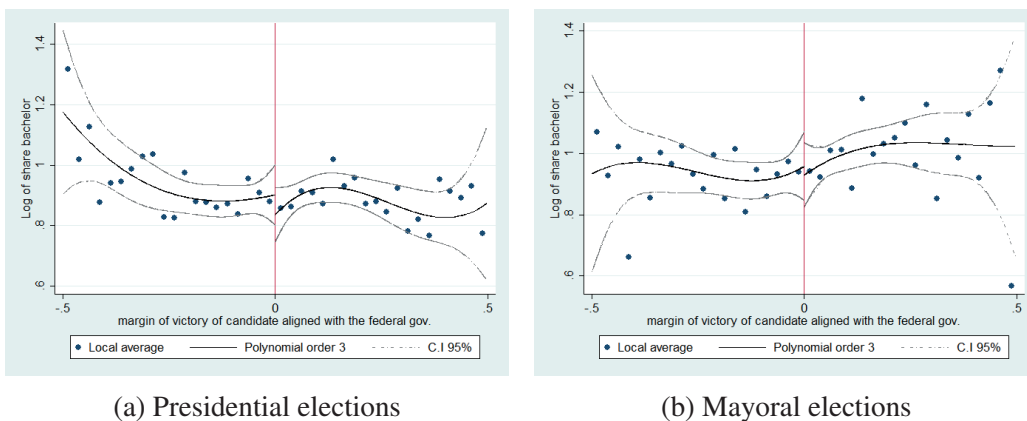
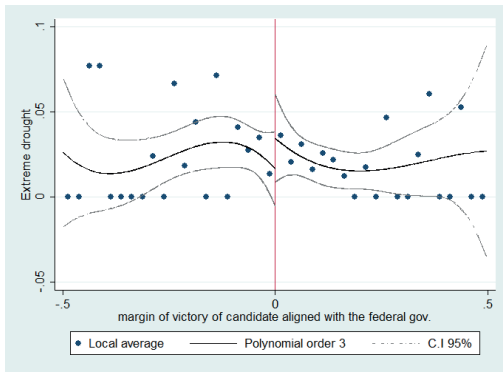
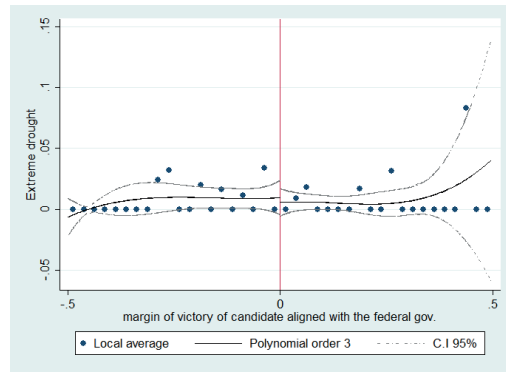


Figure C.2.: Share of graduated citizens

C. RDD Validity

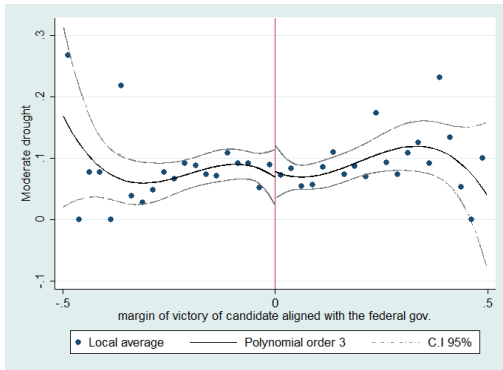


(a) Presidential elections

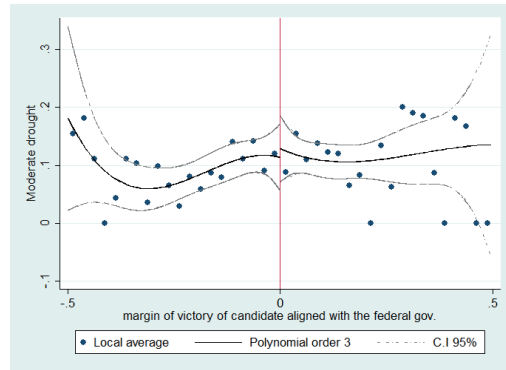


(b) Mayoral elections

Figure C.3.: Extreme drought

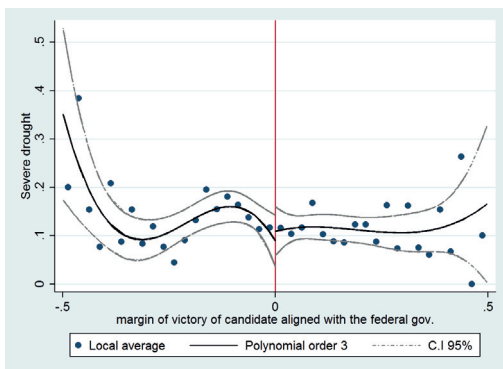


(a) Presidential elections

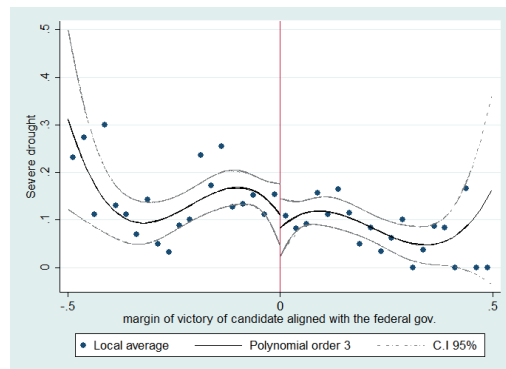


(b) Mayoral elections

Figure C.4.: Moderate drought



(a) Presidential elections



(b) Mayoral elections

Figure C.5.: Severe drought

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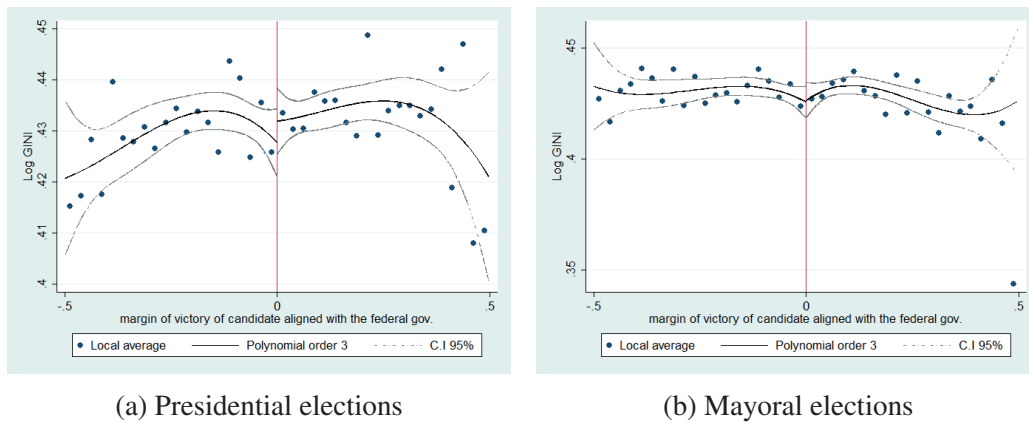


Figure C.6.: GINI coefficient

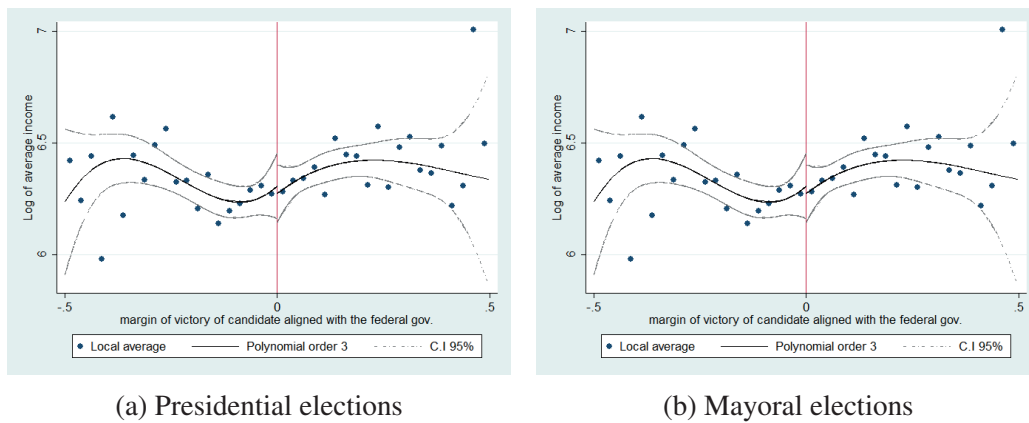


Figure C.7.: Average income

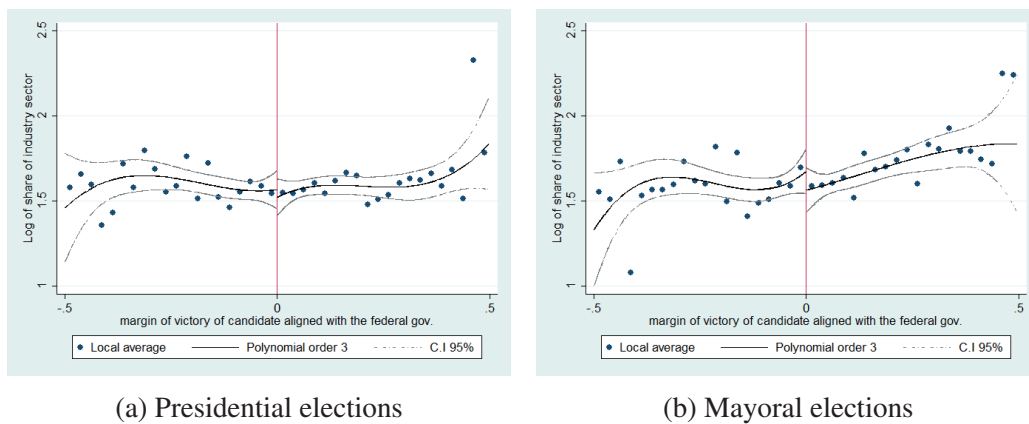
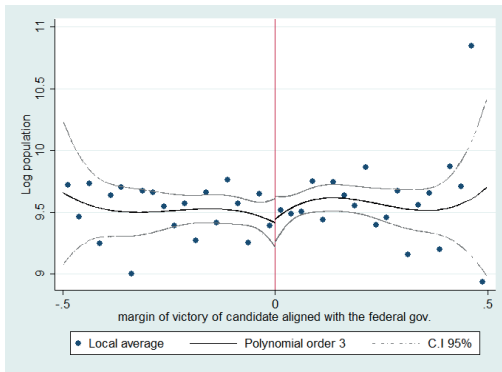
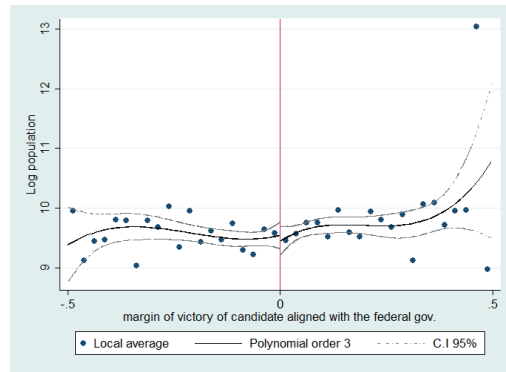


Figure C.8.: Share of workers in industry sector

C. RDD Validity

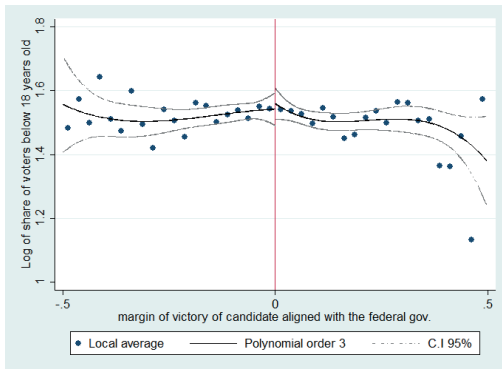


(a) Presidential elections

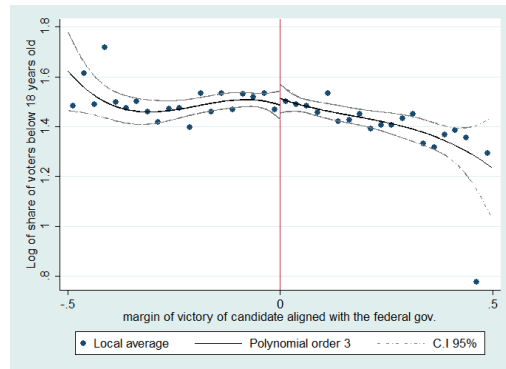


(b) Mayoral elections

Figure C.9.: Population

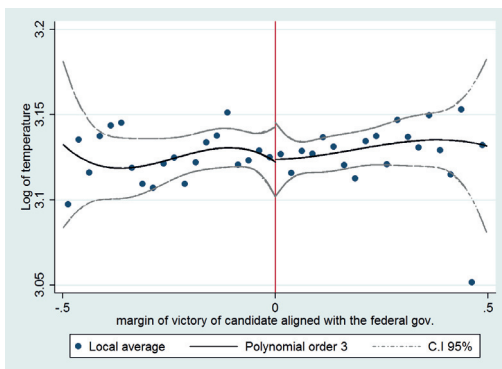


(a) Presidential elections

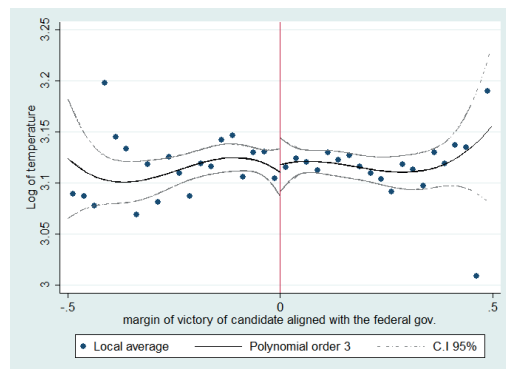


(b) Mayoral elections

Figure C.10.: Share of voters below 18 years



(a) Presidential elections



(b) Mayoral elections

Figure C.11.: Temperature

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Appendix D Data Sources

Table D.1.: Data Sources

Variables	Institutions	Sources
Rainfall	INMET	www.inmet.gov.br
Evaporation	INMET	www.inmet.gov.br
Temperature	INMET	www.inmet.gov.br
Share of graduated citizens	Brazilain Census (1991, 2000, 2010) from IBGE	www.ibge.gov.br
Share of workers in agriculture sector	Brazilain Census (1991, 2000, 2010) from IBGE	www.ibge.gov.br
Share of workers in industry sector	Brazilain Census (1991, 2000, 2010) from IBGE	www.ibge.gov.br
Average income	Brazilain Census (1991, 2000, 2010) from IBGE	www.ibge.gov.br
GINI coefficient	Brazilain Census (1991, 2000, 2010) from IBGE	www.ibge.gov.br
Population	FINBRA	www.tesouro.fazenda.gov.br
Share of voters below 18 years old	Tribunal Superior Eleitoral	www.tse.jus.br
Margin of victory before presidential elections	Tribunal Superior Eleitoral	www.tse.jus.br
Margin of victory before mayoral elections	Tribunal Superior Eleitoral	www.tse.jus.br
Vote share of president's party	Tribunal Superior Eleitoral	www.tse.jus.br
Vote share of mayor's party	Tribunal Superior Eleitoral	www.tse.jus.br
Palliative policy	S2ID	s2id.integracao.gov.br
Prevention policy	Portal da Transparência	www.portaldatransparencia.gov.br

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4. Does Emergency Aid for Drought Increase Corruption? §

4.1. Introduction

Extreme weather events as droughts, floods, and tornadoes are likely to occur more frequently in the coming years as a result of climate change (Pachauri and Reisinger, 2007). This will further intensify the tragedy of natural disasters and the problems they cause (Strömberg, 2007). The economic costs are substantial and escalating. These natural shocks affect various aspects of an economy, including the declining average productivity of assets, lower long-term growth rates, and worsening health and human capital (Maccini and Yang, 2009; Felbermayr and Gröschl, 2014; Rocha and Soares, 2015). The situation is even more dramatic in developing countries, where state capacity is usually deficient and catastrophic deaths are on average the highest (Kahn, 2005). In this scenario, how governments design and implement public policies to mitigate the adverse impacts of natural disasters will play a key role in development economics.

In countries with frequent natural disasters, governments usually prioritize palliative and short-term policies at the expense of prevention and long-term policies.¹ One explanation for this may be the fact that voters behave myopically and reward incumbents for delivering disaster relief, but not for investing in disaster preparedness (Cole et al., 2012). As emergency relief has an immediate impact on the well-being of citizens and is highly discretionary, it has become attractive to policymakers who use it to promote clientelism and favor their constituencies.² Emergency policies are also characterized by low regulation and little transparency, which cre-

[§]The paper in this chapter is single-authored.

¹Neumayer et al. (2014) argue that private individuals tend to under-invest in prevention because of information asymmetry and collective action problems. Cohen and Werker (2008), using a political-economy model of disaster prevention, show that aid relief distorts a government's incentives to prepare well for disasters.

²In fact, the nature of disaster relief as an emergency expenditure which is often not previously reserved in the public budget plan creates room for discretionary allocations, and it is frequently associated with lack of accountability (Downton and Pielke Jr, 2001; Garrett and Sobel, 2003; Cole et al., 2012).

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ates a window of opportunity for rent extractors (Leeson and Sobel, 2008; Nikolova and Marinov, 2017). The diversion of public resources for private use is one of the worst flaws that a public policy design can produce. This paper aims to study whether the allocation of palliative policies creates an increase in the level of corruption in the context of droughts in a developing country. Interestingly, the extent of rent seeking related to these policies is essential for evaluating their cost-benefit and reshaping them to achieve their purposes properly.

This paper proposes an empirical analysis of whether federal palliative transfers to municipalities to address severe water shortages are diverted from their original aim by mayors in municipal governments in the Brazilian setting. Historically, the process to trigger palliative policies by the federal government starts with the recognition of a state of emergency. Then, aid relief can arrive in various forms that are difficult to monetize, such as water distribution and sending the army to help with basic needs. Thus, this study investigates the number of federal emergency declarations against droughts as it relates to municipal corruption. Corruption is measured as the number of irregularities in the local governments' expenditures found by auditors in Brazilian cities during a full mayor's term. To isolate the causal impact of an additional emergency declaration on corruption, I exploit the quasi-random nature of the cycle component of the aridity relative to its trend as an instrumental variable. Additionally, the study implements a battery of robustness checks and examines heterogeneous effects regarding electoral features, social capital, propensity to suffer droughts, and fiscal capacity.

Investigating the relationship between emergency aid for droughts and the level of corruption in the Brazilian context is attractive for at least two reasons. The first is because the Brazilian tragedy of drought is a deeply scrutinized problem and has secular roots. Governmental interventions to mitigate the worst of its effects began in the era of Emperor Dom Pedro II (1825 - 1881) and experienced several changes as experts debated vigorously on how to approach the problem (Callado, 1960; Campos, 2015).³ Second, drought is the most frequent natural disaster in Brazil, and its incidence has increased in recent decades. Between 1970 and 2012, it accounted for more than 50% of all federal declarations of a state of emergency (Nelson and Finan, 2009). In the period between 2013 and 2015, the number of droughts increased by 168%.⁴ Quantitatively, the number of people benefiting from aid relief from drought is superlative. For instance, during the years 2012 to 2013, approximately 1,400,000 people were received government aid, while 660,000 farmers received financial support because of broken harvests (Campos, 2015).

³Section 4.2 provides an historical review of the Brazilian government interventions to address droughts.

⁴For more details see ANA (*Agência Nacional de Águas*).

In recent years, a growing body of literature has claimed that a windfall of resources can have adverse effects through the political process leading to increased rent-seeking (Arezki and Brückner, 2011). Studying the federal funds triggered by diverse types of natural disasters in the U.S, Leeson and Sobel (2008) estimate that additional \$1 per capita in average annual relief increases corruption nearly 2.5 percent in the average state. Using flood-related transfers that the Bulgarian central government distributed to municipalities following torrential rains in 2004 and 2005, Nikolova and Marinov (2017) find that a 10% increase in per capita funds leads to a 9.8% increase in corruption. Other studies investigate the reduced form of the impact of extreme weather events on corruption. For instance, Yamamura (2014) finds that public sector corruption increases in both developing and developed countries when affected by natural disasters. In the same vein, Escaleras and Register (2015) confirm similar results for natural hazards as earthquakes, windstorms, and floods. However, these studies do not specifically address aid relief to handle water scarcity.

This study presents some advantages over previous works. First, I rely on an objective measure of corruption instead of a measurement based on perception. Perception-based measures of corruption are less accurate due to several confounding factors (Olken, 2009).⁵ We construct a proxy for the amount of money involved in illegal procurement practices, fraud, favoritism, and over-invoicing collected from public audits reports produced by a Brazilian anti-corruption agency, *Controladoria Geral da União* (CGU). An unusual characteristic of this anti-corruption program is that the audited municipalities were selected by lottery. As the audit reports provide a detailed picture of how local governments make their expenditures, the sample contains municipalities without accurate corruption cases. This is important for the empirical analysis because it provides an ideal control group for corrupt municipalities. Such a feature is presumably absent in studies on corruption that are based on lawsuits or media reports because the control group can be either municipality without corruption or municipalities where graft and bribery were simply not discovered. Second, our measure of corruption represents the level of corrupt practices throughout the entire term of an incumbent. That is, the study does not consider cases where the audit reports were published when the incumbent was still in office. Thus, it avoids measurement noise related to the behavioral change of incumbent politicians due to information shocks that revealed corruption scandals

⁵The papers of Nikolova and Marinov (2017) and Leeson and Sobel (2008) also use objective measures of corruption. However, while the former builds the measure of corruption based on judicial convictions and may thus miss cases that were not prosecuted, the latter includes in its definition cases of corruption that can be interpreted simply as mismanagement rather than explicitly corrupt practices.

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cause (Timmons and Garfias, 2015).

In previewing the paper's results, I find that emergency relief increases the amount of money involved in corrupt practices. To the best of my knowledge, this is the first evidence of the impact of aid relief for droughts on corruption in the public sector. Precisely, the partial effects of recognitions of the state of emergency lead to an increase of approximately R\$ 5.00 per capita for all observations and R\$ 41.00 per capita for corrupted municipalities of diverted public money.⁶ Although the dataset format precludes panel data analysis, the sizes of the coefficients are robust for the inclusion of economics and political characteristics, incumbent mayor characteristics, and municipal public finances controls. However, the provision of aid relief does not seem to induce an honest mayor to commit acts of corruption. The coefficients are not significant when the dependent variables are the presence of corruption and the fraction of corruption among audited municipal expenditures. Aid relief against drought seems only to open an opportunity for dishonest mayors to steal more public resources. As an extension, the study shows that the heterogeneity of the effect is unclear concerning various dimensions such as electoral characteristics, media presence, history of droughts, and fiscal capacity.

Our results complement the literature on the determinants of corruption (Serra, 2006; Treisman, 2007; Dong and Torgler, 2013). Our study is also related to works that exploit the impact of natural disasters on political outcomes (Hunt, 2007; Bechtel and Hainmueller, 2011; Cole et al., 2012; Quiroz Flores and Smith, 2013). Additionally, this paper is connected to the recent literature that studies Brazil's randomized auditing policy (Ferraz and Finan, 2008, 2011; Brollo et al., 2013; Timmons and Garfias, 2015). As for policy implication, this paper reinforces a call for a re-assessment of how emergency policies are designed to meet the needs of those that deserve it more.

The remainder of this paper is structured as follows. Section 4.2 provides a brief explanation on how the design of public policies against droughts evolved over time in Brazil. Section 4.3 gives a very simple framework of why emergency aid against drought might affect the level of corruption. Section 4.4 presents the data source. Section 4.5 describes the empirical strategy. Section 4.6 presents the results. Section 4.7 concludes.

4.2. Background of Emergency Aid for Droughts

Drought is one of the most common natural disasters in the world. As this phenomenon restricts access to an essential resource for humans, it has disturbing im-

⁶Based on 2008 prices.

4.2. Background of Emergency Aid for Droughts

plications for economic development (Dai, 2011; Freire-Gonzalez et al., 2017). In Brazil, drought has been associated with hunger and mortality. The occurrence of droughts in the Brazilian territory has been reported since the beginning of Portuguese colonization.⁷ However, it was not until the mid-nineteenth century that the state began to implement public policies to mitigate its effects (Campos, 2015).⁸

A continuous debate between experts and the elite over time has determined the design of policies against droughts and the amount of public money disbursed on it.⁹ The dominant view that prevailed for many years was that the cause of the drought was due to the ignorance of the rural workers who did not know how to manage the land properly.¹⁰ This view is later confronted by the argument that climatic factors would be the cause of periods of water scarcity.¹¹ Consequently, in the 1850s, as the first governmental intervention to deal with droughts, ground stations began to be built across the country to monitor the rainfall levels and predict future droughts (Campos, 2015).

The great drought in north-eastern Brazil in 1877 was a turning point, as it elevated this natural tragedy to another level of public debate.¹² As a reaction, the state began to build water reservoirs in drought-prone regions. Although this public policy endured until the early 1950s, these reservoirs had proven to be ineffective in times of water shortage because they were non-perennial and had no water distribution system. Another turning point that switched the design of policies against drought came after the critical drought in 1958, which caused several deaths and displaced thousands of people. Policymakers changed the focus towards humanitarian aid as the social condition of the peasants was increasingly discussed by experts and the elite. At that time, the procedure of declaring a state of emergency

⁷The first reference from the Brazilian drought tragedy date to 1583, when the Jesuit Priest Fernão Cardim (1549 - 1625) wrote that a drought caused death by hunger of approximately five thousand Indians.

⁸However, the state was already concerned about it even before the nineteenth century. In 1777, members of the Camera Official, which represented the Portuguese Kingdom, asked the King for assistance by sending as many slaves as had died from starvation in a drought that killed 7/8 of the cattle.

⁹On May 30, 1856, in a session chaired by the Emperor D. Pedro II, a commission of Brazilian engineers and naturalists was established to explore the problem of lesser-known Brazilian provinces. This became the symbolic moment in which policies against drought began to be formulated.

¹⁰This view, represented by Guilherme de Capanema (1824 - 1908), argued that because of the lack of human capacity in the drought-prone regions, nothing could be done by the state to resolve the problem of droughts.

¹¹The engineer Virato de Medeiros (1823 - 1900) was the voice of this vision and advocated that the state should improve the state capacity to predict coming droughts.

¹²Because of the severity of the water shortage, the Emperor D. Pedro II had promised to sell the crown jewels to solve the problem.

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to trigger individualistic provision of food and water was set up.¹³

The priority given to palliative policies to address drought became common both in the period of military dictatorship (1964 - 1985) and in the democratic periods. Permanent policies that promote environmental sustainability and economic activity have been left out of the public policy agenda.¹⁴ Although humanitarian aid has reduced drought-related deaths and extinguished massive migratory movements, this happened at the expense of rural workers' resilience. Rural workers' dependence on humanitarian aid in times of water crisis created a propitious scenario for a clientelism relationship between politicians and voters. In exchange for votes, incumbent politicians favored their supporters with water distribution. This clientelism relationship became known as the drought industry (*indústria da seca*) (Callado, 1960).

Public scandals involving the misuse of public resources that originally aimed to relieve the consequences of drought are not rare. The increasing amount of public resources earmarked for humanitarian aid combined with low transparency on its implementation has opened a window of opportunity for corrupt activities.¹⁵ In fact, the malfeasance in government programs to address droughts was supported by informal rhetoric in novels and song lyrics suggesting that the Brazilian political economy and the incidence of drought are intrinsically linked. It is not in vain that the drought industry is still used in regional and national political debates (Nelson and Finan, 2009).

¹³This novel approach was led by the creation of the *Superintendência de Desenvolvimento do Nordeste* (SUDENE) by the Brazilian government. Under the supervision of Celso Furtado, the process of implementing public policies against drought became more decentralized, and the target of these policies became more individualistic than before.

¹⁴By the end of the 1990s, the Brazilian federal government added other forms of policy against drought. For example, rural families already included in the *Bolsa Família* conditional cash transfer program became eligible for *Bolsa Estiagem*, a program that increases the earnings of the families suffering from a drought. Another social program that was created focuses on drought is the *Garantia Safra* initiative, which offers insurance to rural farmers who are remunerated during the drought years. These programs are conditional on the declaration of the state of emergency of the residents of a Brazilian municipality.

¹⁵Episodes pointing to misuse of public money have taken place since public policies to fight drought began to be implemented. For instance, a member of the commission formed by the Emperor D. Pedro after the drought in 1877 noted that several commissioners responsible for managing funds allocated by the central government were incompetent and some were dishonest. However, it was notably during the drought of 1958 that corruption in the application of public resources became known in the public domain (Campos, 2015).

4.3. Conceptual Framework

The determinants of corruption have been studied extensively (Treisman, 2007; Dong and Torgler, 2013). Among these determinants, scholars have agreed that rent extraction is an increasing function of budget size (Brollo and Nannicini, 2012). For instance, an incumbent who faces fewer budget constraints can marginally extract more rents without penalizing her re-election prospects. Emergency aid increases the size of the budget since it behaves as unilateral transfers from higher tiers of government and ultimately increases corruption (Leeson and Sobel, 2008; Yamamura, 2014; Nikolova and Marinov, 2017). This section describes intuitively a framework of why aid relief due to drought could affect the level of corruption according to a political agency model (Persson and Tabellini, 2002; Besley and Burgess, 2002).

Consider a population in which citizens differ from each other exclusively by income. Elections follow a majority rule, and individuals decide their vote based on their policy preferences that rely on two dimensions: private consumption and public goods. Politicians are identical in all respects, from the perspective of voters, and voters coordinate their voting strategy based on the observable state of nature. Voters will only vote for incumbent re-election if retrospectively the voters' level of utility were at least higher or equal to a certain threshold, the reservation utility. Otherwise, voters will vote for the challenger candidate.

The incumbent policymaker faces a trade-off between rent extraction while in office and the probability of future re-election. Conceptually, rent extraction could refer to both funding for political parties and a diversion of resources for private use. The public budget can take the form of public goods provision but also the form of rents for politicians. It falls exclusively upon the incumbent to choose the ideal balance of the budget between public goods provision and rents.

In this framework, incumbent politicians face a maximization problem given that if they extract all rents from the public budget, they will not provide sufficient public goods to be re-elected. Incumbent politicians should decline some of the rents to guarantee their re-election chances. Thus, the optimal choice is to provide citizens precisely the level of public goods that equalizes with voter reservation utility. The money left in the budget turns into rents for private use or funding political parties.

Given the rent extraction and public goods provision equilibrium, how could intergovernmental emergency transfers for water scarcity affect the level of corruption? Bearing in mind that emergency aid coming from the federal government to municipalities can be admitted as a lump-sum inflow of public resources, the budget tightening may be reduced. This would imply a higher margin to distribute more resources between rent extraction and provision of public goods. The ultimate impact of emergency aid on the level of corruption would then be determined by the

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incumbents' preferences between re-election and expropriation of public resources for personal gain.

One can also expect that features such as fiscal capacity level, historical drought frequency, level of social capital, and political competition may change the level of rent extraction. Higher levels of fiscal capacity and social capital are related to the development of democratic institutions, which in turn enables individuals to identify corrupt behavior better and thus to prevent malfeasance. For instance, having free and independent media appears to reduce corruption as voters are better informed about government performance (Dong and Torgler, 2013). Drought frequency could be related to corruption. For instance, Yamamura (2014) shows that people who live in disaster-prone areas do so to benefit from disasters, although this finding is only significant for developed countries; it is less evident for developing countries. Political competition can influence corruption because it reinforces the political agency problem (Treisman, 2007). As incumbents are less likely to be re-elected in a highly competitive environment, they need to allocate most of the public budget for the public goods provision, leaving less room for rent extraction.

4.4. Data

The empirical analysis exploits Brazilian data at the municipal level regarding the level of corruption, federal government aid relief, incidence of droughts, economic and political features, mayor's characteristics, and municipal public finances. In the following subsections, I provide the sources and describe how I construct the variables. Summary statistics are shown in Table 4.1.

4.4.1. Corruption

I gather information about corruption at the municipal government level from Brollo et al. (2013). The dataset contains information about the share of public resources that have been diverted from its original purpose. The source comes from audit reports of an anti-corruption program conducted by the Brazilian Federal Government.

The federal agency *Controladoria Geral da União* (CGU), in 2003, began to examine in close detail how mayors were spending federal transfers. Auditors were assigned to personally visit local governments and inspect their public finances. They produced reports providing information about the exact amount of money that was being diverted. These reports were subsequently made publicly available on the internet and had been used in several empirical studies on corruption (Ferraz

Table 4.1.: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.
=1 if at least one corruption episode is reported	0.436	0.496	0	1
Amount involved in corruption episodes over the total amount audited	0.020	0.061	0.000	0.746
Amount involved in corruption episodes per capita	R\$ 7.59	34.48	R\$ 0.00	R\$ 679.75
Log of narrow corruption in level per capita	0.727	1.274	0	6.523
Log of broad corruption in level per capita	1.510	1.633	0	6.523
=1 if aid relief because of drought	0.213	0.41	0	1
Number of aid relief because of drought	0.250	0.517	0	3
Aridity Index (AI)	1.396	1.039	0.219	7.621
Trend component of AI	1.513	1.298	0.167	8.890
Cycle component of AI	-0.117	0.502	-3.755	1.648
Population	24,546	33691	837	425,772
Average income per capita	R\$ 565.60	1158.34	R\$ 5.21	R\$ 14,292.38
Average years of education	2.533	1.009	0.375	6.036
Share of employee working in the agriculture sector	17.468%	8.385	0.069%	58.275%
Share of employee working in the industry sector	5.781%	4.627	0.303%	37.148%
Share of employee working in the public sector	1.382%	0.810	0.129%	8.218%
Share of inhabitants living below the poverty line	55.171%	7.524	31.684%	70.384%
Political alignment with federal government	0.565	0.496	0	1
Herfindahl-Hirshman Index for political competition	0.518	0.115	0.000	0.786
Years of education of the mayor	13.149	4.152	1	17
Second term	0.238	0.426	0	1
Municipal budget debt	64.40%	0.651	0.10%	767.60%
Local tax collection per capita	R\$ 103.50	323.733	R\$ 0.00	R\$ 6761.78
Located within the boundary of the Amazon rainforest	0.132	0.339	0	1
Funded before the 20th century	0.220	0.414	0	1
Presence of local radio	0.416	0.493	0	1
Change in population (1991 - 2000)	0.068	1.505	-0.966	43.183
Change in income per capita (1991 - 2000)	1.824	2.977	-0.995	17.100
Change in level of education (1991 - 2000)	0.48	0.271	-0.266	2.101
Historical aid relief	0.407	0.492	0	1
Obs: 1,081				

and Finan, 2011; Timmons and Garfias, 2015; Brollo and Troiano, 2016).

Unquestionably the most important feature of this program for empirical analysis is the way in which it was designed. A lottery was the selection procedure used to choose the municipalities that would be audited. The randomness characteristic of a lottery avoids concerns about the data-generation process that would bias empirical investigations. That is, the sample accurately contains non-corrupt municipalities as the audit reports give a comprehensive picture of how the incumbent implemented local government spending. In this way, the municipalities found to be without any cases of corruption are an ideal control group for the corrupt municipalities. This is unlikely to be achieved in studies where corruption measurement is based on media news or lawsuits since the control group can be municipalities where graft and bribery were simply not disclosed. It is interesting that the auditors do not limit their inspections to only the mayor; instead, they investigated the expenditures of federally-funded transfers since 1997. During the period of analysis, 29 lotteries collected between 50 and 60 cities each between the years 2003 and 2010, which resulted in a total of 1,600 municipalities that had their finances investigated; this led to an audit of 2,293 terms of mayors. Another significant advantage of these data

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is that they are built as an objective measure of corruption instead of a perception-based measure.

The Brollo et al. (2013) dataset contains two definitions of corruption. I focus on the narrow definition of corruption because it is the measure that explicitly reports irregularities that are undoubtedly serious cases of dishonest behavior. The broad definition of corruption also includes wrongdoings that could also be interpreted as mismanagement instead of unethical behavior. I create a proxy of it by multiplying the share of narrow corruption with the non-mandatory federal transfers.¹⁶ Thus, the outcome variable of this study is the monetary value of theft per capita during the administration of a particular mayor. Due to the skewness of the variable and because some municipalities did not present cases of corruption, for the empirical analysis I add the value of 1 and transform it into its logarithm.

For the sake of consistency, the analysis focuses only on the audit outcomes that have led the auditors to examine the mayor's expenditures over the duration of her entire term. The reason for this is that it does not make sense to compare levels of corruption among municipalities where public spending was inspected for only one year of the mayor being in office with municipalities that had their public spending inspected during all four years, for instance. Additionally, municipalities that had their public expenditures audited in three or fewer years of the mayor's term are those that had their audit reports released during the mayor's term. This is problematic because a shock of information, as in the case of revealed corruption scandals, affects the behavior of both voters and politicians; this reinforces the inadequacy of making comparisons with municipalities that did not have an audit report released during the mayor's term.¹⁷

4.4.2. Federal Aid Relief

Since the 1960s, the Brazilian government has used a state of emergency as a procedure to allocate emergency aid against drought. (Nelson and Finan, 2009).¹⁸ Following the president's recognition of the need to rescue a particular municipality, the federal government publishes an ordinance that triggers several types of benefit to the affected locality.

¹⁶I do not use FPM transfers to compute the amount of corruption because most audits do not scrutinize this type of transfer. See Brollo et al. (2013) for more specific details.

¹⁷For instance, revealed corruption causes voters to punish the incumbent mayor in subsequent elections (Ferraz and Finan, 2011), leads incumbents to reduce municipal property tax (Timmons and Garfias, 2015), and prompts political parties select more educated candidates to run for election (Cavalcanti et al., 2016).

¹⁸State of emergency because of drought was first proposed in 1962 by Celso Furtado at the time chair of SUDENE (Superintendência do Desenvolvimento do Nordeste), in Resolution No: 453.

These benefits include monetary transfers, disclaimer of legal bureaucratic procedures for public spending, investment in infrastructure, distribution of water and food, and sending the army to affected areas to help with basic needs (Campos, 2015). The variety of forms of help makes aid relief challenging to express in monetary terms.

For simplicity, I proxy aid relief as the recognition of a state of emergency or public calamity because of drought by the federal government. The main variable of interest is computed as the amount of aid relief for drought during the term of the mayor of a given city. I cover both definitions of meteorological and hydrological droughts according to Dai (2011).¹⁹ The data are gathered from *Sistema Integrado de Informações sobre Desastres Naturais* (S2ID) for the period of 1996 to 2008.

4.4.3. Drought

I define the level of water scarcity as a measure of aridity of a municipality. For this purpose, I construct the aridity index; this index is similar to the ones used in studies explicitly focused on the hydrology of lands (Wolfe, 1997; Arora, 2002) and calculates the monthly ratio between the amount of rain and the amount of evaporation in a particular municipality during a certain period.

$$AI_{t,m} = \frac{\sum_1^t Evaporation_{t,m}}{\sum_1^t Rainfall_{t,m}}.$$

The subscript t represents the monthly observations of each weather variable, and the subscript m represents the municipality. Since a mayor's term lasts four years, I use the corresponding number of months ($t = 48$). Higher values of the index indicate that the net retention of water is deficient.

The data about monthly rainfall and evaporation are collected from 280 ground weather stations from INMET, the Brazilian Institute of Meteorology, and spatially interpolated among municipalities.²⁰ The coverage of each weather station ranges from 1961 to 2008.²¹

¹⁹I define aid relief for hydrological droughts when the recognition of a state of emergency is because of *secas* and aid relief for meteorological droughts when the recognition of a state of emergency is because of *estiagens*.

²⁰We compute the distance between the municipality's centroid and the closest ground station of each quadrant. The inverse square of such distance is used as a weight to proxy weather variables of each municipality.

²¹Using data from these stations is advantageous because they provide objective weather information. Rainfall levels are recorded with a pluviograph, and levels of evaporation are recorded from a Piche evaporimeter. Objective weather data are superior to those gathered from satellites data because satellites do not directly measure rainfall and evaporation, but instead make inferences (Dell et al., 2014).

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The reason for using this index is because droughts in Brazil are commonly associated with the process of desertification of areas, and the index relies on two indicators of water availability that depicts the net flow of water of a given region. This is important since it describes the capacity of water storage. Using a unique variation of rainfall provides incomplete information about the soil moisture balance.²² Nevertheless, I implement other proxies for drought shocks using variations of rainfall and a different source of precipitation data in the robustness checks section.

To ensure that the drought measure does not merely capture a long-term level of corruption of a municipality that can be correlated with institutional factors, the identification strategy exploits the quasi-random nature of the cycle component of *aridity index*. I decompose the *aridity index* into trend and cycle components following the procedure proposed by Hamilton (2017). That is, I predict the trend component using a very simple forecast using the four most recent values of *aridity index* for each municipality and for each point of time.²³ The cycle component is merely the difference between the actual value of the *aridity index* and the trend component. This procedure guarantees the random walk characteristic of the *aridity index* cycle component, which is presumably exogenous to any structural element. Given its random feature, the cycle component of the *aridity index* is used as an instrumental variable in the empirical analysis.

Figure 4.1 depicts histograms of the trend and cyclical components. Note that the trend component smoothly tracks the level *aridity index* and that the cyclical component is uniformly distributed around zero.

4.4.4. Municipality Characteristics

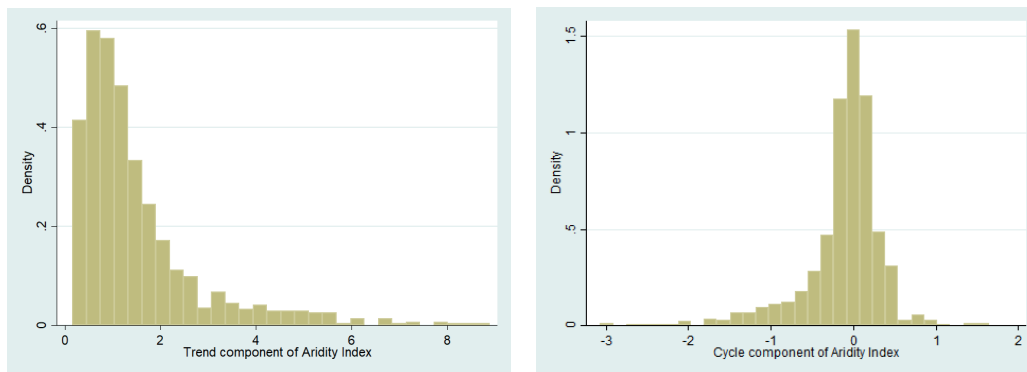
I construct variables that account for the characteristics of the municipalities from 1991 Census of the *Instituto Brasileiro de Geografia e Estatística* (IBGE). The reason is to control for structural features that could determine the long-run pattern of corruption level of a municipality. The control variables are population, average income per capita, the share of people below the poverty line²⁴, average years of schooling, and the proportion of employees in the agriculture, industry, and public sectors.²⁵ I also gather information related to the age of a municipality, whether the

²²For interested readers, Dai (2011) demonstrates why one should not use total precipitation alone to measure changes in aridity or drought.

²³For example, to estimate the trend component of the *aridity index* of a municipality in 2004, I use the *aridity index* of that municipality in the years 2000, 1996, 1992, and 1988.

²⁴People living with less than \$1.90 per day based on 2011 prices.

²⁵Using variables from the 2000 Census provide qualitatively and statistically similar results. However, the use of the 2000 Census would result in the control variables not being prior to the dependent variable, since some cases of corruption date back to 1997.



(a) Trend component

(b) Cycle component

The figures show the histograms of the trend component of *aridity index* and the cycle component of *aridity index*, respectively. Trend component smoothly tracks the level of aridity while the cycle component is uniformly distributed around zero.

Figure 4.1.: Decomposing the *aridity index* in trend and cycle components

city is located within the boundary of the Amazon rainforest, and whether the city is located within the boundary of the semi-arid region. I collect data from the 2000 Census to calculate the percentage change in the level of population, income, and education of a municipality compared to the 1991 Census. Finally, I extract data on whether a municipality has a local radio from the 2006 municipality survey *Perfil dos Municípios Brasileiros: Cultura*.

4.4.5. Political Characteristics

I compute variables that depict political features of a municipality. For an index of political competition, I use the outcomes of the previous mayoral election to construct the Herfindahl-Hirschman Index (Hirschman, 1964). This index provides a measure of how dispersed the votes were among the candidates for mayor in the prior election. Additionally, I construct a dummy variable to note whether a municipality is politically aligned with the federal government.²⁶ Regarding individual characteristics of the incumbent mayors, I build variables that stand for the number of years of education, and whether the mayor is in the second term in office.²⁷ All of these variables are constructed using data from *Tribunal Superior Eleitoral* (TSE).

²⁶We follow the definition of a presidential coalition of parties by Brollo and Nannicini (2012).

²⁷Re-election has been permitted since 1997 (Emenda Constitucional n. 16, 4th of June) but is still limited to two terms.

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4.4.6. Municipal Public Finance

I extract data regarding municipal government budget from *FINBRA*. More specifically, I calculate non-mandatory federal transfers and mandatory federal transfers (FPM) to a municipality during the entire term of each mayor. Furthermore, I calculate a measure of indebtedness of the municipal public finance and the total revenue from local taxes per capita. Monetary values are normalized to 2008 price levels.

4.5. Empirical Strategy

The main objective of this analysis is to test whether disaster relief for droughts has an impact on the level of corruption. More precisely, I investigate whether the number of emergency state declarations due to drought increases the amount of graft found by auditors about federal transfers at the city level.

Hence, I first perform a naive analysis by estimating the following model:

$$C_{i,t} = \beta_1 + \beta_2 A_{i,t} + X_{i,t} \beta_3 + y_t + z_s + \epsilon_{i,t} \quad (4.1)$$

where C represents the amount of corruption per capita of the municipality i at the term t . The coefficient of interest is β which represents the average association of the number of state of emergency recognition due to drought during the full term of the mayor. $X_{i,t}$ represents a matrix with the characteristics of the municipalities and the attributes of the mayor. The terms y_t and z_s represent state and year fixed effects. The ϵ is the error term.

As 66% of the municipalities in my sample do not show evidence of corruption, the dependent variable is left-censored at zero. To accommodate the characteristics of the data on the econometric method, we estimate a Tobit model as the main approach.

Isolating the causal effect of humanitarian aid on the level of corruption is challenging because there are several determinants of intergovernmental transfers. For example, the interested coefficient extracted from the above econometric specification may suffer from bias due to cultural, institutional, and political characteristics in which it is not observable.

To overcome problems caused by bias in the allocation of humanitarian aid, I adopted as the main econometric strategy the method of instrumental variable regression. I exploit the unexpected nature of weather conditions to isolate exogenous variation in the amount of aid relief received by each municipality. For this, the corresponding first stage is given by

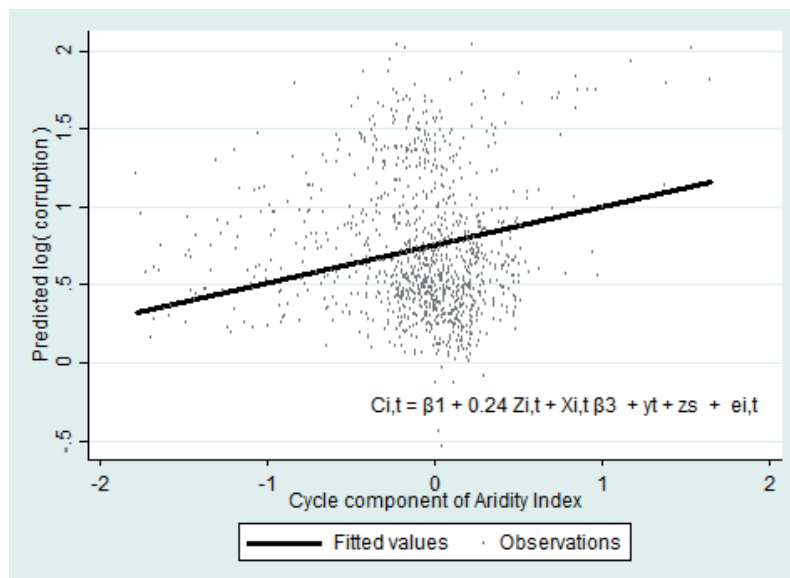
$$A_{i,t} = \beta_1^F + \beta_2^F Z_{i,t} + X_{i,t} \beta_3^F + y_t + z_s + \epsilon_{i,t}, \quad (4.2)$$

where Z represents the cycle component of the *aridity index* of the municipality during the full term of the mayor. The underlying assumption here is that deviations from the aridity trend are exogenous to the incumbent mayor's honesty and the quality of institutions, and consequently Z is not correlated with the error term.

4.6. Results

4.6.1. Main Results

In this section, I report the main results. As a preliminary analysis, I plot in Figure 4.2 the reduced form relationship between the level of corruption and the cycle component of the *aridity index* in a two-way scatter with a regression line fit through it. The figure shows the first evidence suggesting that levels of water scarcity and levels of corruption per capita are intrinsically positively correlated. Holding municipalities and mayor characteristics constant, a one standard deviation increase in the cycle component of the *aridity index* increases around 12 percentage points on the level of narrow corruption per capita.



The figure shows the reduced form relationship between the level of corruption and the cycle component of the aridity index in a two-way scatter with a regression line fit through it.

Figure 4.2.: Reduced form analysis

In Table 4.2, I present the results of the econometric strategy. In Panel A, I present

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the simple OLS and Tobit specifications. Although these estimations cannot be defined as causal, they serve as a useful benchmark for the IV regressions. In Panel B, I present the results of the first stages of the instrumental variables' regressions, and in Panel C, I present the results of the second stages. In the first columns of each panel, I show the results using an OLS estimation. In the subsequent columns of each panel, I implement a Tobit approach. In each panel, I introduce the control variables systematically. In columns (1) to (2), (7) to (8), and (13) to (14), no control is included. In columns (3), (9), and (15), I include the control variables that account for the economic and social structure, such as population, average income, average years of education, share of employees working in the agriculture sector, share of employees working in the industrial sector, share of employees working in the public sector, and share of inhabitants living below the poverty line. In columns (4), (10), and (16), I include control variables that account for political characteristics of a municipality, such as political competition and political alignment with the federal government. In columns (5), (11), and (17), I include control variables that account for the characteristics of the incumbent mayor, such as years of education, gender, whether the incumbent is in the second term and the level of fiscal capacity. In columns (6), (12), and (18), I include control variables such as whether the municipality is located within the boundary of the Amazon rainforest, whether the municipality was founded before the 20th century, and whether the municipality has a local radio.

Going to the interpretation of the results, the coefficients estimated in Panel A suggest a weak association between disaster relief and level of corruption. The coefficients range from -0.010 to 0.045 , but none of them is statistically significant at the usual standards. If the distribution of aid relief due to droughts were exogenous of any other not observable characteristic, I would conclude that such a policy does not affect the level of corruption in a municipality.

However, the results of instrumental regressions suggest that there may be the omitted variables may exert an influence on the latter specifications.²⁸ In the columns of Panel C, the coefficient estimated using two-stage least squares in column (13) is 1.3 and statistically significant at the 10% level. The coefficients of columns (14) to (18) are extracted from the Tobit model that uses maximum likelihood estimation. This econometric approach considers that the dependent variable is left-censored in zero given there are municipalities that did not present any case of corruption. In this case, the coefficients range from 3.2 to 3.4, and they are statistically significant. The first stages of the IV regressions depicted in Panel B suggest that the proposed

²⁸Several studies recognize that public sector corruption is associated with institutional factors that are often omitted or difficult to measure (Escaleras and Register, 2015; Nikolova and Marinov, 2017).

Table 4.2.: Aid relief for droughts and level of corruption

Panel A: OLS and Tobit Regressions						
	(1)	(2)	(3)	(4)	(5)	(6)
Aid Relief	0.001	-0.010	0.004	0.021	0.045	0.033
	(0.080)	(0.211)	(0.212)	(0.210)	(0.212)	(0.214)
R-squared	0.100					
Pseudo R-squared		0.0495	0.0542	0.0553	0.0562	0.0572
Panel B: First Stage of IV Regressions						
	(7)	(8)	(9)	(10)	(11)	(12)
Cycle Component of Aridity Index	0.188***	0.195***	0.178***	0.177***	0.180***	0.176***
	(0.026)	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)
Anderson-Rubin (AR) Test P-value	0.0470	0.0278	0.0335	0.0311	0.0347	0.0376
Wald Test P-value	0.0953	0.0460	0.0562	0.0534	0.0567	0.0611
Panel C: Second Stage of IV Regressions						
	(13)	(14)	(15)	(16)	(17)	(18)
Aid Relief	1.306*	3.228**	3.423*	3.470*	3.384*	3.394*
	(0.780)	(1.618)	(1.793)	(1.797)	(1.772)	(1.809)
Method	OLS	Tobit	Tobit	Tobit	Tobit	Tobit
Observations	1,081	1,081	1,081	1,081	1,081	1,081
Municipal Characteristics			✓	✓	✓	✓
Political Features				✓	✓	✓
Incumbent Characteristics					✓	✓
Others						✓

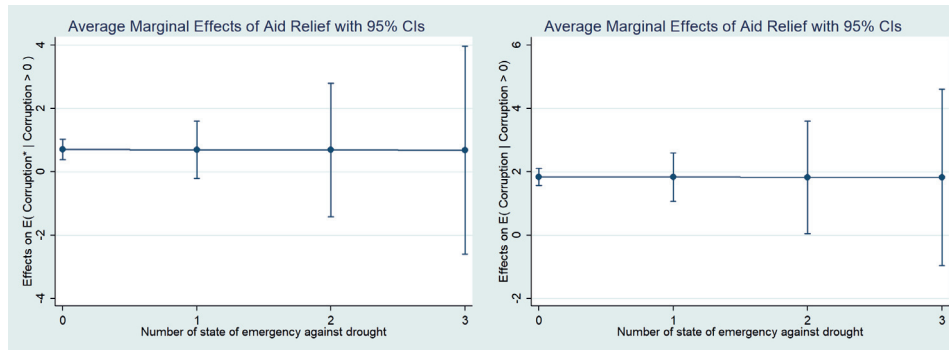
Notes: The dependent variable is the logarithm of monetary value of narrow corruption per capita. Trend component of *aridity index*, states dummies, and terms fixed effects included in all specifications. Robust standard error in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

instrumental variable works well. The Wald Tests reject the hypothesis that the instrumental variable is irrelevant at 90% level of confidence. The Anderson-Rubin tests readily reject the null hypothesis that the coefficient of the endogenous regressor in the structural equation is jointly equal to zero at the 95% level of confidence and indicate that the endogenous regressor is relevant.

To put the results in perspective, I use the estimate drawn in column (18) of Panel C to calculate in Figure 4.3 the margin effects separately for municipalities that were recognized once, twice, and three times in a state of emergency. The margin effect does not vary at each number of state of emergency recognition. Figure 4.3a measures the overall effect of aid relief on corruption for all observations and Figure 4.3b measures the effect of aid relief on corruption for corrupted municipalities. The average partial effects estimated indicates that the amount of narrow corruption per capita increase approximately by 70% for all observations and by 183% for cor-

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rupted municipalities, respectively. Given the average level of narrow corruption in Brazilian municipalities, the estimate suggests that disaster relief against droughts translates to a rise in stolen public money throughout the whole mayor's term of approximately R\$ 5.00 and R\$ 41.00 per capita, respectively.²⁹



(a) For all observations

(b) For corrupt municipalities

Note: The y-axis is the partial effect at the average of aid relief on corruption, and the x-axis represents the quantity of aid relief granted by a municipality during a mayor's 4-year term.

Figure 4.3.: Partial effects

A possible explanation for the sharp difference in the positive evidence in the relationship between aid relief for droughts and the levels of corruption extracted from the IV regressions and the estimates obtained from the simple OLS and Tobit methods may be due to the reverse causality in the naive estimates. The Brazilian central government could decide not to recognize a state of emergency due to droughts in municipalities that historically have high levels of corruption, anticipating that a significant part of this humanitarian aid would be diverted for purposes other than its original aim. Likewise, municipalities that historically have more honest and competent mayors may receive more aid relief and implement it appropriately, which would again attenuate the OLS and Tobit coefficients on aid relief granted.

Although the findings above are, to the best of my knowledge, the first evidence of the effect of disaster relief for water scarcity on corruption, it stands in the similar direction that previous studies found relative to other kinds of natural disasters. For instance, in a close study to our, Nikolova and Marinov (2017) show that flood-related transfers in Bulgaria are associated with spending infringements identified in publicly available audit reports. In the context of U.S., Leeson and Sobel (2008) show that the number of corruption-related crime convictions increased in states that received FEMA disaster relief. Analyzing as a reduced form the impact of

²⁹The average corruption of all observations in the sample is R\$ 7.59 per capita. The average corruption only for corrupt municipalities is R\$ 22.40 per capita. Based on 2008 prices.

natural disaster incidence over corruption, the study of Yamamura (2014) found a positive effect in both developing and developed countries. Investigating if victims of droughts in Peru are more likely to bribe public officials, also as a reduced form, Hunt (2007) finds no significant effect.

4.6.2. Robustness Checks

In this section, I test the main results using alternative variables. Table 4.3 shows the results of the Tobit model for IV specification using different measures of corruption, aid relief, testing the exclusion restriction, and using alternative instrumental variables. The table also presents the p-value of the Anderson-Rubin (AR) test and Wald test for weak instruments with only one endogenous regressor, and additionally the p-value of conditional likelihood ratio (CLR) test for weak instruments when there are more than one endogenous regressor.

In column (1) of Panel A, I use the definition of broad corruption as the dependent variable rather than narrow corruption according to Brollo et al. (2013). The broad definition of corruption also includes practices that could be interpreted as a matter of mayor mismanagement instead of explicitly fraudulent practices. The result remains very similar to the main analysis. In column (2), I use a different proxy to calculate the amount of narrow corruption. Instead of using federal transfers throughout the mayor's term, I exclude the transfers that were made during electoral years. It is documented that central governments often prioritize municipalities that are politically aligned with more public resources in electoral years to influence electoral results (Garrett and Sobel, 2003; Brollo and Nannicini, 2012). Thus, the definition of corruption by construction could incorporate the noise that comes from the political business cycle (Nordhaus, 1975). Nevertheless, the result suggests that this is not the case since the coefficient does not change. Moreover, to ensure that the results are not capturing a mechanical effect of federal transfers to municipalities, I use the federal transfers alone in column (3) as a dependent variable, without taking into account the share of corruption identified by the auditors. The coefficient confirms that the result is robust about this hypothesis since the estimated coefficient is close to zero and statistically insignificant. In column (4), I use as a dependent variable a dummy that takes the value of 1 if an episode of narrow corruption was found by auditors and 0 otherwise. In this case, the coefficient is not statistically significant. A similar result is found in column (5) if I use as the dependent variable the fraction of narrow corruption episodes over the total amount audited. These findings suggest that disaster relief against drought does not turn an honest mayor into a corrupt one but only opens an opportunity for dishonest mayors to steal more public resources.

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Table 4.3.: Robustness checks

Panel A: Alternative Measures of Corruption					
	Broad Corruption (1)	Excluding Election's Year (2)	Total Audited (3)	Dummy Narrow (4)	Fraction Narrow (5)
	2.997** (1.451)	3.065* (1.711)	0.510 (0.318)	0.486 (0.525)	0.084 (0.076)
AR Test P-value	0.0150	0.0495	0.0735	0.3219	0.2587
Wald Test P-value	0.0367	0.0737	0.1016	0.3350	0.2739
Panel B: Alternative Measures of Aid Relief					
	Dummy of Aid Relief (6)	Dummy of Severe Droughts (7)	Dummy of Softy Droughts (8)	Historical (9)	Semi Arid (10)
	5.262* (2.899)	25.509 (19.836)	3.903* (2.140)	1.560 (3.765)	1.339 (1.684)
AR Test P-value	0.0373	0.0365	0.0384	0.0378	0.0369
Wald Test P-value	0.0697	0.2018	0.0685	0.0738	0.0419
Panel C: Exclusion Restrictions					
	Change in Population (a) (16)	Change in Income (b) (17)	Change in Education (c) (18)	Changes in all (a + b + c) (19)	Political Alignment (20)
	3.386* (1.802)	3.368* (1.802)	3.430* (1.817)	3.408* (1.812)	3.679** (1.835)
AR Test P-value	0.0368	0.0380	0.0356	0.0365	0.0500
Wald Test P-value	0.0607	0.0622	0.0595	0.0605	0.0451
CLR Test P-value					0.0250
Panel D: Alternative Measures of Drought					
	Aridity Index of 24 months (11)	Aridity Index of 12 months (12)	Zscore of Aridity Index (13)	Zscore of Rainfall (INMET) (14)	Zscore of Rainfall (NOAA) (15)
	2.458* (1.368)	2.475** (1.254)	2.478 (6.658)	4.394 (12.032)	0.510 (1.401)
AR Test P-value	0.0991	0.2848	0.6926	0.6670	0.7150
Wald Test P-value	0.0734	0.0492	0.6984	0.7071	0.7382
CLR Test P-value	0.0564	0.0425			

Notes: States and terms fixed effects included in all specifications. Controls variables include trend component of *aridity index*, *population*, *average income*, *share of graduated*, *share of employee working in the agriculture sector*, *share of employee working in the industry sector*, *share of employee working in the public sector*, *share of inhabitants living below the poverty line*, *political competition*, *political alignment with the federal government*, *years of education of mayor*, *gender*, *a dummy whether the incumbent is in the second term*, *level of fiscal capacity of the municipal government*, *whether the municipality is located within the boundary of the Amazon rainforest*, *whether the municipality was founded before the 20th century*, and *whether the municipality has a local radio*. Standard error clustered at municipality level in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

Regarding different measures of aid relief, in column (6) of Panel B, I use as an explanatory variable a dummy that takes the value of 1 if there has been at least one recognition of the state of emergency and 0 otherwise. We observe that the

coefficient increases, to some extent. This result is reasonable because it depicts the average difference between municipalities that were granted by humanitarian aid from those municipalities that were not. Thus, as this definition of aid relief does not consider the number of times that a municipality has been recognized in a state of emergency, the coefficient increases to accommodate the municipalities that were granted more than once. In columns (7) and (8), I disentangle the recognition of state emergency between hydrological droughts and meteorological droughts, respectively. The difference between them is that the hydrological drought refers to a prolonged lack of rainfall that has a more dramatic impact on the livelihoods of the citizens and the meteorological drought mainly affects crops. As each type of drought leads to different forms of government aid, I test whether the results are sensitive to this difference.³⁰ In fact, the results of such differentiation indicate that aid relief for hydrological droughts has a low precision in the coefficient of interest, while aid relief for meteorological droughts seems to drive all the effect found in the main analysis. To confirm that the effect of disaster relief on corruption comes from the recognition of a state of emergency against drought during the years in which the mayor is in his term, I propose two falsification tests. First, I use as an alternative measure of disaster relief whether a municipality was recognized in a state of emergency during the years of Brazilian military dictatorship (1964–1986). Second, I use an indicator as an endogenous regressor of whether a municipality is located within the boundary of the semi-arid region. In both cases, the coefficients are not statistically significant, as described in columns (9) and (10), respectively.

The instrumental variable strategy would be valid provided that the cycle component of *aridity index* does not affect corruption, except through the allocation of disaster relief. In Panel C, I test for such an exclusion restriction. Intuitively, natural disasters can trigger massive migration, which would, in turn, reduce the population size, affect the income level of the region, and impact the level of human capital. Thus, if the hypothesis that the droughts impact the level of corruption of a municipality through mechanisms other than public fund windfalls, it would presumably be through the change of these characteristics mentioned above. Thus, if I add such changes in characteristics in the empirical analysis as control variables and the disaster relief against drought estimation remains stable, this finding would advocate against the direct influence of the cyclical component of the *aridity index* on corruption. In columns (16) to (18), I introduce systematically the percentage change in population, income, and education of each municipality using Census of 1991 and 2000, respectively. In column (19) I introduce these control variables simultaneously. Even so, the interested coefficient of disaster relief remains unchanged.

³⁰Hydrological droughts lead to the distribution of basic survival aid, and meteorological droughts often result in financial support for farmers (Campos, 2015).

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Furthermore, to ensure that the effect is actually given through disaster relief, I also incorporate as an additional instrumental variable the interaction between party alignment and the cyclical component of the aridity index. Although party alignment is not an exogenous feature, it is well documented that political alignment is a strong determinant of intergovernmental transfers (Arulampalam et al., 2009; Brollo and Nannicini, 2012). By doing so, I forcefully describe that the impact found in the main analysis is through the proposed mechanism. Again, in column (20), the estimated coefficient remains stable.

I also test the results of the main analysis using different instrumental variables. More specifically, I divide the variation of *aridity index* into the term of the mayor. In column (11), I compute the ratio between evaporation and rainfall for every 24 months for each municipality and do so in column (12) for every 12 months, and I introduce them to capture variation within the mayor's term. Although the coefficient of interest decreases slightly, it remains positive and statistically significant in both cases. Alternatively, in column (13), I use as an instrumental variable the Zscore of the *aridity index*.³¹ In columns (14) and (15), I use as an instrumental variable the rainfall from INMET and NOAA's PRECipitation REConstruction Dataset (Chen et al., 2002), respectively. In each alternative of the instrumental variables using Zscore, the first stage of regression becomes weaker, impairing the precision of the coefficients of interest. These results suggest that the cycle component of the *aridity index* performs better than the indicated alternatives.

4.6.3. Heterogeneous Analysis

This section explores the heterogeneity of the impact of aid relief because of droughts on the level of corruption regarding characteristics that may influence the level of rent extraction. In the main empirical specification, I systematically introduce an interaction between various dimensions and aid relief and use the interaction between the heterogeneous variable and the cycle component of *aridity index* as an additional instrument. I am interested in characteristics that may be related to the electoral contest, individual characteristics of the mayor, social capital, and level of fiscal capacity of a municipality. These characteristics have been shown in previous works that are associated with the level of corruption (Treisman, 2007; Dong and Torgler, 2013; Yamamura, 2014). The coefficients and the p-values of the Anderson-Rubin (AR) test and Wald test for weak instruments are presented in Table 4.4. The estimates depict non-significant findings. The interpretation of the coefficients should

³¹Specifically, the Zscore of a weather variable is the difference between the current weather measure and the historical average of such measure in the municipality during the same years divided by the municipality's historical standard deviation.

be taken only as suggestive and by no means intends to describe an impact inference or causal relation.

Table 4.4.: Heterogeneous analysis

	<i>Electoral Features</i>			<i>Social Capital</i>	
	Political Competition (1)	Incumbent Education (2)	Second Term (3)	Media (4)	Level of Education (5)
Aid Relief	4.069** (1.944)	3.384* (2.035)	3.361 (3.223)	3.526* (1.875)	7.012 (8.090)
x Aid Relief	-11.036 (9.792)	-0.009 (0.731)	-0.181 (7.967)	-3.000 (4.171)	4.541 (8.783)
AR Test P-value	0.0261	0.1149	0.1088	0.1019	0.1157
Wald Test P-value	0.0776	0.1700	0.1509	0.1653	0.3136

	<i>Past Droughts</i>		<i>Fiscal Capacity</i>		
	Historical Aid Relief (6)	Mean of Aridity Index (7)	Indebtedness (8)	Local Tax Revenues (9)	Public Sector (10)
Aid Relief	3.348 (3.573)	8.118 (16.535)	3.200* (1.777)	5.003 (3.720)	2.812 (1.833)
x Aid Relief	1.632 (5.734)	0.337 (6.521)	-2.388 (1.668)	-0.010 (0.017)	-1.272 (1.391)
AR Test P-value	0.1260	0.3172	0.0946	0.1041	0.1022
Wald Test P-value	0.2062	0.6286	0.1567	0.2787	0.1022

Notes: The dependent variable is the logarithm of monetary value of narrow corruption. States and terms fixed effects included in all specifications. Controls variables include trend component of *aridity index*, *population*, *average income*, *share of graduated*, *share of employee working in the agriculture sector*, *share of employee working in the industry sector*, *share of employee working in the public sector*, *share of inhabitants living below the poverty line*, *political competition*, *political alignment with the federal government*, *years of education of mayor*, *gender*, *a dummy whether the incumbent is in the second term*, *level of fiscal capacity of the municipal government*, *whether the municipality is located within the boundary of the Amazon rainforest*, *whether the municipality was founded before the 20th century*, and *whether the municipality has a local radio*. Standard error clustered at municipality level in parentheses. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.10.

In the first three columns, I study dimensions related to electoral features. Column (1) shows the interaction between aid relief and the level of political competition. The proxy used for political competition is the Herfindahl-Hirschman Index. The result shows that municipalities that have low political competition are those that were found to have higher levels of corruption than those that have high political competition. In column (2), I interact with the years of education of the mayors. The results do not suggest a meaningful difference in levels of corruption, regardless of whether the incumbent has high or low human capital. In column (3), I use a dummy that differentiates the sample between mayors who are in the first term and mayor who are lame ducks. It seems that the level of corruption is higher when mayors are in the first term, but the very high standard error indicates that the co-

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efficient points towards zero. If it were true that mayors in the second term are less corrupt, it would contrast the conclusions of Ferraz and Finan (2011), who find less corruption in municipalities where mayors can seek re-election. However, while Ferraz and Finan (2011) study the level of corruption regardless of the nature of federally-transferred funds, this study focuses exclusively on the mechanism of aid relief due to drought. The unexpected characteristic of aid relief combined with low transparency can make corrupt activities particularly tempting compared to waiting to capture other political rents later, and this temptation can be softened if the mayor already has long experience in governing the municipal budget.

In columns (4) and (5), I use as a proxy for social capital the presence of a local radio station in the municipality and the average years of education of the inhabitants of the municipality. Citizens who are exposed to information from local radios are likely to be more informed of the mayor's actions and better able to keep the mayor accountable for her actions. The level of education of the inhabitants of a city can describe the capacity to assimilate social capital knowledge. The results show an inverse relation with the level of social capital.

Subsequently, I study heterogeneity regarding municipalities that have historically faced droughts and that have received aid to fight against them. In column (6), I use a dummy variable that takes a value of one if during the military dictatorship the municipality was recognized as being in a state of emergency because of drought and is zero otherwise. In column (7), I interact with the average of *aridity index* calculated between 1961 to 1996. In the two columns described above, the estimates produce an incredibly high standard error concerning the estimates, suggesting that the coefficients found are close to zero. If the positive coefficients found were not randomly determined, this would suggest that the municipalities that had previous experience with aid relief and droughts are those municipalities that have the highest extraction of rents.

Finally, columns (8) to (10) display the results of the heterogeneous exercise regarding municipal characteristics that express fiscal and administrative state capacity. In column (8), I interact with a proxy that depicts the level of budget debt of a municipality. It seems that municipalities with better financing conditions are those that show a larger effect on the level of corruption than those municipalities with higher fiscal constraint. Similar findings are for municipalities that have greater local tax collection, in column (9). As the size of the public sector correlates with development of the state and the size of the city is associated with the capacity of the state to provide public goods, the result in column (10) suggests that the negative impact of aid relief on governance is stronger in more developed municipalities. These findings are consistent with studies that investigate corruption in countries making the transition to a market economy. In a within-country analysis

in the context of Bulgaria, Nikolova and Marinov (2017) show that the impact of flood assistance on corruption is stronger in municipalities that are more economically developed. Studying also in a within-country analysis but in the context of China, Dong and Torgler (2013) identify a positive relationship between corruption and economic development of the provinces. Nevertheless, it is important to note that there is robust evidence from cross-country analysis showing that economic development is associated with less corruption (Mauro, 1995; Treisman, 2007).

4.7. Concluding Remarks

This paper has assessed whether aid relief for droughts increases the level of corruption at the municipality level in a context of a developing country. Using an instrumental variable strategy with Tobit method this study shows that an additional recognition of a state of emergency indeed increases the levels of corruption. The estimations show that the margin effects of recognitions of the state of emergency lead to an increase of corruption of R\$ 5.00 per capita for all observations and R\$ 41.00 per capita for corrupted municipalities. This finding is alarming because it suggests that there is a misappropriation of emergency relief and this prevents those who need it most from being reached. This paper's findings also suggest that aid relief provision does not imply that honest mayors will begin performing unethically, but the aid relief against drought opens a window of opportunity for dishonest mayors to steal more public resources.

This study has shortcomings that cannot be overlooked. One limitation is due to the arguably weak inference power of the instrument used, which leads to the standard error of the variable of interest becoming high. For this reason, the analysis reveals limited statistical power. However, the usage of other instrumental variables that are commonly used in similar studies of droughts does not perform better than the proposed cycle component of the *aridity index*. Another caveat is that the study does not explicitly observe the amount of corruption, but rather is based on a proxy of how much was stolen. Finally, although Brazil is a vast country in which municipalities are quite heterogeneous regarding social, economic and environmental characteristics, I still cannot say much about the external validity of this study.

Nonetheless, from a policy implication perspective, the results of this study call for a reassessment of how emergency policies are designed to meet those who need them. Although drought programs may invite rent seekers to divert funds, the potential for corruption does not depend on drought policies themselves, but rather on the way these policies are implemented.

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5. Conclusion

Strong institutions are crucial in explaining the difference in the development of nations. Democratic systems together with the rule of law and state capacity are accepted as a pathway towards vigorous institutions (Acemoglu and Robinson, 2013; Fukuyama, 2014). This could be reached by gradually improving the efficiency of governments to promote social protection. The central mechanism by which democracy is good for modern, well-governed states is by electoral accountability. Several models in the political economy literature have focused on this aspect to comprehend agents' behavior (Besley and Coate, 1997; Ferejohn, 1986; Osborne and Slivinski, 1996). This thesis contributes to the literature by shedding light on how political parties, voters, and governments, in a context of regular elections, react when struck by shocks as corruption scandals, droughts, and windfall revenues. The lessons derived from each chapter could serve as benchmarks for enhancing models of electoral accountability and provide insights on which conditions democracy produces either intended or undesired outcomes. Withstanding theoretical models with more realistic assumptions could help these models improve their performance in predicting how shocks change agents' behavior and, ultimately, how they impact the economy.

Political parties

An essential function for electing high-quality public managers is the supply of politicians; however, little has been studied about the strategic behavior of the parties when selecting candidates. Chapter 2 provides some of the newest empirical evidence of the effect of popularity shocks, such as information about the corruption level of government, in the selection of political candidates by parties. By exploring Brazil's city council election data for 2004 and 2008, the study found that an unexpected positive shock over government honesty hurts the quality of candidates put forward by the coalition in the subsequent election. Conversely, the party selects better candidates when a corruption scandal is revealed.

The study also brings parallel lessons about the role of the parties in the selection of candidates. The first lesson is related to the party organization. The study suggests that the results found are, in fact, related to the demand of candidates rather than to

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their supply of candidates because the impact of a political scandal on the characteristics of the selected candidates is stronger in more centralized parties. Centralized parties have more control over the selection process than the decentralized parties since centralized parties usually intervene in local directories by dissolving them and putting in place a temporary committee that follows the instructions of party leaders (Guarnieri, 2011; Avelino et al., 2012).

Another lesson that can be drawn is about the role of the media in political selection. It has been shown that the media are essential for electoral accountability from the perspective of voters because they inform on the conduct of governments (Besley and Burgess, 2002; Ferraz and Finan, 2008; León, 2017). The study demonstrates that the media is also relevant to party behavior. The effect of popularity shocks is higher when the analysis is restricted to municipalities that have easier access to information due to the existence of local radio stations. However, in this case, the role of the media may be harmful as they lead the political parties to select even worse candidates when the information disclosure is positive over the conduct of the coalition of incumbent parties. On the other hand, this does not seem to raise much concern about democratic accountability since the results do not find that changes in the pool of candidates affect the electoral outcome.

Voters

Voter behavior is another component of models of electoral accountability that has been subject to numerous discussions. The analysis in Chapter 3 provides some answers to important questions that have been raised. The first one refers to the inability of voters to differentiate between the outcomes that come from incumbent performance and the outcomes that come from exogenous shocks (Achen and Bartels, 2004; Bagues and Esteve-Volart, 2016). The study shows that voters punish both mayors and presidents for the incidence of droughts, which in principle advocates against the voter rationality assumption as politicians have no control over meteorological variables. On the other hand, voters reward presidents in elections for the provision of mitigating policies against drought but do not reward mayors. As the central government is the one responsible for providing such policies, rather than local governments, voter behavior does not seem to be completely irrational. In other words, it is not exactly the natural disaster shocks that drive voters to punish incumbents; instead, it is the response of governments to these shocks that is considered by voters on the assessment of the incumbent performance (Healy and Malhotra, 2010).

The second draw that one can extract from the analysis of the Chapter 3 concerns voter's myopic behavior (Cole et al., 2012). As voters are short-sighted and put

more emphasis on recent events in their evaluation of government performance, they might reward more short-term effect policies than policies that have a long-term impact. In fact, the findings suggest that voters reward relatively more palliative policies against drought compared to prevention policies, although the difference is not statistically significant. This result is consistent with studies advocating that voters' responses to government policy could distort the incentives of politicians, leading the government to underinvest in drought preparation, thereby producing considerable public welfare losses (Healy and Malhotra, 2009; Neumayer et al., 2014).

The third and main lesson from the analysis in Chapter 3 is the evidence that voters can provide the wrong incentives to elected officials. The electoral response of voters is of paramount importance to politicians because it is through the systematic pattern of voting that politicians learn about the preferences of their electorate. If voters have preferences attached to attributes that do good to governance, then democratic accountability will lead to an improvement in the quality of the provision of public goods by the state. However, the consequences of clientelism are perverse to the efficiency of governments and worsen the functioning of democratic institutions (Acemoglu and Robinson, 2008). The research discovers that in a historically clientelist environment, citizens foresee the opportunistic conduct of governments and therefore respond strategically to maximize their future benefits. Specifically, citizens reward political alignment between local governments and the central government to increase their chances of receiving assistance in times of water shortage, since political alliance is a significant determinant in the allocation of public resources. When voters face a drought, voters' strategic behavior in favor of the political alliance increases, and this reinforces the notion that growing vulnerability leads citizens to demand more clientelism policies. In this way, governments do not have incentives to change the way they operate, and consequently, the problem of bias in the allocation public resources is perpetuated, connecting the dots of a clientelism system.

Governments

Chapter 3 also discusses the strategic behavior of governments to maximize their chances of re-election. The incumbents want to remain in power, and to this end, they could strategically allocate public policies to reap electoral advantages (Arunlampalam et al., 2009; Solé-Ollé and Sorribas-Navarro, 2008; Cole et al., 2012; Brollo and Nannicini, 2012). As it was shown that voters reward the federal government for aid relief for drought, these policies are distorted in favor of politically aligned municipalities. However, this distortion only happens when subsequent

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elections are for president. When the elections are for the municipal government, the federal government does not seem to bias aid relief distribution. These results imply that the type of election matters for the strategic behavior of the government. The bias will be higher when the electoral benefits directly affect the incumbent government responsible for the distribution of public policies.

Another contribution of this thesis on government behavior is related to a shock of an unexpected increase in public revenue (Leeson and Sobel, 2008; Brollo et al., 2013; Nikolova and Marinov, 2017). As emergency relief against drought is characterized by low regulation, little transparency, and highly discretionary spending, it has become a window of opportunity for policymakers to extract rents. The distortion of public resources that initially aimed to rescue citizens deprived by droughts is one of the worst flaws that a public policy design can produce. Chapter 4 aims to study whether the allocation of palliative policies reflects an increase in the level of corruption in the context of droughts in Brazil. Historically, the process to trigger palliative policies by the federal government starts through the recognition of the state of emergency and this is used. Thus, this study investigates the number of federal emergency declarations against droughts as a proxy for aid relief distribution, and its effect on the number of irregularities in the local governments' expenditures found by auditors in Brazilian cities during a mayor's full term. The analysis shows that emergency relief increases the amount of money involved in corrupt practices. Aid relief against drought seems to create an opportunity for dishonest mayors to graft more public resources.

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