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Empirical Essays on Political Economy and Inequality

by

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Declaration of Authorship

I, Bruno Nogueira Lanzer, declare that this thesis titled, 'Empirical Essays on Political Economy and Inequality' and the work presented in it are my own. I confirm that:

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- Where I have consulted the published work of others, this is always clearly attributed.
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Abstract

The first chapter uses a unique dataset on Brazilian party members and variation from mayoral elections to examine the determinants of party membership in Brazil. It starts by examining the effect of winning office on the membership of political parties at the local level. The effect of interest is identified using a differences-in-differences approach that compares changes in membership of parties that assume office with changes in membership of all other political parties registered in a municipality. The results indicate that winning office increases the membership of the party of the mayor by 0.5%. In addition, political alignment with higher levels of government has a significant effect on the membership of the mayoral candidate party. Finally, the paper documents that party switching is one of the drivers of the estimated increase in membership. The paper offers evidence in favor of the hypothesis that party membership is driven by opportunistic motives in addition to ideology.

The second chapter combines data on the universe of recipients of the *Bolsa Família* program from 2005 to 2015 with data on party membership to investigate the returns to political loyalty. Specifically, it uses variation from mayoral elections to investigate whether members of political parties that assume office at the local level are more likely to receive social transfers. Regression results from an IV estimation show that indeed members of the party that gained access to municipal government are significantly more likely to receive the benefit. Additionally, it finds no evidence that members of parties that did not win office are more likely to lose the benefit as a result of the electoral defeat. This chapter offers direct evidence of material rewards to party membership.

The last chapter focuses on the impact of pay transparency on earnings inequality in the Brazilian public sector. Differences-in-differences estimates show that the disclosure of wages reduced the 90/50 decile wage gap across municipalities located in states that adopted wage transparency in comparison to those located in states that did not adopt the policy. There is also no evidence that earnings decile gaps below the median were affected by the salary transparency policy, which indicates that the effect of disclosure in the public sector was mainly concentrated at the upper tail of the log earnings distribution. Finally, evidence presented suggests that the effect on inequality compression is the result of lower returns to top paid occupations rather than changes in employment. The paper suggests that at the margin, top paid public sector employees are insensitive to changes in their earnings, indicating that there are rents that accrue to holding these positions.

To Antônia and Bruna

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Chapter 1

The Determinants of Party Membership in Brazil

1.1 Introduction

This chapter combines longitudinal data on Brazilian party members and mayoral electoral results from 1996 to 2012 to examine how winning public office affects the membership of political parties at the local level.

Economists and political scientists have long been interested in the question of why parties need members and why individuals join political parties. On the demand side, members might be extremely important to run and finance parties, help with political campaigns, provide electoral legitimacy, participate in the recruitment of political candidates, anchor the party in civil society, mobilize voters and help develop party policies (Nalebuff & Shachar (1999), Heidar (2006), Mueller (2007), Mattozzi & Merlo (2015) and Scarrow (2015)).

On the supply side, most works that examine the determinants of party membership build on the typology of incentives proposed by Clark & Wilson (1961) and on the literature on political participation (Heidar (2006) and van Haute & Gauja (2015)). The strand of literature that follows rational choice models typically explains membership through selective and collective incentives (Olson (1965) and

Aldrich (1983a)). On one hand, with non negligible costs, participation solely for promoting collective policy goals would not be rational since individuals could free-ride on other members activities without the need to participate in politics through membership. On the other hand, individuals might also have little incentive to join a party if collective goods are the main driver of membership since the probability that their activity would influence policy goals is typically low. The provision of selective incentives, either related to the outcome of participation (such as material incentives or other types of private returns) or to its process (such as ideological concerns, i.e., the desire to interact with like-minded individuals), that are unrelated to collective goods could partially help to offset costs of membership and help to explain its supply (Seyd & Whiteley (1996)).[1]

However, political parties have a limited ability to offer and deliver selective incentives, which is consistent with the fact that in practice, parties attract only a few individuals out of the voting population. Additionally, of those individuals that join parties only a relatively small number remain active as members after joining. To address these issues, Seyd & Whiteley (1992) propose a general-incentives theory to explain the supply of party membership. Although instrumental motives (described by selective incentives) are still the main factor explaining why individuals engage in membership, the model considers a larger array of incentives that lie outside rational actor models. In addition to selective and collective incentives, non-instrumental motives that are not usually included in a cost-benefit analysis, such as altruism (related to a sense of loyalty or emotional attachment to the party) and social norms (related to the desire of gaining approval or respect of others), also play an important role in explaining membership.

Nonetheless, the existing empirical literature offers few insights into why individuals join political parties. In part, this might be due lack of both adequate micro-data on party members and convincing sources of exogenous variation that would allow one to credibly isolate endogenous membership incentives from other

[1]For instance, Aldrich (1983b) notes that is the mixture of private and public goods incentives that is necessary and sufficient to explain membership and activism of party members. Note also that Seyd & Whiteley (1992) argue that while outcome incentives are private goods, process incentives might be classified as club goods as a result of being non-exclusive for party members.

motives. Most empirical research investigates the determinants of membership through surveys among party members, providing causal relations that might not be credible. In addition, recent empirical studies focus mostly on explaining the decline in membership rates among developed democracies (explained by ideological convergence in party platforms, development of alternative forms of political participation driven by technological changes, social changes that result in individuals having less time to participate in politics, contexts of lower provision of selective incentives, shrinking of group solidarity incentives, and government subsidies to political parties) and its overall consequences for the political process (Scarrow & Gezgor (2010), Whiteley (2011) and van Biezen & Poguntke (2014)).

Employing a unique longitudinal micro-data set on Brazilian party members, this paper attempts to shed light on the determinants of party membership. In particular, it investigates whether an electoral victory at the municipal level affects membership rates of parties that were successful in the electoral race. Note that winning office gives political parties access to a variety of resources (such as public resources, leeway to appoint local bureaucracy, brand recognition, opportunity to implement its preferred policies, etc.) that could be employed to attract party members. This is even more salient at the local level in Brazil given the decentralized structure of the country in which municipal administrations play a large role in the distribution of public goods and services. One could then expect that parties successful in local elections would be more able to attract individuals with selective incentives than parties that were not.

In order to identify the causal effect of winning office on party membership, this study uses a differences-in-differences strategy across electoral cycles, comparing changes in membership of parties that did run for office at the local level and were elected with changes in membership of parties that did not gain access to office. The baseline results indicate that winning office indeed boost the membership political parties. In particular, considering all local elections held in Brazil from 1996 to 2012, differences-in-differences (DID) estimates show that one year after an election the membership of the party of the mayor is roughly 0.5% higher.

To overcome remaining identification concerns and to shed some light on how the degree of political competition might affect membership decisions, this study also uses a Regression Discontinuity design, comparing the membership share of parties that barely won and barely lost office in closely contested elections in a two party set up. The outcome of electoral races decided by a small margin is typically determined by elements that are beyond the control of a political party, providing an exogenous source of variation that allows us to evaluate how party membership is affected by an electoral victory. This set up also allows us to isolate instrumental motives (such as outcome incentives) from other types of incentives that drive membership (such as collective incentives, altruism or social norms).

The estimates from local linear RD regressions suggest that winning a close election indeed increases the membership of the party of the mayor by 1.5%. Note that the comparison with DID estimates for the same sample (electoral races with only two parties) indicates that political parties that won elections by chance seem to attract more members than parties that won by a larger margin.

Overall, the results indicate that selective incentives seem to explain changes in membership of political parties that assume office, which is in line with the fact that local governments of Brazil have a significant power to distribute public resources among their supporters. Consistent with this, the results also suggest that all other parties that run in coalition with the party of the mayor observe an increase in membership after elections (when compared to changes in membership of parties that did not gain access to office), albeit the increase in membership is more pronounced for the party of the mayor.

Additionally, evidence reveals that political parties in the losing coalition experience a decrease in membership as a result of an electoral defeat. These outflows might be in part explained by individuals switching their membership status from member to non-member. On the other hand, members of parties that lost the electoral race might join parties that assume office to enjoy the spoils of victory. If selective outcome incentives explain why individuals join a political party, then

one might expect to observe that individuals leave political parties that were unsuccessful in the electoral race and join those that were successful.

To investigate whether this is the case, this study then focuses on how winning office affects cross party movements. The evidence presented indicates that party switching explains 11% of the rise in membership observed in the main estimates. Note that party switching is very common among Brazilian politicians. Although this behavior is mainly driven by opportunistic motives or electoral opportunities, it is also partially attenuated by policy preferences (Desposato (2006)). One could then expect that party members would be more likely to move to parties that won an election but this opportunistic motive could be mitigated by the ideological distance between the party an individual is initially a member and the party he moves to.

In this direction, this study shows that a one standard deviation increase in the ideological distance between the party an individual starts and the party in power at the local level reduces the probability of switching to the party of the mayor by -0.07%. This indicates the existence of a trade-off between ideology and opportunism, providing evidence in favor of the hypothesis that party membership is driven by opportunistic motives in addition to ideology.

Finally, the effect of winning office on membership is stronger when the party of the mayor is the same as the party of the president in power at the national level. This could indicate that the opportunistic motives driving membership in Brazil are indeed instrumental with individuals joining parties in the expectation of receiving private benefits derived from the relation between the federal and the local governments.

The remainder of the paper is organized as follows. The next section provides a brief background on Brazil's institutions that are of interest in this analysis. Section 1.3 describes the data used and presents basic descriptive statistics. Section 1.4 discusses the empirical strategy and presents the paper's main empirical findings. Section 1.5 examines the dynamics of party affiliation. Finally, Section 1.6 concludes the paper.

1.2 Institutional Background

1.2.1 Government, Electoral System and Political Parties

Brazil is a Federation with more than 5,500 municipalities, 26 states and 1 federal district. Each municipality has an elected mayor (*prefeito*) and a local city council. At the state level Brazilians vote for a governor and a state parliament, and at the federal level there is an elected President of the republic and a national parliament, which is comprised by a lower house and a senate.

Elections are held every four years on the same date throughout the country and usually take place on the first Sunday of October. Federal and state elections occur in year t and municipal elections in year $t+2$. Elected officials assume public office in the first day of the year following an election (that is, for an election held in October of year t the political mandate starts on the first day of January of year $t+1$). Voting is compulsory for literate citizens between 18 and 70 years old and since 1996 Brazilians have used electronic voting machines to cast their votes.[2]

Mayors are elected by a simple majority rule for municipalities under 200,000 voters and by an absolute majority rule (two-round system) for those over 200,000 voters.[3] Once in office, mayors are subject to a two-term limit. City councillors are elected under a proportional representation system with an open-list and face no term limits.

Brazil has a multiparty system that is arguably weakly institutionalized (Desposato (2006) and Klanja & Titiunik (2017)). There are currently 35 political parties registered at the Electoral Supreme Court. Political parties typically run in coalitions, but votes cast on the ballot in mayoral elections are an expression of

[2]For illiterate citizens and voters between 16 and 17 and more than 70 years old voting is optional. Note also that since 2008 the Electoral Supreme Court (which is the federal electoral authority) started the re-registration of voters with collection of biometric data (in order to identify voters through their fingerprints).

[3]If no candidate achieves the absolute majority in the first round, a runoff shall be held gathering the two candidates with the highest number of votes (top two candidates), and the candidate who wins the majority of valid votes (50 percent plus at least one vote) shall be considered elected.

preferences for the mayor's candidate party.[4] There are no term limits for political parties.

1.2.2 Party Membership in Brazil

As reported in official records, 11.3% of Brazilian voters were members of a political party as of October of 2016.[5] Albeit the standard view suggests that official statistics on party membership are usually overestimated, in October of 2009 the Brazilian Electoral Supreme Court (Tribunal Superior Eleitoral - TSE) introduced an online platform designed for a better management of party membership records with the intent to mitigate existing discrepancies.[6]

The new platform, named Filiaweb, allowed parties to submit their membership lists via web, helping the TSE to identify and cancel membership of fictitious or deceased individuals. It also allowed the electoral court to regularly detect other types of irregularities (mostly related to individuals holding multiple affiliations). It finally enabled full public disclosure of membership information, which was a step forward into the adoption of measures that promote transparency, which is in compliance with the Brazil's Access to Information Act (Law 12,527/2011).

In Brazil, party members play a key role in internal party disputes for leadership and candidate selections. Based on the electoral law, parties must hold conventions during electoral years to deliberate on coalitions and nominate candidates that will run for public office.[7] In some occasions, party members might participate

[4] Although it is mandatory to display all acronyms of political parties under the coalition's name in advertisement or publicity materials related to an election, when casting a vote in elections for executive bodies (mayoral, governor and presidential elections) voters must type the candidate's number in the electronic voting machines.

[5] See Scarrow (2015) and van Biezen & Poguntke (2014) for an international comparison of party membership rates.

[6] For instance, political parties might exaggerate their membership rates on purpose since members are an important source of public legitimation. In addition, membership rates might be inaccurate since there is no verification by external sources (Mair & van Biezen (2001)). Speck (2013) suggests, however, that in Brazil most of these concerns were mitigated by the introduction of the Filiaweb, with outdated records and unchecked databases being less the case. Likewise, Ribeiro (2014) indicates that bias in membership rates seem to affect all parties.

[7] In particular, the Brazilian electoral law establishes that parties must hold electoral conventions in electoral years between the 20th of July and 5th of August.

in primary elections, directly deciding who will represent them in the electoral race.[8] However, even when there are no primaries, members might indirectly select candidates through the election of party leaders that will be ultimately responsible for choosing party candidates.[9]

Membership with a party is also one of the legal conditions to run for public office in Brazil. In observance with this requirement parties must forward (twice a year) a list featuring basic information on their members to the TSE that keeps records and publishes the data.[10]

There is anedoctal evidence suggesting that Brazilian party members are rewarded with employment in the public sector.[11] There is also evidence of public resources, in particular from social transfers, being diverted towards party members.[12]

Despite the fact that the bulk of political parties have written in their statutes that members should make financial contributions to parties, membership fees are typically negligible. An exception is when members are competing for office or when they are civil servants directly appointed by the party to the local bureaucracy (Speck & Costa (2015)).

[8]For media coverage of these events see: g1.globo.com/sao-paulo/noticia/2016/02/candidatos-do-psdb-prefeitura-de-sp-votam-nas-previas-do-partido or inaldosampaio.com.br/termina-a-eleicao-primaria-do-pt.

[9]See for instance: anoticia.clicrbs.com.br/sc/an-jaragua/noticia/2015/05/filiados-ao-psdb-escolhem-presidentes-dos-diretorios-municipais. Note that intra-party competition for candidate selection (among party members) may vary across parties and localities. Strong and institutionalized parties are usually more prone to promote participation of their members. Nonetheless, anedoctal evidence indicates that in some cases boards at the state or national level select the candidates that will run for office at the local level.

[10]Note that the TSE is also responsible for enforcing compliance with the electoral law. Specifically, the Brazilian Political Parties Act (Law N. 9,096/1995) establishes that candidates must meet the following eligibility criteria to run for elections: to be a Brazilian national; to be capable to fully exercise their political rights; to be registered to vote; to have their electoral domicile established in their electoral district; to be a member of a unique political party; and to meet the minimum age requirements for holding office for the different levels of government.

[11]The following examples of media coverage provide insights on these practices: livre.jor.br/36-dos-comissionados-da-prefeitura-de-curitiba-sao-filiados-a-partidos-politicos and fernandorodrigues.blogosfera.uol.com.br/2015/10/01/so-13-dos-23-mil-comissionados-sao-filiados

[12]See for instance: folha.uol.com.br/poder/2013/10/1355336-mais-de-2-mil-politicos-eleitos-receberam-bolsa-familia-ilegalmente

1.3 Data Description and Descriptive Statistics

1.3.1 Data Description

The main data source for this study is a dataset on Brazilian party members published by the Electoral Supreme Court. This data lists all individuals that are currently members of a political party in Brazil, with their names and surnames, date of birth, gender, a geographical location of their affiliation (that is, the municipality and electoral jurisdiction in which those individuals are registered to vote), their party of affiliation, their electoral identification number (that is, their voter's card number), and the start and end dates of their membership (in case of termination).

Individuals can have multiple spells of membership in the data (where a membership spell is defined as a continuous period of membership with a given party) but the electoral law restricts membership to a single party and voting district at the same time.[13] Additionally, the data is longitudinal but incomplete since political parties that ceased to exist (either because of merging or extinction) before the data was posted online by the TSE do not appear in the dataset.[14]

Table 1.1 shows basic characteristics of Brazilian party members as of October of 2016, including information on demographics and length of membership. As mentioned, roughly 11.3% of Brazilian voters were reported as members of a political party as of this date. As shown in the Table, women represent around 45% of all party members. The majority of members are between 35 and 59 years of age and have joined a party when they were, on average, 37 years old. In addition,

[13]Table 1.A1 in the Appendix shows the number of party members per occurrences in the data. There are roughly 16,5 million individuals affiliated to a party with 78% of them observed only once (that is, the majority of individuals either joined a party and are currently affiliated with it or joined and left, without switching to other political parties).

[14]It is noteworthy that party merging is not an uncommon phenomena in Brazil. For instance, PST, a party that won 11 races in the elections of 1996 and 2000 was incorporated into PL in 2003. Additionally, PRONA, that elected 3 mayors in the 2004 elections, merged with PL in 2006 creating a new party called PR. As an additional example, PSD, a political party that won office in more than 100 municipalities between the 1996 and 2000 elections, was absorbed by PTB in 2003. Nonetheless, PSD was later recreated in 2011 (although with a different party number) by former members of three other parties (DEM, PP and PSDB). A list with all political party acronyms and names is presented in the Appendix of this chapter.

a typical member remains in a political party for 7 years before either moving to another party or changing their membership status to being a non-member.

In turn, Figure 1.1 illustrates the proportion of party members among voters distributed in four education categories. Note that Brazilian party members seem to be typically clustered among low and high educated individuals when compared to voters in general.

In order to examine how winning office affects political membership at the local level, this study combines data on party membership with municipal electoral outcomes (also provided by the TSE). The electoral data contains detailed results of all local elections held from 1996 to 2012. For the elections held in 2004, 2008 and 2012 it is also possible to identify all political parties that were running in coalition with the party of the mayoral candidate. I also use data on elections for higher levels of government (specifically, presidential elections) to examine whether political alignment with the the president of the country plays any role in the individual decision to become a member of a political party at the local level. In particular, I use data on presidential elections for all the races that took place between 1994 and 2010.

1.3.2 Descriptive Statistics

Panel A of Figure 1.2 presents the monthly evolution of the stock of party members from September 1992. Dates highlighted in the horizontal axis correspond to electoral periods with dotted lines indicating local elections and solid lines federal and state elections. Although the top panel of the figure suggests that there was an upward trend in membership over time, the increase in the number of party members is mostly in line with the growth of the Brazilian electorate over the period.[15] Panel A also shows discrete gaps in membership, in particular between federal/state and local elections, which are largely driven by inflows of new members, as confirmed by Panel B shown in the bottom of the figure. Note also that,

[15]See Figure 1.A2 in the Appendix.

although smaller in magnitude, Panel B also shows evidence of discrete increase in membership after local elections, which could be an indication that political parties that were successful in electing a mayor could also be successful in attracting members.

Table 1.2 displays basic descriptive statistics on Brazilian mayoral elections from 1996 to 2012. Due to the nature of the data, electoral races with candidates of political parties that merged with others or ceased to exist (and hence do not appear in the party membership dataset) were dropped from the empirical analysis. A total of 902 races were dropped from the final sample (out of 27,645) and of those 834 were from the local elections of 1996 and 2000.

Over the period, elections with two candidates accounted for 50% of all mayoral races as illustrated by comparing columns (2) and (3) of the Table.[16] As shown in column (4), there are typically 5 parties in each coalition competing for local office. Candidates that won an election normally receive roughly 56% of votes.[17]

Finally, columns (6) to (11) illustrate the membership share of parties that won and lost the electoral race in the month the election occurred. Columns (8) and (11) highlight that in the month of the election the membership of those that won office is significantly higher if compared to the membership of those parties that lost the mayoral elections. In particular, as described in the last row of column (8), the membership share of parties that had a candidate running and won the election is roughly 5 percentage points higher than the share of members of parties that had a candidate but lost the electoral race.

[16]Table 1.A2 in the Appendix illustrates the number of races of two candidates for each local election by margin of victory of the winning candidate. Note that a large proportion of Brazil's mayoral races are highly competitive. For instance, races decided by an absolute margin of 5% represent more than 25% of all races with two coalitions. Furthermore, elections decided by an absolute margin of 2% account for more than 11% of all elections.

[17]Note that the vote share is expressed as the number of votes that the party of the mayoral candidate received over the total valid votes in a municipality.

1.4 Electoral outcomes and Party Membership

This section examines if individuals join political parties as a result of an electoral victory at the municipal level, which would indicate that selective incentives are determinants of party membership. It begins by describing the empirical strategy used to identify the effect of interest and then shows the impact of winning public office on the membership of the party of the mayoral candidate. It then considers whether other parties that were in coalition with the mayoral candidate during the electoral race also face positive changes in membership and whether those in the opposition face a drop in their membership as a result of the electoral defeat. It finally examines if political alignment with higher levels of government boosts the membership of parties that gained access to office.

1.4.1 Empirical Strategy

To estimate the effect of winning public office on party membership this study relies on a differences-in-differences strategy that compares differential changes in membership of political parties across electoral cycles. Although the data provided by the TSE allows us to follow individuals over time and across political parties and localities, this study focuses on a party level analysis, since an individual level exercise with the full sample would be computationally challenging. The unit of observation in the empirical analysis is then a party in a given municipality and month. Note, however, that the party level model would provide us the same estimate for the coefficient of interest than the individual level model with the caveat that it does not include individual level controls in the regressions. Nonetheless, the within party variation does not matter for the identification since the treatment takes place at the party and municipality level. Focusing on one electoral cycle, this study starts by estimating the following regression model:

$$Y_{pmt} = \alpha + \beta W_{pm} * d_t + \delta_{pm} + \delta_{pt} + \delta_{mt} + u_{pmt} \tag{1.1}$$

where Y_{pmt} is the membership share of party p in municipality m in a given month t, W_{pm} is a variable that equates 1 if a party p won the election in municipality m and 0 otherwise, d_t is a dummy for observations post election, δ_{pm} denotes party x municipality fixed effects, δ_{pt} party and time fixed effects, δ_{mt} municipality and time fixed effects, and u_{pmt} is an error term.

This empirical strategy uses all political parties that lost the mayoral electoral race in a municipality as control for those parties that were successful in the mayoral elections. The parameter β identifies then the effect of winning office through the comparison of changes in membership of parties that won the local election with changes in membership of parties that lost the election.

Note that this strategy has the advantage of controlling for party municipal specific effects with δ_{pm}, which can account for unobserved characteristics of political parties that change with locality (such as party ideology). It also absorbs any party specific effects that are time varying and could explain changes in membership of party p independently of the fact that this party gained access to office, such as nationwide trends in membership or in policy preferences of political parties, with the δ_{pt} control. It finally controls for changes in local socioeconomic conditions or municipal institutions, potentially correlated both with electoral results and changes in membership, through δ_{mt}.

It should also be noted that when all mayoral elections from 1996 to 2012 are used to estimate the model, the parameter of interest, β, is a pooled coefficient, since the observations from each of the five electoral cycles are pooled. As a result, in the pooled regressions, the control δ_{pm} is not restricted to be the same across elections and it does account for party municipal specific effects that vary with the electoral cycle.

One potential concern with this identification strategy is that voters could anticipate electoral results and strategically move in and out of political parties before the local elections are held, which would bias the estimated effect of winning office. In addition, the effect might be driven by a reverse causality, since membership

might be the cause of the electoral victory and not the other way around. To address these issues, throughout the empirical analysis the posterior membership share of parties (that is, their membership in each month after the electoral race) is compared to their membership share in the month just before the elections were held.

This study finally uses a Regression Discontinuity (RD) design following Lee (2008), that exploits exogenous variation from close elections in a two party set up, to deal with any remaining concerns regarding the proposed DID strategy and to examine the potential bias in the DID estimates. To perform this exercise, the sample is restricted to electoral races with only two parties (or two candidates) competing for office. In each electoral cycle, a political party p is now assigned to treatment in municipality m if $W_{pm} = 1[MV_{pm} \geq 0]$, where MV_{pm} is defined as the vote share of the winner minus the vote share of its strongest opponent and $1[.]$ the indicator function. At the threshold $MV_{pm} = 0$, assignment to treatment, given by W_{pm}, sharply changes from 0 to 1.

The identification of the RD strategy relies on the assumption that in elections decided by a narrow margin unobservables play a negligible role. Parties that barely won and barely lost by a close margin should then be very similar in observable and unobservable covariates with the assignment to treatment around the cutoff considered as good as random. To examine whether winning a close election increases the membership of parties this work uses the following model:

$$Y_{pmt} = \alpha + \beta W_{pm} + f(MV_{pm}) + \delta_{pm} + \delta_{pt} + \delta_{mt} + u_{pmt} \tag{1.2}$$

where Y_{pmt} and W_{pm} are defined as before and MV_{pm} is the margin of victory. The function $f(.)$ is a smooth function of the margin of victory and includes an interaction between W_{pm} and MV_{pm}. The controls δ_{pm}, δ_{pt}, and δ_{mt} are defined as in equation 1.1 and u_{pmt} is an error term.

The proposed RD specification captures the effect of winning office on membership at the cutoff by comparing the membership of parties that barely won and barely lost an election within a locality. The RD model is then estimated both using a parametric specification fitting a third order polynomial with a spline on either side of the threshold $MV_{pm} = 0$ and local linear regressions that restrict the sample to observations that lie within a close vicinity of the threshold. Note that the parameter β identifies now a slightly different effect in comparison to the one described by equation 1.1, since the comparison group is restricted to the strongest opponent of the party of the mayor that assumed office in a two party race set up and the effects are measured around the cutoff MV=0.

1.4.2 Baseline Results

Table 1.3 presents DID estimates of equation (1.1) for different time windows after the local elections were held. Note that for each electoral cycle the membership share of each monthly window reported in columns (1) to (3) is compared to the membership in a baseline period defined as the month just before the local elections took place. The dependent variable in each regression is the membership share of a political party p in municipality m and month t (multiplied by 100). Standard errors are clustered at the municipal level and regressions are weighted by the overall number of party members in each municipality.

Overall, estimated coefficients presented in columns (1) to (3) of the table indicate that winning an election significantly increases the membership of political parties. For instance, estimates in column (1) suggest that, 1 to 12 months after an election, the change in membership of the party of the mayor is roughly 0.1 percentage point higher than the change in membership of parties that lost the elections. That is, in the first twelve months after the electoral race, the membership of the party of the mayor (which typically accounts for 20% of all party members) increases by 0.5% as a result of assuming office at the local level.

Additionally, estimates reported in columns (2) and (3) reveal that the rise in membership is significantly positive but relatively stable throughout an electoral cycle. Specifically, the share of members of the mayoral candidate's party rises by 0.75% and 0.5% in the second and the third years after the elections took place, respectively. For simplicity, the rest of the empirical analysis will focus on the first twelve months after the mayoral electoral race, which comprehends the first year of the mayoral term.

Although results of the DID regressions presented in Table 1.3 can be explicitly interpreted as evidence of selective incentives as drivers of membership as discussed by Seyd & Whiteley (1992) and Seyd & Whiteley (1996), these estimates might be downward biased if movements in and out of political parties mainly take place before the elections for races where the margin of victory is expected to be large. In an attempt to examine this issue, this study also investigates whether winning office at the local level affects party membership in closely contested elections where anticipation is less likely to happen.

Columns (1) and (2) of Table 1.4 report estimated coefficients of non-parametric and parametric RD regressions of the effect of winning the election on party membership for the first year after the elections were held. The nonparametric RD specification is based on kernel estimators with an optimal bandwidth while the parametric RD regression is estimated with a third order polynomial that uses the full bandwidth.[18] For comparison, column (3) in the table includes estimates of a DID specification for the same sample (that includes only two party races) and period. Note that columns (2) and (3) include the same set of controls as described in Table 1.3.

[18]The optimal bandwidth was selected based on Calonico *et al.* (2014). Note that this procedure selects a bandwidth that minimizes the mean squared error (as suggested by conventional RD inference) and also corrects the standard errors to produce robust confidence intervals (Klanja & Titiunik (2017)). Nonetheless, Table 1.A3 in the Appendix present estimates of alternative RD specifications. Columns (1) and (2) of the table reports parametric RD estimates with the control function approximated with 1^{st} and 2^{nd} order polynomials. Column (3), reports the point estimate of a local linear regression with the optimal bandwidth chosen using Imbens & Kalyanaraman (2012). The estimated coefficients reported in the table are in line with those presented in Table 1.4.

Estimates presented in columns (1) and (2) are in line with those presented in Table 1.4, showing that winning office indeed increases the membership of the party of the mayor. Specifically, column (1) shows that winning office by a small margin increases the membership of a political party by roughly 1.5%. Consistent with this result, the point estimate of the parametric RD reported in column (2) shows that the membership of parties increases by 3%.

Note also that the RD design allows me to contrast close with safe electoral races. Theoretically, it is not clear whether estimated coefficients of a DID specification should be higher or lower than those of RD specifications that compare political parties in highly competitive races. On one hand, parties that won by a larger margin might have more political capital or support to implement policies than parties that won by chance, which could explain a higher membership share in the former with respect to the later. On the other hand, if elections are not decided by chance voters could arguably be able to predict the electoral outcome and might have no incentive to join parties after the race.

Column (3) reports estimated coefficients of a DID regression, which compares changes in membership of parties competing in the two party race set up. The presented point estimate indicates that the membership of parties that assumed government at the local level rises by 0.6%. The sizable differences of estimates of columns (1) to (3) suggest that indeed the degree of political competition matters. The membership of political parties that won office in competitive races tend to increase more than those of parties that won by a larger margin.

Figure 1.3 presents an RD plot that illustrates the effect described in column (2) of Table 1.4. The figure shows the membership share in the future against the vote margin received by the party with a running candidate in the previous election. The lines in the graph correspond to 3^{rd} order polynomial fits for control and treatment units separately and the dots correspond to the sample mean within each chosen bin (constructed using evenly spaced bins).[19]

[19]Note that the number of bins was selected in order to minimize the Integrated Mean Square Error (IMSE) of the underlying regression function estimator as proposed by Calonico *et al.* (2015).

To check the robustness of the results from the proposed RD specification, Table 1.A4 in the Appendix of this chapter presents placebo local linear regressions of the effect of winning the election on the pre-treatment membership share of political parties that barely won office (in comparison to the membership of those parties that barely lost the local elections). Point estimates presented in the table suggest that there was no anticipation effect. That is, an electoral victory in t had no effect on the membership share of parties that had a candidate and barely won office if one looks to the period of twelve months before the elections were held.

1.4.3 Heterogeneous Effects

Mayoral Coalitions

As discussed in subsection 1.2.1, Brazilian mayors compete for office together with vice-mayors and usually in coalition with other political parties. In addition, the decentralized structure of the country makes municipalities responsible for the provision of a wide range of public goods and services, giving mayors considerable leeway to distribute public resources among supporters and appoint bureaucracy to civil service jobs (Colonelli *et al.* (2017)). It is noteworthy to mention that mayors typically appoint as deputy mayor's (*secretarios*) both politicians or party members of the other parties that composed their winning coalition. As a result, the effect of an electoral victory on membership might not be restricted to the party of the mayor and could drive affiliation of all parties within the coalition that gained access to office at the municipal level. On the other hand, those political parties that did compete for office but lost the electoral race might experience a differential decrease in membership. This could also be particularly more salient for the party of the incumbent that was replaced in office. To investigate whether this is the case, this study then estimates the effect of winning office on the membership after elections for parties in all these different groups.

Note that information on the party composition of mayoral coalitions is only available for the local elections held in 2004, 2008 and 2012 (as argued in subsection

1.2.1). In practice, this restricts the sample used in the proposed exercise to these sub period with three electoral cycles (out of five), excluding the mayoral elections held in 1996 and 2000. The empirical strategy follows the specification illustrated by the equation 1.1 and includes interactions for each group of parties that compose the party coalitions competing for office.

Estimated coefficients of this exercise are reported in columns (1) to (4) of Table 1.5. The first column shows estimates of the effect of winning the election on the membership share of all parties that compose a winning coalition. That is, all political parties that competed for office but lost the electoral race are included in the control group. The second column, in turn, includes a dummy for the party of the mayoral candidate that is in office, measuring if there is any additional effect on the membership of the party of the mayor.

The point estimate reported in column (1) indicates that the membership of parties in the winning coalition rises as a result of an electoral victory when compared to the membership of parties that did not assume office. Specifically, winning the elections increases the membership of the parties of the winning coalition by 0.1%. This effect is, however, mainly concentrated in the party of the mayoral candidate, as suggested by estimates displayed in column (2) of the table. In particular, column(2) shows that while gaining access to office increases the membership of the party of the mayor by 0.5%, the affiliation share of the other parties in the winning coalition rises only by 0.02%. Note this effect could be in part explained by the fact that the party of the mayor has arguably more leeway than the other parties in a winning coalition to allocate local resources and to select public sector workers.

The specification presented in the third column includes dummies for parties that were part of the losing coalition. Note that the control group is now restricted to the parties registered in the municipality that did not compete for office in the election. Hence, changes in the membership of parties that compete for office and won and changes in membership of those parties that did compete but lost are now compared to changes in membership of parties that did not compete in the

local election. Finally, the fourth column includes an additional interaction for the party of the incumbent that lost office.

Notably, parties in a losing coalition face a decrease in membership as a result of losing the election. Specifically, the point estimate reported in the third row of column (3) suggests that the membership of parties in the losing coalition falls by -0.03%. Interestingly, this is effect is stronger for parties that lost office (losing incumbents) as shown by the estimate reported in the last row of column (4). In particular, the decrease in membership of incumbent parties that lost the election is of -0.5%.

In sum, the evidence presented in Table 1.5 reveals that the party of the mayor is the main beneficiary of an electoral victory in terms of membership, in line with results reported in Table 1.3. This result is robust to the comparison with different control groups. Additionally, the membership of other parties in the winning coalition also increases once this parties assume local government (when compared to changes in membership of parties that did compete for office but lost and of those that did not compete in the electoral race). Finally, the membership of parties in a losing coalition decreases as a result of an electoral defeat, which indicates that there is a net outflow of individuals from these parties.

Political Alignment

Intergovernmental transfers constitute an important source of resources to Brazilian municipalities, representing on average more than 80% of the municipal revenue. One could then expect that alignment with higher levels of government might increase municipal resources. In fact, empirical evidence suggests that transfers are employed to reward mayors aligned with the presidential coalition (Brollo & Nannicini (2012)).[20] That could be a source of influence in the individual decision

[20]In particular, Brollo & Nannicini (2012) exploit variation from close elections to investigate whether discretionary federal transfers are used to reward aligned mayors able to run for a second mandate. Their findings indicate that aligned mayors are more likely to receive transfers from the federal government in the last two years of a first mayoral term.

to join a political party, in particular, when this party assumes public office at the local level.

Table 1.6 presents DID estimates based on equation 1.1 that attempt to examine whether there is any additional effect of winning an election if the party of the mayor is aligned with the party of the president of the Brazilian republic. In practice, the estimated specification includes an additional interaction between the treatment dummy with a dummy equal to 1 if the party of the mayor that won office is the same party as the president in power at the national level. Controls included in this regression are the same as those described in the main DID regression described by equation 1.1.

As expected, the point estimate presented in the first row of the Table is in line with those illustrated in Table 1.3, although smaller in magnitude. In particular, there is a significant differential change in the membership of the mayoral candidate's party in comparison to the changes in membership of all the other parties registered in a municipality in the twelve month period after the electoral race (using the month just before the election as basis).

Interestingly, the estimate reported in the second row of the Table shows that political alignment significantly boosts the membership of the party of the mayor. Precisely, when the mayor belongs to the same party as the head of the central government, the membership of the mayoral candidate's party increases by 1.7%.

This result is consistent with opportunistic motives for membership, with individuals joining political parties not only as a result of ideological preferences or unobserved time varying characteristics of parties (that are partially controlled by local specific effects that vary with municipalities and electoral cycle) but in expectation of selective rewards derived from the relation between the local and the federal government. It could also indicate that this opportunistic behavior is driven by instrumental incentives (such as material rewards) rather than simply showing that individuals prefer to be associated with parties that succeeded in the local elections.

1.5 Dynamics of Party Affiliation

Consistent with selective motives being one of the determinants membership, the baseline estimates presented in section 1.4 suggest that the membership of political parties that were successful in local elections increases if compared to the membership of parties that were unsuccessful. Part of this increase in membership could be explained by party switching, which arguably implies a trade-off between the opportunistic behavior of an individual and his ideological preferences. This section attempts to shed light on this issue.

1.5.1 Role of Cross-Party Movements

Before examining whether ideology attenuates the opportunistic behavior of those joining parties after the elections, this work first attempts to ascertain whether the differential change in membership of parties that assume office is also driven by cross-party movements and not only due to inflows of individuals that were not previously members. To perform this exercise, I estimate the DID model described by equation 1.1 for a restricted sample of individuals. Specifically, for each electoral cycle, the sample includes only those individuals that are continuously observed in the period of twelve months after the mayoral elections were held, using as basis the month before each election takes place. This sample selection guarantees that the membership share of parties in a municipality only changes as a result of cross-party movements and not of inflows of new members or outflows from a membership status to a non-membership status.

Table 1.7 reports estimates of the DID regressions for this restricted sample. As in Table 1.3, the control group includes all parties that were not successful in the local elections. The point estimate reported in column (1) is positive and statistically significant, which indicates that individuals are indeed switching from parties that did not assume office to those that won office. Specifically, winning office increases the membership of the party of the mayor by 0.1% in this sample.

Note finally that this estimate is 0.07 percentage point smaller than the point estimate reported in column (1) of Table 1.3.

1.5.2 Ideology versus Opportunism

The previous subsection has established that individuals strategically move from political parties that did not assume office at the local level to those that won the electoral race. However, party switching is a costly activity and its costs might vary depending on the ideological distance between the party an individual is initially a member and the party he decides to move (Desposato (2006)). One might expect that opportunistic individuals are more likely to join parties that assume government but would prefer not to join a party that is far from their own policy preferences in the ideological spectrum in order to lower the costs of switching.

To measure the relative probability of moving from a party k in a baseline period before local elections to a party j in time period t after elections, conditional on the fact that party j is in office in t, and to assess whether the ideological distance between parties affects the relative probability of switching, this study estimates the following regression model:

$$n_{kjmt} = \alpha + \beta_0 W_{jmt} + \beta_1 W_{jmt} * distance_{kj} + \delta_{kmt} + \delta_{kjm} + u_{kjmt} \tag{1.3}$$

where n_{kjmt} is the proportion of party members that are in party j in month t and municipality m but were in party k, in the same locality, one month before the local elections were held (or the log of this ratio), W_{jmt} is a variable that equates to 1 if the party of destination j is in office in t in municipality m, $distance_{kj}$ is equal to $|ideology_k - ideology_j|$ that measures the absolute value of the difference in ideology (or policy preferences) between party k and party j, δ_{kjm} is a fixed effect that controls for time invariant unobservables that affect both party k and

j in municipality m,δ_{kmt} is a fixed effect that controls for unobservables that vary with the party of origin k over time in a certain locality, and u_{kjmt} an error term.

Note that this specification controls for local differences in ideology of party k and party j, with δ_{kjm}. It also absorbs municipal specific trends in ideology or in membership of party k, with δ_{kmt}. The Appendix of this chapter also includes a fully saturated model that additionally controls forδ_{jmt} and δ_{kjt} and is discussed below.

If cross-party movements occur in the direction of the party that did assume office at the local level (the party of the mayor), as previously established in subsection 1.5.1, one would expect β_0 to be positive in the above specification. Nonetheless, if individuals are willing to switch to the party of the mayor but this effect is attenuated by the distance in party position of party k and party j, then one would also expect β_1 in equation 1.3 to be negative.

Empirically, to perform this exercise, I consider individuals that were members of any political party registered in a municipality at baseline (one month before the local elections were held) and then follow their movements from baseline to a maximum period of 12 months after the elections were held. As in subsection 1.5.1, the sample is restricted to individuals continuously observed as members during this period.

To measure the distance in ideological preferences described by $distance_{kj}$ in equation 1.3, this study uses an index of party positions given by Power & Zucco (2009). The index is based on survey responses from members of the Brazilian National Congress and ranks parties from left to right on a scale of 1 to 10, respectively. Estimates of party ideology, however, are only provided for the 11 main Brazilian political parties. In practice, this additionally restricts the sample of the analysis to 11 parties (that is, movements can only take place across these 11 parties). Note, however, that despite the large number of political parties registered in Brazil, as discussed in subsection 1.2.1, there is evidence of political concentration both in terms of mayoral offices and party members towards some of the parties. For instance, considering all mayoral elections that took place between 1996 and

2012, the 11 main Brazilian parties elected 87% of mayors (on average). In addition, in the month each election was held, more than 93.3% of all party members were affiliated to one of these 11 parties.[21]

In order to confirm that cross party movements also drive the effect of winning the local elections on membership in this restricted sample of 11 parties, column (2) of Table 1.7 shows the point estimate of the same DID exercise described in the previous subsection but including only the 11 parties for which Power & Zucco (2009) provide information on ideological preferences. In contrast to subsection 1.5.1, the control group in this specification does not include all other parties registered in a municipality but is restricted to 10 out of the 11 parties for which the information on ideology is available. Note, that the point estimate presented in column (2) of Table 1.7 is positive and statistically significant and of roughly the same magnitude than the one reported in column (1) of the same table.

Finally, Table 1.8 presents estimates of equation 1.3 that examines how winning the election affects the relative probability of switching party and how this transition rate is further impacted by the distance in party position between the party individuals are at baseline and the party they move to after elections. Columns (1) and (2) of the table focus on the proportion of changers while columns (3) and (4) on the log of this proportion. All regressions are weighted by the number of party members in the party of origin k and robust standard errors are clustered at the municipal level and reported in brackets.

Focusing on the log specification presented in the table, the point estimate reported in the first row of column (3) indicates that individuals are significantly more likely to switch to the party of the mayor after elections rather than staying in any other party of the winning coalition or in the parties that did compete for office and lost or even in the parties that did not compete in the election. Specifically, the relative probability of moving is 0.72% higher for the party of the mayor that assumed office in comparison to all other political parties registered in the municipality.

[21] To illustrate, Panels A and B of Figure 1.A3 in the Appendix of this chapter shows trends of the five major Brazilian parties. At local elections held in 2012 these 5 parties together had over 56% of their candidates elected as mayors. Moreover, over 50% of all party members were affiliated to one of these five political parties.

Note, however, that once the distance in ideology between the party voters start at baseline and the party they move to during the first year after local elections is taken into account, the relative probability of moving to a party j that is in office rather than staying in party k decreases, although the estimated coefficient is not significantly different from zero. Precisely, a one standard deviation increase in the ideological distance of party k and party j implies a decrease of -0.10% in the probability of moving to party j relative to staying in party k, as illustrated in the second row of column (3).

Column (4) of Table 1.8 presents a specification that additionally controls for time varying local specific characteristics of the party of destination j, such as changes in policy preferences. Note that once this control is included the effect of the party of the mayor being in office per se can no longer be identified. The estimated coefficient presented in the second row of the column is negative and statistically significant, which confirms the intuition that switching to winners is less likely to take place the higher the ideological difference between the party that assume office and the party an individual is initially a member. In particular, a one standard deviation increase in the ideological distance of party k and party j decreases the probability of switching to the party of the mayor (relative to staying in other parties) by -0.07%

As a final attempt to add transparency and ascertain the robustness of the results illustrated in Table 1.8, columns (3) and (4) of Table 1.A5 presented in the Appendix of this chapter show regressions of the same model in logs described by equation 1.3, including an additional control to account for unobservables that change with the party of origin k and the party of destination j over time (such as the distance of policy preferences). Note that the inclusion of this control does not alter the significance and barely changes the magnitude of point estimates of equation 1.3 in comparison to those presented in Table 1.8.

In sum, the estimates suggest that selective incentives are one of the determinants of party membership, with individuals switching from other political parties to the party of the mayor. In addition, the evidence presented also indicates that there

seem to be a trade-off between opportunistic and ideological motives for joining a party, with ideological preferences attenuating the opportunistic behavior of entering a party that was successful in local elections.

1.6 Summary and Conclusions

This paper exploits variation from mayoral elections to investigate the determinants of party membership in Brazil. It first establishes that winning office significantly increases the membership of the party of the mayoral candidate successful in the elections, which is consistent with the hypothesis that party membership is explained by selective incentives.

Given that Brazil is a very decentralized country and municipalities are responsible for providing a significant set of public goods and services, political parties that assume office at the local level might have a significant leeway in allocating resources to supporters and in appointing individuals to work in the local bureaucracy. In part, this might explain the effect of an increase in membership of parties in power. It could additionally explain an increase in membership of parties that run in coalition with the mayoral candidate's party.

As expected, when compared to changes in membership of parties that did not gain access to office, all parties in a winning coalition seem to face a rise in membership. Nonetheless, the effect of winning office on membership is more salient for the party of the mayor. Additionally, parties in the losing coalition experience a decrease in membership as a result of the electoral defeat, which could indicate that individuals are switching to the parties that assumed government.

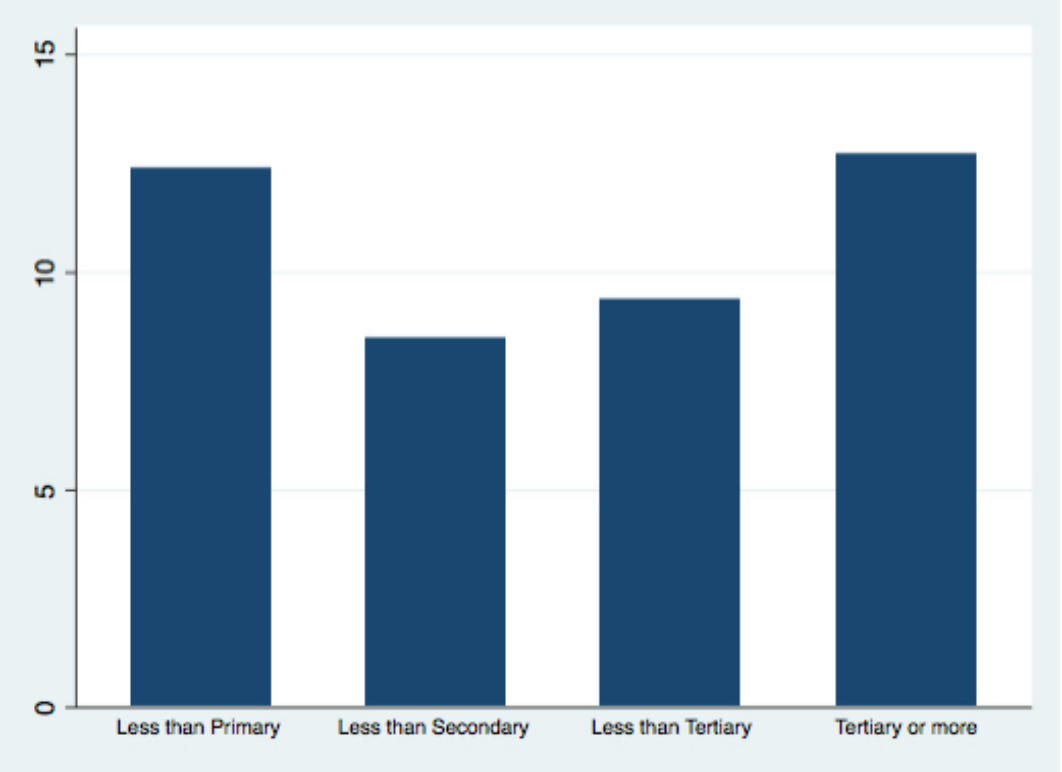
Consistent with this, the results presented in this chapter show that cross-party movements explain 11% of the differential change in membership of the party of the mayor caused by a mayoral electoral victory. In addition, the evidence presented suggests that individuals indeed switch to those parties that assume local government, which is in line with the observed reduction in membership of

parties that did compete for office but lost the elections. Party switching towards winners is, however, attenuated by ideological concerns.

Finally, this study provides evidence that alignment with the party of the president of the republic seem to explain most of the differential effect observed in the membership of the party of the mayor. This result is also consistent with the interpretation that party membership is driven by opportunistic motives in addition to ideology.

Tables and Figures

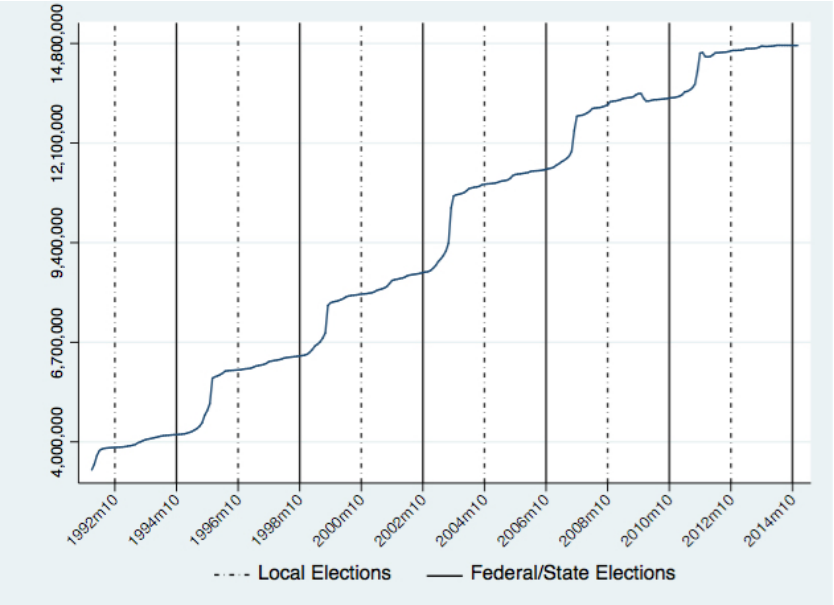
FIGURE 1.1 Proportion of party members among voters by level of education



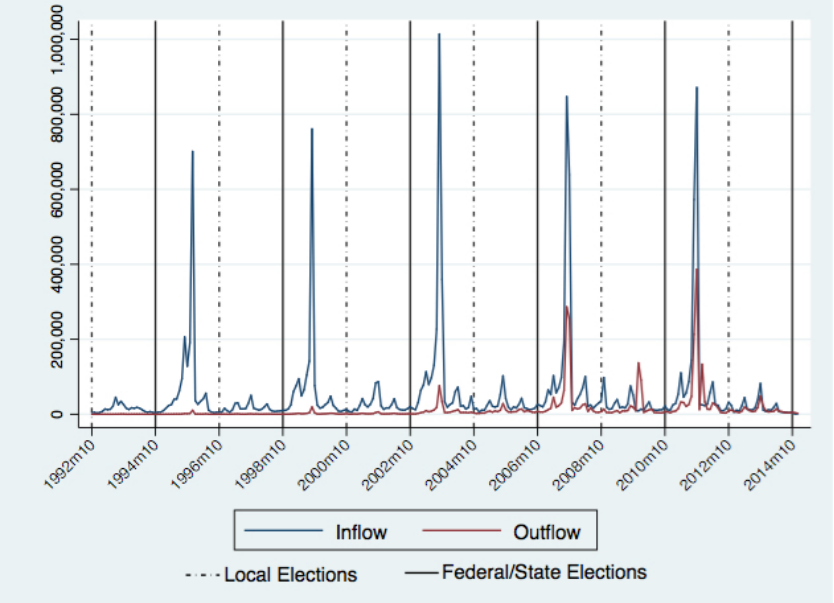
Notes: the figure shows the share of party members among voters by level of education as of October 2016.

FIGURE 1.2 Evolution of Party Membership in Brazil

Panel A: Stock of Members

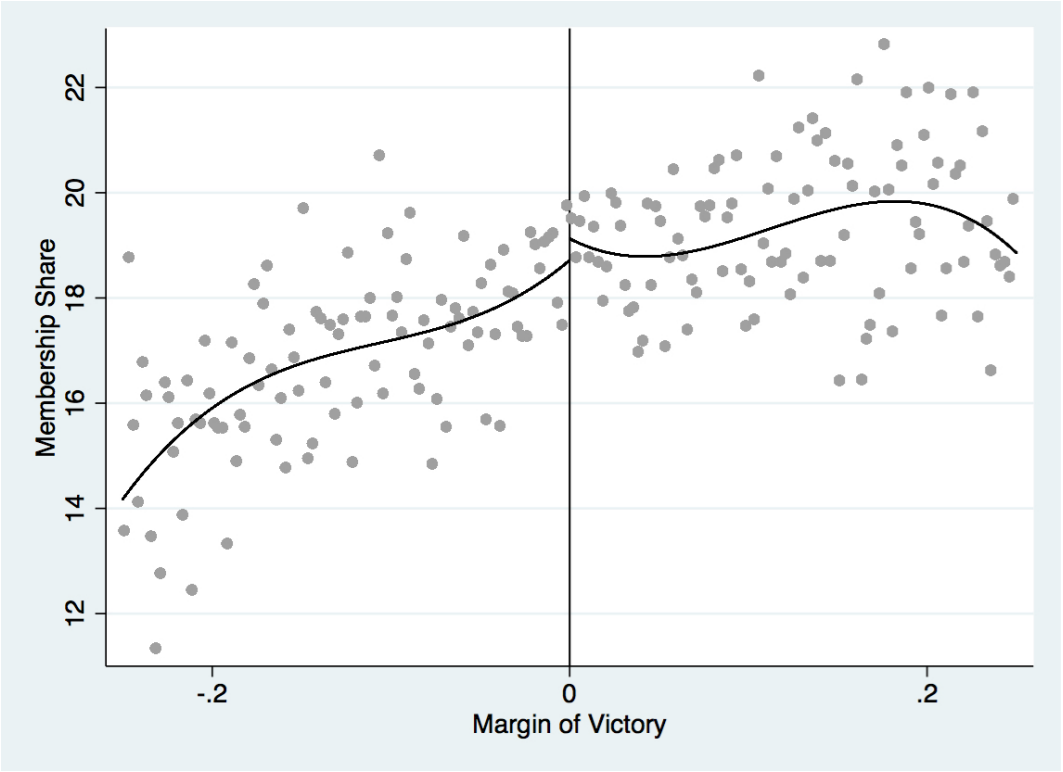


Panel B: Inflows and Outflows of Members



Notes: the figures show the monthly evolution of party membership in Brazil from Oct/92 to Oct/14. Panel A illustrates the evolution of the stock of party members while Panel B shows the inflows and outflows. Vertical dashed lines indicate the time period that local elections took place while solid lines indicate dates of federal and state elections.

FIGURE 1.3 RD effect of winning the election on party membership



Notes: the figure presents a graphical description of the RD results described in Table 1.4 for races of two candidates. The graph shows the fitted values from a third order polynomial regression estimated separately on each side of the cutoff point. Note that the running variable is the difference between the share of votes of the party of the winning candidate and the losing candidate in the previous election. The range of the graph is limited to winning margins of 25 percent or less (in absolute terms).

Table 1.1 Descriptive Statistics: Brazilian Party Members

Variables	-
% of members among voters	11.3
% of members that are women	45.1
% of members by age	
16-17	0.0
18-24	2.3
25-34	13.8
35-44	19.9
45-59	36.1
60-69	15.4
70-79	8.0
>79	4.6
average age when joining a party	36.7
average years as a member	7.3

Notes: The table presents descriptive statistics on Brazilian party members as of October of 2016. The first row shows the share of voters that are members of a political party and the second row the share of party members by gender. The third row illustrates the distribution of party members by age. Finally, the fourth and last rows present the average age of party members when joining a political party and the duration of their membership. Source: TSE.

TABLE 1.2 Descriptive Statistics: Brazilian Mayoral Elections

					Membership Share					
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Elections	No of Races	No of Races (2 coalitions)	No of parties per coalition	Vote Share (win. coal)	winners (party of mayor)	losers	Diff.	winners (coalitions)	losers	Diff.
1996	4,983	2,456	-	0.551	0.222	0.158	0.066***	-	-	-
2000	5,145	2,760	-	0.562	0.205	0.141	0.063***	-	-	-
2004	5,492	2,633	4.1	0.543	0.164	0.112	0.047***	0.419	0.329	0.084***
2008	5,556	2,891	4.7	0.571	0.150	0.106	0.044***	0.439	0.329	0.102***
2012	5,567	2,984	5.4	0.562	0.126	0.095	0.030***	0.416	0.334	0.075***
Average	5,349	2,745	4.8	0.558	0.192	0.165	0.049***	0.424	0.331	0.087***

Notes: The table shows descriptive statistics for Brazilian mayoral elections. Column (2) shows the number of races per election and column (3) presents the number of races where only two coalitions (or two candidates) were running for office. Column (4) reports the average number of parties within each running coalition. Column (5) presents the average vote share received by a winning coalition in each election. In addition, columns (6) and (7) present the share of party members of parties with running candidates in each election. In turn, columns (9) and (10) show the average membership share of all parties in a running coalition. Finally, columns (8) and (11) show a simple difference in means between columns (6) and (7) and (9) and (10), respectively.

TABLE 1.3 DID Estimates of the Effect of Winning the elections on Party Membership: local elections 1996 to 2012

	(1 to 12 months)	(13 to 24 months)	(25 to 36 months)
	(1)	(2)	(3)
winning party	0.091***	0.143***	0.094**
	(0.018)	(0.025)	(0.040)
N	12,671,540	12,671,715	12,671,715
R^2	0.999	0.999	0.992
Weights	Yes	Yes	Yes
Time x Mun. FE	Yes	Yes	Yes
Time x Party FE	Yes	Yes	Yes
Municipality x Party x Electoral Cycle FE	Yes	Yes	Yes

Notes: Columns (1) to (3) of the table report DID estimates of the monthly membership share of political parties on a dummy that equals to 1 if the party with a running candidate won the local election and 0 otherwise. Each column compares the month just before the local elections were held with different periods of time after the elections. In particular, columns (1) to (3) compare the month before a mayoral election takes place with the first, second, and third years after this period, respectively. The unit of observation is party p in municipality m and month t. All regressions include time x municipality fixed effects, time x party fixed effects and municipality x party x electoral cycle fixed effects, and are weighted by the overall number of party members in each municipality. Robust standard errors are clustered by municipality and reported in parentheses. Significance ***at 1% level, **at 5%level, *at 10%level.

TABLE 1.4 DID and RD Estimates of the effect of winning the election on Party Membership (races with two candidates): local elections 1996 to 2012

	Non Parametric RD	Parametric RD	DID
	(1)	(2)	(3)
winning party	0.320*	0.594***	0.133***
	(0.170)	(0.210)	(0.039)
p-order		3	
h	0.14		
BW Type	CCT		
N	186,144	329,306	356,682

Notes: The table shows DID and RD estimates of the effect of winning the local elections on the membership share of political parties. The sample is restricted to races with only two candidates competing for office. Column (1) reports the point estimate of the non-parametric RD regression while column (2) shows the estimate for the parametric RD based on equation 1.2. Estimated coefficients reported in column (3) refer to the DID regression described in equation 1.1. Controls displayed in the specifications of columns (2) and (3) include time x party fixed effects, municipality x party fixed effects and municipality x party fixed effects. Regressions in columns (2) and (3) are weighted by the number of party members per municipality with robust standard errors clustered by municipality and reported in parentheses. Significance ***at 1% level, **at 5%level, *at 10%level.

TABLE 1.5 DID Estimates of the effect of Winning office on Party Membership of Coalitions: local elections 2004-2012

	(1)	(2)	(3)	(4)
winning coalition	0.023***	0.008***	0.002	0.001
	(0.004)	(0.002)	(0.003)	(0.003)
winning party		0.104***	0.102***	0.101***
		(0.025)	(0.026)	(0.025)
losing coalition			-0.011***	-0.011***
			(0.013)	(0.004)
losing party			-0.002	0.007
			(0.005)	(0.006)
losing incumbent				-0.083***
				(0.017)
Weights	Yes	Yes	Yes	Yes
Time x Muni. FE	Yes	Yes	Yes	Yes
Time x Party FE	Yes	Yes	Yes	Yes
Muni. x Party x Electoral Cycle FE	Yes	Yes	Yes	Yes
N	7,603,050	7,603,050	7,603,050	7,603,050
R^2	0.999	0.999	0.999	0.999

Notes: The Table shows DID regressions of the membership share of political parties on dummies that distinguish the different groups composing a party coalition that run for office at the local level. All regressions include time x municipality fixed effects, time x party fixed effects and municipality x party x electoral cycle fixed effects, and are weighted by the overall number of party members in each municipality. Robust standard errors are clustered by municipality and reported in parentheses. Significance ***at 1% level, **at 5%level, *at 10%level.

TABLE 1.6 DID Estimates of the effect of Winning the election on Party Membership of Politically Aligned candidates: local elections 1996 to 2012

	(1)
winning party	0.039***
	(0.008)
winning party*aligned-president	0.345***
	(0.091)
Weights	Yes
Time x Mun. FE	Yes
Time x Party FE	Yes
Mun. x Party x Electoral Cylce FE	Yes
Observations	12,671,540
R^2	0.999

Notes: the table reports DID estimates based on equation 1.1 with an additional interaction of the treatment effect with a dummy that equates 1 if the mayor that just assumed local office is from the same party of the president of the republic. All regressions are weighted by the overall number of party members per municipality and include time x municipality fixed effects, time x party fixed effects, municipality x party x electoral cycle fixed effects. Robust standard errors are clustered by municipality and reported in parentheses. Significance ***at 1% level, **at 5%level, *at 10%level.

TABLE 1.7 DID Estimates of the effect of winning the election on Party Membership: local elections 1996 to 2012 - cross party movements

	DID	DID-11 parties
	(1)	(2)
winning party	0.010***	0.011***
	(0.001)	(0.001)
winning party*ideology		
Weights	Yes	Yes
Time x Party FE	Yes	Yes
Mun. x Party FE	Yes	Yes
Mun. x Party x Electoral Cycle FE	Yes	Yes
N	6,147,596	3,513,172
R^2	0.999	0.999

Notes: the table reports DID estimates based on equation 1.1 for a restricted sample of party members. In particular, it includes only individuals that joined a party in any period before local elections and did not leave the sample in the 12 months after each election (that is, it excludes individuals that become non members and it does not include those that joined a party during this time period). Controls include time x party fixed effects, municipality x party x electoral cycle fixed effects and municipality x time fixed effects. Regressions are weighted by the number of party members per municipality. Robust standard errors are clustered by municipality and reported in parentheses. Significance ***at 1% level, **at 5%level, *at 10%level.

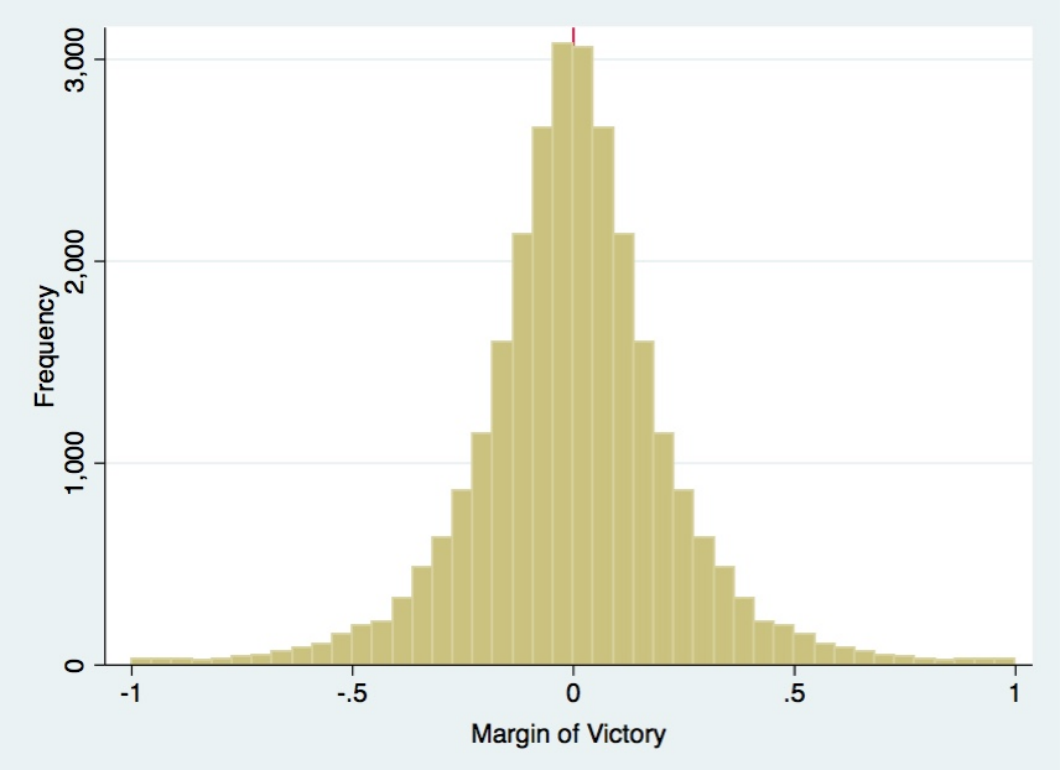
TABLE 1.8 Effect of winning the election on the probability of changing party

	(1)	(2)	(3)	(4)
	proportion of changers		log proportion of changers	
winning party	0.002		0.721***	
	(0.001)		(0.222)	
winning*ideo_distance	0.000	-0.001**	-0.102	-0.077**
	(0.001)	(0.000)	(0.066)	(0.035)
N	19,386,360	19,386,360	19,386,360	19,386,360
R^2	0.139	0.403	0.324	0.616
Party of Origin x Muni. x Time FE	Yes	Yes	Yes	Yes
Party of Origin x Party of Destin. x Muni. FE	Yes	Yes	Yes	Yes
Party of Destin. x Muni. x Time FE	No	Yes	No	Yes

Notes: the table reports regressions of the probability of changing party in the first year after the local elections conditional on the party of destination having won office and on the ideological distance between the party of origin and the party of destination. The dependent variable is the proportion (or the log of the proportion) of party members that moved to a destination party j in time t but were in the party of origin k in the month before the mayoral elections were held. The independent variables of interest are a dummy that equals 1 if the party of destination is in office at the local level in time t and the interaction of this dummy with the absolute value of the ideological distance between party of origin and party of destination. Robust standard errors are clustered by municipality and reported in parentheses and regressions are weighted by the overall number of members in the party of origin in the baseline period t_0. Significance ***at 1% level, **at 5%level, *at 10%level.

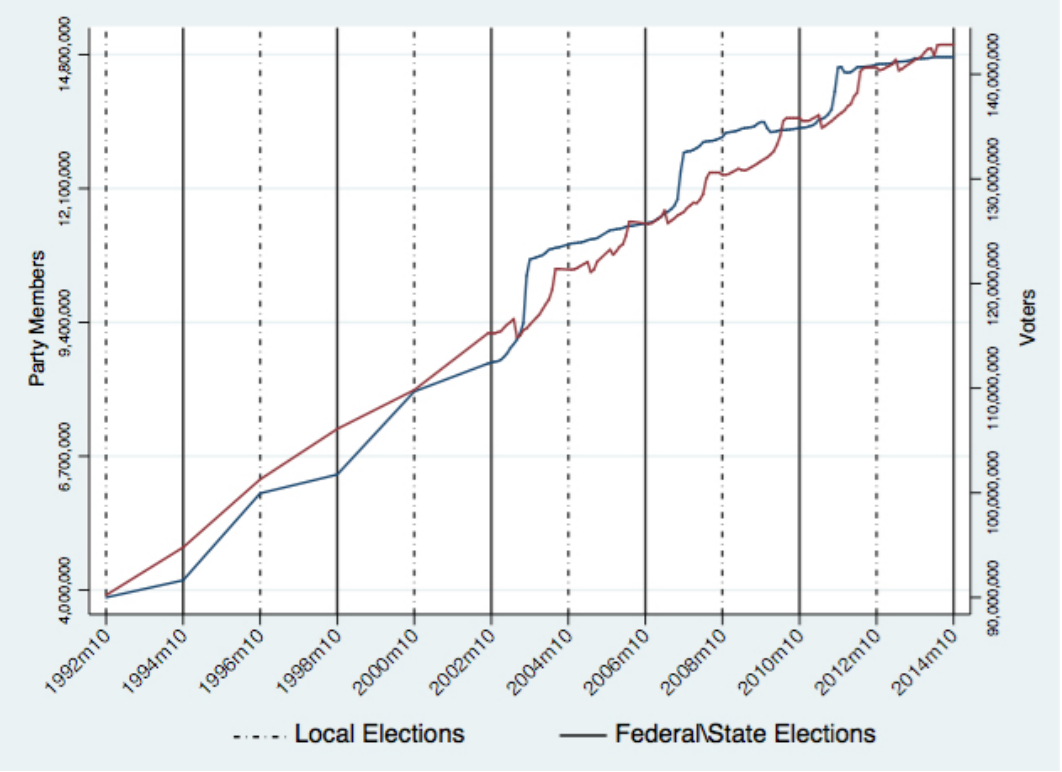
Appendix 1.A Supplementary tables and figures

FIGURE 1.A1 Histogram of Margin of Victory



Notes: the figure shows the histogram of the margin of victory in the two party electoral race.

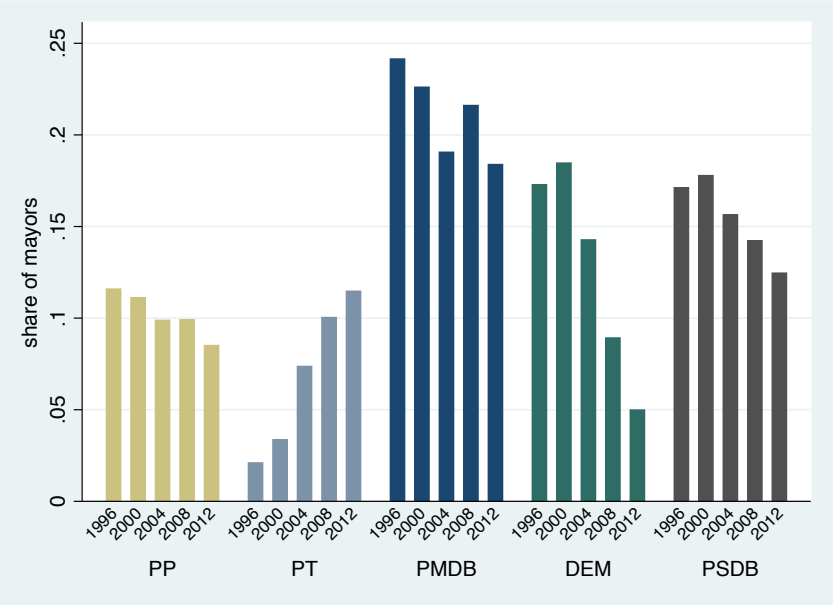
FIGURE 1.A2 Evolution of Voters and Party Members



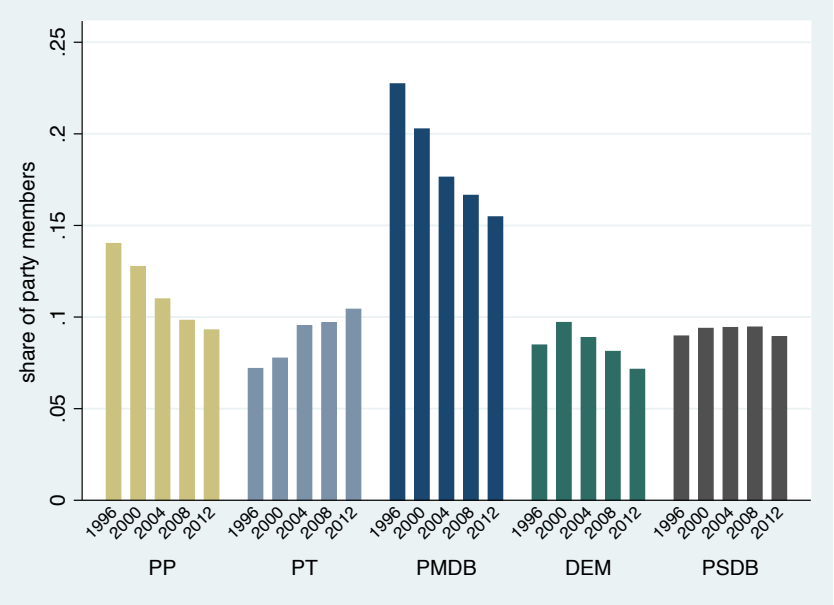
Notes: the figure reports the evolution of voters and party members by electoral period. The dashed vertical lines refer to local elections while the solid lines to federal and state elections.

FIGURE 1.A3 Descriptive Statistics on the Five Major Brazilian Political Parties

Panel A: Share of Elected Mayors by Election



Panel B: Share of Party Members by Election



Notes: the figures show the monthly evolution of party membership in Brazil from Oct/92 to Oct/14. Panel A illustrates the evolution of the stock of party members while Panel B the inflows and outflows. Vertical dashed lines indicate the time period in which local elections took place while solid lines indicate dates of federal and state elections.

TABLE 1.A1 Party members per number of occurrences in the data

Occurrences	Individuals	%
1	14,016,010	77.9
2	2,675,288	14.9
3	996,894	5.5
4	233,892	1.3
5	55,270	0.3
>5	16,076	0.1
Total	17,993,430	100.0

Notes: The table shows the number of party members by occurrence in the data.

TABLE 1.A2 Number of Elections with 2 candidates by Margin of Victory

Elections	Total	MV≤ 0.10	%	MV≤ 0.05	%	MV≤ 0.02	%
1996	2,577	1,105	45.0	603	24.6	260	10.6
2000	2,887	1,090	39.5	572	20.7	237	8.6
2004	2,639	1,232	46.8	654	24.8	253	9.6
2008	2,852	1,284	44.4	711	24.6	251	8.7
2012	2,988	1,434	48.1	766	25.7	330	11.1

Notes: The table shows the number and the proportion of two candidate races in Brazil by margin of victory of the winning candidate for each mayoral election held between 1996 and 2012.

Table 1.A3 RD Estimates (races with two candidates) - Alternative Specifications: local elections 1996-2012

	Parametric RD	Parametric RD	Non Parametric RD
	(1)	(2)	(3)
winning party	-0.057	0.285	0.467**
	(0.227)	(0.241)	(0.170)
p-order	1	2	
h			0.06
BW Type			IK
N	329,306	329,306	92,400
R^2	0.981	0.981	

Notes: The table shows parametric and non parametric RD estimates of the effect of winning the local elections on the membership share of political parties for different polynomial specifications and alternative bandwidth types. Significance ***at 1% level, **at 5%level, *at 10%level.

Table 1.A4 Placebo RD Estimates: local elections 1996-2012

	Non Parametric RD	Non Parametric RD
	(1)	(2)
winning party	0.196	0.285
	(0.152)	(0.308)
h	0.14	0.06
BW Type	CCT	IK
N	230,950	134,968

Notes: The table reports Placebo RD estimates of the effect of winning the local elections on the membership share of political parties for a window of 12 months before local elections. Significance ***at 1% level, **at 5%level, *at 10%level.

TABLE 1.A5 Effect of winning the election on the probability of changing party - Additional Controls

	(1)	(2)	(3)	(4)
	proportion of changers		log proportion of changers	
winning party	0.002		0.686***	
	(0.001)		(0.197)	
winning*distance	0.000	-0.001**	-0.097*	-0.072**
	(0.000)	(0.000)	(0.057)	(0.033)
N	19,386,360	19,386,360	19,386,360	19,386,360
R^2	0.142	0.403	0.338	0.623
Party of Origin x Muni. x Time FE	Yes	Yes	Yes	Yes
Party of Origin x Party of Destin. x Muni. FE	Yes	Yes	Yes	Yes
Party of Destin. x Time FE	Yes	Yes	Yes	Yes
Party of Destin. x Muni. x Time FE	No	Yes	No	Yes
Party of Origin x Party of Destin. x Time FE	No	Yes	No	Yes

Notes: the table reports regressions of the probability of changing party in the first year after the local elections conditional on the party of destination having won office and on the ideological distance between the party of origin and the party of destination. The dependent variable is the proportion (or the log of the proportion) of party members that moved to a destination party j in time t but were in the party of origin k in the month before the mayoral elections were held. The independent variables of interest are a dummy that equals 1 if the party of destination is in office at the local level in time t and the interaction of this dummy with the absolute value of the ideological distance between party of origin and party of destination. Robust standard errors are clustered by municipality and reported in parentheses and regressions are weighted by the overall number of members in the party of origin in the baseline period t_0. Significance ***at 1% level, **at 5%level, *at 10%level.

Chapter 2

The Returns to Political Loyalty: Evidence from the Bolsa Família Program

2.1 Introduction

The previous chapter ascertained that individuals opportunistically join political parties as a result of an electoral victory at the local level. This chapter combines the data set on Brazilian party members presented in the first chapter with data on the universe of recipients of the *Bolsa Família*, the largest Conditional Cash Transfer (CCT) program in the world, to study whether members of political parties in office are rewarded with targeted transfers. Specifically, it uses variation from the Brazilian mayoral elections of 2008 and 2012 to explore the returns to membership, at the local level, for the period 2005 to 2015.

There is extensive evidence that politicians distort the allocation of public resources (Finan & Mazzocco (2016)). Discretionary transfers, in particular, might be allocated to specific districts or particular geographic areas as typical forms of pork-barrel politics (Levitt & Jr. (1997), Voigtlaender & Voth (2014), and Carozzi & Repetto (2016)).

There is also convincing evidence that politicians claim electoral credit for income transfers targeted to particular groups of voters (Manacorda *et al.* (2011), de Janvry *et al.* (2012), De La O (2013), Labonne (2013) and Zucco (2013)).[1] Since voters respond to government policies, distortions driven by incentives to win and retain office might leave room for electoral exchanges between politicians and voters.

Indeed, a well-established body of literature on distributive politics discusses how political parties (once in office) allocate targetable goods promised before elections (Dixit & Londregan (1996)). One strand of literature follows Cox & McCubbins (1986) in arguing that transfers should be allocated to core supporters (rather than swing or opposing voters) given that the political preferences of this group of voters are known. Another strand of literature contends that allocating promised benefits to core voters might be a waste of resources, since members of this group would not switch their vote choices (Lindbeck & Weibull (1987)). However, once strategic interactions are taken into account, it remains unclear which group (swing or core voters) would be targeted with promised transfers (Cox (2009)).

On one hand, the secrecy of ballots might represent an obstacle to electoral exchanges since it restrains the ability of political machines to monitor voters' choices. In addition, politicians might not be able to credibly commit *ex ante* to particular types of *ex post* transfers (Robinson & Verdier (2013)). Yet, there is evidence of clientelism and vote buying, which suggests that, in practice, agents are able to circumvent the two-sided commitment problem (Brusco *et al.* (2013) and Diaz-Cayeros *et al.* (forthcoming)).

It has been argued that politicians might use knowledge of voters' social networks to infer individual vote choices or might hire local brokers to target more reciprocal individuals (Stokes (2005), Finan & Schechter (2012) and Larreguy *et al.* (2016).

[1]Note that while pork-barrel spending is typically associated with transfers that benefit all constituents in a certain district or area, redistributive transfers are usually in the form of redistributions from rich to poor voters. See also Baez *et al.* (2012) for a related study that shows evidence that targeted transfers influence political participation and political views.

Political machines might also implement alternative strategies of mobilization such as turnout buying (Dunning & Stokes (2007) and Nichter (2008)).[2]

Despite this, there is scarce empirical evidence on whether individuals are rewarded for their loyalty after casting a vote. In part this is due to the lack of adequate micro-data linking political support to the recipiency of benefits. Information on party membership at the individual level is usually private and not publicly disclosed, since it could be used to identify and punish political adversaries. Moreover, ballots are secret in modern democracies to ensure privacy and anonymity of votes.

In this respect, the paper by Hsieh *et al.* (2011), which documents punishments of opponents of the former President of Venezuela Hugo Chávez is a notable exception. Combining data from unsuccessful petitions that intended to remove Chávez from office with individual labor market outcomes, the study provides evidence that petition signers faced lower earnings and lower employment probability than non-signers once these lists were publicized.[3] The work by Maurer (2015), that investigates whether the German National Socialist party (NSDAP) used resources to benefit their voters after achieving a parliamentary majority in 1933, is also closely related.[4] Nonetheless, in authoritarian regimes, the ability to reward supporters or punish opponents is substantially higher than in established democracies at most times. Thus, whether and how individuals are rewarded when the party they support assumes office remains largely unexplored.

This study attempts to contribute to this literature. In particular it examines whether the probability of recipiency of a *Bolsa Família* transfer changes for party members and non members when political parties assume or leave municipal office.

[2]Note that Finan & Schechter (2012) show that political machines might use local brokers to target more reciprocal individuals either because they know individual political preferences, and prefer to "buy" their turnout (rather than their vote) or because reciprocity facilitates cooperation in a repeated game.

[3]In particular, Hsieh *et al.* (2011) use information consolidated in a database called *Maisanta* which contained a list of all registered voters as of March 2004 and an indicator of whether the individual signed the last of three referenda for a recall vote against former President Chávez. This information was later matched with labor market outcomes included in the Venezuelan Household Survey and used for political retaliation against opponents of the Chávez regime.

[4]Specifically, Maurer (2015) shows that public employment increased between 1933 and 1939 in cities where the NSDAP received a higher vote share.

The *Bolsa Família* is a major decentralized CCT program that was launched by the federal government of Brazil in the end of 2003, currently covering more than 23% of the Brazilian population (or 14 million households). Although widely advertised as a federal program, municipal governments play a central role in the implementation of *Bolsa Família*: they are responsible for selecting potential beneficiaries and for enforcing the program conditionalities. That could create incentives for local politicians or local bureaucrats to manipulate the program in their own favor. Indeed Brollo *et al.* (2017) find evidence that politicians with high electoral incentives manipulate the enforcement of conditionalities before elections (in particular those related to school attendance). In contrast to their work, this study focuses on whether politicians allocate transfers among voters after elections and notably on whether these transfers are distributed to supporters of political parties that gained access to office.

As a motivating evidence, this chapter starts by investigating whether shifts of party in power affect the recipiency of *Bolsa Família* at the municipal level. Results indicate that the recipiency of *Bolsa Família* increases by 0.4% in municipalities where a new party is in office.

In order to investigate whether these transfers are distributed to supporters of the party in office at the local level, this work then estimates the returns to party membership. The baseline results from the OLS regressions suggest that party members are 0.5% more likely to receive a BF transfer when their party is in office.

To deal with endogeneity issues from the OLS model that might bias estimates (since membership might be driven both by changes of party in office and endogenous movements in and out of political parties) and to rule out reverse causality issues (membership causing recipiency and not the other way around), this study proposes an instrumental variable approach. Specifically, it exploits the structure of the data and uses the membership of an individual in a baseline period pre-policy as an instrument for its membership in period t, which allows the variation to come entirely from parties assuming and leaving office and not from inflows and

outflows of members from political parties. The estimates from the IV regressions indicate that the returns to membership is of 0.3%.

This chapter later examines whether political parties allied to the party of the mayor are also rewarded with transfers and whether those in the opposition are more likely to be punished (losing transfers). Surprisingly, voters linked to groups other than the party of the elected mayor do not seem to be significantly rewarded with transfers as a result of their party being in office. This might be a signal that parties in office are distributing *Bolsa Família* transfers to core as oppose to swing voters. In addition, there seems to be no punishment for members of parties that competed in an election but lost the race, which could be explained by the fact that including additional individuals in the list of recipients of BF transfers might be less costly than removing them from the list of beneficiaries.

Finally, this study presents evidence that returns for members that join the party just before the local elections are higher than for those that join the party far in the past.

Overall, the evidence presented in this study seems consistent with a model where voters and political machines interact in a dynamic context, as largely sustained in works on vote-buying (Stokes (2005) and Finan & Schechter (2012)). An important feature of the data on membership is that the lists of party members are publicly available prior to elections. Thus, while politicians cannot observe individual votes, information on political membership could be used to infer the degree of loyalty of members or predict future vote choices. This work presents evidence that membership prior to local elections correlates highly with the vote share a party will receive in the election. Thus, political parties seem to be targeting individuals that were arguably loyal when casting their vote and that are arguably more responsive to transfers. Furthermore, since political support might be a function of the history of exchanges between voters and the parties they support, preserving partisan loyalties might explain how political machines deal with the commitment problem of rewarding individuals ex post (Diaz-Cayeros *et al.* (forthcoming)).

In estimating the returns to political affiliation this study closely relates to the literature that attempts to establish the value of political connections. In particular, it relates to the works of Fafchamps & Labonne (2017) and Gagliarducci & Manacorda (2017) that focus on returns of holding public office.[5] It also relates to works that explore whether those connections influence the allocation of road construction contracts or investments in land improvements (Lehne *et al.* (2016), Markussen & Tarp (2014)) In contrast to these studies, this chapter looks to the allocation of targeted transfers and provide evidence that political connections seem to be relevant when politicians distribute those resources at the local level.

This work finally contributes to the literature on the manipulation of policies near electoral periods (Ferraz (2007), Camacho & Conover (2011) and Brollo *et al.* (2017)). If targeted government policies affect voter choices, as widely documented, then politicians or bureaucrats may have high incentives to strategically manipulate these policies.[6]

The remainder of the paper is organized as follows. Section 2.2 provides a brief background on Brazils institutions, and Section 2.3 a description of the data used in the analysis. Section 2.4 documents motivating evidence used for this study. The empirical strategy to measure returns on party membership is discussed in Section 2.5 together with the paper's main empirical findings and possible interpretations. Section 2.6 concludes the paper.

2.2 Institutional Background

The analysis carried out in this chapter strongly relies on the institutional background introduced in chapter 1 (refer to section 1.2). In what follows I will then

[5] For measures of the value of political connections at the firm level see Fisman (2001) Khwaja & Mian (2005), Faccio (2006).

[6] Ferraz (2007) documents that politicians influence bureaucrats to manipulate the approval of environmental licenses for new industrial plants in urban Brazil. Camacho & Conover (2011) establish patterns of manipulation and strategic behavior by local politicians during the implementation of the first Census of the Poor in Colombia that was used as a targeting instrument to identify potential beneficiaries for a variety of social welfare programs in the country. Brollo *et al.* (2017) highlight that politicians manipulate the degree of enforcement of the conditionalities of the *Bolsa Família* program

focus only on the main features of the Bolsa Família Program.

2.2.1 Bolsa Família Program

Bolsa Família (BF) is a Conditional Cash Transfer (CCT) program targeted to the poor. It was created in 2003 by the federal government of Brazil as a result of the merging of four existing transfer programs (*Vale Gás, Vale Alimentação, Bolsa Escola* and *Fome Zero*). It is also the largest CCT program in the world, covering around 14 million households, or 23% of the Brazilian population.

BF is largely decentralized with each level of government playing a different and complementary role in the program. The Federal Government, through the Ministry of Social and Agrarian Development (MDSA), defines the budget, targets, eligibility criteria and conditionalities of the program. Municipal governments, in turn, are responsible for identifying and selecting potential beneficiaries. They are required to collect a rich set of information on poor families and include them into a national database called *Cadastro Único*, with registration in the *Cadastro* being a necessary condition for recipiency of a BF benefit.[7] This information is later processed by a federal bank (Caixa Econômica Federal - CEF) according to eligibility conditions defined by the MDSA. The MDSA thus decides who is eligible to receive the benefit (according to budget availability) and allows the CEF to provide the payments.[8]

Local governments are also responsible for enforcing the program's conditionalities (that are mainly school attendance for all school-age children in a family and certain health checkups and vaccinations for mothers and their children). Families that fail to comply with conditionalities might face different penalties that range from simple warnings to the loss of the BF transfer.

In contrast to other CCTs around the world such as the Mexican *Progresa* or the Colombian *Familias en Acción*, where the eligibility criteria depend on a composite

[7] The *Cadastro Único* was created in 2001 by the Federal Government and is the main registry for the purpose of programs and policies directed at low-income populations in Brazil.

[8] Recipients withdraw their stipend using ATM electronic cards.

score of a household's characteristics that proxy household income, BF benefits are granted based on self-reported income.[9] Households with declared monthly per capita income below certain thresholds qualify for the program and are entitled to different types of benefits. The first type of benefit is a fixed amount that follows an income criteria (basic benefit), while the second varies depending on the demographic composition of the household (variable benefit).[10] On average, a family receives *R*$163.06 ($49) per month and the yearly cost of the program is around 0.4% of the GDP (or 2.4% of federal expenditures).[11]

BF was not randomly implemented across the country. The MDSA first constructed poverty maps based on a national household survey (Pesquisa Nacional por Amostra de Domicílios - PNAD) and the Brazilian census to estimate the number of poor families in each municipality. It then promoted targeting with municipal quotas that were complemented with the information on poor families contained in the *Cadastro Único*.[12] However, there is evidence that local quotas are not strictly binding and in practice some municipalities have more beneficiaries than estimated measures would suggest.[13]

Despite arguments that BF is a typical example of a successful programmatic policy, recent official reports point to sizable irregularities in the program.[14] For instance, in one of the biggest attempts to scrutinize the BF program, the MDSA audited all BF transfers given during the period between 2013 to 2016 (in a joint work with federal prosecutors). The audits showed irregularities in more than 1 million monthly benefits (which is roughly 8% of all benefits given in a year). As a

[9] According to Soares *et al.* (2009) BF follows means test targeting that is only partially verified since information on income provided by municipalities used is basis to assign recipiency might be strictly self-declared and not always cross-checked with other national databases such as the *Cadastro Único* itself or social security data.

[10] Note that following the income threshold for participating in the program families are divided into poor and extremely poor. Extremely poor households qualify for both types of benefits and are not subject to conditionalities. Poor families, on the other hand, can only request variable benefits being subject to the program conditionalities.

[11] Information based on 2015 data.

[12] Note that, according to the MDSA, municipal quotas are updated throughout time.

[13] See Soares *et al.* (2009) for a discussion of the targeting process of *Bolsa Família*.

[14] See Fried (2012) or Hunter & Sugiyama (2013) for arguments of BF as a poverty program less prone to clientelism.

result of this work, 469 thousand families lost their benefit and other 654 thousand had them blocked.[15]

Furthermore, given the decentralized structure of the BF program, local politicians and bureaucrats might have the incentive to manipulate it in their favor. Indeed, Brollo *et al.* (2017) find evidence that politicians become more lenient with the enforcement of conditionalities (specifically those related to school attendance) in order to influence electoral outcomes. In particular, their results show that mayors that face higher electoral incentives (first-term mayors), and are connected with the federal government, tend to reduce the level enforcement of the program conditionalities before elections.[16]

2.3 Data

2.3.1 Data Description

The primary data source of this study is the longitudinal micro data on Brazilian party members described in subsection 1.3.1 of the first chapter. To measure the returns to party membership I use data on the universe of recipients of the *Bolsa Família* program, which was obtained from the MDSA. These data provides monthly information on all BF recipients since the start of the program (January of 2004), including names and surnames of family heads, their municipality of residence, the amount each family receives per month, and a social identification number that uniquely identifies each beneficiary.

Since the aim of this study is to investigate whether parties, once in power, reward their members at the local level, the data on party membership is matched with

[15]The audits consisted of federal prosecutors cross-checking information of recipients with other databases such as employment registries, electoral donations, company registrations, etc. Most irregularities discovered were associated to actual income of beneficiaries being higher than their (self) declared income. See www.raioxbolsafamilia.mpf.mp.br for more details on the investigation.

[16]There are also studies documenting manipulation of the eligibility status with individuals reducing their labor supply in order to participate in the program (Firpo *et al.* (2014)).

the data on mayoral electoral outcomes for the elections held in 2008 and 2012, which was presented in detail in the first chapter (refer to subsection 1.3.1).

To account for local characteristics that might be correlated both with membership and recipiency of the *Bolsa Família* this study also uses a large array of auxiliary data from different sources. Baseline characteristics of municipalities come from the 2000 census provided by the IBGE and include the share of the population with at least high school, share of white, share of men, illiteracy rate, share of urban population and Gini inequality index. Time varying covariates include administrative data from local budget from 2004 to 2015 provided by the National Treasury (Secretaria do Tesouro). I also use data on local population, GDP, birth rate, mortality rate, homicide rate and perinatal death rate which are also provided by the IBGE.[17] I finally use a cross-sectional dataset that describes the level of education of party members by municipality and party for the year of 2016 also obtained from the TSE.

2.3.2 Data Selection and Descriptive Statistics

As discussed in the previous chapter, the data on party members is longitudinal but incomplete by construction (since political parties that disappeared over the period are not observed in the data). Thus, the final sample of this analysis is composed of an average of 5,553 municipalities per year (out of 5,566), excluding electoral races that cannot be combined with membership data.

It should be noted that the unit of observation both in the data on beneficiaries of *Bolsa Família* and on party members is at the individual level. However, there is no unique information that allows us to perfectly match individuals across the data sets. In the empirical analysis, I then construct a panel with observations aggregated at the municipal level for each year of the period 2005 to 2015. To

[17]Note that rates for birth, mortality, homicide and perinatal death are measured per 100,000 inhabitants. In addition, GDP and data on local budget are deflated for 2015 prices using the IPCA, which is a consumer price index measured by the IBGE.

aggregate the observations at the yearly level this study uses the number of recipients and members of each political party as of December for each year of the period.

Table 2.1 reports descriptive statistics on averages of municipal characteristics for variables of interest between 2005 and 2015. As shown in the table roughly 13% of voters are recipients of *Bolsa Famıilia* and more than 16% are members of a political party. In addition, approximately 2.6% of voters are members of the party of the mayor and 4.4% are members of those parties in coalition with the mayoral candidate's party in an election. In contrast, members of parties that compete for office but lost represent 6.8% of voters, on average. Finally, 2.7% of voters are members of a party that did not compete for office at the local level.

2.4 Motivating Evidence

This chapter starts by documenting how gaining access to public office affects the overall recipiency of *Bolsa Família* at the local level. As discussed in the first chapter, winning municipal office gives political parties the access to public resources that might be distributed to their supporters. As a first piece of evidence on the returns to membership, one could then expect to observe a differential change in the probability of receiving a transfer in localities that experienced a change of party in office in comparison to those that did not. To examine if this is the case, this study exploits variation from the 2008 and 2012 Brazilian mayoral elections and estimates the following regression model:

$$b_{mt} = \alpha + \gamma D_{mt} + d_m + d_t + u_{mt} \tag{2.1}$$

where b_{mt} is the share of recipients of *Bolsa Família* among the voting population in municipality m and year t, D_{mt} is a dummy that equals 1 if there is a new party

in office in municipality m in year t and 0 otherwise, d_m is a municipality fixed effect, d_t is a time fixed effect and u_{mt} an error term.

The parameter of interest, γ, measures the differential change in the probability of recipiency and is identified through the comparison of changes in the share of beneficiaries of *Bolsa Família* in municipalities that did and did not change the party in office for each year of the period between 2005 and 2015.

Note that *ex ante* it is unclear which sign one should expect from point estimates of equation (2.1). For instance, parties that remain in power might have more political capital and a larger network than parties that just gained access to public office, which could result in greater success in rewarding voters with targeted transfers. On the other hand, politicians that just entered office might be very keen to build future political support or to reward those groups that helped them get elected in the first place. However, if *Bolsa Família* is a program shielded from the influence of local politicians, as sometimes argued, then the ability of incumbents to claim credit from the program would be limited, which could mitigate any electoral benefit from a higher coverage of the program.

Columns (1) to (5) of Table 2.2 present estimates of equation (2.1) with each column referring to a different specification. Robust standard errors are clustered at the municipality level in all regressions and reported in parentheses. Additive municipal and year dummies are included in all specifications with columns (2) to (5) also including the interaction between year and state dummies.

Results reported in the table indicate a positive and significant change in the probability of recipiency of the *Bolsa Família* in municipalities where a new party assumed office in comparison to municipalities where the same party remained in power. These results are also robust to the inclusion of a rich set of time varying covariates, baseline controls based on the 2000 census interacted with year dummies and municipality specific linear time trends. The inclusion of these controls attempt to account for the fact that municipalities where a new party is in office might differ in observed and unobserved time varying characteristics from

those localities where the same party remained in power, which could then affect the recipiency of BF transfers regardless of a change of party in power.[18]

Focusing on the estimate from the most saturated specification displayed in column (5), results suggest that the probability of recipiency increases by 0.4% (from a baseline probability of 13%) when there is a change of party in office at the local level in comparison to when there is not.

For robustness, Figure 2.1 shows an event study examining the existence of pre-trends in the recipiency of BF transfers as a result of changes in government at the local level. Since municipalities might experience various changes of party in office over time the sample of the exercise is restricted to municipalities that experienced a unique change of party in power between 2005 and 2015. That is, observations are dropped for localities that did not have any episode of shift in government. For those that changed party in power more than once I include only the first event in the sample. The model is estimated with a 3-year window before and after the electoral years of 2008 and 2012. Point estimates are standardized to their values in the electoral year. Overall, the Figure indicates that in localities with changes in government recipiency increases exclusively after a new party is in office.

As a final piece of motivating evidence, this chapter looks at the relation between membership of a political party and the votes received in an election. Although vote choices are anonymous, membership lists are publicly available to Brazilian political parties prior to local elections. Hence, while parties might not be able to observe vote choices at the individual level, information on membership could be used to infer the degree of loyalty of their members in an election or to predict future vote choices.

Table 2.3 combines electoral results of the four mayoral elections held between 1996 and 2012, and party membership data to examine this issue. Specifically, the table shows regressions of the lagged membership share of a given political party

[18]Time varying controls include the change in the real GDP per capita, number of voters (log) birth rate, death rate, perinatal death rate, real transfers per capita (log), vote share of the winning coalition, number of candidates that competed for office in the previous local election, electoral turnout and percentage of valid votes.

(for different periods before an election) on the vote share this party received in a mayoral election. For simplicity, the sample used in this analysis is restricted to the parties of the mayoral candidates. The unit of observation is a party in a given municipality in a certain election, with membership share defined as the ratio between the number of members of a certain party over the total number of members in a municipality. Note that all regressions include time x municipality fixed effects (that account for changes in local economic conditions or local institutions), time x party fixed effects (that control for nationwide changes in policy preferences of political parties or scandals that affect a party at the national level) and municipality x party fixed effects (that account for unobserved party characteristics that vary with locality). Robust standard errors are clustered at the municipal level and reported in brackets.

The point estimates presented in columns (1) to (5) indicate that membership prior to local elections is highly correlated with future vote shares. Although the magnitude of the estimates decreases monotonically with the lag of membership, the observed correlation still holds if one looks to the membership of a party almost two years before an election, as shown in column (5) of the table. Albeit the regressions do not necessarily identify a causal effect, the results seem to suggest that political parties would not be entirely mistaken in expecting some loyalty from their members at the ballots. This could explain, in part, the nature of the exchange relation between party members and political parties with parties targeting members *ex post* to sustain this dynamic relationship.

2.5 Returns to Party Membership

2.5.1 Empirical Model

As argued in subsection 2.3.2 the structure of the data does not allow me to match recipients of *Bolsa Família* with members of political parties. To estimate the effect of being connected to a party in office on the recipiency of *Bolsa Família*,

this study then relies on a municipal-level model, which is based on an individual-level specification presented in the Appendix of this chapter. The aggregated model to be estimated is as follows:[19]

$$b_{mt} = \alpha + \sum_{p=1}^{P} \beta_p a_{pmt} + \sum_{p=1}^{P} \gamma a_{pmt} * D_{pmt} + X'_{mt}\eta + d_m + d_t + u_{mt} \tag{2.2}$$

where b_{mt} is the share of recipients of *Bolsa Família* among the voting population in municipality m at a given period of time t, a_{pmt} is the share of members of party p in the same period and locality, D_{pmt} is a dummy variable that equates to 1 if party p is in office and 0 otherwise, X'_{mt} is a vector with a set of municipal characteristics, d_m is a municipality fixed effect, d_t year dummies, and u_{mt} is an error term.

Note that this strategy is flexible since it allows each party p to have different preferences with respect to the recipiency of the BF transfers as measured by parameter β_p. It has also the advantage of controlling for unobserved characteristics of municipalities (with d_m) and unobservable shocks that equally affect municipalities at the same time (with the control d_t). It finally controls for time varying observable municipal characteristics that are likely to be correlated both with recipiency of *Bolsa Família* and membership of a political party with X_{mt}.

The parameter γ effectively measures the change in the probability of recipiency for a member of party p before and after party p assumes office relative to changes in the probability of recipiency for members of other political parties and non members over the same period. In the aggregate model this effect is identified through changes in the share of recipients in a locality driven by changes in the share of members of the party in office.[20]

[19] See Appendix 2.A 1 for details on the aggregation of the model from the individual level.

[20] As discussed in subsection 1.3.1 of the previous chapter, the micro level of the data contains names and surnames of both party members and recipients of *Bolsa Família*. Potentially then, even if party members cannot be perfectly matched with beneficiaries, the model could be estimated at the the family level, linking party members and recipients through surnames, letting different families be connected to different parties at the same time. The causal effect of interest would then be identified through changes in the number of family members connected to a party that assumed public office, which would, in turn, still be driven by changes of party in power

Note, however, that in equation 2.2, the effect of membership is identified using both variation of parties assuming and leaving office and due to inflows and outflows of members into and out of political parties (which might be endogenous as discussed in the first chapter). Point estimates of γ might then be biased as a consequence of this strategical movements of party members since individuals could join (or leave) political parties just before (or after) the local elections in order to receive rewards (or avoid punishments). Additionally, it might be difficult to disentangle whether voters support parties as a result of being targeted or whether they are targeted as a result of their support.

In order to shed light on the causal effect of membership on recipiency and deal with the reverse causality mentioned, this study proposes an instrumental variable approach using the membership in a baseline period t_0 (pre-policy) as an instrument for membership in t. As argued in the first chapter, the structure of the data on party membership captures the timing of individual movements in and out of political parties. Restricting party membership to a baseline period before the start of the program thus allows all variation used to identify γ to come exclusively from changes of party in office. Explicitly, in the aggregate model, this identification strategy would allow the comparison of municipalities with similar characteristics and where the same party assumed office, with the variation coming solely from different membership shares (among voters) at baseline.

2.5.2 Baseline Results: Mayoral Candidate's Party

Table 2.4 presents OLS and IV estimates of equation (2.2). In particular, the Table displays results of the returns to membership in a given municipality, measured in terms of recipiency of *Bolsa Família*, for the years between 2005 and 2015.[21] The dependent variable is the share of beneficiaries in year t in a given municipality m and the variable of interest is the share of members among voters of the

at the local level. Nonetheless, that would require the use of data on the overall distribution of surnames in a municipality, which is not available.

[21]Estimates for reduced form and first-stage regressions are displayed in Table 2.A2 in the Appendix B.

party in government, for the same period and locality (with party in government defined as the party of the mayoral candidate). The unit of observation is thus a municipality in a given year. Regressions are weighted by the number of voters in each municipality. All specifications include municipality fixed effects and year dummies as a minimum set of controls. Robust standard errors are clustered at the municipal level and reported in parentheses.

Overall, OLS estimates (columns 1 to 5) show that the returns to membership are significantly positive. Note that estimates are robust to the inclusion of several controls that range from state dummies interacted with year dummies that account for unobservables that might vary with states over time (column 2), time varying covariates that include the percentage of valid votes, electoral turnout, number of candidates competing in the electoral race, vote share of the winning coalition, amount of federal transfers, birth rate, homicide rate, death rate, perinatal death rate, change of GDP per capita, number of voters (column 3), baseline covariates interacted with year dummies that include literacy rate, share of white, share of men, share of population with at least high school, Gini index, share of urban population (column 4) and baseline membership of parties interacted with year dummies that allows the baseline membership of each political party to follow a different trend over time in each municipality (column 5).

Focusing on the estimated coefficient of the most saturated OLS regression presented in column (5), the estimate show that the probability that a party member receives a transfer increases by 0.5% when his party assumes office. Reconciling this estimate with results displayed in column (4) of Table 2.2, this would imply that all individuals who receive a BF transfer due to changes of party in power would be members of the party that assumed local government (that is, members of the party of the mayor).

To deal with the fact that the OLS specification might not be able to disentangle the causal effect of membership on recipiency and that OLS estimates might suffer from an upward bias, columns (6) to (10) of the same table present estimates of the IV strategy proposed in the last subsection. In all regressions the share of

members of the party in government in year t is instrumented with its share at a baseline year before the start of the *Bolsa Família* program (set at 2003). Controls reported are the same as those used in OLS regressions (columns 1 to 5) and are included in the same order.

Although point estimates in columns (6) to (8) are not statistically significant, all estimated coefficients from the IV specification in columns (6) to (10) are lower in magnitude than the OLS coefficients (columns 1 to 5), as one would expect. Additionally, the inclusion of baseline covariates interacted with year dummies (column 9) brings significance to estimated coefficients, which indicates that the inclusion of further controls reduces the residual variance and gives more precision to estimates. This is also confirmed by the most saturated specification presented in column (10) (that has the same set of controls as the OLS model reported in column (5) of the table). Explicitly, the point estimate presented in column (10) indicate that the probability that a party member receives a BF transfer increases by 0.3% when his party assumes office.

2.5.3 Party Coalitions

The structure of the electoral data provided by the TSE enables me to distinguish all pertinent groups running for local office.[22] Notably, political parties can be grouped, according to mayoral electoral results, in the following categories: winning coalition, losing coalition, and parties not running for office. This allows me to examine how rewards are distributed within a winning coalition and whether those identifying with the opposition are more likely to be punished (as one would expect).

It is worth mentioning that when voting in electronic ballots in mayoral elections, Brazilians have to type the number of the party with a candidate to cast a valid vote. In other words, votes are not distributed among coalition allies and are

[22] As argued in subsection 2.3.1, Brazilian political parties usually run in coalition when competing in mayoral elections.

rather concentrated in the mayoral candidate's party (which might imply an expression of preferences for the party with a running candidate). For this reason, the winning and losing coalitions are further decomposed into the parties of the mayoral candidates and those allied that also composed the coalition competing for office (since effects might be more salient for members in the party of the mayor or for those in parties with candidates that lost an election).

Columns (1) to (10) of Table 2.5 show OLS and IV regressions of the effect of membership on the recipiency of BF transfers for all of those party groups. All specifications include municipality fixed effects and year dummies as a minimum set of controls. Regressions are weighted by the number of voters in each municipality. Robust standard errors are clustered at the municipal level and showed in brackets.

OLS estimates (columns 1 to 5) suggest that the probability that a party member receives a transfer indeed increases when his party assumes office. However, point estimates indicate that returns to membership are only statistically significant for members of the party of the mayor, which indicates that parties that assume office are not allocating transfers to the other parties of the government coalition. Furthermore, members of the parties that competed for office but lost the election do not seem to be punished as a result of being in the opposition. Focusing on the specification displayed in column (5), the point estimate reported in the first row show that members of the party of the mayor are 0.5% more likely to receive a BF transfer than non members or members of parties that did not compete for office in that locality.

To deal with the endogeneity and reverse causality issues mentioned in subsection 2.5.1, columns (6) to (10) of the same Table present IV estimates. In line with point estimates of the OLS regressions, estimates of the IV specifications are not significant for groups other than the members of the party of the mayor. Additionally, estimates loose precision when the model is fully saturated as showed in column (10) of the table.

Summing up, results presented so far can be interpreted as a sign of patronage with political parties targeting individuals that are expected to be loyal when casting their vote and are arguably more responsive to transfers. Considering that politicians run for office multiple times in subsequent periods, and also in different types of elections (majoritarian and proportional), this clientelistic relation could be the result of a sustained cooperation between voters and politicians. In this respect, note that political parties do not face term limits and can remain in office at the local level for an indefinite period (in contrast to mayors that face a two-term limit).

In addition, the fact that members of parties that lost the electoral race are not less likely to receive benefits might be explained by the fact that excluding individuals from the list of beneficiaries could be both more difficult and costly for municipal administrations than including them in the list of beneficiaries. Finally, note that negligible returns to members of other parties in the winning coalition that not the party of the mayor could be interpreted as sign that parties in office are targeting core in oppose to swing voters.

2.5.4 Robustness Checks

The validity of the research design relies on the assumption that members of a given party do not predict their party's electoral victory and therefore returns to membership are not anticipated. That is, the membership of a particular party only affects the local recipiency of *Bolsa Família* once this party assumes public office. I then perform an event study analysis of the effect of membership on the recipiency of *Bolsa Família* to investigate the existence of pre-trends on returns to membership. To perform this exercise, the sample is restricted to a single event per municipality with an event being defined as a locality having experienced a change of party in power between 2005 and 2015. Localities that did not change the party in power within this period are excluded from the sample. The model described below is built with a time window of three years around 2008 and 2012, which are the years where mayoral elections take place. Estimated coefficients are

measured relative to the electoral periods, which are the first year prior an entry in government and hence omitted. In formulas,

$$b_{mt} = \alpha + \sum_{k=-3}^{3} \gamma_k a_{mt_F} * D_k + X'_{mt}\eta + d_m + d_t + u_{mt} \tag{2.3}$$

Note that a_{mt_F} is the share of voters affiliated to the party in government in t_F for each municipality m, where t_F is equal to the first year after a change of party in power. Controls are the same as those included in (2.2).

Figure 2.2 graphically illustrates this event study analysis. Estimated coefficients are displayed in the figure with 95 percent confidence intervals.[23] For a given municipality, the dashed vertical line refers to 2008 or 2012 (the electoral years).

The figure shows that point estimates are nearly zero before an electoral year, which suggests no pre-trends on the returns to membership prior to a party assuming office. Moreover, there is a change in the gradient of the estimated coefficient precisely when a new party assumes government, which indicates that the effect of entry is significantly positive (that is, members of the party of the mayor are only rewarded when their party assumes office and not before), alleviating concerns with the validity of the proposed identification strategy.

Another possible concern with the regression model presented in equation (2.2) is that there might still be omitted variables correlated both with membership and recipiency that would confound the estimates. One major issue is that the estimated effect might be the result of parties rewarding voters rather than party members.

I try to address this issue including the vote share of each party competing for local office as an additional control in the most saturated OLS and IV specifications displayed in Table 2.4. Note that the vote share of those parties that are registered in a municipality but did not compete for office is set as equal to zero.

[23] Point estimates are depicted in column (2) of Table 2.A1 included in the Appendix 2.A 2. For transparency, results following different specifications of equation 3.3 are also reported in the Table.

Table 2.A3 present OLS and IV regressions of the effect of membership on the recipiency of BF with this additional control. Reassuringly, estimates of OLS and IV presented columns 1 and 3, respectively, are significant and very close in magnitude to estimates presented in columns (4) and (9) of Table 2.4, suggesting that these concerns are of second order.

2.5.5 Heterogeneous Effects

EDUCATION OF PARTY MEMBERS. Given that BF transfers are targeted to the poor, one could expect the returns to membership to be more salient to those members that are in the lower tail of the income distribution. As an attempt to examine this issue, this study uses a cross-sectional data on the level of education of party members for the year 2016.

These data detail the number of members per party and municipality in 8 different education categories (illiterate, literate, incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary) as of December 2016, which enables us to compute the proportion of members of party p in one of these 8 k education categories in a given locality. Relying on the assumption that this proportion is time invariant, I then reconstruct the membership level of parties by education category and municipality for the baseline year 2003.

Columns (1) to (5) of Table 2.6 show results of IV regressions of this heterogeneous effect. The model estimated follows closely (2.2) with the difference that the independent variable of interest (the share of members of the party in office) is disaggregated in those 8 education categories. Explicitly, each education category is defined as the proportion of members of the party in government among voters, measured as of 2003. All specifications include municipality fixed effects, year dummies and the baseline education of party members interacted with year fixed effects.

Results are presented in Table 2.6 and reveal that returns to membership are concentrated on literate individuals (defined as those with no schooling but able to read or write), which indicates that BF transfers are more likely to be distributed to those party members that are likely candidates to receive a *Bolsa Família* transfer rather to those in the middle and upper tail of the income distribution.

MARGIN OF VICTORY. The empirical findings have shown so far that members of a party that assumed office at the local level are significantly more likely to receive BF transfers. Nonetheless, it is still an open question whether parties that won an election with a large margin of victory (and therefore might have more political capital) are more able to reward members than those that won a close electoral race.

To address this issue, this chapter compares the returns to membership in close and safe electoral races. Given that Brazil has a multi-party system, to simplify the interpretation of results, sample used in this analysis is restricted to electoral races with only two candidates (or coalitions), which excludes roughly 50% of the observations used in the original sample. In practice, the variable of interest in equation (2.2) (the share of members of the party in office) is interacted with several dummies that group municipalities according to different margins of victory. Note that for races with two candidates the margin of victory is explicitly measured as the vote share of the winning candidate minus the vote share of the losing candidate.

Table 2.7 presents point estimates of IV regressions of the returns to membership by the margin of victory of the winning party.[24] Focusing on the specification reported in column (5) of the table, one can see a clear gradient across estimates. As illustrated in the first and second rows of column (5), returns to party membership seem to be considerably higher in localities where parties that won office were elected with a vote share higher than 55% (in comparison to localities where parties won tight races). Notably, there also seem to be significant returns to party

[24]For transparency, Table 2.A4 displayed in the Appendix 2.A 2 reports estimates of (2.2) for the restricted sample. Estimates are overall robust and in line with those presented in Table 2.4

membership in municipalities where mayors were elected in competitive elections, as can be seen in the third and fourth rows of column (5), which alleviate concerns that the estimated effect presented presented in Table 2.4 is driven by differential trends in unobservable characteristics of municipalities that are not accounted for in the main specification.

TIMING OF MEMBERSHIP. Causal estimates presented so far focused on members that joined a political party before 2004. However, returns to membership might be more salient to recent members than to those than joined the party far in the past. Additionally, the instrument proposed in subsection 2.5.2, that used membership shares based on a fixed year (set as 2003), cannot fully account for endogenous movements of party members (given that there is no variation in membership of parties that were registered in a municipality but did not compete for office in a certain electoral cycle).

Estimates presented in Table 2.A5 take these issues into consideration. Columns (1) to (4) of the Table report estimates of different IV regressions based on equation (2.2) with each column of the Table using the same set of controls but a different set of instruments that attempt to account for the endogeneity of membership.

Column (1) simply replicates the point estimate presented in column (9) of Table 2.4. As argued in subsection 2.5.2, this specification instruments the share of members of the party in government in t with its share in the baseline year 2003 (pre-policy). Column (2) to (4), in turn, present instrumented regressions based on membership shares of the party in government for the periods of 1 year, 6 months and 3 months before local elections, respectively. As discussed above, the IV specifications presented in columns (2) to (4) additionally instrument the actual membership share of the political parties that did not compete for office in an election.

The analysis of columns (1) to (4) of the table suggest that the magnitude of estimates is significantly higher when the instrument is based on more recent members rather than on older members. In particular, the point estimate of

column (4) is 1.1 percentage points higher than the one presented in column (1), which indicates that members of the party of the mayor that joined the party just before elections seem to derive slightly higher returns than those members that join the party far in the past.

2.6 Conclusion

This study employed a micro longitudinal data on Brazilian party members and data on the universe of recipients of the *Bolsa Família* program to examine whether voters are rewarded for their loyalty after casting their vote. First, it documents that voters are more likely to receive *Bolsa Família* in municipalities with a new party in power, which could be a signal that political parties that are successful in the elections might be allocating resources towards its supporters.

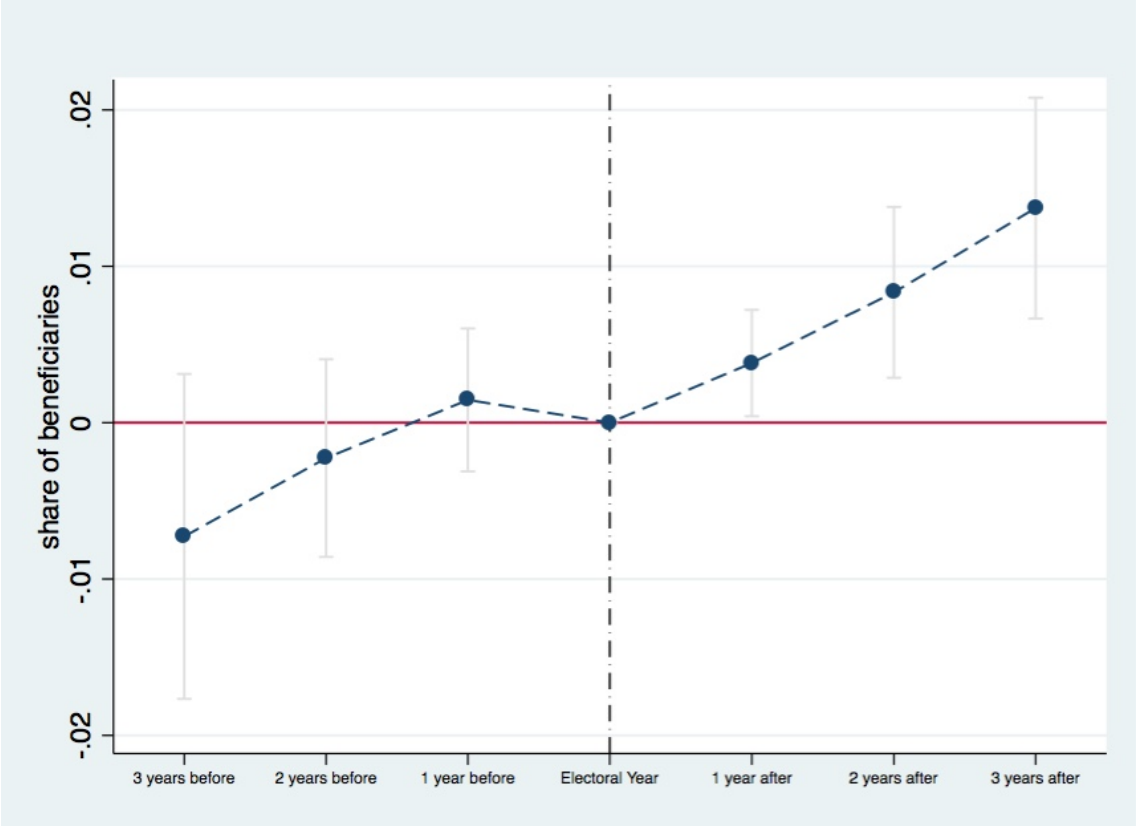
In this direction, the results of this chapter show that the observed effect on recipiency is mostly explained by an increase in the probability that members of the party of the mayor receive a BF transfer. In addition, returns to membership are concentrated on individuals with no schooling, which suggests that BF transfers are more likely to be distributed to party members that are likely candidates to receive the CCT transfer.

In contrast, members of parties that are in the opposition are not less likely to receive the BF transfers, which indicates that parties that gained access to office might be targeting solely their own supporters or might be too costly to penalize adversaries.

The evidence presented in this study seems consistent with vote-buying models with repeated interactions between political parties and voters. It additionally provides direct evidence of material rewards to party membership.

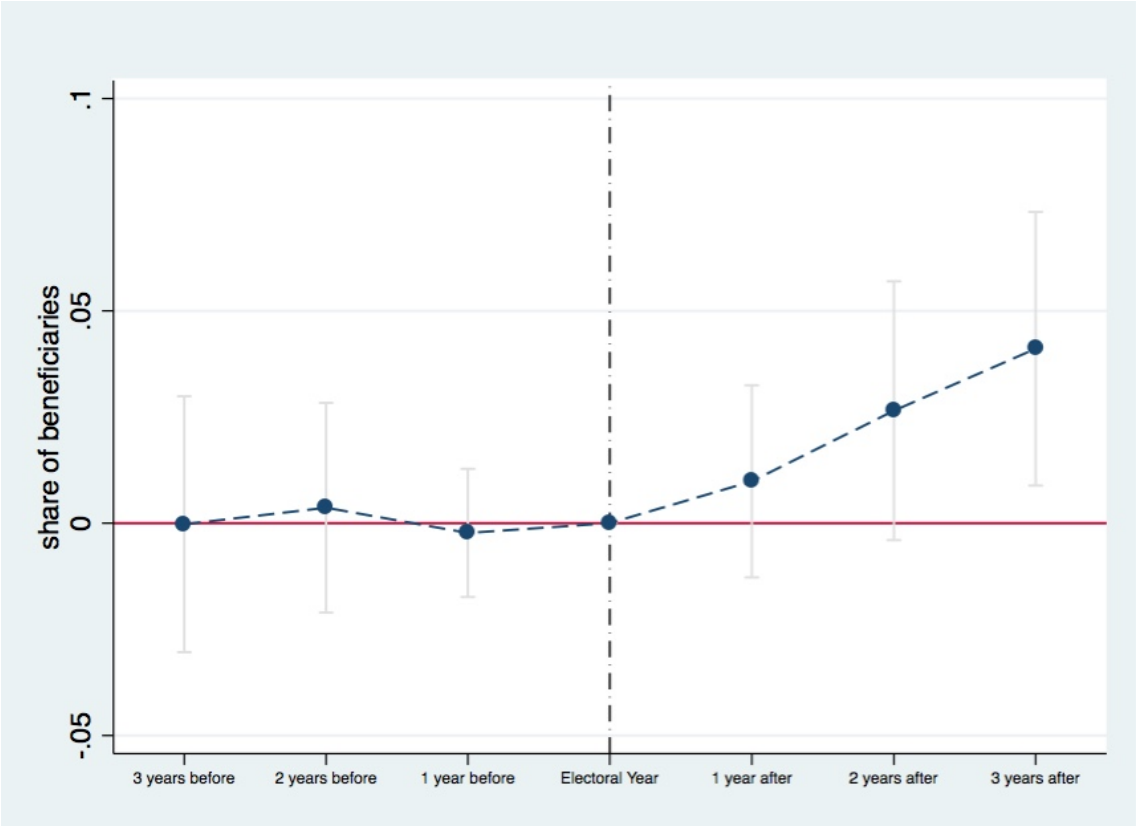
Tables and Figures

FIGURE 2.1 Event study of trends in the recipiency of BF driven by shifts of party in office: 2005-2015



Notes: the figure shows an event study of trends in the recipiency of *Bolsa Famılia* that are result of changes of party in office at the local level. The sample is restricted to a single event per locality during the period between 2005 and 2015, with an event defined as a change of party in power. The model is estimated with a 3-year window around the electoral years of 2008 and 2012 with point estimates standardized to their values in the electoral year. For reference, see section 2.4.

FIGURE 2.2 Event study of trends in returns to party membership: 2005-2015



Notes: the figure shows an event study of pre-trends in the recipiency of *Bolsa Família* as a result of changes in membership caused by parties assuming and leaving office in municipalities. The sample is restricted to a single event per locality during the period between 2005 and 2015, with an event defined as a change of party in power. The model is estimated with a 3-year window around the electoral years of 2008 and 2012 with point estimates standardized to their values in the electoral year. Estimated coefficients presented in the Figure are based on regressions illustrated in column (2) of Table 2.A1. See section 2.5.4 for reference.

Table 2.1 Descriptive Statistics

Municipal Characteristics: 2005-2015

	Mean	Std Dev.
% of beneficiaries	0.133	0.072
% of party members	0.165	0.077
% of members in the party of the mayor	0.026	0.030
% of members in party's allied to the mayor	0.044	0.037
% of members in parties that lost an election	0.068	0.045
% of members in parties that did not run	0.027	0.029
N	60,764	

Notes: the table reports descriptive statistics on averages of municipal characteristics for the variables of interest in the empirical analysis for the years between 2005 and 2015. Variables presented in each row of the table are reported as share of voters. The table shows the share of recipients of *Bolsa Famılia*, party members, party members affiliated with the party of mayor, party members of parties in coalition with the party of the mayor, party members affiliated to parties that lost an election and party members affiliated to parties that did not compete for local office.

TABLE 2.2 Effect of changes of party in office on the recipiency of *Bolsa Família*: 2005-2015

dep. variable: share of beneficiaries	(1)	(2)	(3)	(4)	(5)
change of party	0.141***	0.053*	0.074**	0.065**	0.059**
	(0.038)	(0.031)	(0.031)	(0.030)	(0.026)
N	60,794	60,794	60,794	60,794	60,794
R^2	0.919	0.946	0.950	0.989	0.995
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year x State FE	No	Yes	Yes	Yes	Yes
Time Varying Controls	No	No	Yes	Yes	Yes
Baseline Controls x Year FE	No	No	No	Yes	Yes
Municipality Trends	No	No	No	No	Yes

Notes: Columns (1) to (5) of the Table show DID estimates of the model described by equation (2.1) for the period 2005-2015. The dependent variable is the share of beneficiaries of *Bolsa Família* among the voters of a municipality. The independent variable is a dummy that equals 1 when there is a change of party in local government and 0 otherwise. Time varying controls include the change of the real GDP per capita, number of voters (log), birth rate, death rate, homicide rate, perinatal death rate, real transfers (log), vote share of the winner, number of coalitions competing the election, electoral turnout and percentage of valid votes. Pre-treatment covariates interacted with year dummies are based on 2000 Brazilian Census and include the share of the population with at least high school education, share of white, share of men, illiteracy rate, share of urban population, and local Gini Index. Robust standard errors are clustered by municipality and reported in parentheses. Significance *** at 1% level, ** at 5% level, * at 10% level.

TABLE 2.3 Effect of party membership on future vote share of parties: 1996-2012 mayoral elections

	Number of Months Before Elections				
	(1 to 3)	(4 to 6)	(7 to 12)	(13 to 18)	(19 to 24)
vote share $_t$	(1)	(2)	(3)	(4)	(5)
membership share $_{t-k}$	0.340***	0.327***	0.232***	0.075***	0.051***
	(0.028)	(0.028)	(0.021)	(0.014)	(0.014)
Time x Municipality FE	Yes	Yes	Yes	Yes	Yes
Time x Party FE	Yes	Yes	Yes	Yes	Yes
Municipality x Party FE	Yes	Yes	Yes	Yes	Yes
N	228,997	215,436	430,871	430,848	430,806
R^2	0.916	0.916	0.915	0.915	0.915

Notes: Columns (1) to (5) of the Table report regressions of the electoral vote share of a political party on their lagged membership share for different time windows before elections, considering all mayoral elections between 1996 and 2012. The dependent variable is defined as the number of votes a party received in a mayoral election over the total number of valid votes in that election. The lagged independent variable is defined as the number of members a party has in a certain period of time and municipality over the total number of party members in the same period and locality. Controls include time x municipality fixed effects, time x party fixed effects and municipality x party fixed effects. Robust standard errors are clustered by municipality and reported in parentheses. Significance ***at 1% level, **at 5%level, *at 10%level.

TABLE 2.4 Effect of being connected to a party in office on the recipiency of BF benefits: 2005 to 2015

dep. var.: share of beneficiaries	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) IV	(7) IV	(8) IV	(9) IV	(10) IV
share of gov members	0.085	0.075*	0.079**	0.072**	0.065**	0.052	0.042	0.042	0.053**	0.038*
	(0.055)	(0.042)	(0.038)	(0.028)	(0.027)	(0.045)	(0.033)	(0.029)	(0.023)	(0.022)
N	61,055	61,055	61,052	61,052	61,052	61,055	61,055	61,052	61,052	61,052
F	14.04	9.396	11.31	9.999	8.956	14.02	9.376	11.22	9.995	8.932
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FE	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Time Varying Controls	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Baseline Controls x Year FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Baseline Membership x Year FE	No	No	No	No	Yes	No	No	No	No	Yes

Notes: the table presents OLS (columns 1 to 5) and IV (columns 6 to 10) estimates of the effect of being member of a party that won office on the share of recipients of *Bolsa Família* at the municipal level for the period 2005 to 2015. In IV regressions the share of members of a party that gained access to office in t is instrumented by the share of members of the same party in a baseline period t_0 (pre-policy), which is set as 2003. All regressions are weighted by the number of voters per municipality. Time varying municipal controls include the change of the real GDP per capita, number of voters (log), birth rate, death rate, homicide rate, perinatal death rate, real transfers (log), vote share of the winner, number of coalitions competing the election, electoral turnout and percentage of valid votes. Pre-treatment local covariates (interacted with year dummies) are based on the 2000 Brazilian census and include the share of the population with at least high school, share of white, share of men, illiteracy rate, share of urban population, and local Gini Index. Baseline membership (interacted with time dummies) is defined as the share of voters affiliated to each political party at the baseline year 2003 (at the municipal level). Robust standard errors are clustered by municipality and reported in parentheses. Significance *** at 1% level, ** at 5% level, * at 10% level.

TABLE 2.5 Effect of party membership on the recipiency of BF benefits: 2005 to 2015

dep. var.: share of beneficiaries	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) IV	(7) IV	(8) IV	(9) IV	(10) IV
share of gov members	0.090*	0.091**	0.083**	0.082***	0.075**	0.037	0.051	0.036	0.068**	0.048
	(0.054)	(0.044)	(0.038)	(0.031)	(0.030)	(0.047)	(0.039)	(0.035)	(0.030)	(0.030)
share of gov coalition	0.013	0.027	0.010	0.016	0.019	-0.013	0.016	-0.001	0.017	0.017
	(0.023)	(0.017)	(0.017)	(0.017)	(0.017)	(0.026)	(0.018)	(0.019)	(0.018)	(0.018)
share of losing members	-0.002	0.013	0.003	0.003	0.006	-0.026	0.003	-0.009	0.003	0.002
	(0.023)	(0.017)	(0.018)	(0.017)	(0.017)	(0.026)	(0.019)	(0.020)	(0.019)	(0.019)
share of losing coalition	0.003	0.013	0.001	0.008	0.007	-0.013	0.009	-0.008	0.019	0.010
	(0.025)	(0.021)	(0.022)	(0.021)	(0.021)	(0.026)	(0.022)	(0.023)	(0.022)	(0.023)
N	61,055	61,055	61,052	61,052	61,052	61,055	61,055	61,052	61,052	61,052
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FE	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Time Varying Controls	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Baseline Controls x Year FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Baseline Membership x Year FE	No	No	No	No	Yes	No	No	No	No	Yes

Notes: the table presents OLS (columns 1 to 5) and IV (columns 6 to 10) estimates of the effect of membership changes (driven by party switches in local office) on the share of recipients of *Bolsa Família* at the municipal level for the period 2005 to 2015. The dependent variable is the share of recipients (among voters) in a given locality in a certain year. The independent variables of interest are defined as the number of party members in each group among voters. In IV regressions the share of members of each group in t is instrumented by its share in a baseline period t_0 (pre-policy), which is set as 2003. All regressions are weighted by the number of voters per municipality. Time varying municipal controls include the change of the real GDP per capita, number of voters (log), birth rate, death rate, homicide rate, perinatal death rate, real transfers (log), vote share of the winner, number of coalitions competing the election, electoral turnout and percentage of valid votes. Pre-treatment local covariates (interacted with year dummies) are based on the 2000 Brazilian census and include the share of the population with at least high school, share of white, share of men, illiteracy rate, share of urban population, and local Gini Index. Baseline membership (interacted with time dummies) is defined as the share of voters affiliated to each political party at the baseline year 2003 (at the municipal level). Robust standard errors are clustered by municipality and reported in parentheses. Significance *** at 1% level, ** at 5% level, * at 10% level.

TABLE 2.6 Heterogeneous effect of being connected to a party in office on the recipiency of BF by level of education of party members: 2005 to 2015

	(1)	(2)	(3)	(4)	(5)
dep. var.: share of beneficiaries					
illiterate	-0.102	-0.137	-0.082	-0.143	-0.143
	(0.199)	(0.180)	(0.175)	(0.168)	(0.169)
literate	0.098*	0.095*	0.076	0.087*	0.087*
	(0.055)	(0.051)	(0.049)	(0.048)	(0.048)
incomplete primary	-0.030	-0.029	-0.022	-0.024	-0.024
	(0.046)	(0.042)	(0.042)	(0.041)	(0.041)
complete primary	0.435*	0.347*	0.285	0.205	0.205
	(0.233)	(0.210)	(0.206)	(0.206)	(0.206)
incomplete secondary	-0.349***	-0.301**	-0.249**	-0.161	-0.161
	(0.135)	(0.122)	(0.118)	(0.115)	(0.115)
complete secondary	-0.184	-0.135	-0.146	-0.192	-0.192
	(0.143)	(0.129)	(0.127)	(0.126)	(0.126)
incomplete tertiary	0.027	0.212	0.117	0.200	0.200
	(0.474)	(0.426)	(0.415)	(0.412)	(0.414)
complete tertiary	0.045	0.025	0.069	0.148	0.148
	(0.207)	(0.185)	(0.183)	(0.184)	(0.185)
N	61,055	61,055	61,052	61,052	61,052
F	10.32	9.97	16.41	16.09	15.99
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Baseline Educ. of Members x Year FE	Yes	Yes	Yes	Yes	Yes
Year x State FE	No	Yes	Yes	Yes	Yes
Time Varying Controls	No	No	Yes	Yes	Yes
Baseline Controls x Year FE	No	No	No	Yes	Yes
Baseline Membership x Year FE	No	No	No	No	Yes

Notes: Columns (1) to (5) of the table show estimates of the effect of being connected to a party in office on the share of recipients of *Bolsa Família* for the period 2005 to 2015. Regressions follow the model described by equation (2.2) with the difference that the variable of interest is disaggregated in eight education categories. Each category is defined as the proportion of members of the party in government among voters (all measured at the baseline year of 2003). Time varying municipal controls include the real gdp per capita (log), number of voters (log), birth rate, death rate, homicide rate, real transfers (log), vote share of the winner, number of coalitions competing the election, electoral turnout and percentage of valid votes. Pre-treatment local covariates (interacted with year dummies) are the same as those used in Table 2.4. Baseline membership is defined as before and also interacted with year dummies. Party members baseline education is defined as the share of members in each party per education category in a certain municipality and it is also interacted with year fixed effects. Robust standard errors are clustered by municipality and reported in parentheses. Significance *** at 1% level, ** at 5% level, * at 10% level.

TABLE 2.7 Heterogeneous effect of being connected to a party in office on the recipiency of BF benefits in races of two candidates by margin of victory: 2005-2015

dep. var.: share of beneficiaries	(1)	(2)	(3)	(4)	(5)
share of gov*$[MV > 0.20]$	0.136	0.102***	0.099***	0.119***	0.107***
	(0.089)	(0.036)	(0.035)	(0.031)	(0.031)
share of gov*$[0.20 \geq MV > 0.10]$	0.223*	0.151***	0.127***	0.115***	0.101***
	(0.134)	(0.055)	(0.045)	(0.035)	(0.032)
share of gov*$[0.10 \geq MV > 0.05]$	0.172*	0.057*	0.061**	0.075***	0.070***
	(0.088)	(0.033)	(0.030)	(0.026)	(0.026)
share of gov*$[MV \leq 0.05]$	0.140*	0.063*	0.053*	0.051**	0.046**
	(0.081)	(0.033)	(0.028)	(0.023)	(0.023)
N	31,035	31,035	31,035	31,035	31,035
F	8.42	7.43	8.61	9.21	8.46
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year x State FE	No	Yes	Yes	Yes	Yes
Time Varying Controls	No	No	Yes	Yes	Yes
Baseline Controls x Year FE	No	No	No	Yes	Yes
Baseline Membership x Year FE	No	No	No	No	Yes

Notes: Columns (1) to (5) of the table report estimates of the heterogeneous effect of being connected to a party that assume office on the share of recipients of *Bolsa Família* for the period 2005 to 2015, at the municipal level. The estimated model follows (2.2) but the variable of interest is interacted with 4 dummies of the margin of victory of parties that won the previous election and therefore are in office. The sample is restricted to races of two candidates (or two coalitions). Controls follow those presented in 2.4. Robust standard errors are clustered by municipality and reported in parentheses. Significance *** at 1% level, ** at 5% level, * at 10% level.

Appendix 2.A 1 Empirical Model

At the individual level the econometric model is described by the following equation:

$$B_{ipmt} = \alpha + \beta_p A_{ipmt} + \eta D_{pmt} + \gamma A_{ipmt} * D_{pmt} + u_{ipmt} \tag{2.4}$$

where B_{ipmt} is a variable equal to 1 if individual i is a recipient of Bolsa Família in municipality m and time t, A_{ipmt} is a variable that equals 1 if individual i is a member of party p in municipality m and time t, D_{pmt} is a dummy variable that is equal to 1 if party p is in office in the same locality and time period and 0 otherwise, and u_{ipmt} an error term.

At the municipal level:

$$B_{mt} = N_{mt}\alpha + \beta_p \sum_{p=1}^{P} A_{pmt} + \eta N_{mt} + \gamma \sum_{p=1}^{P} A_{pmt} * D_{pmt} + N_{mt} * u_{mt} \tag{2.5}$$

where B_{mt} is the number recipients of Bolsa Família in locality m and time t, N_{mt} is the total number of individuals in municipality m and time t, A_{pmt} is the number of party members from party p in the same locality and time period, d_t time fixed effects, and u_{mt} an error term.

Dividing (5) by N_{mt}, which is defined as the voting population in municipality m and year t:

$$b_{mt} = \alpha + \beta_p \sum_{p=1}^{P} a_{pmt} + d_t + \gamma \sum_{p=1}^{P} a_{pmt} * D_{pmt} + e_{mt} \tag{2.6}$$

where b_{mt} is the share of individuals receiving benefits among the voters in year t

and locality m, a_{pmt} is the of members of party p among the voters in the same locality and year, d_t year dummies, D_{pmt} is defined as above, and e_{mt} an error term.

Appendix 2.A 2 Supplementary tables and figures

Table 2.A1 Event study of trends in the returns to party membership: 2005 to 2015

	(1)	(2)	(3)
	share of beneficiaries		
[3 years before]*share of gov members	-0.023	-0.000	0.002
	(0.028)	(0.015)	(0.014)
[2 years before]*share of gov members	-0.008	0.004	-0.001
	(0.016)	(0.013)	(0.012)
[1 year before]*share of gov members	-0.010	-0.002	-0.002
	(0.011)	(0.008)	(0.007)
[Electoral Year]*share of gov members	-	-	-
[1 year after]*share of gov members	0.018	0.010	0.002
	(0.017)	(0.012)	(0.011)
[2 years after]*share of gov members	0.048*	0.026*	0.014
	(0.028)	(0.017)	(0.013)
[3 years after]*share of gov members	0.066**	0.041**	0.025*
	(0.047)	(0.016)	(0.020)
N	34,733	34,729	34,729
F	2.40	1.77	1.01
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Baseline Membership x Year FE	Yes	Yes	Yes
Year x State FE	No	Yes	Yes
Baseline Controls x Time FE	No	No	Yes

Notes: Columns (1) to (3) of the table present estimates of an event study that investigates the pre-trends in returns to party membership. The model estimated is based on equation (2.2) and uses a three year window around the electoral years of 2008 and 2012. Point estimates are standardized to their value in the electoral years. The sample is restricted to a unique event per municipality. That is, municipalities that did not experience a change of party between 2005 and 2015 are excluded from the analysis. In addition, for localities that faced more than one shift of party in government during the same period I only include the first event in the sample. Robust standard errors are clustered by municipality and reported in parentheses. Significance *** at 1% level, ** at 5% level, * at 10% level.

TABLE 2.A2 Reduced form and first stage regressions of effect of party membership on the recipiency of BF benefits: 2005 to 2015

dep. var.: share of beneficiaries	(1) reduced	(2) reduced	(3) reduced	(4) reduced	(5) reduced	(6) first stage	(7) first stage	(8) first stage	(9) first stage	(10) first stage
share of gov members	0.044	0.035	0.035	0.044**	0.031*	0.840***	0.833***	0.832***	0.832***	0.833***
	(0.038)	(0.027)	(0.025)	(0.020)	(0.019)	(0.018)	(0.013)	(0.013)	(0.012)	(0.012)
Observations	61,055	61,055	61,052	61,052	61,052	61,055	61,055	61,052	61,052	61,052
F	14.02	9.373	11.20	9.973	8.918	2,148	3,927	4,004	4,924	5,199
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year x State FE	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Time Varying Controls	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Baseline Controls x Year FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Baseline Membership x Year FE	No	No	No	No	Yes	No	No	No	No	Yes

Notes: the table presents estimates of reduced form (columns 1 to 5) and first stage (columns 6 to 10) regressions of the effect of being a member of a party that won office on the share of recipients of *Bolsa Família* at the municipal level for the period 2005 to 2015. Robust standard errors are clustered by municipality and reported in parentheses. Significance *** at 1% level, ** at 5% level, * at 10% level.

TABLE 2.A3 Robustness Checks: effect of being connected to a party in office on the recipiency of BF benefits controlling for the vote share: 2005-2015

dep. var.: share of beneficiaries	(1) OLS	(2) OLS	(3) IV	(4) IV
share of gov members	0.062***	0.053***	0.048**	0.031
	(0.020)	(0.019)	(0.020)	(0.019)
N	61,052	61,052	61,052	61,052
F	7.86	7.23	7.83	7.21
Municipality FE	Yes	Yes	Yes	Yes
Year x State FE	Yes	Yes	Yes	Yes
Baseline Controls x Year FE	Yes	Yes	Yes	Yes
Time Varying Controls	Yes	Yes	Yes	Yes
Baseline Membership x Year FE	No	Yes	No	Yes
Coalition's Vote Share	Yes	Yes	Yes	Yes

Notes: the Columns (1) to (4) of the table show estimates of the effect of being a member of a party that won office on the share of recipients of *Bolsa Família* at the municipal level for the period 2005 to 2015. In IV regressions the share of members of a party that gained access to office in t is instrumented by the share of members of the same party in a baseline period t_0 (pre-policy), which is set as 2003. All regressions are weighted by the number of voters per municipality. For details on time varying municipal controls, pre-treatment local covariates and baseline membership of each party see Table X. A coalition's vote share is defined as the vote share of each party participating in an election. The vote share of those that did not run is set to zero. Robust standard errors are clustered by municipality and reported in parentheses. Significance *** at 1% level, ** at 5% level, * at 10% level.

TABLE 2.A4 Effect of being connected to a party in office on the recipiency of BF benefits in races with two candidates: 2005 to 2015

	(1)	(2)	(3)	(4)	(5)
dep. var.: share of beneficiaries	IV	IV	IV	IV	IV
share of gov members	0.101	0.056**	0.042*	0.063***	0.048**
	(0.063)	(0.027)	(0.024)	(0.022)	(0.022)
N	31,035	31,035	31,035	31,035	31,035
F	8.98	7.93	9.10	9.54	8.81
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year x State FE	No	Yes	Yes	Yes	Yes
Time Varying Controls	No	No	Yes	Yes	Yes
Baseline Controls x Year FE	No	No	Yes	Yes	
Baseline Membership x Year FE	No	No	No	No	Yes

Notes: the table presents IV estimates of the effect of being member of a party that won office on the share of recipients of *Bolsa Família* at the municipal level for the period 2005 to 2015. The sample is restricted to races with only two candidates (or two coalitions). The share of members of a party that gained access to office in t is instrumented by the share of members of the same party in a baseline period t_0 (pre-policy), which is set as 2003. All regressions are weighted by the number of voters per municipality. Time varying municipal controls include the change of the real GDP per capita, number of voters (log), birth rate, death rate, homicide rate, perinatal death rate, real transfers (log), vote share of the winner, number of coalitions competing the election, electoral turnout and percentage of valid votes. Pre-treatment local covariates (interacted with year dummies) are based on the 2000 Brazilian census and include the share of the population with at least high school education, share of white, share of men, illiteracy rate, share of urban population, and local Gini Index. Baseline membership (interacted with time dummies) is defined as the share of voters affiliated to each political party at the baseline year 2003 (at the municipal level). Robust standard errors are clustered by municipality and reported in parentheses. Significance *** at 1% level, ** at 5% level, * at 10% level.

TABLE 2.A5 IV regressions of the effect of being connected to a party in office on the recipiency of BF benefits: 2005 to 2015

share of beneficiaries	(1) baseline 2003	(2) 1 year before	(3) 6 months before	(4) 3 months before
share of gov members	0.053**	0.059**	0.064**	0.064**
	(0.023)	(0.027)	(0.029)	(0.028)
N	61,065	61,065	61,068	61,068
F	9.99	20.49	18.58	18.58
Municipality FE	Yes	Yes	Yes	Yes
Year x State FE	Yes	Yes	Yes	Yes
Time Varying Controls	Yes	Yes	Yes	Yes
Baseline Controls x Time FE	Yes	Yes	Yes	Yes

Notes: the table presents IV estimates of the effect of being member of a party that won office on the share of recipients of *Bolsa Família* at the municipal level for the period 2005 to 2015. In column (1) the share of members of a party that gained access to office in t is instrumented by its share in a baseline period t_0 (pre-policy), which is set as 2003. In turn, in columns (2) to (4) the share of members of the party of the mayor t is instrumented by its share in different periods before local elections (1 year, 6 months and 3 months, respectively). All regressions are weighted by the number of voters per municipality. Time varying controls and baseline controls interacted with year dummies are defined as in Table 2.4. Robust standard errors are clustered by municipality and reported in parentheses. Significance *** at 1% level, ** at 5% level, * at 10% level.

Chapter 3

Wage Transparency in Brazil

3.1 Introduction

This chapter uses data on the universe of Brazilian public sector employees from 2003 to 2015 and variation from a wage transparency policy implemented at the state level to shed light on the effects of transparency in wages inequality in the public sector.

The main idea behind transparency policies is that the access to publicly available information on government activities, institutions and policies enable voters to hold public officials accountable for their performance (Persson & Tabellini (2000), Besley & Burgess (2002) and Abrahams *et al.* (2016)). Public disclosure of information might also improve governance since better informed citizens can demand effectiveness in the delivery of public goods and services (Pande (2011), Olken & Pande (2012), Kosack & Fung (2014)).

Indeed there is documented evidence that voters respond to disclosure of information on actions of those in government, for instance, disciplining incumbent politicians that misbehaved in office (Ferraz & Finan (2008), Banerjee *et al.* (2011) and Bobonis *et al.* (2016)). Empirical studies also suggest that politicians respond to better informed citizens and that new information affects governance (Ferraz

& Finan (2011) and Banerjee *et al.* (2016)). There is also evidence that the disclosure of information on politicians is correlated with measures of the quality of governments (Djankov *et al.* (2010)).

There is less evidence, however, on the effects of wage transparency policies in the public sector. In part, this is explained by the fact that salaries in the public sector tend to be based on a rigid pay structure with little dispersion relative to their counterparts in the private sector, even if civil servants enjoy a substantial wage premium when compared to private sector workers (Finan *et al.* (2017)). Additionally, pay transparency policies are controversial, since disclosure of salaries by name could undermine individual privacy, and are not widely adopted across the world. Recent studies have also documented unintended effects of wage transparency, such as the decline in job and pay satisfaction among workers with earnings below the median and increase in the well-being gap between rich and poor (Card *et al.* (2012) and Perez-Truglia (2016)).[1] As a result, individual level data on public sector earnings is typically not available.

This paper aims to provide evidence on the impact of pay transparency on earnings inequality in the public sector. Following a law that mandated disclosure of salaries, in June of 2012 the central government of Brazil published individual level wages of all its employees on a transparency website. Although the law mandated wage disclosure exclusively for public employees working for the executive branch of the federal government, evidence suggests that the executive power of states and local governments followed the central government and also adopted pay transparency policies in different points in time.

To estimate the effect of disclosure on wages inequality of civil servants I then use variation from the timing state governments of Brazil posted salaries online

[1] In particular, employing a survey on a random sample of faculty and staff members of the University of California that had their salaries published online after a court decision, Card *et al.* (2012) documented that individuals with wages below the median reported lower job and pay satisfaction and were more likely to search for a new job after comparing their earnings with their peers. Perez-Truglia (2016) shows that income transparency significantly affected the well-being of individuals in Norway. Specifically, online disclosure of tax records data increased the well-being gap between rich and poor Norwegians as a result of self-image concerns, driven by changes in self-perceived income rankings.

together with administrative data on public sector pay from a matched employer-employee database. These data provide universal coverage of the Brazilian public sector for each year of the period between 2003 to 2015 and in particular detail, for every public employee, the occupation and the average monthly wage over the employment spell during the year. This helps to overcome the fact that wages posted online by state administrations are not displayed in a single repository and typically do not contain pre-disclosure information. Additionally, although I do not have information that allows tracking individuals over time or across multiple employers, I use the fact that state employees work in offices located in different localities within a state with the detailed information on occupation from the data to perform aggregate analysis both at the municipal and occupational level. Focusing on state rather than federal level employees also allows me to separate the effect of disclosure from a time trend. Finally, each state government has administrative autonomy to set pay scales and define salaries of public employees working at the state level.

Overall, this study documents that disclosure of wages significantly reduced wage dispersion of state level employees, with effects concentrated in the upper tail of the log earnings distribution. On average, the 90/50 wage gap of public servants fell by 8.3 percentage points in states that adopted pay transparency in comparison to those that did not. There is no evidence that decile gaps below the fifth decile were differentially affected by the policy.

Note that the adoption of wage transparency policies was not randomly implemented by state administrations. Thus, municipalities located in states that did not adopt the policy might not represent a valid control group to those in states that did adopt pay transparency. To address this concern and to control for unobserved characteristics of states that might vary over time and could be correlated with the adoption of transparency, I propose a triple-differences design that compares localities within states. Specifically, this strategy compares municipalities with high and low baseline levels of wage inequality (measured in the pre-disclosure period) in states that did and did not disclose information on pay of civil servants at the individual level. The evidence presented indicates that wages compressed

within states as a result of pay transparency. In particular, the average 90/50 earnings gap of public employees decreased by 2.2 percentage points if one considers a baseline difference of 10 percentage points in the wage dispersion of municipalities with high and low levels of inequality.

In estimating the impact of pay disclosure on the wage structure of the public sector this work is closely related to Mas (2017) who provides evidence that mandated wage disclosure of municipal employees of California reduced pay compensation of city managers. It also relates to Mas (2016) that investigates how mandated disclosure of earnings affected CEOs compensation. [2] Consistent with his findings this paper shows that the effect of disclosure is more salient for top earners in the public sector. In contrast to his work, however, this study does not focus on a particular geographical area within a country and also examines whether transparency affected other parts of the public sector log earnings distribution (rather than the highest paid job). It additionally provides evidence on the occupations that were mostly affected by the adoption of pay transparency policies within a state.

When analyzing the effects of transparency on earnings inequality this study also relates to the literature on causes and consequences of income inequality (Lee (1999), Autor *et al.* (2008), Bosch & Manacorda (2010) and Autor *et al.* (2016)). And in particular to the strand of this literature that examines how occupations contribute to changes in the wage structure (Firpo *et al.* (2011) and Fortin & Lemieux (2016)). This paper provides evidence that the observed reduction in the gap between top deciles in comparison to the median is driven by lower returns to top paid jobs in the public sector rather than by changes in the composition of top paid occupations.

[2] Specifically, Mas (2017) uses variation from a law that mandated disclosure of pay compensation of municipal employees in California and compares early adopters (cities that disclosed salaries previous to the mandate) to late adopters (cities that disclosed salaries only after the mandate). The study shows that disclosure compressed wages of city managers (in particular in cities where salaries were initially higher) and that this effect was driven by public aversion to high salaries (rather than accountability mechanisms). In turn, Mas (2016) presents evidence that rather than compressing average earnings of CEOs as intended, the policy seemed to have reduced the gap between lower and higher paid CEOs.

The remainder of this paper is structured as follows. Section 3.2 provides a brief description of Brazil's institutions that are of interest in this analysis. Section 3.3 describes the data and Section 3.4 presents basic descriptive statistics. Section 3.5 discusses the identification strategy and Section 3.6 describes the paper's main empirical findings. Finally, Section 3.7 summarizes and concludes the paper.

3.2 Institutional Setting

3.2.1 Structure of Government and Electoral System

Brazil is a Federal Presidential Republic which is largely decentralized. The country has three autonomous levels of government with equal status as guaranteed by the constitution. Municipalities are the lowest administrative level and are managed by an elected mayor (chief of the executive local branch) and a local city council. The federal district and each of the 26 Brazilian states are administered by an elected governor and a state chamber.[3] The federal government, which is the highest level, is headed by an elected president and a national congress (comprised by a lower house and a senate).

Each level of government is composed of three independent branches of power: the executive, legislative and judicial.[4] Across those three separate institutional layers, the executive branch has the main responsibility for the provision of public goods and services in the country (such as health, security or educational services).

The Brazilian constitution also determines which activities are to be exclusively performed or regulated by each level of government and those in which responsibilities are shared. For instance, the executive power of states and local governments

[3]Note that the federal district, where Brasília (the capital of the country) is located, is administered by an elected governor and a district chamber (which functions both as a state chamber and a city council).

[4]An exception is the Public Prosecutor's Office (Ministério Publico) that is an autonomous institution not subordinated to any of the three branches of power of the Brazilian government. Furthermore, the judicial branch is organized in states and at the federal level and municipalities holds no judicial powers.

share responsibilities regarding the provision of education as well as health care services. In particular, municipalities are mainly responsible for early childhood and primary education programs while the provision of secondary education is shared by states and local governments. Services such as urban planning, public lighting, garbage collection and public transportation, however, are mostly municipal responsibilities. In turn, police institutions responsible for providing public security are mainly part of the executive branch of either federal or state governments.[5]

Elections in Brazil take place every four years on the same date throughout the country with local elections occurring in year t and state and federal elections in year $t+2$ (that is, elections are perfectly staggered by two years). Executive chiefs of all levels of government compete for a four year-term and can remain in office for no more than two consecutive terms. Governors of Brazilian states and the president of the republic are elected by a plurality rule (runoff voting). Mayors, in turn, are elected by a simple majority rule in cities with less than 200,000 voters and by a plurality rule in those with more than 200,000 voters.[6] Legislators are elected for a four year-term with no term limits, under a proportional representation system with an open-list.[7]

3.2.2 Transparency in Brazil

The basis for the adoption of transparency laws in Brazil dates back to 1988 when the first Brazilian constitution was promulgated after the military regime. The 1988 constitution established publicity of information and administrative actions

[5]Police institutions at the federal level are the Federal Police, Federal Highway Police and Federal Railway Police. The Military Police, Fire Brigade and the Civil Police are under the control state government authorities. Note also that municipal authorities have the power to create municipal guards. City guards, however, are mainly responsible for protecting municipal parks, properties, installations and the interior of municipal councils and city halls and not citizens as police institutions at the state and federal level.

[6]If no candidate attains an absolute majority in the first round, a runoff shall be held gathering the two candidates with the highest number of votes (top two candidates), and the candidate who wins the majority of valid votes (50 percent plus at least one vote) shall be considered elected.

[7]Note that members of the senate are elected by a majority rule for a term of eight years and not four as all other elected officials throughout the country.

as primary citizens' rights.[8] As a result, several laws and initiatives followed with the aim of promoting government openness across the country. An early example was the law on fiscal responsibility passed in May 2000 which mandated disclosure of key budget documents and established guidelines on transparency, control and oversight of public finances for all three levels of government in Brazil.[9]

In May 2003, taking a further step to enhance transparency, the federal government created the Federal Office of Comptroller General (Controladoria Geral da União - CGU). The CGU was created as an anti-corruption agency that centralized all internal control activities within the federal executive branch, also being in charge of strengthening transparency and fighting corruption within the public sector. The agency was also responsible for coordinating ombudsman activities and promoting public audits of federal funds.

In November 2004, less than two years after its creation, the CGU launched a transparency portal with the objective of ensuring real-time free access of information on budget execution of federal funds. The portal was created as an online tool that enabled citizens to track and monitor revenues and expenditures of the federal government. As a direct spillover, in May of 2009 the Brazilian president sent a bill to the national congress mandating online publishing of comprehensive budgetary information (in real-time) to all levels of government.[10]

To finally regulate the constitutional principle that entitled citizens to the right to access public information, in November 2011, the central government passed

[8] In particular, the article 5 of the Brazilian constitution in its subsection XXXIII states that "all persons have the right to receive, from the public agencies, information of private interest to such persons, or of collective or general interest, which shall be provided within the period established by law, subject to liability, except for the information whose secrecy is essential to the security of society and of the State."

[9] The law of fiscal responsibility was also the result of a fiscal crisis that exposed massive deficits associated with excessive spending, and over borrowing in subnational government budgets across the Brazil and led to a federal bailout of subnational debt.

[10] The bill, known as transparency law, was approved in May 2010 by the national congress. Compliance with the law was staggered and based on demographic characteristics of federal entities. States, the federal district and municipalities with more than 100,000 inhabitants were given one year to comply with the law. Municipalities with between 50,000 and 100,000 inhabitants were given two years, and those with less than 50,000 inhabitants were given 4 years. In case of non-compliance the federal government can withhold voluntary intergovernmental transfers.

a federal bill in congress known as *Lei de Acesso à Informação* (LAI). The law established a legal framework of guidelines for opening data from all levels of government (federal, state and municipal) and branches of power (executive, legislative and judiciary). Similar to a Freedom of Information Act (FOI), the law guarantees any person the right to request non confidential information kept by Brazilian public bodies. Non-compliance with the law might result in penalties that range from fines to loss of mandate (for those holding public office). The aim was also to promote active transparency in the public sector establishing minimum levels of information that should be publicly available to citizens, preferably in online transparency portals.[11]

The LAI was regulated in May 2012 and among other features mandated disclosure of wages of all civil servants working at the executive branch of the federal government (that is, judiciary and legislative powers at the federal level were not obliged to disclose salaries of their workers). Although other public bodies of Brazil were not mandated to release individual information on pay, there is evidence that the executive branch of states and municipalities followed the central government and also adopted wage transparency policies. For instance, according to a ranking created by the federal public prosecutor's office (Ministério Público Federal - MPF) roughly 88% of Brazilian states (22 out of 26 states) and 29% of municipal governments (1,605 out of 5,568 municipalities) disclosed pay of their bureaucracy at the individual level as of 2016.[12]

Following LAI's regulation, the central government of Brazil posted salaries online by name of employee in June 2012. Anedoctal evidence suggests that wage disclosure was considered a sensitive issue by trade unions and public servants with several legal actions placed against government bodies that disclosed data

[11] According to Law 12,527/2011, the transparency portals should contain at least: a) a record of competence and organizational structure, addresses and telephone numbers of the respective units and working hours for the public; b) records of any passing or transfers of financial resources; c) records of expenses; d) information concerning bidding procedures, including the respective notices and results, as well as all contracts; e) general data for the monitoring of programs, actions, projects and works of agencies and entities; and f) answers to frequently asked questions from society. Municipalities with less than 10,000 inhabitants are not required to publish this information in electronic portals but are still subject to the law that mandates online disclosure of detailed information on budget execution in real-time.

[12] For the ranking elaborated by the MPF see: http://combateacorrupcao.mpf.mp.br/ranking.

on pay at the individual level.[13] On the other hand, there is also evidence that exposing privileged public employees revealed the wide disparity in wages within the Brazilian public bureaucracy.[14]

3.3 Data

As discussed in the previous section, the Brazilian FOI Act mandated disclosure on pay of public employees of the central government in May 2012 with salaries being posted online in the following month. Thus, public servants of the executive branch of the federal government were equally affected by the law in the same period of time across the country, which makes it impossible to separate the effect of transparency on pay of these employees from a time trend. In order to investigate the effects of wage disclosure in the public sector this study uses the fact that state governments followed the central government and passively adopted pay transparency in different time periods and focuses then on civil servants working at the executive power of state administrations.

To perform this analysis, I first collected data on the exact date the executive branch of each Brazilian state disclosed information on wages of their civil servants at the individual level. These data was collected mainly through public online requests made to each state using the guidelines of the LAI. To check if the information on the timing of disclosure provided by the state governments matched with its availability to the general public this study also relied on other sources such as regional or local newspapers.

Data on pay available in transparency websites of executive governments typically contain information on the name of employees, their salary, position and office. Importantly, data provided is not standardized across public sector bodies and there is no single repository of data. Figure 3.1 illustrates how these data is

[13]For evidence: http://www.brasil.gov.br/governo/2012/07/portal-da-transparencia-volta-a-divulgar-remuneracao-dos-servidores-do-executivo-federal.

[14]For instance: http://www.economist.com/node/21556916 or nytimes.com/brazil-seethes-over-public-officials-super-salaries.

displayed to citizens on the webpages of two state governments of Brazil. The top figure shows data for the state of Amapá and the bottom for the state of Rio de Janeiro. While in the state of Amapá, citizens can easily access detailed information on each public employee in the state of Rio de Janeiro access to this data is only granted if one has information on the name of the employee or his/her tax identification number. An additional caveat of transparency websites is that pre-disclosure data is usually not available.

To overcome those issues I use data on public sector pay from the *Relação Anual de Informações Sociais* (RAIS), which is available for each year of the period between 2003 to 2015. RAIS is a matched employer-employee database that provides universal coverage for Brazilian formal sector employees. For each tax-registered plant, RAIS records detailed information for every worker in its employment during the preceding calendar year. In particular, the data provides information on occupation (six-digit code), industry, tenure, age, contractual hours, gender and the (gross) average monthly wage of a worker over the employment spell during the year. Earnings reported in RAIS include any forms of payment subject to social security contributions or taxable income such as regular salaries, holiday or vacation bonuses, commissions and fees, hazard compensation, tips and gratuities.[15]

The RAIS database also provides detailed information on the legal form of the employer establishment. This information is used to classify individuals as workers of the private or public sector and to identify the specific level of government and branch of power in which a civil servant works.

There are two groups of workers who do not appear on RAIS: elected politicians and self-employed individuals. Hence, measures of wage dispersion of civil servants constructed from information on earnings and used in the empirical analysis exclude salaries of elected officials.

Finally, although RAIS data is reported at the individual level, I have no information that allows tracking individuals over time or across multiple employers. In

[15]See Menezes-Filho *et al.* (2008) for a full description of earnings reported in RAIS.

the empirical analysis, I then use aggregate data on RAIS either at the municipal or occupational level.

To account for differences in characteristics of municipalities located in states that adopted transparency on pay and those located in states that did not adopt the policy I use a large array of municipal covariates (both time invariant and time varying). Time varying controls used in municipal level analysis for each year between 2003 to 2015 are provided by the Brazilian Institute of Geography and Statistics (IBGE) and include log population and log of real GDP per capita. I additionally use administrative data on local budgets from a dataset named FINBRA, provided by the Ministry of Finance (Secretaria do Tesouro Nacional), to construct measures of intergovernmental transfers, expenditures and revenues for each year between 2003 and 2015.

Time invariant municipal controls come from the 2000 census and were also provided by the IBGE. Variables used in local level analysis include the share of the population with at least high school education, literacy rate, share of urban population, share of households with TV, share of households with radio, share of households with a PC, and the Gini inequality index.

Data on gubernatorial electoral results for the period between 2002 and 2014 is provided by the Electoral Supreme Court (TSE). This data contains detailed results for elections held at the state level, including vote totals, electoral turnout, names and surnames of candidates, their party of affiliation, etc.

This study finally uses data from RAIS on average wages, age, tenure, number of workers, share of female workers and share of workers with at least a secondary degree, both at the municipal and occupational levels, for the period between 2003 to 2015.

Nominal wages, intergovernmental transfers, revenues and expenditures are all measured in real terms and expressed in 2015 Brazilian Reals using the National Consumer Price Index (Índice Nacional de Preços ao Consumidor - Amplo, IPCA), which is also provided by the IBGE.

3.4 Descriptive Statistics

This study focuses on civil servants of the executive branch, which is the branch of government that is primarly responsible for the provision of public goods and services across the country and as a result accounts for the majority of public employees. To illustrate, Figure 3.2 shows the average distribution of civil servants between 2003 and 2015 across the different branches of power in the country. As shown in the figure, more than 90% of Brazilian public employees work in the executive branch of either the federal, state or municipal governments.[16] The empirical analysis also restricts the sample to civil servants of state governments. Figure 3.3 show that those workers account for roughly 35% of all public servants working in the executive branch of the different administrative levels of the Brazilian government.

The timing of wage disclosures across state governments of Brazil is illustrated in Figure 3.4. Note that most states adopted the transparency policy just after the central government posted earnings of its employees by name (June 2012). In total, 15 out of the 26 states disclosed information on pay between July and August of 2012. Nonetheless, there was substantial variation in adoption over time and across states, with 7 states adopting the policy in different points in time between January of 2013 and February of 2016. Finally, as of 2017, 4 state governments had not yet disclosed data on pay of its employees.[17] Since data on RAIS is not available for years after 2015, states that adopted the policy after or at the end of 2015 are classified as non adopters throughout the empirical analysis.

Data reported on RAIS use a six-digit numerical system to define each occupation, following the 2002 Brazil's classification code called CBO (Classificação Brasileira de Ocupações). In order to describe the typical occupations of public employees working for state governments I use the first two of these six digits and group occupations into 48 categories (instead of 1,716 individual occupations from the six-digit level). I then mapped the CBO-2002 into the commonly used ISCO-88

[16] Others include civil servants of the Public Prosecutor's Office.
[17] Note that the federal district is excluded from the analysis since it has no municipalities.

(International Standard Classification of Occupations). These data is reported in Table 3.1, which shows the average proportion of workers in the 10 most common occupations in the period 2003-2015. Note that those occupations account for more than 95% of all public servants working for state governments.

Consistent with the fact that state governments are responsible for providing educational services and public security and also share with municipalities the obligation for providing health services, the table shows that Teaching workers, Police Officers and those in Health related occupations account for roughly 58% of all state employees. Office Clerks, in turn, which is a common white-collar occupation across government bureaucracies, represent around 19% of all state workers. Finally, Personal Services Workers and workers in senior managerial positions at the state level, account for 8% and 7.8% of all the state's civil servants, respectively.

To describe the evolution of earnings inequality in Brazil, I use municipal level regressions of the top and bottom decile gaps (relative to the median) on year and state dummies for each year between 2003 and 2015. Results are illustrated in Figure 3.5 which presents trends in wage dispersion since 2003 for workers of the executive branches in all levels of government and the private sector. In all regressions, coefficients are measured using the first year of the series as baseline. The dotted vertical line identifies the year the federal government passed the law that mandated wage disclosure of its own employees. Panels A and B of the figure indicate that there was an upward trend in wage inequality during the period both at the upper and lower tails of the log earnings distribution. However, focusing on the upper tail earnings inequality measured for central and state government civil servants (illustrated in Panel A of the figure) the evidence suggests that there was not much of a trend since 2011, which could be the result of pay disclosure policies.[18] Although the effect of pay disclosure on earnings of central government employees cannot be separated from a time trend, the next section will examine how pay transparency affected wage inequality of state government civil servants.

[18]Figure 3.A1 in the Appendix show trends in earnings inequality for state government employees using other decile gaps.

3.5 Empirical Strategy

In order to investigate how wage transparency affects inequality of earnings in the public sector, I start by using a differences-in-differences (DID) strategy that exploits variation in the timing of adoption of pay transparency policies by state governments of Brazil. In formulas,

$$Y_{mt} = \beta_0 + \beta_1 disc_{st} + \gamma X_{mt} + d_m + d_t + u_{mt} \tag{3.1}$$

where Y_{mt} measures the difference in logs of different wage percentiles with respect to the median in municipality m located in state s in year t (that is, $Y_{mt} = w^p_{mt} - w^{50}_{mt}$, with p indicating different percentiles of the log wages distribution), $disc_{st}$ is a variable that equates 1 when state s discloses information on pay of civil servants, X_{mt} is a vector that includes both time varying local characteristics and baseline municipal controls interacted with year dummies, d_m are municipal dummies that control for municipal specific characteristics, d_t year dummies that control for unobservable shocks that equally affect all municipalities, and u_{mt} an error term.

Building on the fact that state government employees might work in offices located in different municipalities within a state, the unit of observation of this analysis is a municipality m in a given year t. The proposed empirical strategy effectively compares changes in wage dispersion of municipalities located in states that did and did not adopt pay transparency before and after salaries were posted online. Note that this strategy does not restrict the control group to states that never posted data on earnings. Each municipality in states that did not disclose information on pay at a particular point in time acts as control for municipalities in states that already adopted pay transparency in that time period (even if the non-disclosing state adopts the disclosure policy later on). The parameter β_1 identifies the causal effect of disclosure under the assumption that the policy adoption by

state governments is not driven by unobservable characteristics of states that vary over time.

An important caveat of the above analysis is that information on pay was not disclosed at random across states. Thus, municipalities in non-disclosing states might not represent a valid control group to those in states that adopted the policy (even after conditioning on observables). The estimated effect of the policy based on equation (3.1) might then be simply picking the effect of differential trends in state-specific conditions.

To provide causal estimates of the effect of interest, I then employ an identification strategy that uses the cross-state variation induced by the timing of adoption but compares localities within states. In particular, it compares changes in the wage dispersion of municipalities with high and low levels of inequality within states that disclosed and did not disclose information on salaries. Inequality is measured in the pre-policy period between 2003 and 2009 and defined as the average gap between the different deciles of the log earnings distribution and the median of civil servants working for that state government in different municipalities.

This strategy has the advantage of accounting for shocks contemporaneous to the time a state government disclosed data on pay of its civil servants, such as inequality trends across states or law changes correlated with the transparency policy. Furthermore, the baseline level of earnings inequality for civil servants working in a particular municipality is arguably exogenous to the fact that a state government decided to disclose data on pay in a particular point in time. In formulas,

$$Y_{mt} = \alpha_0 + \alpha_1 ineq_{mt_0} * disc_{st} + \gamma X_{mt} + d_m + d_{st} + u_{mt} \tag{3.2}$$

where Y_{mst} is defined as in equation (3.1), $ineq_{mt_0}$ is a measure of wage dispersion for state government employees that work in municipality m in a baseline period t_0 before the policy was adopted by state s (that is, $ineq_{mt_0} = w^p_{mt_0} - w^{50}_{mt_0}$), d_m

is a municipality fixed effect, d_{st} state dummies interacted with time effects, and u_{mt} an error term.

3.6 Empirical Results

3.6.1 Baseline Results

Table 3.2 presents estimated coefficients of the DID model described by equation (3.1). Each row of the Table corresponds to a different set of municipal level regressions that estimate the average effect of the disclosure policy on decile wage gaps with respect to the median, for civil servants of state governments, during the period between 2003 and 2015. Robust standard errors shown in brackets are clustered at the state level. All specifications include additive year and municipal dummies.

Note that the inclusion of year dummies in regressions control for any shock that contemporaneously affect all state governments, such as the transparency law passed in 2010 that mandated online publishing of detailed information on budget discussed in subsection 3.2.2. Municipality fixed effects, in turn, account for latent wage differentials in localities across states. Nonetheless, municipalities in states that disclosed and did not disclose salaries of civil servants might also differ in observable characteristics that vary over time. To account for the fact that those characteristics might be correlated with the transparency policy adopted by state governments, in columns (2) and (3) I also estimate specifications that include municipal time varying controls and local baseline characteristics interacted with year effects, respectively. Additionally, in an attempt to control for trends in wage differentials, the specification presented in column (4) includes the average gap for each decile in a baseline period (between 2003 and 2009) interacted with year effects. Finally, in regressions in column (5) I add municipal linear trends to address the fact that the effect of the policy on earnings differentials might also be driven by differential trends in municipal-specific conditions.

Focusing on estimated coefficients for decile gaps above median, in particular above the eighth decile, evidence presented in Table 3.2 indicates that wage inequality reduced significantly at the upper tail of the wage distribution as a result of the adoption of pay transparency policies. Precisely, columns (1) to (4) in the first row of the table show that the gap between the 99th percentile and median earnings consistently decreased in all specifications presented. For instance, estimates presented in column (4) show that disclosure reduces this gap by 7.6 percentage points.

Note, additionally, that point estimates for percentiles above the median become smaller in magnitude after the inclusion of municipal linear trends in the specification presented in column (5) of Table 3.2. Moreover, the point estimate of the gap between the 99th and the 50th percentiles becomes statistically insignificant after the inclusion of this control. Nevertheless, the estimated coefficient for the gap between the top decile and the median illustrated in the second row of column (5) suggests that pay transparency reduced the 90-50 log inequality by 8.3 p.p., comparing states that adopted the policy with those that did not.

In turn, point estimates for decile gaps below the median, illustrated in columns (1) to (5) of Table 3.2, do not suggest any significant changes in the wage inequality at the bottom of the pay distribution. As shown in the table, this result is robust to the inclusion of a different set of controls.

Overall, across state comparisons suggest that wage disclosure significantly changed earnings differentials in the public sector, compressing wages at the top of the distribution. Interestingly, point estimates for all decile gaps above the median are monotonically decreasing, despite being non significant up to the 8th decile, which is consistent with the intuition that the effect of disclosure is expected to be more salient at the top of the log wages distribution.

Nonetheless, it may be still of concern that some or all of the effect that has been estimated using equation (3.1) is due to contemporaneous policy changes at the state level that cannot be captured by municipality linear trends. In addition, the timing of the online disclosure of pay information by state governments is

not exogenous since the adoption of transparency policies was not random across states.

In order to disentangle the effect of disclosure this study thus relies on a within state comparison of municipalities with high and low levels of inequality at baseline (before pay is disclosed by state governments). As defined in the previous subsection inequality at baseline is simply a measure of the average difference between the log wage at the pth percentile and the log of the median at the municipal level for the period 2003-2009 (for public servants working in state governments). Importantly, the baseline level of wage dispersion in a particular municipality is exogenous to the state decision to disclose the pay of its employees by name.

Table 3.3 presents regression results of the DID strategy based on equation (3.2). As before, each row in the table refers to a separate regression of the gap between consecutive deciles of the log wages distribution and the fifth decile. The independent variable that measures the causal effect of interest, however, is the interaction between the disclosure dummy and the level of wage dispersion measured in the pre-policy baseline period. In addition to municipality fixed effects, all specifications presented in the table include state dummies interacted with year effects. This allows us to additionally control for unobserved state-specific factors that might change with states over time and could be driving the effects of pay transparency.

Estimates presented in columns (1) to (3) of the table indicate that earnings were significantly compressed within states for decile gaps above the median with the opposite occurring for gaps below the fifth decile. For instance, regression coefficients of the gap between the $9th$ and the fifth decile shown in the second row of column (3), suggest that if the baseline difference in wage dispersion of municipalities with high and low levels of inequality is 10 percentage points apart (comparing localities in states that disclosed data on pay) this gap is reduced by 2.7 p.p. once data on salaries of employees are posted online by states. In turn, the point estimate of the gap between the bottom decile and the median, presented in

the last row of column (3), shows that the adoption of the policy further increases this gap by roughly 1.7 p.p.

Regressions presented in column (4) of the table include an interaction between the municipal level of inequality measured at baseline and year dummies. This specification allows the baseline level of inequality to have different effects at different points in time and localities, controlling also for differential trends in inequality. Note that point estimates of wage differentials displayed in this column are now mostly statistically insignificant, indicating that part of the effects described in columns (1) to (3) of the table were driven by inequality trends.

On the other hand, the estimated coefficient of the 90-50 decile gap is significantly negative even after accounting for trends in wage dispersion of state government employees. While the effects are smaller in magnitude in comparison to those presented in column (3), this result indicates that wage disclosure significantly affected the top of the log earnings distribution. Precisely, the point estimate of this wage gap shown in the second row of column (4) suggests that the adoption of pay transparency by states reduced this log earnings gap by 1.5 p.p. in localities where the baseline gap was 10 p.p. apart.

In an additional attempt to control for specific-local shocks that might also affect the wage gap of state government employees, the specification presented in column (5) of Table 3.3 includes municipal linear trends. Importantly, point estimates for the ninth and eighth deciles relative to the fifth presented in the second and third rows of column (5) are both significantly negative. Finally, estimates for gaps at the bottom decile of the distribution are non significant, suggesting that disclosure of pay affects mostly the upper tail of the log earnings distribution in the public sector.

3.6.2 Returns to Occupations versus Composition

The previous subsection established that disclosure of wages of the state bureaucracy compressed pay in the public sector. The evidence presented also suggests

that effects are concentrated among workers with wages above median earnings and in particular at the upper tail of the log earnings distribution. Occupations might be an important channel to assess how the wage structure in the public sector was affected by pay transparency. Furthermore, point estimates indicating wage compression at the local level might be the result of compositional changes in top paid occupations rather than changes in returns to these occupations. To shed light on these, this study uses the following regression to examine how the average returns and the composition of workers in top paid jobs of state governments were affected by pay disclosure policies:

$$Y_{imt} = \delta_0 + \delta_1 Top_{is} * disc_{st} + d_{im} + d_{mt} + d_{it} + u_{imt} \tag{3.3}$$

where Y_{imt} is the average log earnings or the share of workers in occupation i in municipality m and year t, Top_{is} is the share of top earners in occupation i and state s, $disc_{st}$ is defined as previously, d_{im} is an occupation and municipality fixed effect that controls for unobservables that might affect an occupation in a given municipality, d_{mt} is a municipality and year fixed effect that control for municipal trends or institutional changes that might affect different municipalities over time, d_{it} is an occupation and time fixed effect that accounts for occupations time specific effects, and u_{imt} an error term.[19]

As described in section 3.4, an occupation is defined using the first two out of the six digits provided by the CBO-2002 classification system, which leads to 48 occupations. Top paid jobs in state governments are defined using a ranking of occupations and earnings within states to account for the fact that each state government has autonomy to decide the pay of their employees and for differences in earnings across states. Specifically, a top paid job is defined as the average share of workers in the top vingtile, decile or percentile, in a given occupation i and state s over the period 2003-2015.

[19]Note that equation (3.3) follows from equation (3.1) but estimated at the occupational level.

Table 3.4 describes the 10 highest-paying jobs (out of 48 jobs) in state governments across Brazil. Occupations in the table are ranked according to the share of workers in the top percentile as shown in column (1). Note that the 10 jobs displayed in the table account for roughly 17% of all state civil servants, as illustrated in column (4). Among the occupations with highest share of earners in the top percentile are Legal Professionals, Social, Arts and Humanities Professionals, Science and Engineering Professionals, Life Science Professionals and Health Professionals (together with Firefighters, managers of different offices at the state level and senior officials working for state governments).[20]

Columns (1) to (6) of Table 3.5 present estimated coefficients of regressions based on equation (3.3). Panel A describes the effect of pay disclosure on the returns to top paid occupations while Panel B shows the impact on the composition of those occupations. All regressions are weighted by the number of workers in occupation i, state s and year t and robust standard errors are clustered at the state level and reported in brackets. Note also that regressions displayed in columns (2), (4) and (6) of the table additionally include time varying occupational controls with respect to those presented in equation (3.3). Those controls attempt to account for trends in characteristics of occupations that might also vary by municipality. Specifically, those specifications include average age, tenure, number of workers, share of female workers and share of workers with at least high school education in a certain occupation i, municipality m and year t.

Focusing on point estimates illustrated in columns (1) to (6) of the top panel of the table, results indicate that the average returns to top paid jobs consistently decreased as a result of the adoption of pay transparency policies. For instance, the point estimate presented in column (1) indicates that, once data on pay is posted online in the state where an occupation is distributed, a 10 percentage

[20]Top paid earners classified as Legal Professionals include state prosecutors, attorneys and police commissioners (chief officers). Social, Arts and Humanities Professionals include mainly tax auditors, economists and political scientists. Science and Engineering Professionals include workers in different engineering sectors, IT workers, aircraft pilots and architects. Life Science Professionals include forensic experts, plant biology and agronomy researchers. Note also that Firefighters are an auxiliary force of the Brazilian Army. Thus, Firefighters among top earners are mainly those individuals with the highest military ranks within Brazilian Military Firefighters.

point increase in the share of earners in the top 20% decreases the returns to this occupation by 2.1%. Similarly, the estimated coefficient displayed in column (3) shows that a 10 percentage point increase in the share of earners in the top 10% reduces the returns to this job by 3.1%. Note also that additionally controlling for time varying characteristics of occupations at the local level does not alter the significance of point estimates, as illustrated in columns (2) and (4), which provides evidence that changes in returns were not driven by differential changes in characteristics of top paid jobs, such as changes in gender or age structure.

Interestingly, the effect of disclosure on returns to top paid jobs displayed in columns (1) to (6) of Panel A is increasing in absolute terms according to the definition of a top paid occupation. That is, although non statistically significant as illustrated in columns (5) and (6), the impact of pay transparency is stronger for top paid jobs defined as the share of workers in the top 1% than for those defined as the share in the top 10% or 20%, which seems consistent with findings presented in Table 3.2.[21]

Figure 3.6 illustrates the relation between the compression in returns as a result of disclosure and the share of workers in top paid jobs. Each figure present scatter plots of estimated coefficients drawn from regressions of the the interaction between the disclosure dummy and occupation dummies on the average log earnings by occupation and the share of workers in the top vingtile (Panel A) and top decile (Panel B).[22] Point estimates of regressions on returns per occupation are measured using the occupation with the lowest share of workers in the top 20% and 10% as baseline (that is, other workers of conservation, maintenance and repair) and presented in the vertical axis. The share of earners in the top 20% and 10%, in turn, is displayed in the horizontal axis of each figure.

[21]For transparency, Table 3.A1 in the Appendix presents point estimates for regressions using an alternative definition of top paid jobs. In particular, occupations are ranked according to earnings in all the country rather than by state. Similarly, a top paid occupation is then defined as the average share of workers in the top vingtile, decile or percentile, in a given occupation i throughout the period between 2003-2015. Results in the table are consistent to those illustrated in Table 3.5.

[22]Returns by occupation are estimated using the following regression: $Y_{imt} = \delta_0 + \delta_2 disc_{st} * d_i + d_{im} + d_{mt} + d_{it} + \eta X_{imt} + u_{imt}$, with X_{imt} being a vector of time varying local characteristics of occupations as previously described.

Consistent with results presented in columns (2) and (4) of Table 3.5, the fitted red lines in Figure 3.6 indicate that there is a strong negative correlation between the share of workers in the top vingtile or decile of an occupation and the estimated returns for this job once pay information is posted online by state governments.[23] Notably, as one would expect, occupations such as Legal Professionals, Life Science Professionals, Science and Engineering Professionals and Administrative Managers, which are listed in Table 3.4, that rank occupations according to the share of top earners, are among the most affected by the pay transparency policies.

Finally, the results presented in columns (1) to (6) of Panel B of Table 3.5 indicate that the effect of releasing data on pay of civil servants on the share of workers in top paid occupations is not significantly different from zero. As shown in columns (2), (4) and (6) of the table, this result is also robust to the inclusion of local time varying observable characteristics of occupations. Indeed Figure 3.7 shows that the correlation between the share of top paid workers per occupation and estimated coefficients from regressions of the interaction between the disclosure dummy and occupation dummies on the share of workers is approximately zero, suggesting that disclosure did not change the size of top paid occupations relative to low paying jobs.

3.6.3 Robustness Checks

The main analysis of this study focuses on earnings differentials of civil servants working for state governments. Although some spillover effects might propagate to the local bureaucracy, impacting wage differentials of municipal public employees, it is not likely that the policy adopted by states would have affected workers of the central government. [24] As discussed, disclosure of information on pay (mandated

[23]Estimated coefficients of regressions presented in Panels A and B of the figure illustrated by the red fitted lines are -0.36 and -0.39, respectively, with both being significantly negative at 5%.

[24]It should be noted that one could have expected that municipalities were pushed to adopt pay transparency policies by state governments or other public bodies at the state level, such as the state public prosecutor's office, just after state administrations where this municipalities are located disclosed wages online.

by law) affected all central government employees across the country at the same point in time. As a placebo, I then use the timing of adoption of pay transparency policies by states to examine the effects of disclosure on wage differentials of public servants working for the federal and local governments.

Table 3.A2 in the Appendix of this chapter presents regression results of this placebo exercise using within state comparisons described by equation (3.2). Estimated coefficients reported in columns (1) to (3) and (6) to (8) of the table follow the pattern illustrated in Table 3.3 with wage inequality within states decreasing at the upper tail of the wage distribution and increasing at the lower tail as a result of disclosure (both for employees of the central and local governments). Note, however, that once the interaction between year dummies and the baseline level of inequality is included in regressions, most of the effect of the pay disclosure vanishes, as shown in columns (4) and (9) of the table. These results are confirmed in the specification that additionally controls for municipal time specific effects presented in columns (5) and (10).

In sum, the results in Table 3.A2 indicate that there was no differential change on earnings inequality of civil servants of the central government as a result of states having disclosed salaries of their employees. Additionally, evidence suggests no spillover effects in the wage dispersion of municipal employees.

3.6.4 Possible Mechanisms

The evidence discussed so far suggests that earnings at the top of the distribution compressed in comparison to the median (both across and within states) as a result of state governments adopting pay transparency policies. Since in Brazil elected officials for the executive branch of power face a two-term limit, electoral accountability might be a plausible explanation for earnings compression at the top. Governors in their first term could be more likely to reduce the gap between top and median earners within the public sector than those in their second term

since they fear being punished by voters when running for reelection (in particular if the information their administrations disclose reveal a high disconnection between salaries and skills of civil servants, which could signal to rents that accrue to holding public sector jobs).

In order to shed light on this mechanism I further interact the independent variable of interest of equation (3.1) with a dummy that equals one if there is a first term governor in power in state s and year t, and zero otherwise. Table 3.A3 in the Appendix presents estimated coefficients of this interaction, focusing on decile gaps starting from the 6^{th} decile relative to median earnings.

Results displayed in columns (1) to (6) of the table show that earnings inequality at the upper tail of the log earnings distribution is more likely to decrease when there is a first term governor in power in a disclosing state. It should be noted, however, that estimates are only significant when running the specification presented in column (3), that additionally controls for time varying local characteristics and allows for baseline covariates to have specific effects in each municipality. After allowing for the baseline level of wages inequality to have time specific effects in each locality, which could possibly account for inequality trends from baseline, point estimates lose precision and become non significant, as shown in column (4) of the table.

3.7 Conclusion

In this paper I used linked employer-employee data from Brazil for the period between 2003 and 2015 to analyze the effect of pay transparency on the wage dispersion within the public sector. The study presents evidence that the adoption of transparency by Brazilian states affected earnings inequality both across and within states, with effects concentrated at top of the log earnings distribution. Importantly, decile gaps below the median do not seem to have been differentially affected by transparency policies both comparing localities across and within states.

In line with evidence presented, top paid occupations of state governments, such as Legal Professionals, Life Science Professionals or Administrative Managers, faced a higher compression on earnings than lower paid occupations as a result of state administrations having adopted pay transparency policies. Moreover, there was no evidence that the share of workers in top paid jobs was affected by disclosure, which seems to suggest that indeed the compression at the upper tail of the log wages distribution is driven by lower returns rather than compositional effects.

Although the estimates presented are not robust it seems that governors in their first term are more likely to reduce earnings inequality for civil servants working at the state level than their second-term counterparts, which is consistent with a mechanism of electoral accountability.

Finally, the results presented seem consistent with previous works that analyzed the effect of mandated disclosure of salaries on pay compensation of workers and showed that the effect of policies that revealed wages are more likely to affect top earners (Mas (2016) and Mas (2017)).

Tables and Figures

FIGURE 3.1 Information on Pay of Civil Servants of State Governments

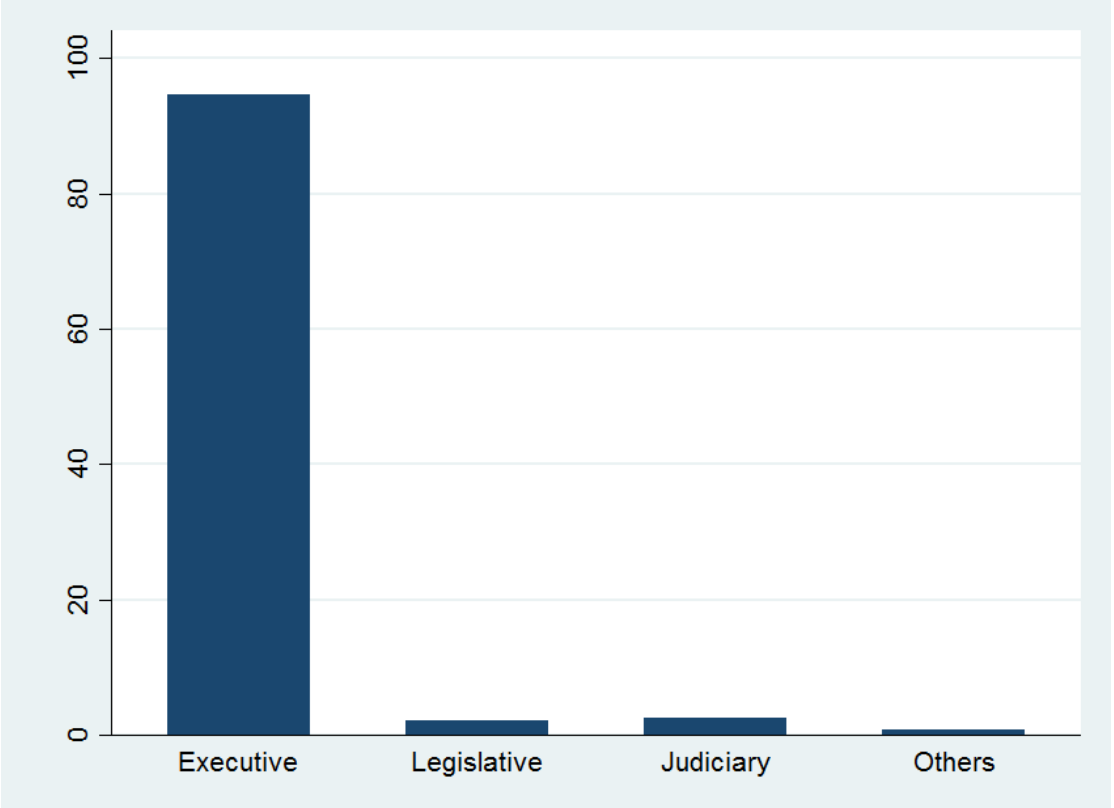
Panel A: State of Amapa

#	Nome do Funcionario	Sigla Org?o	Cargo	Dt. Admiss?o	Valor Bruto	Descontos	Valor Bruto - Descontos
	Nome do Funciona	Sigla Org?o	Cargo	Dt. Admiss?o	Valor Bruto	DESCONTO	LIQUIDO
1	ANTONIO WALDEZ GOES DA SILVA	GABI	GOVERNADOR	01/01/2015	24.376,88	5.834,28	18.542,60
2	HUELTON CORREA MEDEIROS	GABI	CHEFE DE GABINETE	01/01/2015	10.040,22	362,24	9.677,98
3	MARCELO IGNACIO DA ROZA	GABI	CHEFE DE GABINETE	01/01/2015	11.920,00	2.822,53	9.097,47
4	JOSE ARISTEU ARAUJO TAVARES	GABI	MOTORISTA	02/01/2015	601,18	0,00	601,18
5	LUIS ROBERTO SILVA DOS SANTOS	GABI	CHEFE DE DIVISAO	01/01/2015	6.910,00	770,20	6.139,80
6	ALAN PATRICK SANTOS DIAS	GABI	CHEFE DE UNIDADE	13/07/2015	1.703,38	153,30	1.550,08

Panel B: State of Rio de Janeiro

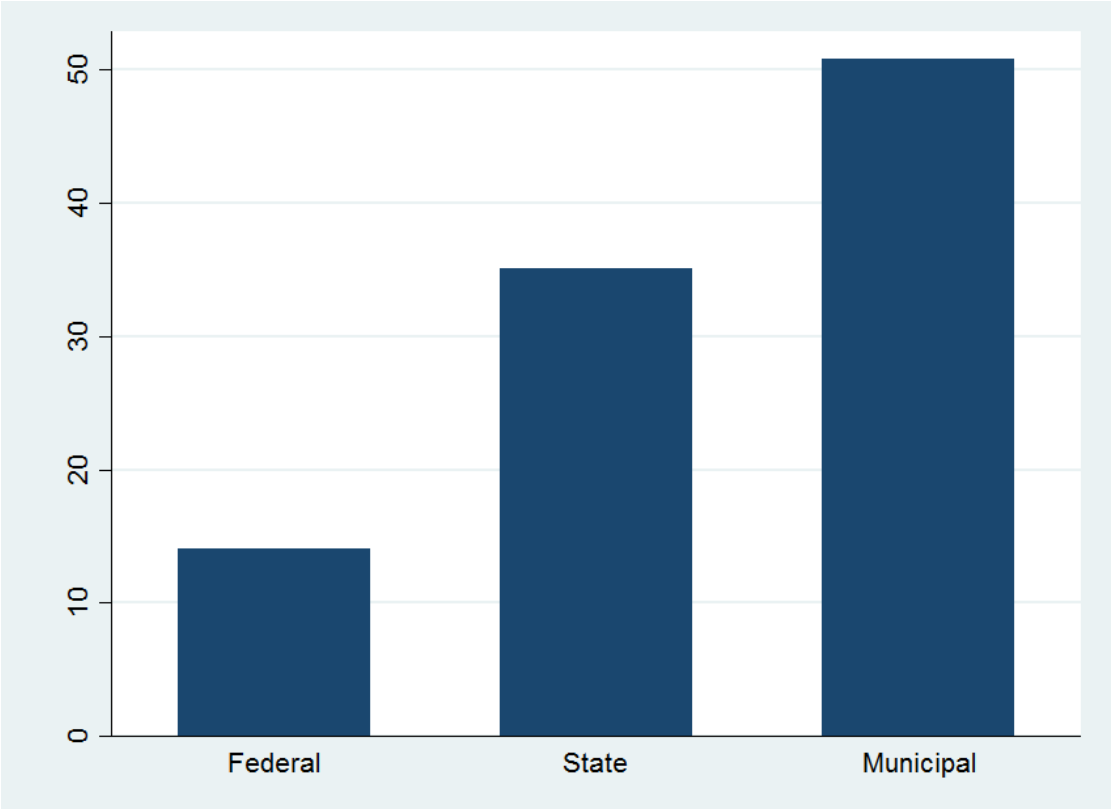
Notes: the figures illustrate how data on pay of state government employees is displayed to citizens on transparency websites of two Brazilian states. The first panel show data for the state of Amapá while the second for the state of Rio de Janeiro.

FIGURE 3.2 Civil Servants by Branch of Power - %



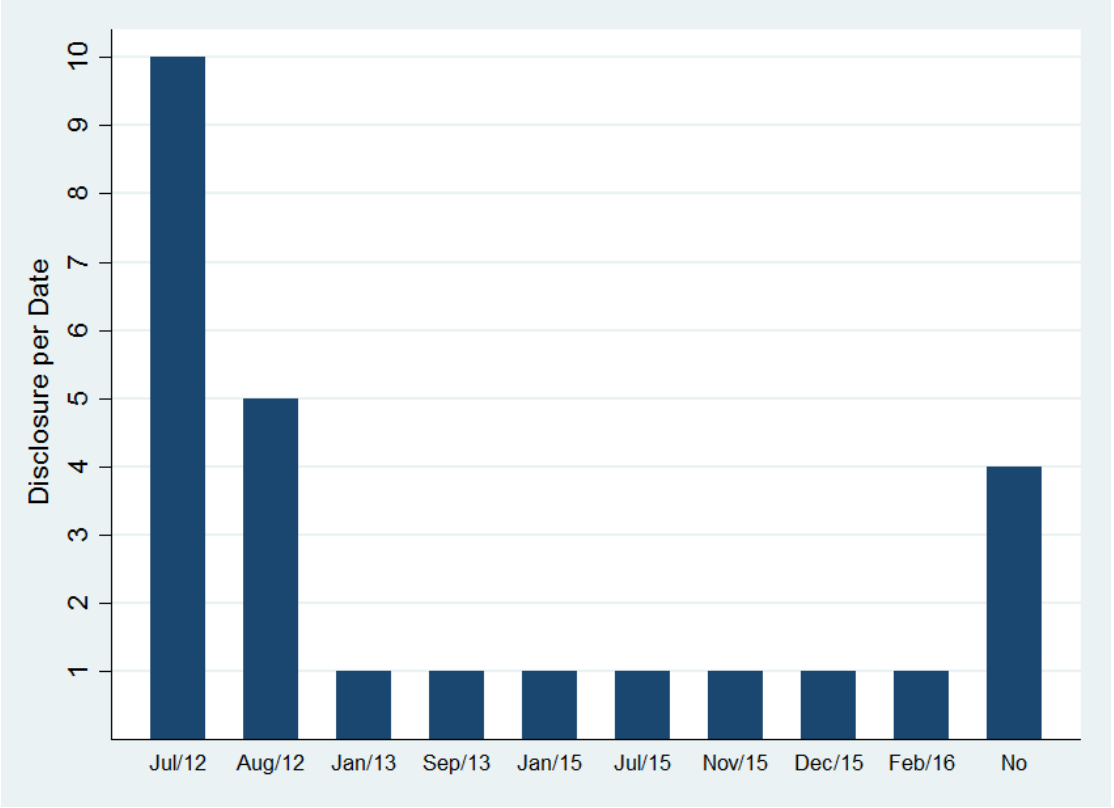
Notes: The figure shows the average percentage of civil servants working in the different branches of government of Brazil from 2003 to 2015. The Others category in the figure refers to Prosecutor's Public Office employees.

FIGURE 3.3 Civil Servants in the Executive Branch by Administrative Level - %



Notes: the figure shows the average percentage of public servants working for the executive branch of power in one of the three administrative levels of government of Brazil (Federal, State and Municipal) between 2003-2015.

FIGURE 3.4 Timing of Disclosure of Civil Servants Pay by State Governments



Notes: the figure illustrates the timing of disclosure of information on pay at the individual level by each state government of Brazil.

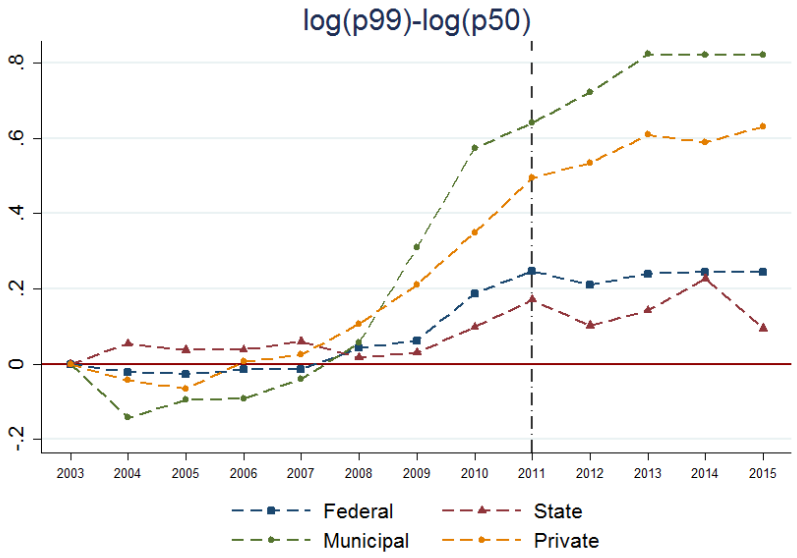
Table 3.1 Ranking of Occupations in State Governments

	Ni/N
	(1)
Teaching Professionals	22.6
Office Clerks	18.7
Teaching Associate Professionals	16.1
Police Officers	12.0
Personal Services Workers	8.0
Chief Executives and Senior Officials	7.8
Health Associate Professionals	3.6
Health Professionals	3.5
Business Associate Professionals	3.4
Social, Arts and Humanities Professionals	1.8

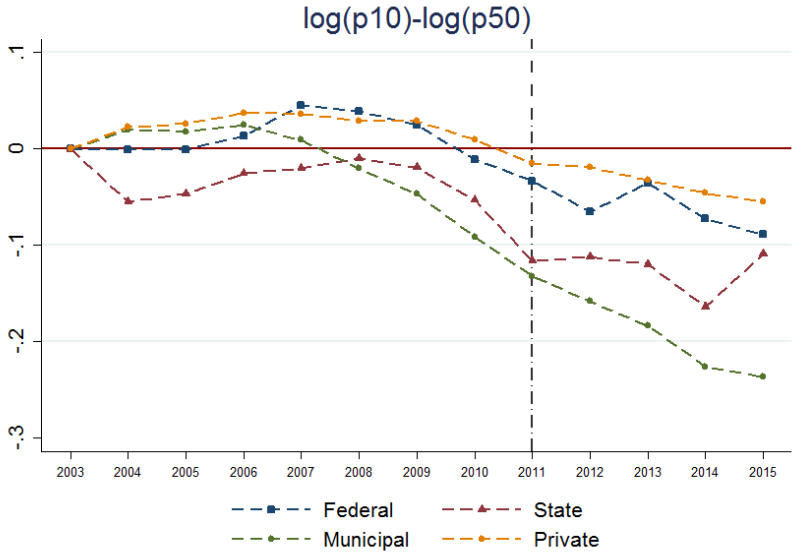
Notes: the Table shows the average proportion of civil servants working for state governments for the period between 2003 and 2015 in the 10 most common occupations. Occupations are aggregated at the two-digit level using RAIS data and ranked by 1 to 10.

FIGURE 3.5 Inequality Trends

Panel A: Evolution of upper tail inequality



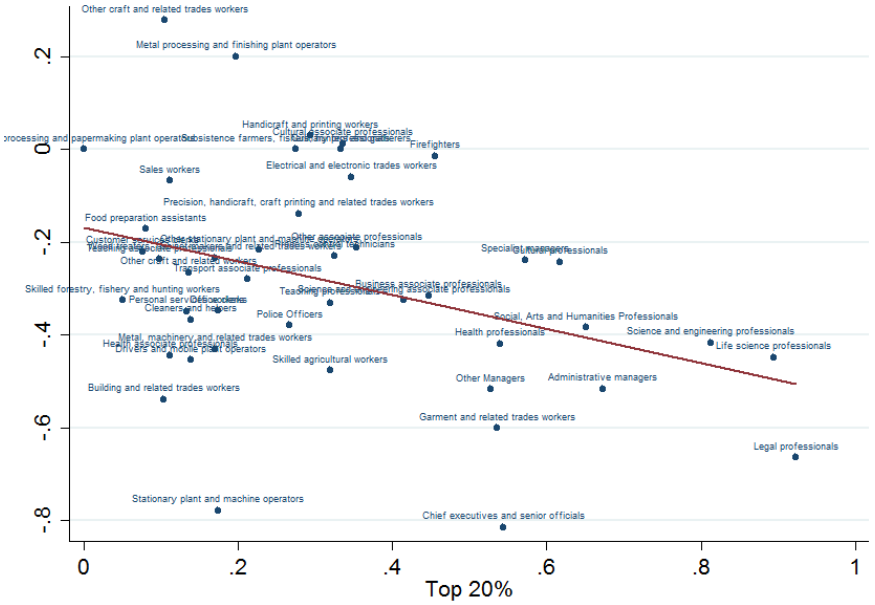
Panel B: Evolution of lower tail inequality



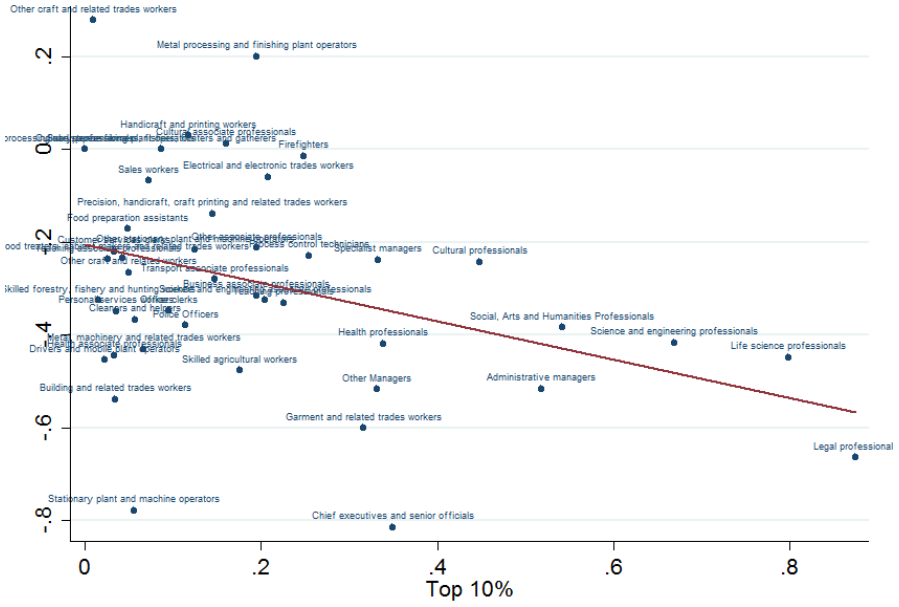
Notes: the figures illustrate inequality trends at the upper (Panel A) and the lower tails of the log earnings distribution (Panel B) from 2003 to 2015 for civil servants of the executive branch of the central, state and local governments and also private sector workers. Each point in the figure represents an estimated coefficient from regressions of the gap between the top and bottom deciles with respect to median wages on year and state dummies. Estimated coefficients are measured using the year of 2003 as baseline.

FIGURE 3.6 Returns to Occupations and Share of Top Paid Workers

Panel A: Top 20%



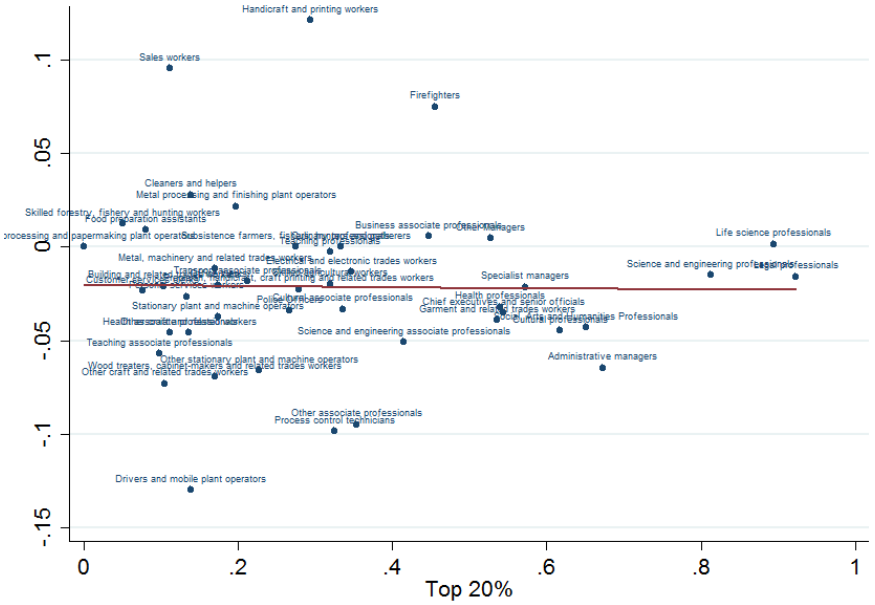
Panel B: Top 10%



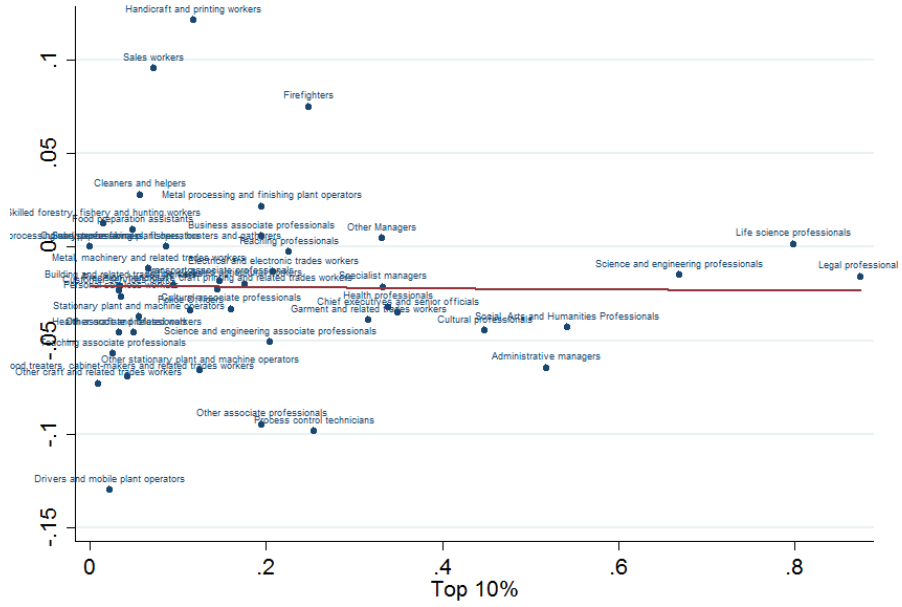
Notes: the figures show the relation between the returns to occupations in disclosing states and the share of workers in top paid occupations. The vertical axis of each panel displays the point estimates from the regression of the interaction of the disclosure dummy with occupation dummies on average log earnings by occupation. Point estimates of this regression are measured relative to the occupation with the lowest share of top paid workers. The horizontal axis show the share of workers in each occupation that are in the top vingtile (Panel A) and top decile (Panel B).

FIGURE 3.7 Composition of Occupations and Share of Top Paid Workers

Panel A: Top 20%



Panel B: Top 10%



Notes: the figures show the relation between the composition of occupations in disclosing states and the share of workers in top paid occupations. The vertical axis of each panel displays the point estimates from the regression of the interaction of the disclosure dummy with occupation dummies on average log earnings by occupation. Point estimates of this regression are measured relative to the occupation with the lowest share of top paid workers. The horizontal axis show the share of workers in each occupation that are in the top vingtile (Panel A) and top decile (Panel B).

TABLE 3.2 Effect of transparency in the public sector on wage differentials across states: 2003 to 2015

	(1)	(2)	(3)	(4)	(5)
dep. variables	State Level Employees				
p99-50	-0.129*	-0.115**	-0.061*	-0.076**	-0.097
	(0.074)	(0.050)	(0.035)	(0.031)	(0.064)
p90-50	-0.058*	-0.052	-0.050	-0.055*	-0.083*
	(0.029)	(0.031)	(0.030)	(0.030)	(0.043)
p80-50	-0.027	-0.028	-0.016	-0.023	-0.046
	(0.037)	(0.038)	(0.028)	(0.028)	(0.030)
p70-50	-0.012	-0.009	-0.003	-0.012	-0.026
	(0.021)	(0.023)	(0.017)	(0.018)	(0.027)
p60-50	-0.000	0.005	0.010	0.004	-0.007
	(0.012)	(0.013)	(0.016)	(0.014)	(0.016)
p40-50	0.006	-0.001	-0.002	-0.018	-0.004
	(0.012)	(0.012)	(0.012)	(0.015)	(0.013)
p30-50	-0.003	-0.015	-0.019	-0.028	-0.004
	(0.015)	(0.015)	(0.016)	(0.023)	(0.021)
p20-50	0.003	-0.011	-0.005	-0.012	0.022
	(0.018)	(0.018)	(0.019)	(0.027)	(0.025)
p10-50	0.001	-0.013	0.018	0.010	0.028
	(0.031)	(0.027)	(0.033)	(0.040)	(0.028)
N	3,256	3,256	3,256	3,062	3,062
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Time Varying Controls	No	Yes	Yes	Yes	Yes
Baseline Controls x Time FE	No	No	Yes	Yes	Yes
Baseline Inequality x Time FE	No	No	No	Yes	Yes
Municipal Linear Trends	No	No	No	No	Yes

Notes: The table shows point estimates of DID regressions of equation (3.1). The dependent variable is the log(p)-log(p50) in municipality m and year t, where p indicates the percentile. The independent variable is a dummy that equals 1 when state governments disclose pay information. Time varying municipal controls include logs of population, real GDP per capita, real transfers per capita, real expenditure, real revenue and number of civil servants. It also include average age, tenure, share of females and share of workers with at least high school degree. Baseline municipal covariates interacted with year dummies are based on the 2000 Brazilian Census and include the literacy rate, share of households with radio, share of households with TV, share of households with a PC, share of urban population and Gini index. Robust standard errors are clustered at the state level and reported in parentheses. Significance ***at 1% level, **at 5%level, *at 10%level.

TABLE 3.3 Effect of transparency in the public sector on wage differentials within states: 2003 to 2015

	(1)	(2)	(3)	(4)	(5)
dep. variables	State Level Employees				
p99-50	-0.188***	-0.171***	-0.216***	-0.057	-0.068
	(0.049)	(0.032)	(0.031)	(0.050)	(0.102)
p90-50	-0.237***	-0.219***	-0.269***	-0.147*	-0.221***
	(0.061)	(0.048)	(0.049)	(0.074)	(0.055)
p80-50	-0.274***	-0.260***	-0.309***	-0.080	-0.171*
	(0.030)	(0.028)	(0.031)	(0.057)	(0.088)
p70-50	-0.350***	-0.342***	-0.405***	-0.075	-0.101
	(0.033)	(0.036)	(0.047)	(0.114)	(0.134)
p60-50	-0.338***	-0.324***	-0.360***	-0.139	-0.089
	(0.062)	(0.061)	(0.074)	(0.178)	(0.290)
p40-50	-0.277**	-0.256**	-0.276**	-0.104	0.066
	(0.103)	(0.103)	(0.114)	(0.140)	(0.171)
p30-50	-0.233***	-0.207**	-0.233**	-0.201*	-0.111
	(0.083)	(0.079)	(0.088)	(0.105)	(0.145)
p20-50	-0.209***	-0.183***	-0.210***	-0.321***	-0.201
	(0.048)	(0.053)	(0.056)	(0.085)	(0.136)
p10-50	-0.166***	-0.138**	-0.167**	-0.117	-0.120
	(0.056)	(0.064)	(0.071)	(0.172)	(0.129)
N	2,993	2,993	2,993	2,993	2,993
Year FE x State FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Time Varying Controls	No	Yes	Yes	Yes	Yes
Baseline Controls x Time FE	No	No	Yes	Yes	Yes
Baseline Inequality x Time FE	No	No	No	Yes	Yes
Municipal Linear Trends	No	No	No	No	Yes

Notes: The table shows point estimates of DID regressions of equation (3.2). The dependent variable is the log(p)-log(p50) in municipality m and year t, where p indicates the percentile. The independent variable is the interaction of a dummy that equals 1 when state governments disclose pay information and the average log(p)-log(p50) in municipality m at baseline. Time varying municipal controls include logs of population, real GDP per capita, real transfers per capita, real expenditure, real revenue and number of civil servants. It also include average age, tenure, share of females and share of workers with at least high school degree. Baseline municipal covariates interacted with year dummies are based on the 2000 Brazilian Census and include the literacy rate, share of households with radio, share of households with TV, share of households with a PC, share of urban population and Gini index. Robust standard errors are clustered at the state level and reported in parentheses. Significance ***at 1% level, **at 5%level, *at 10%level.

Table 3.4 Ranking of Top Paid Occupations in State Governments

	Ni_{99}/Ni	Ni_{90}/Ni	Ni_{80}/Ni	Ni/N
	(1)	(2)	(3)	(4)
Legal Professionals	40.2	83.7	89.6	0.9
Social, Arts and Humanities Professionals	13.9	54.8	67.8	1.8
Administrative Managers	9.2	57.9	70.2	0.3
Chief Executives and Senior Officials	5.8	35.2	48.0	7.8
Science and Engineering Professionals	4.5	56.8	75.1	0.3
Firefighters	2.9	24.5	42.4	1.7
Life Science Professionals	1.8	74.9	86.4	0.2
Other Managers	1.5	33.7	49.3	0.2
Specialist Managers	1.3	34.0	51.2	0.7
Health Professionals	1.1	39.9	63.6	3.5

Notes: the Table shows the proportion of civil servants in top paid occupations of state governments. Occupations are aggregated at the two-digit level using RAIS data as described in subsection 3.4. Specifically, columns (1) to (3) illustrate the percentage of workers in occupation i that earn equal or above the top percentile, decile and vingtile, respectively. Column (4) shows the proportion of workers in occupation i among all public servants of state governments.

TABLE 3.5 Returns and Composition of Top Paying Occupations from 2003 to 2015

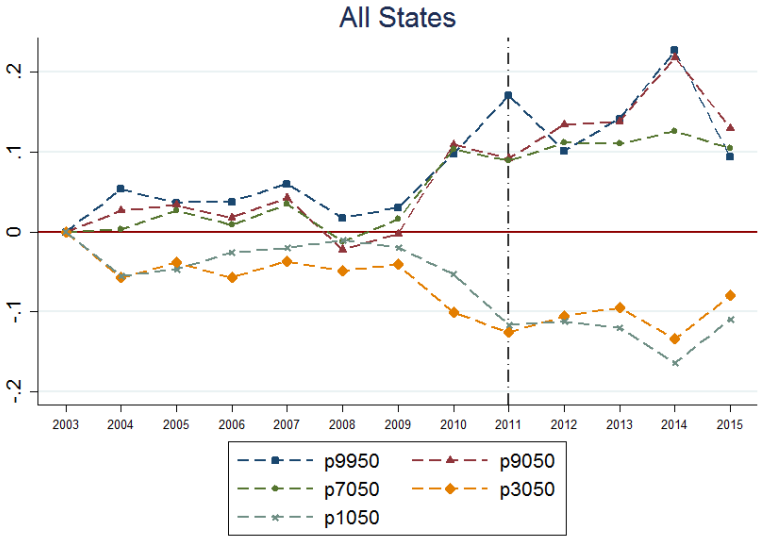
	Top 20%		Top 10%		Top 1%	
dep. variables	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average returns to occupations						
log(wages)	-0.215**	-0.190**	-0.312***	-0.257**	-0.474	-0.378
	(0.098)	(0.086)	(0.102)	(0.096)	(0.369)	(0.323)
R-squared	0.945	0.954	0.945	0.954	0.945	0.954
Panel B: Composition of occupations						
share of workers	-0.045	-0.039	-0.045	-0.039	0.309	0.196
	(0.047)	(0.035)	(0.046)	(0.033)	(0.223)	(0.117)
R-squared	0.960	0.976	0.960	0.976	0.960	0.976
N	27,885	27,885	27,885	27,885	27,885	27,885
Weights	Yes	Yes	Yes	Yes	Yes	Yes
Occupation x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation x Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Time Varying Occupational Controls	No	Yes	No	Yes	No	Yes

Notes: The table shows the effect of disclosure on average log wages or the share of workers in top paying occupations of state governments. The unit of observation is an occupation in a given municipality and year. The dependent variable is the average log wages (Panel A) or the share of workers (Panel B) in occupation i, municipality m, year t. The independent variable is the interaction of a dummy that equals 1 once state governments disclose data on pay (and 0 otherwise) and the share of top paid workers in occupation i and state s. Top paid occupations are defined, respectively, as the share of workers in the top 20%, 10% or 1% of the log earnings distribution of a given state in each occupation. All specifications are weighted by the number of workers per occupation in a given state. Time varying controls at the occupational level include the average share of female workers, tenure, age, number of workers and the average share of workers with at least primary education. Robust standard errors are clustered at the state level and reported in parentheses. Significance ***at 1% level, **at 5%level, *at 10%level.

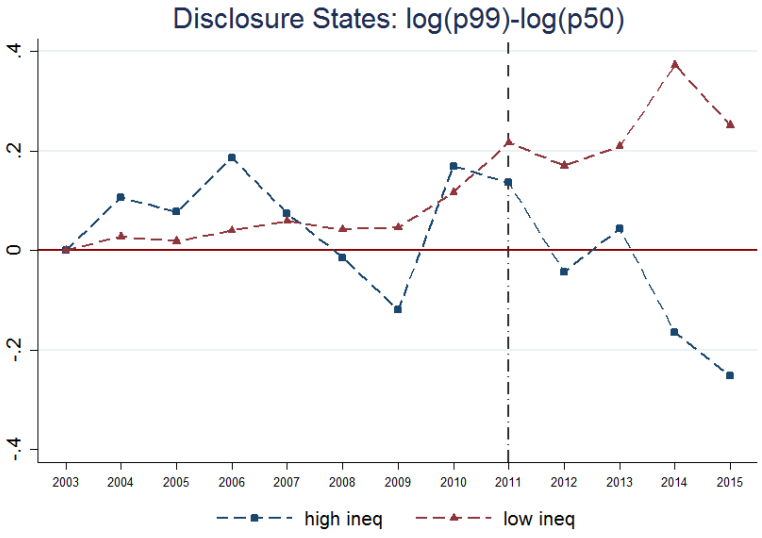
Appendix 3.A Supplementary tables and figures

FIGURE 3.A1 Inequality Trends - State Governments

Panel A: Evolution Inequality in State Governments



Panel B: Evolution of lower and upper tail inequality



Notes: panel A of the figure reports inequality trends for state governments for different decile gaps of the log earnings distribution. Panel B of the figure reports trends in the decile gap p(90)-p(50) for states with high and low levels of inequality measure in the period 2003-2009.

TABLE 3.A1 Returns and Composition of Top Paying Occupations - Nationwide Ranking

	Top 20%		Top 10%		Top 1%	
dep. variables	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average returns to occupations						
log(wages)	-0.157	-0.246**	-0.279	-0.314*	-0.874*	-0.662
	(0.154)	(0.116)	(0.174)	(0.167)	(0.431)	(0.480)
R-squared	0.945	0.960	0.945	0.960	0.945	0.960
Panel B: Composition of occupations						
share of workers	-0.048	0.021	-0.066	0.007	-0.185	-0.052
	(0.051)	(0.035)	(0.065)	(0.040)	(0.188)	(0.110)
R-squared	0.960	0.976	0.960	0.976	0.960	0.976
N	27,885	27,885	27,885	27,885	27,885	27,885
Weights	Yes	Yes	Yes	Yes	Yes	Yes
Occupation x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation x Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Time Varying Occupational Controls	No	Yes	No	Yes	No	Yes

Notes: The table shows the effect of disclosure on average log wages or the share of workers in top paying occupations of state governments. The unit of observation is an occupation in a given municipality and year. The dependent variable is the average log wages (Panel A) or the share of workers (Panel B) in occupation i, municipality m, year t. The independent variable is the interaction of a dummy that equals 1 once state governments disclose data on pay (and 0 otherwise) and the share of top paid workers in occupation i. Top paid occupations are defined, respectively, as the share of workers in the top 20%, 10% or 1% of the log earnings distribution of Brazil in each occupation. All specifications are weighted by the number of workers per occupation in a given state. Time varying controls at the occupational level include the average share of female workers, tenure, age, number of workers and the average share of workers with least primary education. Robust standard errors are clustered at the state level and reported in parentheses. Significance ***at 1% level, **at 5%level, *at 10%level.

TABLE 3.A2 Placebo test on employees of other levels of government: 2003 to 2015

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
dep. variables	Federal Level					Municipal Level				
p99-50	-0.375***	-0.319***	-0.301***	-0.021	-0.009	-0.490***	-0.463***	-0.474***	0.046	-0.028
	(0.021)	(0.019)	(0.020)	(0.039)	(0.034)	(0.023)	(0.021)	(0.020)	(0.036)	(0.046)
p90-50	-0.328***	-0.323***	-0.321***	-0.045	0.104	-0.471***	-0.456***	-0.466***	-0.058	0.014
	(0.029)	(0.029)	(0.032)	(0.064)	(0.077)	(0.028)	(0.027)	(0.024)	(0.052)	(0.045)
p80-50	-0.341***	-0.361***	-0.361***	-0.070	0.060	-0.457***	-0.445***	-0.452***	-0.071	-0.043
	(0.036)	(0.038)	(0.044)	(0.059)	(0.070)	(0.023)	(0.023)	(0.020)	(0.048)	(0.036)
p70-50	-0.376***	-0.391***	-0.402***	-0.100	0.043	-0.483***	-0.477***	-0.483***	-0.041	-0.071
	(0.044)	(0.045)	(0.041)	(0.064)	(0.052)	(0.020)	(0.018)	(0.015)	(0.027)	(0.047)
p60-50	-0.436***	-0.449***	-0.463***	-0.054	-0.042	-0.560***	-0.557***	-0.561***	0.009	-0.101
	(0.077)	(0.074)	(0.067)	(0.137)	(0.128)	(0.032)	(0.030)	(0.026)	(0.044)	(0.061)
p40-50	-0.426***	-0.436***	-0.460***	-0.004	0.064	-0.522***	-0.524***	-0.530***	0.034	0.027
	(0.055)	(0.053)	(0.047)	(0.149)	(0.185)	(0.027)	(0.026)	(0.027)	(0.037)	(0.078)
p30-50	-0.364***	-0.379***	-0.429***	-0.047	-0.062	-0.444***	-0.445***	-0.460***	0.050	0.018
	(0.037)	(0.038)	(0.041)	(0.107)	(0.264)	(0.036)	(0.035)	(0.033)	(0.034)	(0.053)
p20-50	-0.296***	-0.314***	-0.366***	0.034	-0.033	-0.414***	-0.417***	-0.439***	0.032	-0.002
	(0.032)	(0.036)	(0.043)	(0.116)	(0.206)	(0.049)	(0.049)	(0.047)	(0.031)	(0.037)
p10-50	-0.194***	-0.201***	-0.252***	0.002	0.039	-0.387***	-0.389***	-0.415***	0.004	-0.022
	(0.040)	(0.043)	(0.043)	(0.080)	(0.159)	(0.043)	(0.043)	(0.040)	(0.033)	(0.036)
N	10,727	10,727	10,727	10,727	10,727	71,160	71,160	71,160	71,160	71,160
Year FE x State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Varying Controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Baseline Controls x Time FE	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Baseline Ineq. x Time FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Municipality Linear Trends	No	No	No	No	Yes	No	No	No	No	Yes

Notes: The table shows DID regressions of equation (3.2). The dependent variable is the log(p)-log(p50) in municipality m and year t, where p indicates the percentile. The independent variable is the interaction of a dummy that equals 1 when state governments disclose pay information and the average log(p)-log(p50) in municipality m at baseline. Time varying municipal controls include logs of population, real GDP per capita, real transfers per capita, real expenditure, real revenue and number of civil servants. It also include average age, tenure, share of women and share of workers with at least high school degree. Baseline municipal covariates interacted with year dummies are based on the 2000 Brazilian Census and include the literacy rate, share of households with radio, share of households with TV, share of households with a PC, share of urban population and Gini index. Robust standard errors are clustered at the state level and reported in parentheses. Significance ***at 1% level, **at 5%level, *at 10%level.

TABLE 3.A3 Governor Term Limits and Wage Transparency across states: 2003 to 2015

dep. variables	(1)	(2)	(3)	(4)
p99-50	-0.021	-0.020	-0.037	-0.021
	(0.075)	(0.058)	(0.042)	(0.053)
p90-50	-0.021	-0.013	-0.038*	-0.024
	(0.034)	(0.025)	(0.022)	(0.026)
p80-50	-0.037	-0.032	-0.055**	-0.040
	(0.026)	(0.021)	(0.022)	(0.026)
p70-50	-0.021	-0.019	-0.036*	-0.025
	(0.021)	(0.018)	(0.018)	(0.021)
p60-50	-0.015	-0.013	-0.014	-0.012
	(0.016)	(0.014)	(0.014)	(0.016)
N	3,256	3,256	3,256	3,062
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Time Varying Controls	No	Yes	Yes	Yes
Baseline Controls x Time FE	No	No	Yes	Yes
Baseline Inequality x Time FE	No	No	No	Yes

Notes: The table shows point estimates of DID regressions based on equation (3.1). The dependent variable is the log(p)-log(p50) in municipality m and year t, where p indicates the percentile. The independent variable of interest is the interaction of a dummy that equals 1 when state governments disclose pay information and a dummy that is equal to one when there is a first term governor in state s and time t. Time varying municipal controls include logs of population, real GDP per capita, real transfers per capita, real expenditure, real revenue and number of civil servants. It also include average age, tenure, share of females and share of workers with at least high school degree. Baseline municipal covariates interacted with year dummies are based on the 2000 Brazilian Census and include the literacy rate, share of households with radio, share of households with TV, share of households with a PC, share of urban population and Gini index. Robust standard errors are clustered at the state level and reported in parentheses. Significance ***at 1% level, **at 5%level, *at 10%level.

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