Money Matters: Municipal Budgets and Asset Accumulation of Brazilian Mayors

Sigurd Storehaug Arntzen

Thesis submitted for the degree of
Master of Philosophy in Economics
30 credits

Department of Economics
Faculty of Social Sciences
University of Oslo

May 2021

Money Matters: Municipal Budgets and Asset Accumulation of Brazilian Mayors

http://www.duo.uio.no/

Printed: Reptrosentral, University of Oslo

Money Matters: Municipal Budgets and Asset Accumulation of Brazilian Mayors

A study of the Asset Accumulation of Brazilian Mayors

Sigurd Storehaug Arntzen

Acknowledgements

First and foremost, I would like to thank my supervisor, Henrik Sigstad. He has truly been vital in forming my interests in political economics, and without him, this thesis would probably never have seen the light of the day. I am grateful for having had the opportunity to work with the most inspirational and resourceful economist I have met. I could not imagine a better supervisor!

I give thanks to my parents and my sister for always believing in me and supporting me regardless of my choices in life.

Finally, I would like to thank my girlfriend, Nasra, for showing her love and support. You are my sunshine and always brighten up my day.

Needless to say, any remaining errors are my own.
All data tidying and estimations are done in RStudio.

Abstract

This paper studies the effect of additional government revenues on the asset accumulation of elected politicians. The data refers to the municipal governments in Brazil, where the federal transfers to the municipalities change exogenously and discontinuously at given population thresholds. This key feature of the federal transfers allows me to employ a regression discontinuity design. I find that larger transfers to the municipal governments increase the asset growth of the mayors. The estimate is robust to a wide range of specifications but lacks robustness for some bandwidth selections. However, the estimate remains positive for all bandwidth selections. I explore several different explanations for the variation in asset accumulation between mayors. I am unable to find any evidence of which mechanism is more important in determining the asset growth, and therefore conclude that the possible mechanism of corruption cannot be taken off the table.

Contents

List of Figures

List of Tables

1 Introduction

The recent literature on the importance of resources for economic development has emphasized the role of high-quality institutions in promoting accountability and state competence for these resources to benefit a country and its population (Sachs and Warner, 1995; Robinson et al., 2006; Mehlum et al., 2006). The idea is that it is not resources in themselves that hamper the economic development, but rather the quality of the institutions. If the institutions are of poor quality, a windfall of resources will subsequently hamper economic growth due to resource misallocation in the rest of the economy. The literature on the political resource curse has emphasized the grabbing of resource revenues by politicians, but no one has demonstrated that politicians can enrich themselves due to a windfall of government resources. In this paper, I use a regression discontinuity design to assess if the private returns to holding public office depend on the size of the government budget. Specifically, I do so in a decentralized Brazilian context by comparing the declared assets of the mayors to the size of federal transfers received by the municipalities.

The obvious problem with testing the effect of municipal budgets on private returns to holding public office is that municipal revenue can be correlated with the pool of political candidates. As an example, municipalities with higher revenues might attract politicians of higher quality or ability that likely would increase the monetary value of their assets by much, even in absence of the high municipal revenue. In order to overcome the potential problem of endogeneity in the selection of mayors, I take advantage of a key feature of the federal transfers in Brazil: the transfers made to the municipalities change exogenously and discontinuously at given population thresholds. In other words, the main source of variation in municipal resources stems from the population in that municipality. These thresholds create discontinuities in federal transfers to local governments, which I exploit to estimate the effect of federal transfers on the elected mayors' asset accumulation by using a regression discontinuity (RD) design. Results suggest that mayors in municipalities receiving higher federal transfers are increasing the monetary value of their assets by more than their counterparts in municipalities receiving less in transfers. The point estimate indicates that increasing the federal transfers by 1,559,442 Reais leads to the mayors increasing the monetary value of their assets by 73,009 Reais more, an amount equal to 37,551 USD at the average 2012 exchange rate. More precisely, I find that, on average, 4.7 % of the increased federal transfers ends up in the pocket of the mayors. However, this effect is only statistically significant for some specifications.

I perform a series of tests and robustness checks to assess the validity of the results. First, I check for manipulative sorting around the population thresholds by performing a McCrary sorting test. Second, I verify whether the treated and untreated units are comparable in a number of characteristics realized before the treatment. I do this by testing for discontinuities in pre-treatment covariates at the threshold. Third, I test whether the estimates are robust to different bandwidth selections. Fourth, I check if the estimates are robust to the inclusion of several pre-treatment covariates that might affect the outcome. Fifth, I show that the point estimate is robust to the exclusion of outliers. Sixth, I explore if the point estimate is robust to the exclusion of more populous municipalities.

For the most part, the point estimate is robust to both the inclusion of pre-treatment covariates and different functional specifications. Furthermore, the point estimate is mostly robust to less conservative ways of removing the richest outliers and to the removal of more populous municipalities. However, as I explore the robustness of the point estimate to a different bandwidth selection, I find that the point estimate is only statistically significant for some bandwidth selections. For bandwidth selections closer to the cutoff, the effect of receiving more in federal transfers on the asset growth of the mayor is statistically strong. However, for bandwidth selections further away from the cutoff, the estimated effect is less statistically significant. The estimate is, however, robust in the sense that it always returns a positive estimate, regardless of the bandwidth selection.

Although the main result suggests an effect of higher federal transfers on the asset growth of the mayor, the result is not necessarily synonymous with a theory of corruption. There might exist other channels through which the mayors ruling over larger budgets legitimately can generate a higher asset growth than their counterparts ruling over smaller budgets. In an attempt to relate the findings with a theory of corruption, I disentangle the sources of variation in the asset growth of elected mayors. Specifically, I explore four different possible sources of variation.

First, I explore the possibility that higher federal transfers to a municipality are associated with an increase in economic activity. Such an effect would mean that the mayors in these municipalities have a comparative advantage in generating a higher asset growth. Consistent with this theory I find that higher transfers are associated with higher GDP per capita in that year and the year following the payment. This is a clear indication that the increase in federal transfers leads to an increase in the economic activity in that municipality. However, this effect should also be present for the rest of the population in that municipality. To test for this effect, I use the placebo group of unelected mayoral candidates who ran for office in two subsequent elections. I find no evidence of an effect of increased federal transfers on the asset growth in the placebo group consisting of unelected mayoral candidates, a result in contrast to a theory about internal municipal demand driving the main result.

Second, I explore a possible heterogeneity in the mayoral wage. Because there is no centralized data source on mayoral wages, I am not able to exclude the possibility that the main result is generated by variations in the official wages. However, I argue that these adjustments are less common because all wage adjustments need to be sanctioned by the legislature. Most of the Brazilian municipalities are facing a critical fiscal situation, and it, therefore, seems to be little scope for the municipalities to adjust the wages. Hence, I cannot exclude the possibility that the main result is driven by differences in the official mayoral wages, but I perceive it as less likely.

Third, I discuss whether the mayors possess an informational advantage over other investors. The mayors might possess information about forthcoming events that can affect the markets in one way or another. They can use this information to generate a higher asset growth before it becomes public knowledge. Typically, this may include investing in real estate or shares with the certain knowledge that the price will rise sharply in the future. Fourth, I explore the channel of corruption. Brollo et al. (2013) show that an increase in federal transfers in Brazil leads to an increase in observed corruption. It is therefore likely that at least part of the increase in asset growth can be explained by a theory of corruption. I explore different types of corruption that could explain the variation in asset growth. To find out if the main result can be explained by theories of informational advantage or corruption, I split the declared assets into five different categories. I then perform a regression discontinuity, using the same specifications as for the main result, in an attempt to disentangle the source of variation in the asset growth. Somewhat inconsistent with a theory about informational advantage I find very little evidence of an effect of increased federal transfers on the asset growth for assets declared within the categories "Properties" and "Shareholdings, Investments, Funds." I find that the main source of variation in asset growth between mayors stems from assets declared within the category "Deposits, Credits, Savings." This could very much be consistent with a theory of informational advantage, although it then is somewhat surprising that the estimates within the other mentioned categories are so ambiguous. Similar arguments apply to the possible explanation of corruption.

I am unable to conclude that the difference in asset growth is generated through illicit activities, but because I have not been able to find any plausible explanation to the main result, the possible explanation corruption cannot be taken off the table.

This paper contributes to the literature in two main ways. First, while a lot of researchers have investigated the monetary returns to holding public office, few have discussed the role of the official government budgets. Fisman et al. (2014) investigate the wealth accumulation of Indian state politicians and find that the marginal winners have a 3-5 percent higher annual asset growth than the runners-up. They also find that the effect is greater in more corrupt states. Cunha (2019) uses data on declared assets by political candidates in Brazil and finds that marginally winning politicians have a higher asset growth than their marginally losing counterparts. None of the above have discussed a possible heterogeneity in the asset growth of winners based on the government budget. Understanding the role of budget size in determining the private benefits to holding public office might provide further evidence for understanding the incentives that politicians are facing. To this extent, researchers have investigated the effect of the official salary of politicians on the entry of political candidates.[1] My contribution to this strand of literature will be to identify that government finances beyond mayoral wages might affect the incentives to political entry. Similarly, Asher and Novosad (2018) study how natural resource rents affect the selection and behavior of holders of public office in India. They find that politicians commit more crimes and accumulate greater wealth when mineral prices rise during their term in office. However, they argue that the politicians have direct influence over mining operations but no access to fiscal windfalls from mining. Hence, to the best of my knowledge, I am the first to estimate the causal effect of the official government resources on the asset accumulation of elected politicians. Second, this paper might contribute to deepening the understanding of the mechanisms leading to the *resource curse*. A lot of research has been done on the aggregated result leading to the resource curse. The closest antecedent to this extent must be *political resource curse* as suggested by Brollo, Nannicini, Perotti, and Tabellini (2013). They use the same variation in federal transfers to find that higher federal transfers to the municipalities increase observed corruption and reduce the average education of the mayors. I contribute to this strand of literature by deepening the understanding of how the mayors in Brazilian municipalities are responding to a sudden increase in federal transfers. Although the results in this paper do not necessarily imply the existence of a resource curse in this context, the results might help us to better understand how an increase in available resources might incentivize politicians to divert funds and thereby neglecting the maximization of society's utility.

This paper also relates to other strands of the existing literature. Litschig (2012) investigates the variation in federal transfers in Brazil and finds evidence of manipulation of the population numbers during the '90s, which ultimately decides the size of federal transfers received by a municipality. He finds evidence of the manipulation being political in nature,

[1] Ferraz and Finan (2009) find that higher wages attract better-educated candidates among legislators in Brazilian municipalities. Gagliarducci and Nannicini (2013) investigate the effect of wages of mayors on the pool of political candidates in Italy. They find that higher wages attract more-educated candidates. Kotakorpi and Poutvaara (2011) take advantage of a reform in 2000 in Finland regarding the salaries of the MPs. They find that higher salaries increased the fraction of candidates with higher education among female candidates. Fisman et al. (2015) examine the impact of salaries on the composition and the behavior of legislators among Members of the European Parliament. They find that higher salaries induce more political competition.

with the right-wing central government targeting swing voters in municipalities with roughly equal right-wing and non-right-wing voters. Similarly, I find evidence of manipulation of the population estimates for some years. I contribute to this paper by highlighting that even rules-based government programs are not shielded from manipulation, and might even be subject of special-interest politics. However, I have not investigated if this manipulation was political in nature. Litschig (2013) uses the same variation in federal transfers to find the effect of unrestricted federal transfers on local spending, schooling, and learning in Brazil. He finds that the transfers increase local public spending almost one for one. Consistent with Litschig (2013), I find that the increase in federal transfers has a sharp effect on GDP in that municipality in the year of the payment and the year following the payment.

The remainder of the paper is organized as follows: Section 2 provides institutional background on the political responsibilities given to Brazilian mayors and on the federal transfers as a revenue-sharing mechanism. Section 3 describes the data and some possible limitations. Section 4 presents the econometric strategy and the theoretical foundations for the RD design. Parts of this section can be skipped for those with pre-knowledge of the RD design. Section 4 also contains a discussion of whether the RD design is applicable in the setting studied in this paper. Section 5 provides validity checks. Section 6 presents the estimation results. The reader is referred to the Appendix for several of the robustness checks. Section 7 discusses possible mechanisms that could explain the estimate. Section 8 concludes.

2 Institutional Background

This section describes the institutional background for the empirical analysis. I start by explaining the electoral system in Brazil and the role of the mayors in local politics. I then go on to discuss municipal finances, before I discuss the most important source of revenue for small to medium-sized municipalities: the Fundo de Participação dos Municipios (FPM). The FPM transfers received by a municipality are decided by that municipality's population number. I will therefore go into some detail on how the population numbers are estimated.

2.1 Electoral System and Mayoral Duties

Brazil consists of 5,570 municipalities governed by a mayor together with a council of local legislators. The local elections are held every four years, with the most recent election held in November 2020.[2] For municipalities with less than 200,000 registered voters, which represent about 98.3 % of all municipalities, the mayors are elected by plurality rule. For municipalities with 200,000 or more registered voters, the mayors are elected by a majority runoff rule. All elected mayors are term-limited, allowed to be in office for a maximum of two consecutive terms. Before running for office, all political candidates have to fill out forms made by the Brazilian Superior Electoral Court (Tribunal Superior Eleitoral (TSE)), specifying their name, date of birth, and education. Since 2006, the political candidates are also required to declare a full list of their assets and their monetary value. A candidate who is later known to have misreported his or her assets can be sentenced to prison, with a maximum sentence of five years. As of Article 37 XI of the constitution, the mayors have a uniform wage cap, with no mayor allowed to earn more than the Justices of the Supreme Federal Court.

Brazil is one of the most decentralized countries in the world (Nickson, 1995; Ferraz and Finan, 2011). Local governments received, on average 35 billion dollars from the federal government in 2011 (Ferraz and Finan, 2011). Although the constitution restricts a share of these resources to be allocated to certain sectors, the mayor and local legislators have the mandate to decide how the remaining share is to be spent. It is the responsibility of the mayor to yearly propose a detailed budget, specifying spending on all programs and public work projects. The local legislature will then analyze the proposed budget and return it to the mayor after revising items. After having received the revised budget, the mayor decides how much is to be spent on each of the approved items. Brazilian municipal governments are therefore enjoying substantial autonomy in designing and implementing policies.

2.2 Municipal Finances

The municipal governments are in charge of providing a large share of public goods and services related to health, education, and infrastructure projects. However, most municipalities

[2] The elected mayors and councilors start their four-year term in January of the subsequent year.

are highly dependent on state and federal transfers: In 2016, 81,7 % of the municipalities generated less than 20 % of their own total revenue (Firjan, 2017). For the municipalities in our sample, which are restricted to have less than 40,753 inhabitants for reasons discussed below, local taxes only represent a small part of their total revenue and these municipalities are therefore highly dependent on federal or state transfers. The most important source of income for municipal revenue is the Fundo de Participação dos Municipios (FPM), a largely unconditional transfer made to the local authorities as a measure to alleviate regional inequalities (Brollo et al., 2013). In total, the FPM transfers amount to about 75 percent of all federal transfers. According to the rules for allocation of these funds, 15 percent of the transfers are restricted to be spent on education and 15 percent is restricted to be spent on health care. The rest of the fund is unrestricted and can be spent in whatever way the municipal government pleases. The remainder of this paper will focus on the FPM transfer due to several reasons. One reason is that these types of transfers contribute significantly to the financing in small to medium-sized municipalities and that they are largely unconditional. The most important reason, however, is that the FPM transfers are allocated according to population size in a discontinuous manner, which will allow me to use the regression discontinuity design as the identification strategy.

As of Decree 1881 of 1981, the FPM transfers received by each municipality are decided by different exogenously given population thresholds. More specifically, based on population estimates each municipality is assigned a coefficient. Municipalities with less than 10,189 inhabitants are assigned the coefficient 0.6; municipalities from 10,189 to 13,584 inhabitants are assigned the coefficient 0.8 and so forth. The population thresholds and the associated FPM coefficients are reported in Table 1. These coefficients are used by the states to allocate the FPM resources among the municipalities. In order to clarify how the FPM transfers are allocated, define $FPM_{m,s}$ as the FPM transfer received by municipality m in state s in a given year. Then the revenue-sharing mechanism is

$$FPM_{m,s} = \frac{\lambda_m}{\sum_{m \in s} \lambda_m} FPM_s \tag{1}$$

where FPM_s is the FPM transfers allocated to state s and λ_m is the coefficient assigned to municipality m as according to the step function presented in Table 1. It then becomes clear that the only way two municipalities within the same population bracket will receive the same amount of FPM transfer is if they are located in the same state.

There are 3,396 inhabitants in the interval between the first three thresholds, while the intervals between the subsequent thresholds are twice as much (6,792 inhabitants). Similar to both Brollo et al. (2013) and Litschig and Morrison (2013), I restrict the sample to municipalities from 3,396 inhabitants below the first population threshold, mainly with symmetry in mind, and up to 3,396 inhabitants above the sixth threshold of 37,357 inhabitants. This means that I am only studying municipalities in the population interval 6,793 to 40,753, still constituting about 54 % of all Brazilian municipalities as per the 2020 population estimates.

According to Litschig and Morrison (2013), the increase in FPM transfers at the cutoffs in more populous municipalities is too small to affect the overall budget. This could imply that there is no "first stage" in terms of the budget size for the municipality. Litschig and Morrison (2013) therefore focus on municipalities within the first three population brackets. Instead, I follow Brollo et al. (2013) and include more populous municipalities. In the Appendix, I will explore how robust the estimates are to the exclusion of more populous municipalities.

Table 1: Population Thresholds and FPM Coefficients

Population Interval	FPM Coefficient
Below 10,189	0.6
10,189-13,584	0.8
13,585-16,980	1.0
16,981-23,772	1.2
23,773-30,564	1.4
30,565-37,356	1.6
37,357-44,148	1.8
44,149-50,940	2.0
Above 50,940	2.2-4

Notes: This table displays the population intervals and the associated FPM coefficients that are decisive in determining how much a municipality receives in FPM transfers. Source: Decree 1881/81.

Figure 1 depicts the yearly FPM transfers made to the municipalities from 2014 to 2020 against the population size. The scatterplot is averaged over 100-inhabitant bins, with the

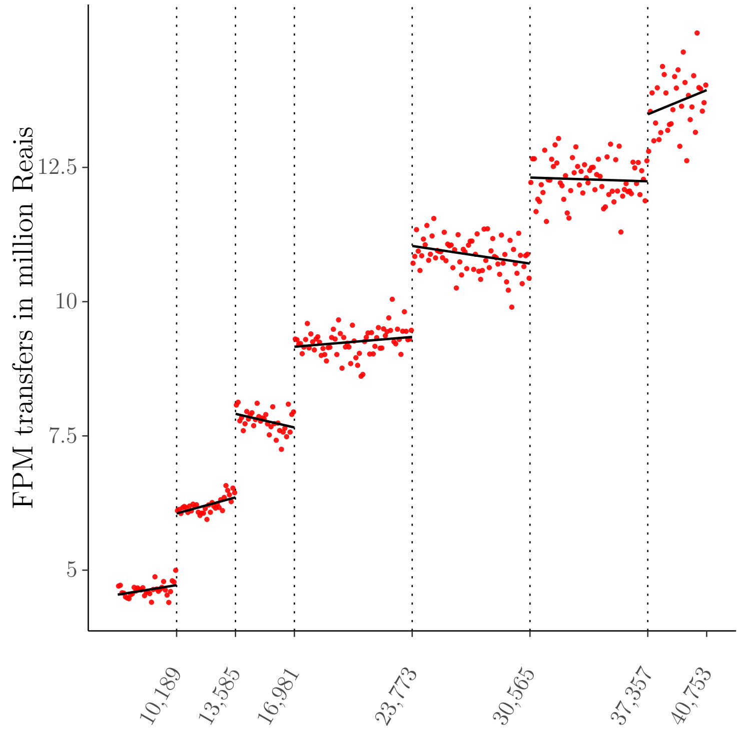


Figure 1: A scatterplot of yearly FPM transfers against population size for the final sample in the years 2014 to 2020. The scatterplot is averaged over 100-inhabitant bins while running a mean-smoothing separately in each regression line and making sure that no bins are containing observations on both sides of the threshold.

smoothed average of transfers calculated in each interval, while making sure that no bins are containing observations on both sides of the thresholds.

2.3 Population Estimates

The coefficient given to each municipality is decided by the Federal Court of Accounts (Union Tribunal de Contas União (TCU)), using the yearly population estimates from the Brazilian Institute of Geography and Statistics (Instituto Brasileiro de Geografia e Estatística (IBGE)). From the beginning of 1989, the population estimates have been updated yearly, with the exception of years where there is a national census, in which alternative strategies have been used to calculate the number of inhabitants. The IBGE currently uses a top-down approach by first estimating the population growth in the whole country, then finding consistent estimates for the lower-level units such as states or municipalities (Litschig, 2012).

First, the IBGE creates a population estimate for Brazil based on mortality rates, birth rates, and net migration. The 26 states are then assigned their share of the population growth based on previous census population numbers. The municipalities are then put into 20 different groups based on population number according to previous census and past population growth in-between census-years. These 20 groups of municipalities are then assigned their share of the population estimate in that specific state. Finally, the municipality is assigned its population estimate. As this process has a big impact on the finances in small to medium-sized municipalities, the municipal governments have the opportunity to make a complaint about the official IBGE estimate. In these cases, an official court of justice will decide the final outcome.

3 Data

In this section, I present the data sources and discuss how I have constructed the final data sample used for the analysis. I will also discuss some limitations of the data.

3.1 FPM Transfers and Population Estimates

Every year, the IBGE publishes the population estimates for all the municipalities. These estimates are available online and the same estimates are sent to the TCU who will decide the coefficients. The data on FPM transfers is updated monthly by the TCU and is available online. For the balance tests performed in Section 5, I rely mostly on the 2010 census made by the IBGE containing pre-treatment characteristics for every municipality in the final sample.

3.2 Electoral Data

The Brazilian Superior Electoral Court (Tribunal Superior Eleitoral (TSE)), which is the highest judicial body of the Brazilian Electoral Justice, is responsible for publishing a publicly available database containing information about every election in Brazil. The data contains information on electoral results, characteristics of the political candidate (such as name, age, education, skin color), and a list of all the declared assets and its monetary values of the political candidate in an election year.

The data has some limitations that might be a concern. The first is that I am only able to observe the asset growth of political candidates running in two subsequent elections. This implies that candidates running for office in the 2012 election, but not in the 2016 election are excluded from the analysis. A second concern is misreporting of assets. There seems to be little reason why candidates would overreport the monetary value of their assets. Cunha (2019) finds that the Brazilian federal tax authority (Receita Federal) uses information on the candidates' assets as a measure to detect tax irregularities. Hence, there is a strong incentive to underreport the assets, and I expect candidates to exclude unlaundered assets or assets obtained in illegal ways. For the purpose of this paper, the question is: Are candidates running for mayor in more populous municipalities more able to hide their assets than candidates in less populous municipalities, or do they have a bigger incentive to misreport their assets? Performing a series of balance tests in Section 5 on pre-treatment covariates such as the declared assets of mayors pre-election show that there are very few discontinuities, implying that mayors "just above" and "just below" the cutoff should be just as able to evade taxes.

Another concern about the possible misreporting of assets is that people have been misreporting by coincidence. I have found some instances of candidates misreporting the value of their assets seemingly by accident by merely adding an extra 0.[3] However, I did find very few cases of such by-accident misreporting. If misreporting is happening by coincidence, there is little reason to believe that it should threaten the validity of the research significantly. Furthermore, I exclude 7 percent of the richest mayors in terms of declared pre-election assets, which also is likely to exclude some of the most severe cases of misreporting.

To assess the monetary rent of being in office, I only use data for candidates that ran for mayor in two subsequent elections, either in 2012 and 2016 or in 2016 and 2020. In the municipalities in my sample, 8,335 candidates ran for mayor in 2012, of whom 2,699 ran again in 2016, while 8,517 candidates ran for mayor in 2016, of whom 2,912 ran again in 2020.

[3] For example, the mayor in Japaratuba declared a Hyundai from 2001 to be worth about 20 million dollars ahead of the 2020 election. Other candidates report that the same property 10 or 100-doubled its value in just four years.

From the TSE database, these restrictions leave me with 5,611 mayoral candidates, of whom 2,324 went on to be elected and 3,327 were not. For the final data sample, I remove about 7 percent of the richest mayoral candidates in terms of their declared assets pre-election. That is, all candidates with the value of declared assets exceeding 2,000,000 Reais, corresponding to about 1,000,000 USD, are excluded. After removing these outliers, I am left with 2,161 elected mayors and 3,082 candidates in the placebo group of unelected mayoral candidates. All the asset values are deflated to the 2012 level according to the Extended National Consumer Price Index (Índice Nacional de Preços ao Consumidor Amplo (IPCA)) from the IBGE. Specifically, I will use two different measures of asset growth as the outcome variable. The first is by looking at the absolute increase in assets measured in Reais from one election to the next one. The term absolute asset growth refers to the fact that the value of declared assets is deflated to the 2012 level, and hence will measure the real asset growth. The second measure is the percentage growth in assets. This method is more sensitive to mayors that declared low asset value (e.g. 0.01 Reais) in the first period.

3.3 Summary Statistics

Summary statistics for all candidates running for mayor in two subsequent elections, both elected and unelected, are presented in Panel A of Table 2. About 42 % of the candidates that are running for mayor in both 2012 and 2016 or in both 2016 and 2020 went on to be elected. Most of the candidates are running for the party MDB, but a considerable amount is also running for PSDB and PT. Most of the candidates are male, of white skin color, and have finished higher education. Summary statistics for the municipalities, in which the political candidates are running, are reported in Panel B of Table 2.

4 Estimation Approach

The main objective of this paper is to test the effect of additional FPM transfers on the asset growth of the elected mayors. One way to do this is by regressing the FPM transfers made to a municipality on the asset growth of the mayor in that municipality. Assuming a linear relationship between asset growth and the municipal budget, a simple way of estimating this

Table 2: Summary Statistics

Statistic	Mean	St. Dev.	N
Panel A: Candidate characterstics			
Sum assets pre election (in 1,000 Reais)	633.897	2,163.205	19,151
Sum assets post election (in 1,000 Reais)	772.587	8,692.921	19,151
Candidate male	0.888	0.315	19,151
Primary school	0.086	0.280	19,151
High school	0.344	0.475	19,151
Higher education	0.504	0.500	19,151
Married	0.719	0.449	19,151
Single	0.181	0.385	19,151
Divorced	0.071	0.258	19,151
Brown skin color	0.318	0.466	19,151
Black skin color	0.027	0.163	19,151
White skin color	0.646	0.478	19,151
Running for MDB	0.153	0.360	19,151
Running for PT	0.079	0.270	19,151
Running for PSDB	0.108	0.310	19,151
Running for PP	0.072	0.259	19,151
Running for PDT	0.066	0.248	19,151
Running for PTB	0.045	0.206	19,151
Running for DEM	0.045	0.208	19,151
Running for PL	0.056	0.229	19,151
Running for PSB	0.065	0.247	19,151
Age at election day	47.692	10.284	19,151
Elected	0.424	0.494	19,151
Panel B: Municipal characteristics			
Estimated population	18,004.200	8,783.326	19,151
Proximity to closest threshold	−198.585	1,756.460	19,151
Areal	1,967.624	6,409.053	19,151
Gini coefficient	0.471	0.049	19,151
Literacy rate	0.832	0.096	19,151
Unemployment rate	0.068	0.035	19,151
Human Development Index	0.647	0.071	19,151
North	0.097	0.296	19,151
Northeast	0.358	0.479	19,151
Center	0.086	0.281	19,151
South	0.165	0.371	19,151
Southeast	0.295	0.456	19,151

Notes: Summary statistics displaying means and standard deviations of various characteristics for the municipalities and the political candidates running for mayor. Panel A shows statistics on politician characteristics such as declared assets, educational attainment, sex, marital status, skin color, age, and political affiliation. Panel B shows statistics for municipality-level characteristics.

is by fitting the linear regression

$$y_{im} = \alpha + \beta_1 \; FPM_m + \epsilon_{im} \tag{2}$$

where y_{im} is the asset growth of mayor i in municipality m, FPM_m is the FPM transfers received by municipality m and ϵ_{im} is the usual error term. A potential problem with this strategy is, however, that the size of transfers made to a municipality is correlated with the politicians running for election. Consider for example that higher transfers to a municipality might attract a different set of politicians of higher ability. Observing higher asset growth of the mayors in richer municipalities might be due to the fact these mayors also are more able, and *not* because they rule over a larger budget. It is reasonable to expect that this naïve approach would lead to biased estimations. A solution to this is of course to control for individual characteristics or municipality characteristics, such as education, skin color, age, GDP per capita, and similar. I could then fit the linear regression

$$\begin{aligned} y_{im} = \; & \beta_1 \; FPM_m + \beta_2 \; education_i + \beta_3 \; skincolour_i + \\ & \beta_4 \; age_i + ... + \beta_{12} GDP \; per \; capita_m + \sigma_t + \delta_s + \; \epsilon_{im} \end{aligned} \tag{3}$$

where y_{im} is the asset growth of mayor i in municipality m. I control for education, skin color, age, GDP per capita, and several other characteristics of the mayor and the municipality in which he/she is running. All of the control variables included will be presented below. I include the year fixed effects σ_t and the state fixed effects δ_s.

The inclusion of control variables might remove some of the potential bias in Equation 2. But how can I be sure that I have controlled for all variables affecting the asset growth of the mayor? There is no way to be sure of this. So to estimate the effect of additional FPM transfers on the asset growth of the mayors, it is necessary to overcome the problem of endogeneity in the selection of mayors. In the next section, I will present a possible solution.

4.1 Theoretical Foundations of the RD Design

The regression discontinuity (RD) design is a non-experimental research strategy used to study the causal effect of a treatment. The distinctive feature of the RD design is that all units receive a score, and all units whose score exceeds a certain threshold or cutoff are offered a treatment, while all units below that threshold remain untreated. As long as the units' characteristics do not change abruptly at the threshold, the change in treatment status decided by the discontinuous treatment assignment can be used to study different causal treatment effects on outcomes of interest (De la Cuesta and Imai, 2016; Cattaneo et al., 2019). I will now present the main assumptions behind the RD design closely following the notation from Cattaneo et al. (2019) before I go on to discuss how this identification strategy is applicable in the Brazilian context studied in this paper.

Denote S_m as the distance in population number to the closest threshold in municipality m. A municipality with 11,000 inhabitants will be closest to the first threshold of 10,189 inhabitants. Hence, the distance to the closest threshold for this municipality will be $S_m = 11,000 - 10,189 = 811$. Similarly, a municipality with 16,000 inhabitants will be closest to the third threshold of 16,981 inhabitants, so that $S_m = 16,000 - 16,981 = -981$. Because the municipalities receive treatment when their score exceeds the closest population threshold, while municipalities below the threshold remain untreated, it is appropriate to apply a sharp RD design, implying that the assignment of treatment coincides with the actual treatment taken. The sharp RD treatment assignment can then be written as

$$T_{im} = \mathbb{1}(S_m \geq 0) = \begin{cases} 1 & \text{if } S_m \geq 0 \\ 0 & \text{if } S_m < 0 \end{cases} \tag{4}$$

where $\mathbb{1}(\cdot)$ is the indicator function and the cutoff-value is defined as $c = 0$. In our case $T_{im} = \mathbb{1}(S_m > 0)$ is the binary variable that indicates whether mayor i is mayor in a municipality m above or below the closest population threshold as defined in Table 1.

The goal of this paper is to assess the effect of the binary treatment T_{im} on the asset growth of the elected mayors. In this framework, there are two possible outcomes for every unit in the population. $y_{im}(1)$ is the asset growth for mayor i in presence of the treatment ($T_{im} = 1$), while $y_{im}(0)$ is the asset growth in absence of treatment ($T_{im} = 0$). It then clearly follows that the treatment effect for mayor i in municipality m is defined as the difference in potential outcomes, $\tau_{im} = y_{im}(1) - y_{im}(0)$. The problem with estimating τ_{im} is, however, that we are only able to observe one of the potential outcomes, while the other remains counterfactual. This is known as the *fundamental problem of causal interference* (Holland, 1986).

The RD design offers a way to address this problem by comparing treated units that are slightly above the cutoff and units that are slightly below the cutoff. The idea is that under the appropriate assumptions, treated and untreated units in a small neighborhood very close to the cutoff are similar in observed and unobserved characteristics, with the only exception being the treatment status.

Under this setup, we can define the continuity assumption as

$$E[y_{im}(1)|S_m = 0] = \lim_{s \downarrow 0} E[y_{im}(1)|S_m = s] \tag{5}$$

$$E[y_{im}(0)|S_m = 0] = \lim_{s \uparrow 0} E[y_{im}(0)|S_m = s] \tag{6}$$

where the limit is taken from above the threshold in the first equation and below the threshold in the second equation. Equations (5) and (6) imply that there is no discontinuous jump in the conditional expectation function of the potential outcomes at the cutoff. The continuity assumption states that the observed and unobserved characteristics that determine the average potential outcomes do not jump suddenly at the threshold. When this assumption holds, the only difference between units that are "right above" and "right below" the cutoff is their treatment status. Furthermore, the units above and below the threshold can be very different in terms of observable and unobservable characteristics affecting the outcome of interest, but as their scores approach the cutoff value and therefore become similar in that aspect, the only remaining difference between treated and untreated units is their treatment status. This will ensure comparability between the treated and untreated units. Formally, when the conditional expectation function is continuous at the cutoff value $c = 0$, we can define the average treatment effect at the threshold as

$$\tau(0) = \lim_{s \downarrow 0} E[y_{im}|S_m = s] - \lim_{s \uparrow 0} E[y_{im}|S_m = s] \tag{7}$$

The first conditional expectation measures the expected asset growth for mayors at the threshold in municipalities that are slightly above the population threshold. The second conditional expectation measures the expected asset growth for mayors at the threshold in municipalities that are slightly below the population threshold. Given that the continuity assumption holds, Equation (7) would equal

$$E[y_{im}(1) - y_{im}(0)|S_m = 0] \tag{8}$$

Equation (8) is the average treatment effect at the cutoff $c = 0$. So to summarize, the continuity condition enables us to use the average outcome for those slightly below the threshold ($T_{im} = 0$) and those slightly above the threshold ($T_{im} = 1$), to estimate the causal effect of the treatment.

However, there are some problems related to this estimation strategy. Since the score is continuous, there are in fact very few units with a score exactly equal to the cutoff value. To estimate the limits of $E[y_{im}|S_m = s]$ as s approaches 0 from either above or below I will therefore have to extrapolate from the cutoff. It is therefore required to specify a bandwidth around the cutoff in which to approximate the regression function $E[y_{im}|S_m = s]$. This approximation will then be used to calculate the value that the function has exactly at $s = 0$. This means that the calculated value at the cutoff, and hence also the estimate, is sensitive to the bandwidth selection. While selecting the bandwidth, there is typically a trade-off. On the one hand, selecting a small bandwidth around the cutoff might reduce the misspecification error in the approximation and hence reduce the bias. On the other hand, a small bandwidth will require discarding a large fraction of the observations, meaning a smaller sample which typically will lead to estimators with a larger variance. The goal of any bandwidth selection should therefore be to balance these two opposing interests.

In order to avoid possible subjectivity and researchers' discretion in the choice of bandwidth, I use the data-driven *mean squared error* (MSE) criterion (Imbens and Kalyanaraman, 2012; Calonico et al., 2019). More specifically, I use the bias-corrected estimator proposed by Calonico, Cattaneo, and Titiunik (2014), henceforth "the CCT estimator", employing a local-linear regression for the estimates and a local quadratic regression for the bias-correction. The idea is to estimate the bias of the local RD estimator by using a local regression of polynomial p+1 to take into consideration the variance in the bias estimation. The CCT estimator is equal to the sum of the conventional estimator and the bias-correction term (Pei et al., 2021). I use a triangular kernel function to construct the local-polynomial estimator. All standard errors are clustered at the municipality-level.

4.2 Estimating the Causal Effect of FPM Transfers

In order to estimate the causal effect of increased federal transfers on the asset growth of the mayor, the ideal experiment would either be to randomly assign mayors to the municipalities, or similarly, randomly assign the transfers to the municipalities. With a random assignment of the treatment, I should be able to estimate the causal effect of federal transfers on the mayor's asset growth. For obvious reasons I am not able to run this experiment, so I exploit the fact that FPM transfers are allocated to the municipalities in a discontinuous manner according to population size as according to Table 1. The idea is that the population size for municipalities close to the threshold is as good as randomly determined. Thus, the mayors and municipalities above and below the population thresholds should on average be similar in characteristics, and any difference in the asset growth of the mayors has to be due to the difference in FPM transfers. The only difference and discontinuity we observe around this threshold are that the municipalities above a population threshold, on average, receive 22% more in FPM transfers than the municipalities below the threshold. Formally, for the main specification I use the CCT estimator proposed by Calonico, Cattaneo and Titiunik (2014) with local linear regression for the estimate and a local quadratic regerssiom for the bias-correction. The specification for the local linear regression is

$$y_{imt} = \alpha + \beta D_{mt} + \gamma S_{mt} + \delta D_{mt} S_{mt} + \epsilon_{imt} \tag{9}$$

where m is the municipality in which mayor i is elected and t is the year. D_{mt} is a dummy variable taking the value 1 if the municipality is above the threshold in a year and 0 otherwise, and S_{mt} is the distance in population for that municipality to the closest threshold that year. ϵ_{imt} is the usual standard error clustered at the municipality level. The outcome y_{imt} will vary, but all outcomes will measure the asset growth in some way.

In the next section, I will discuss whether the continuity assumption is satisfied within this context.

5 Validity Checks

A crucial assumption that needs to be satisfied for the regression discontinuity design to be valid is the so-called "continuity assumption", which states that the only change that occurs at the point of discontinuity, is the change in the treatment status (Hahn et al., 2001). If the individuals can precisely manipulate the running variable, this could represent a violation of the continuity assumption. I check for this by performing a McCrary sorting test. Furthermore, I check if the continuity assumption holds by performing a series of balance tests on pre-treatment covariates. The results of both of these tests are presented in this section.

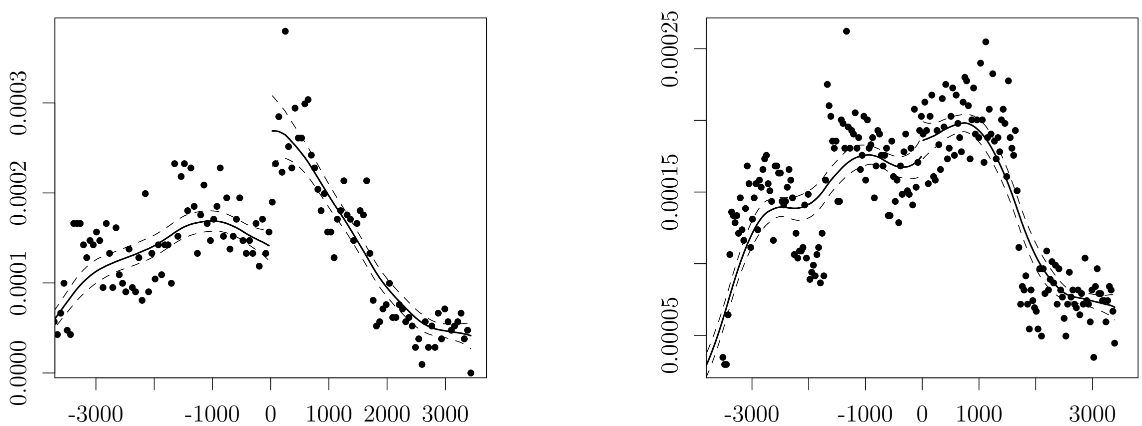
5.1 McCrary Sorting Test

In the setting studied in this paper, if the municipalities can manipulate their population estimates, the continuity assumption will break and thereby invalidate the regression discontinuity design. Specifically, at two points in the process of deciding the FPM transfers, there is a possibility for manipulating the population estimates. The first possibility is that the TCU population estimates do not correspond with the IBGE estimates. Similar to Brollo et al. (2013) this problem is addressed by manually comparing the TCU estimates with the IBGE estimates for some years. Based on the years I have checked, there is no reason to believe that the TCU are systematically manipulating the coefficients to favor certain municipalities. Furthermore, at the end of every year, the TCU makes a report containing the population estimates and the corresponding coefficients publicly available. Any mismatch in population estimates or corresponding coefficients is therefore very likely to be detected.

The second possibility of manipulation of the population estimates is through the official IBGE estimates. I use the McCrary-test to check for sorting by estimating discontinuities in the density function of the running variable around the population thresholds (McCrary, 2008). This test is justified as the treatment in this study is monotonic, i.e. there is a clear benefit of manipulating population size to be slightly above the threshold, while there is little reason to believe why a municipality would want to be below a threshold. Formally, I test the null hypothesis of continuity in the density of the running variable at the cutoff value against the alternative hypothesis of a jump in the density function at the cutoff. To reject the possibility of sorting around the threshold, I have to implement a local linear density estimator. This is done in a two-step process. In the first step, I create an undersmoothed histogram, making sure that the bins for the histogram are defined such that not one bin includes points to both sides of the point of discontinuity. The second step is to create a local linear smoothing of the scatterplot. To identify the potential discontinuity in the density, the local linear smoothing is conducted separately for the bins on each side of the threshold, with a bandwidth selection as according to McCrary (2008).

I find strong evidence that the population estimates were manipulated in the years 2010, 2011, and 2012, showing a statistically significant discontinuity in the distribution around most of the thresholds. In Figure 2 panel (a) I show the density of the distance to the population thresholds in the estimates from the IBGE in the years 2010, 2011, and 2012. There is a clear discontinuity in the density at the cutoff point with a p-value smaller than 0.00001, implying that I can reject the null hypothesis of continuity in the density of the running variable at the cutoff at a 1 percent significance level. The McCrary test has also been performed separately for each threshold for each year, and the discontinuity seen in Figure 2 is maintained through most of the thresholds in the years 2010 to 2012.

I have not been able to find any information that the IBGE changed their calculation method for population estimates within this time period. So, it rather seems that the discontinuity at the cutoff stems from a population census conducted in 2010. Similarly, there was a trial population census in 2007 where I find strong evidence of sorting around the population



(a) Distribution of Population Estimates Years 2010-2012

(b) Distribution of Population Estimates Years 2013-2019

Figure 2: Graphical output from McCrary tests with the density on the y-axis and the proximity to the closest population threshold on the x-axis. Panel (a) shows a clear discontinuity in the density of distance to population threshold suggesting that in the period 2010-2012, the municipalities were somehow able to manipulate their population estimates above the threshold. Panel (b) shows the density of proximity to the population thresholds in the subsequent years 2013-2019, showing a smooth distribution of the running variable at the cutoff, implying that the massive manipulation of population estimates had been corrected.

thresholds. The subsequent population estimates for 2008 and 2009 disregarded the trial census from 2007 and did again possess a continuous distribution around the thresholds, suggesting that the possibility of manipulating population estimates increases in census years. Furthermore, the census is likely to create a strong incentive for the municipal governments to manipulate the population as this will create the foundation for the population estimates in the subsequent years and hence could imply higher FPM transfers of magnitude to the municipal government.

Similar to this result, Litschig (2012) finds strong evidence of population estimates being manipulated after the 1991 population census, and that this manipulation ultimately led to wrong population estimates in the subsequent years. This is also what we observe in the data. The population estimates in the two subsequent years after the 2010 census all possessed the same discontinuity around the population thresholds. The 2011 estimates were one-to-one with the population reported in the 2010 census, and the 2012 population estimates possess the same discontinuous "jump" around the threshold. By 2013, the population estimates again possess smoothness around the thresholds, implying that the massive manipulation that happened in the years 2010-2012 was corrected. The McCrary sorting test on the running variable in the years 2013-2019 results in a p-value of 0.37 implying that I cannot reject the null hypothesis of continuity in the density of the running variable at the cutoff while using a 10 percent significance level. This is graphically shown in panel (b) of Figure 2, showing a smooth density around the threshold compared to panel (a). The McCrary sorting test has been performed on all thresholds and all years. A table showing the resulting p-values calculated separately for each threshold in the years 2007-2019 is presented in Table 10 of the Appendix.

In the continuity-based approach, the regression functions must be smooth functions at the cutoff. If this assumption is violated, it imposes a threat to the validity of the estimation approach. Sorting around the threshold could be a violation of this underlying assumption, and the years in which there is strong evidence of manipulation will thus have to be excluded. Therefore, I will only use the population estimates from 2013 to 2019, implying that I am only looking at the FPM transfers made from 2014 to 2020.

The McCrary sorting test is, however, not able to detect more sophisticated or subtle ways of manipulating the population estimates. Nonrandom sorting could for example be that the highest bidders receive beneficial population estimates, or that the manipulation of population estimates is political in nature. This will be explained in the next section.

5.2 Balance Tests

If the variation in the treatment around the threshold is "as good as randomized", then it follows that the baseline characteristics of the mayors or municipalities should have the same distribution just above and below the cutoff point. If there is a discontinuity in one or more of the baseline covariates, likely, the assumption of an individual's ability to manipulate the running variable is unwarranted (Lee and Lemieux, 2010).

Thus, I use the baseline covariates - that is all those variables determined prior to the realization of the treatment - to test the validity of the RD design. E.g. if we observed nonrandom sorting that was political in nature, it is expected to observe discontinuities at the cutoff for mayors from certain political parties, indicating that mayors from these political parties are more likely to be above the cutoff than they are to be below. Table 3 shows the balance tests performed on pre-treatment covariates comparing municipalities and elected mayors that are slightly above and below the threshold. Out of the 31 mayor characteristics I test for, there is some evidence of imbalance in 2 of the variables. The estimate on the covariate "white skin color" indicates that in our sample, there is a discontinuity around the threshold for mayors of white skin color, with more of these mayors being slightly below the population threshold than slightly above. The estimated difference is significant at a 5 percent level with a p-value of 0.024. Logically, there is also a discontinuity around the threshold for mayors of brown skin color, with a p-value of 0.032. Finding imbalances in 2 out of 31 mayor characteristics at a 10 percent significance level is however consistent with chance. Similarly, I perform balance tests on 29 municipal pre-treatment covariates. Other than the obvious and reassuring discontinuity in the FPM transfers, I find no evidence of any difference between the treated and untreated units in 28 municipal characteristics determined prior to the treatment around the threshold.

In total, I find some evidence of imbalance for 2 of the 52 observed covariates, using a 10 percent significance level. This is a small fraction that is largely consistent with chance. Still, to show that these imbalances are unlikely to impose a threat to the estimation strategy, I show that the estimates are robust to the inclusion of these covariates as controls. In general, there is little evidence of discontinuities in the baseline characteristics of the mayors or municipalities, and the data, therefore, fails to reject the continuity assumption around the threshold.

Table 3: Balance Test

	Difference	Mean	P-value	Observations
Panel A: Candidate characterstics				
Declared assets pre-election (in 1,000 Reais)	−63.700	392.813	0.168	7,595
Max contribution party (in 1,000 Reais)	−5.303	162.046	0.867	7,595
Male	0.027	0.878	0.476	7,595
Primary school	−0.029	0.079	0.384	7,595
High school	−0.013	0.393	0.853	7,595
Higher educational attainment	0.105	0.444	0.151	7,595
Married	−0.033	0.710	0.604	7,595
Single	0.005	0.187	0.921	7,595
Divorced	0.030	0.075	0.395	7,595
Widow	−0.002	0.010	0.906	7,595
Separated	0.012	0.015	0.450	7,595
Brown skin color	0.129**	0.243	0.032	7,595
Black skin color	0.025	0.028	0.418	7,595
White skin color	−0.150**	0.726	0.024	7,595
Running for MDB	−0.052	0.185	0.351	7,595
Running for PT	0.002	0.070	0.961	7,595
Running for PSDB	−0.027	0.060	0.406	7,595
Running for PP	−0.007	0.060	0.826	7,595
Running for PDT	0.019	0.057	0.541	7,595
Running for PTB	0.018	0.015	0.384	7,595
Running for DEM	0.047	0.070	0.214	7,595
Running for PL	0.005	0.056	0.875	7,595
Running for PSB	0.042	0.099	0.322	7,595
Age at election day	−1.043	46.632	0.450	7,595
Working in agriculture	0.012	0.048	0.686	7,595
Working in municipal administration	0.024	0.020	0.362	7,595
Merchant	0.004	0.085	0.905	7,595
Business person	−0.019	0.189	0.698	7,595
Councilor	−0.0004	0.041	0.985	7,595
Retired	0.012	0.012	0.507	7,595
Teacher	0.038	0.033	0.154	7,595
Panel B: Municipal characteristics				
FPM transfers (in millions)	1.578***	7.082	0.00000	7,595
GDP per capita (in 1,000,000 Reais)	0.743	11.237	0.489	7,595
Areal	−348.750	1,726.057	0.497	7,595
Agriculture as share of GDP	0.0005	0.190	0.979	7,595

Industry as share of GDP	0.011	0.176	0.544	7,595
Service as share of GDP	−0.009	0.629	0.635	7,595
Share with incomplete elementary school	−0.014	0.621	0.318	7,595
Share with completed elementary school	0.006	0.162	0.195	7,595
Share with completed high school	0.006	0.173	0.421	7,595
Share with completed higher education	0.002	0.039	0.471	7,595
Mean hours worked	0.334	38.892	0.515	7,595
Rate of people commuting	−0.006	0.688	0.656	7,595
Human Development Index	0.013	0.636	0.197	7,595
Mean per capita income	30.343	426.731	0.306	7,595
Life expectancy at birth	0.619	72.167	0.114	7,595
Existence of anti-forced labour program	0.020	0.257	0.748	7,591
North	−0.013	0.085	0.692	7,595
Northeast	−0.048	0.419	0.472	7,595
Center	0.012	0.040	0.606	7,595
South	−0.027	0.217	0.610	7,595
Southeast	0.091	0.227	0.146	7,595
Share employed in agriculture	−0.012	0.365	0.627	7,595
Share employed in industry	0.007	0.174	0.577	7,595
Share employed in service	0.006	0.462	0.716	7,595
Share not working or studying (15-24 years)	0.0003	0.230	0.977	7,595
Share of children aged 10-17 years working	−0.001	0.177	0.946	7,595
Share of people over 65 receiving pensions	−0.001	0.883	0.938	7,595
Share receiving transfers from social programs	−0.008	0.319	0.678	7,595
Value of transfers Bolsa familiar (in millions)	0.002	3.212	0.995	7,595

Notes: Panel A displays regression discontinuity coefficients on the estimated difference between the elected mayors in municipalities slightly above and slightly below the population threshold. The column "Mean" displays the mean of elected mayors that fell slightly below the population threshold, while the column "Difference" displays the coefficient on being above the population threshold on the pretreatment covariate. Panel B displays the regression discontinuity coefficients on the estimated difference in municipality-level characteristics of being slightly above the threshold. The balance test is performed on the main estimation sample. The running variable is the proximity in inhabitants to the closest population threshold. There are no control variables included, and the standard errors are clustered at municipal-level. The coefficients are estimated using the CCT bias-corrected estimator, using a local linear regression for the estimates and local quadratic regression for the bias-correction.

6 Results

In this section, I first present the ordinary least squares (OLS) regression results showing the correlation between the size of FPM transfers and the asset growth of the mayors. I then go on to present the main regression discontinuity result comparing the asset growth of mayors in municipalities slightly below the population threshold with those slightly above. I will also go into some detail about how robust the results are.

6.1 Ordinary Least Squares Results

Ordinary least squares results showing the correlation between FPM transfers and asset growth of the elected mayors are reported in Table 4. Formally, I use a linear model to see how an increase in the FPM-transfers of 1,000,000 Reais affect the expected absolute asset growth measured in 1,000 Reais of the elected mayor while adding year fixed effects, state fixed effect, and controlling for mayor characteristics and municipality characteristics.

Column (1) reports the estimate of an increase of one million Reais in FPM transfers on the absolute asset growth without controlling for any fixed effects, while I include state and year fixed effects in Column (2). In column (3) I control for the mayor characteristics and municipality characteristics. Mayor characteristics include skin color, gender, marital status, and age. Municipality characteristics include GDP per capita, mean per capita income, the share of the population receiving bolsa familiar, and the share of GDP related to industry, service, and agriculture. Column (4) includes both the fixed effects and the control variables. It becomes clear that the coefficient on FPM transfers is sensitive to the inclusion of fixed effects and control variables. E.g. the coefficient in Column (3) implies that an increase in the FPM transfers of 1,000,000 Reais is associated with a decrease in the asset growth of the mayor by 2,504 Reais. However, adding the fixed effects in Column (4), the coefficient changes sign, implying that the estimates suffer from omitted variable bias. I am not able to produce any significant results on the coefficient on FPM transfers for any of the OLS specifications. In fact, the coefficient in Column (4) implies that an increase in FPM transfers of 1 million is associated with an increase in absolute asset growth of 409 Reais. This would mean that 0.000409 % of the increased transfers end up in the pocket of the mayor. All of the specifications are telling a similar story. It is tempting to conclude that there is zero correlation between the size of FPM transfers and the asset growth of the mayor measured in absolute growth.

However, the inclusion of different control variables highlights the fact that the regression specification might suffer from omitted variables. It is not unlikely that there are even more omitted variables that might affect the asset growth of a mayor. Thus to know if higher FPM transfers to a municipality make it easier for a mayor to generate a higher asset growth, I need a source of random variation in the FPM transfers. Such a variation is studied in the next subsection.

Table 4: OLS Regression Results

	Dependent variable:			
	Absolute Asset Growth in 1,000 Reais			
	(1)	(2)	(3)	(4)
FPM Transfers (in 1,000,000 Reais)	0.433	3.936	−2.504	0.409
	(7.277)	(7.906)	(7.488)	(8.263)
Mean Dependent Variable	90.8	90.8	90.8	90.8
Control Politician	*No*	*No*	*Yes*	*Yes*
Control Municipality	*No*	*No*	*Yes*	*Yes*
Year FE	*No*	*Yes*	*No*	*Yes*
State FE	*No*	*Yes*	*No*	*Yes*
Observations	7,595	7,595	7,595	7,595
R^2	0.00000	0.036	0.019	0.054
Adjusted R^2	-0.0001	0.032	0.016	0.047

Note: *p<0.1; **p<0.05; ***p<0.01

6.2 Regression Discontinuity Results

The main regression discontinuity results are reported in Column (1) of Table 5. At first glance, the result is eye-catching. Mayors elected in municipalities slightly above the threshold increase the value of their assets by $73,099$ Reais more than mayors slightly below the threshold. The effect is significant at a 10 percent level with a p-value of 0.094. This is an amount equal to 37,551 USD at the average 2012 exchange rate. For the municipalities in the final sample, exceeding the closest population threshold and thereby becoming a treated unit means, on average, an increase in FPM transfers equal to $1,559,422$ Reais. The main result, therefore, implies that 4.7% of the increased FPM transfers ends up in the mayor's pocket. A clear jump at the threshold is visible in the discontinuity plot in Figure 3.

Similarly, there seems to be a clear effect of being above a population threshold on the asset growth rate of the elected mayors. The estimate of 0.676 suggests that mayors slightly above the population threshold have an asset growth rate of 0.676 percentage points higher than their counterparts slightly below the threshold. The estimate is significant at a 10 percent level with a p-value of 0.054. This estimate might, however, seem a bit high. One reason for this estimate to be that high is that I have removed the richest outliers in terms of pre-election assets. The richest candidates are more likely to have a higher asset growth in absolute terms, and lower asset growth in terms of the growth rate. As a consequence, the removal of the richest outliers might drive the estimate on asset growth rate up.

In the balance test performed in Table 3 I tested for imbalance in 53 pre-treatment covariates and found significant discontinuities at a 10 percent level for only two of the covariates. Although this is very much consistent with chance, I perform the main RD regression with these two variables as controls. Table 6 reports the estimated effect of treatment while controlling for if the mayor is of white or brown skin color. The estimate on absolute asset growth becomes a bit larger and slightly more significant with the inclusion of the control variables. More importantly, the estimates in Table 6 shows that both of the main regression results are robust to the inclusion of these two control variables.

Table 5: Main Regression Discontinuity Results

	Absolute Asset Growth (1)	Asset Growth Rate (2)
Above Threshold	$73,099.200^{*}$	0.676^{*}
(SE)	43,684.620	0.351
P-value	0.094	0.054
Observations	7,595.000	7,595.000
Bandwidth	401.000	262.000
Mean (T=0)	24,237.380	0.322

Notes: This table displays the main regression discontinuity estimates. Column (1) reports the estimated effect of being slightly above the population threshold on the absolute asset growth of the mayor measured in Reais. Column (2) reports the the same effect measured in asset growth rate of the mayor. Mean (T=0) reports the mean for the mayors that fell slightly below the population threshold. I use the CCT estimator by fitting a linear regression for the estimate and a local quadratic regression for the bias-correction. The running variable is the proximity to the closest population threshold. There are no control variables included, and the standard errors are clustered at the municipality-level. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

In Figure 4 I plot the coefficient on nominal asset growth for different bandwidth selections and the associated confidence intervals at a 5 percent level. This clarifies that the main regression discontinuity estimate presented in Table 5 is sensitive to the bandwidth selection.

Table 6: RD Results Controlling for Skin Color

	Absolute Asset Growth (1)	Asset Growth Rate (2)
Above Threshold	81,309.220*	0.686*
(SE)	43,597.210	0.351
P-value	0.062	0.051
Observations	7,595.000	7,595.000
Bandwidth	401.000	263.000
Mean (T=0)	11,984.830	0.078
Control Variables	*Yes*	*Yes*

Notes: This table displays the main regression discontinuity estimates while controlling for the skin color of the mayors, which were the only discontinuities found in the balance test in Table 3. Mean (T=0) reports the estimated mean for the outcome variable for the mayors in municipalities slightly below the population threshold. I use the CCT estimator by fitting a linear regression for the estimate and a local quadratic regression for the bias correction. The running variable is the proximity to the closest population threshold. The standard errors are clustered at the municipality level. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

For bandwidth selections closer to the cutoff, the coefficient seems to be positive and close to being statistically significant at a 5 percent level. However, as I increase the bandwidth to include municipalities further away from the threshold the relationship seems to be weaker before it again turns becomes seemingly stronger, albeit with larger standard errors and not significant at a 5 percent level. Hence, although the main regression results presented in this paper seem to tell us the story that the mayors slightly above the population threshold are getting richer than the mayors slightly below the threshold, the significance is less robust to other bandwidth selections. Although not statistically significant for most of the bandwidth selections, the fact that the coefficient stays positive for all bandwidth selections does, however, suggest some robustness.

In the Appendix Table 11 and 12, I provide further robustness to the main regression discontinuity results by controlling for several variables and running the same regression with a local fit of polynomial 1st, 2nd, 3rd, and 4th degree. In Table 13 I also provide the RD results for the unrestricted sample, which highlights the fact that the inclusion of the richest candidates increases the estimates measured in Reais. In Table 14 I present the estimated effect of being above a population threshold while removing different outliers to

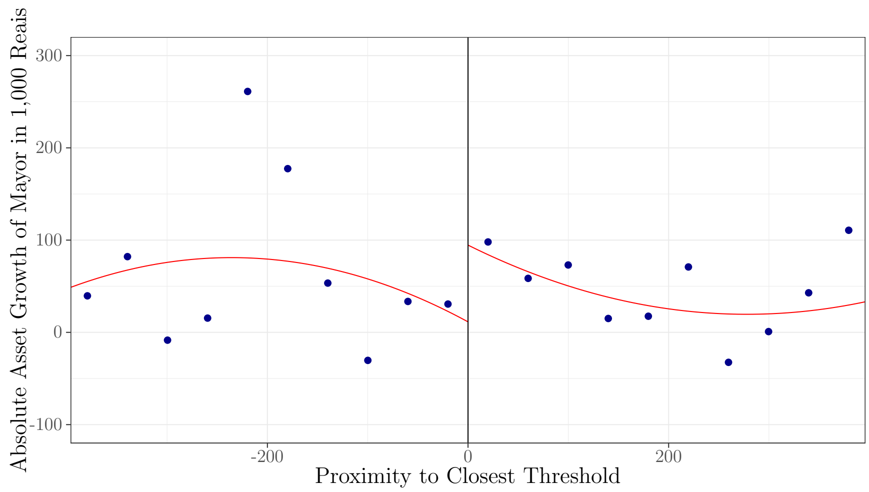


Figure 3: The main regression discontinuity plot fitting regression lines of the second polynomial on each side of the cutoff. The plot is constructed by first creating evenly spaced bins on each side of the cutoff while making sure that no bin consists of observation on both sides of the threshold. I then fit a local polynomial regression on each side of the cutoff. Furthermore, I specify the regression function to be polynomial of the second degree to closely mimick the main RD results for which I used local-polynomial of the second degree to construct the bias-correction (Calonico et al., 2015; Calonico et al., 2017).

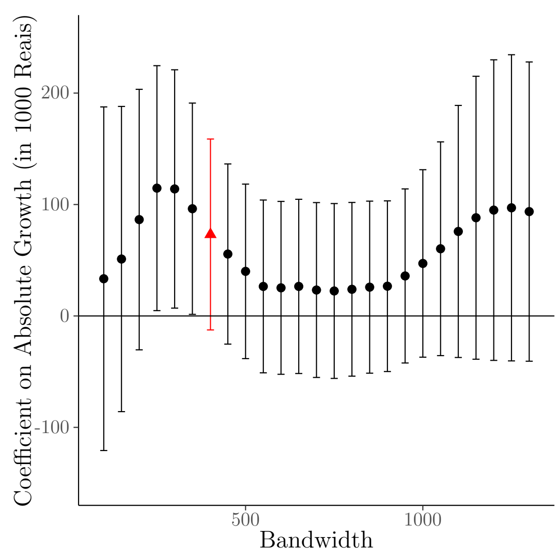


Figure 4: Coefficient on absolute asset growth in 1000's Reais for different bandwidth selections with the associated 95 percent confidence intervals. The red triangle represents the optimal CCT bandwidth selection which is used throughout this paper. The figure displays the coefficient for different bandwidths for the elected mayors. I keep the ratio between the main bandwidth and the bias bandwidth constant and equal to the ratio between main and bias bandwidth for the optimal bandwidth selection.

show that the estimates are robust to less conservative ways of removing the outliers.

7 Mechanisms

Although the point estimate suggests that the size of FPM transfers affects the asset accumulation of mayors, it is not clear exactly why. In this section, I explore the channels that might generate a higher asset growth for mayors receiving more in federal transfers. I will explore four possible determinants of the difference in the private returns to holding public office: increased internal demand in the municipality, wages, informational advantage possessed by the mayor, and corruption. Both wages and increased internal demand offer two legitimate sources through which the mayors in municipalities receiving more in FPM transfers might generate a higher asset growth. I will discuss both of these mechanisms before I go on to discuss the possibility of the point estimate indicating either use of government information for private benefit or corruption. Corruption is difficult to detect for several reasons, and it is, therefore, difficult to conclude that the point estimate is driven by illicit activities. However, I will try to tie the results in this paper to specific theories of corruption that could explain the difference in asset growth.

7.1 Increased Internal Demand and Market Mechanisms

First, an explanation to why higher FPM transfers might lead to higher asset growth for mayors is that increased municipal revenue might be associated with higher internal demand in the municipality. The general idea is that the increase in FPM transfers leads to an increase in municipal spending associated with several long-run effects beneficial to the municipality. One of these effects is that the increase in the municipal revenue generates higher economic activity, which ultimately will accrue to the mayor through market mechanisms. As an example consider a mayor who also owns a bakery. The municipality increases its revenue by 22 % as they are exceeding a population threshold, and the municipal government decides to refurbish the local library. Coincidentally, the workers that are refurbishing the library also buy their lunch at the bakery owned by the mayor. Consequently, the mayor will generate a higher income which evidently will show up in his/her declared assets before the next election. This is a very direct effect of the increase in the FPM transfers, and some of these effects might be even more indirect through multiplier effects because of the increased municipal spending.

To check if an increase in the FPM transfers leads to an increase in internal economic activity in the municipalities, I compare the treatment status with GDP per capita in the municipality in the year of the FPM payment and the year following the treatment. If in fact, the FPM transfers lead to higher economic activity, a discontinuity in GDP per capita at the threshold should be observed *at least* in the year of the payment. Table 7 reports the effect of being above a population threshold on GDP per capita in the year of the payment and the year following. I only have data on GDP in the municipalities until 2018, so I am only able to estimate the effect of being above the population threshold on the GDP per capita in the subsequent year for the years 2014-2017. There is, however, a clear indication that the treatment status leads to a sharp increase in GDP per capita in the municipality. Being above a population threshold is associated with an increase of 5,097 Reais in GDP per capita in the municipality in that year. The estimate is significant at a 5 percent level with a p-value of 0.023. Similarly, the effect of treatment status in one year on GDP per capita in that municipality in the subsequent year is estimated to be 4,591 Reais. This estimate is less significant with a p-value of 0.051. However, I cannot exclude that both of these results are generated from the treatment status in the previous year. The results in Table 7 should therefore be thought of as an indication that higher FPM transfers is associated with an increase in the internal economic activity, rather than the causal effect of treatment on GDP per capita.

However, this theory cannot be limited to the economic activity related to the mayor. If in fact, the increase in FPM transfers creates an effect that makes it easier for the mayors to generate a higher asset growth, this effect should also be present for the inhabitants in that municipality. As a check of the validity of the theory, I estimate the effect of being above a population threshold on the asset growth of the unelected mayoral candidates. These results are presented in Table 8. Note that I have used the same specifications as for the main RD

Table 7: RD Results on GDP per Capita in Subsequent Years

	Year t	Year t+1
Above Threshold	$5,097.681^{**}$	$4,590.910^{*}$
(SE)	2,244.522	2,355.473
P-Value	0.023	0.051
Observations	5,242.000	4,057.000
Bandwidth	565.783	655.635
Mean	17,206.500	18,109.090

Notes: This table displays the RD regression results of being slightly above a population threshold on GDP per capita in that municipality in the years following the payment of the FPM fund. Year t corresponds to GDP per capita in the year of the payment, while Year t+1 corresponds to GDP per capita in the municipality the year following the payment. Mean reports the estimated mean of GDP per capita at the cutoff in the year before. I use the CCT estimator by fitting a linear regression for the estimate and a local quadratic regression for the bias-correction. The running variable is the proximity in population to the closest threshold. Standard errors are clustered at the municipality level. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

results presented for the elected mayors in Section 6. In terms of absolute asset growth, the estimated effect of being above a population threshold for the placebo group of unelected mayors is 19,480 Reais. Similarly, the estimated effect of being above a population threshold on the asset growth rate for the placebo group is 0.003 percentage points. None of the coefficients are statistically significant at a 10 percent level. The estimated treatment effect for the placebo group is relatively small compared to the treatment effect for the elected mayors. The difference in the coefficients for the group of elected mayors and the placebo group could partly be explained by a theory of ability. The elected mayors are more able to take advantage of the increase in economic activity because of their ability. Their high ability is the reason why they were elected in the first place, and their high ability is the reason why they can generate a higher asset growth than the unelected mayoral candidates. However, regardless of the ability of the political candidate, the increase in economic activity should generate *some* increase in the asset growth for the placebo group, albeit smaller than for their possibly more "able" and elected counterparts. A counterargument is that the placebo group of unelected mayoral candidates might have more time to work on their businesses, which should make them more capable of generating a higher asset growth. In any case, I do not find any evidence that the treatment status in a municipality leads to an increase in asset growth for the unelected mayoral candidates in that municipality. This might represent a result contrary to a theory of increased economic activity generating the main results from this paper.

In the Appendix Section A.3 I explore the robustness of the results for the placebo group of unelected mayoral candidates by showing how the coefficient change for different bandwidth selections and by showing how sensitive the estimates are to the removal of the richest outliers.

Table 8: Main RD Results for Placebo Group

	Absolute Asset Growth (1)	Asset Growth Rate (2)
Above Threshold	19,479.560	0.003
(SE)	52,975.450	0.175
P-value	0.713	0.986
Observations	10,625.000	10,625.000
Bandwidth	833.000	791.000
Mean (T=0)	64,478.290	0.516

Notes: This table displays the main regression discontinuity estimates for the placebo group of unelected mayoral candidates. Column (1) reports the estimated effect of being slightly above the population threshold on the absolute asset growth for the placebo group measured in Reais. Column (2) reports the same effect on the asset growth rate. Mean (T=0) reports the mean for the mayoral candidates in municipalities that fell slightly below the population threshold. I use the CCT estimator by fitting a linear regression for the estimate and a local quadratic regression for the bias-correction. The running variable is the proximity to the closest population threshold. There are no control variables included, and the standard errors are clustered at the municipality-level. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Although the results in Table 7 indicate that the increase in FPM transfers is associated with increased economic activity, this does not prove that the source of the variation in asset growth between mayors slightly below and slightly above the threshold is legitimate. The results do, however, provide evidence of increased internal demand, suggestive of a market mechanism through which the treated mayors can increase their asset growth at a higher rate than their untreated counterparts. If in fact treatment in one year leads to higher economic activity, this sharp increase in GDP should accrue the population as a whole and not just the elected politicians. I check for this mechanism by estimating the treatment effect on the placebo group of unelected political candidates, and I find no evidence of a treatment effect on the asset growth of the placebo group.

7.2 Wage Heterogeneity

Second, there is some degree of heterogeneity in the wages of Brazilian mayors. All Brazilian mayors face a uniform wage cap with no mayor allowed to earn more than the minister of the Supreme Federal Court. The wage cap is equal to $39,293$ Reais per month. Other than that, any changes made in the salary of the mayor and vice-mayor have to be approved in the *câmara municipal* (municipal chamber) by the councilors.

Unfortunately, there is no centralized data source on the wages of the mayors. Consequently, I am not able to run a balance test on the mayoral wage. If exceeding a population threshold instantly was associated with a wage increase for the mayor, the estimates presented in this paper could be a direct effect of the wage increase on asset accumulation *rather* than the effect of the increased FPM transfers on asset accumulation.

However, there are arguments as to why it is less likely that a sudden increase in FPM transfers is associated with an immediate wage increase. First, any change in the mayoral wage needs to be sanctioned by the councilors in the municipal chamber. Hence, unless the mayor has a majority in the municipal chamber, it might be difficult to get the councilors to sanction the wage adjustment unless of course there also is a wage adjustment for the councilors. Second, there is a degree of uncertainty about financing in the municipalities in the coming years. Treatment status in one year does not necessarily imply treatment status in the next year. Even if treatment was associated with an immediate increase in the mayoral wage, this wage is likely to be present even if the municipality loses its treatment status in the subsequent year. In the final sample, 172 municipalities change treatment status within the time period I am studying. In total, there are 202 cases of change in the treatment status. 136 of these cases are regarding municipalities that are being downgraded and lose their treatment status, while there are 66 cases of municipalities being upgraded and thereby gaining treatment status. Hence, this makes it more difficult to argue that the wage slightly below the cutoff is very different from the one slightly above the cutoff given the argument that wage reductions are less common, especially considering that there are so many cases of downgrading of municipalities. To conclude, I cannot exclude the possibility of an imbalance in the mayoral wages at the cutoff, but the uncertainty about the financing that the municipalities are facing is making it less probable that a sudden increase in the FPM transfers is associated with an immediate increase in the wage of the mayor. I also perceive it as less likely that a downgrade in treatment status is associated with an immediate decrease in the mayoral wage, which consequently makes an imbalance in the mayoral wage at the cutoff less likely.

It is also worth mentioning that most of the Brazilian municipalities are in a critical fiscal situation. As of 2018, 74 % of the Brazilian municipalities were facing a critical fiscal situation, a story that above all is representative for small to medium-sized municipalities (Firjan, 2019). 1/3 of the Brazilian municipalities were unable to obtain enough revenue to pay for the salaries of mayors, councilors, and secretaries in 2018. According to Firjan (2019), almost half of the 5,337 municipalities they analyzed spent more than 54 % of their net revenue on public administration, which means that they exceeded the alert spending limit defined by the Law of Fiscal Responsibility (Lei de Responsabilidade Fiscal). In the worst case, the federal government stops all transfers if the municipality fails to comply with rules for municipal spending on administration. In other cases, the State Accounting Court (Tribunal de Contas do Estado (TCE)) takes over the control of the municipal finances, in which case it will be even more difficult to change the salaries.[4] In general, there therefore seem to be little scope for wage adjustments in many municipalities.

7.3 Informational Advantages and Corruption

A third channel that might generate the variation in asset accumulation between mayors, is the informational advantage. Because the mayors are prominent participants in government decision-making in the municipalities, they might have information about forthcoming gov-

[4] Recent examples of the TCE taking over the municipal finances are Pescaria Brava (SC) and Minas (MG). For more information, see: "Brasil tem quase 2 mil cidades inviáveis, sem receita nem para o salário do prefeito", *Gazeta do Povo*, August 26 2018

ernment action before the information becomes public (Ziobrowski et al., 2004). The idea is that this could provide the mayors with an informational advantage over other investors. As an example, the mayor might have detailed knowledge about a forthcoming major construction project that will drive up real estate prices in that area. If the mayor is confident that the project will be passed, he/she might decide to invest heavily in properties in close vicinity to the area where the construction project is happening in certain knowledge that the real estate prices in this area will rise sharply after the conclusion of the project. The informational advantage might also be very present in the stock market. E.g. the mayors might invest in a company they are certain will increase its value in the future due to government action. However, this raises the question: How is the informational advantage changing with the size of FPM transfers? It is likely that mayors in all municipalities, regardless of the size of the FPM transfers, possess the same informational advantage. The increase in FPM transfers might make the information more valuable. Consider the example of a construction project driving up prices for real estate in the vicinity of the construction site. Larger FPM transfers might be associated with larger investments, which might drive up the real estate prices even more. As a consequence, the informational advantage for mayors slightly above the population threshold might be relatively more valuable.

If none of the theories above serves to explain all of the increase in asset growth for mayors slightly above the population threshold, this leaves us with a fourth possible channel: Corruption. An elected mayor could use his/her political power and position to trade favors and thereby increase his/her asset growth. If the mayor rules over a larger budget, there is possibly a higher private return to diverting the public funds for the mayor. However, since corruption usually involves illicit transfers, assets derived from illegal sources are unlikely to show up in the declared assets of the political candidate. These assets will have to be laundered before they show up in the list of declared items by the mayor.

I have no evidence of corruption being the driving force behind the point estimate. However, Brollo et al. (2013) show that an increase in FPM transfers leads to an increase in observed corruption. It is therefore likely that at least part of the increase in asset growth for mayors slightly above the population threshold can be explained by a theory of corruption. However, what kind of corruption could explain the increase in asset growth for mayors slightly above the population threshold?

According to Ferraz and Finan (2011) most corruption schemes in local governments in Brazil are associated with three types of violations: (i) fraud in the acquisition of public goods and services; (ii) diversion of public funds for private benefit; and (iii) over-invoicing of goods and services. These three schemes will typically involve illegal tendering processes, with contracts rewarded to "friendly companies." These companies are "friendly" in the sense that they are directly connected to the mayor and/or his/her family. This might include the use of non-existing firms in the bidding process, use of fake receipts, and/or over-invoicing to increase the price of the public good or service, thereby generating a higher income for the company. In other cases, federal transfers will simply "disappear" from the municipal bank accounts.

A higher municipal budget makes it easier for the mayors to conclude suspect agreements. E.g. under the refurbishing of the new local library, the increase in FPM transfers might lead to an even more ambitious plan including a new computer room for the citizens. In some cases, the mayor might pay the construction firm a price higher than the market price for their service, and the quality of the computer room will be lower than what the price paid indicates. In other cases, the construction firm will subcontract another firm, often owned by the mayor or the family of the mayor, to perform parts of the construction project. The construction done by the subcontractor tends to be *at best* of low quality, at worst no construction is done at all. All of these cases illustrate how an increase in FPM transfers could lead to a higher number of corrupt violations.

For the corrupt violations to affect the observed asset growth of the elected mayors, the funds need to be laundered first. In the cases where the mayor has an ownership interest in the company or the subcontracting company working on the library, this will either happen because the company increases its income and its value so that the owners can increase their dividends. In the cases where the mayor hires a "friendly" firm in which he/she has no ownership interest, the owner of the "friendly" firm will have to repay the mayor in other ways. Common ways of doing so are for example by selling the mayor a property well below market value or by refurbishing the existing property owned by the mayor.

To assess if the effect of FPM transfers on asset accumulation of mayors can be tied to a theory of informational advantage or corruption, I decompose the assets into five broadly defined categories to see where the variation in asset accumulation is stemming from. Table 9 displays the estimated effect of being above a population threshold on the asset growth within each category. Note that I have specified the bandwidth to be equal to the optimal bandwidth selection in the main RD results. The Column "Mean" displays the mean value of the declared assets at the cutoff before getting elected.

If the informational advantage possessed by the mayors is the driving force of the point estimate, we should expect mayors slightly above the population threshold to generate a higher asset growth from assets declared as real estate, shareholdings, funds, investments, and similar. However, as seen in Table 9, the coefficients within these categories are relatively small and seem to be less of an important source to the point estimate. E.g. the coefficient on "Properties" suggests that mayors slightly above the population thresholds increase the value of their properties by 37,764 Reais more than their counterparts slightly below. The coefficient is, however, not statistically significant at a 10 percent level with a p-value of 0.247. Furthermore, this estimate seems rather small compared to the Column "Mean", specifying the mean value at the cutoff of declared assets within this category before getting elected. Similarly, I find no evidence of a treatment effect on asset growth within the category "Shareholdings, Investments, Funds." An increase within the category "Deposits, Credits, Savings" could also be consistent with a theory of informational advantage if the mayors sell their assets gained through an informational advantage before declaring their assets as incumbents. However, it is likely to also observe asset growth within the other mentioned categories, as some of these assets might be less liquid.

In contrary to what we would expect if the informational advantage was the main source of the difference in asset growth between mayors slightly above and below the threshold, Table 9 shows that the main driver for the point estimate comes from the category "Deposits, Credits, Savings." Although this is not necessarily inconsistent with a theory of informational advantage, it is somewhat surprising that the effect on the categories "Real Estate" and "Shareholdings, Investments, Funds" is so ambiguous.

Table 9: RD Results on Assets per Category (in 1,000 Reais)

Category	Coefficient	(SE)	P-value	Mean	Observations
Properties	37.764	32.637	0.247	206.491	7,017
Vehicles	15.143	18.459	0.412	73.575	6,682
Shareholdings, Investments, Funds	27.823	34.265	0.417	75.442	4,875
Deposits, Credits, Savings	51.511**	21.832	0.018	52.521	4,983
Other Goods	−101.487	77.656	0.191	16.276	1,945

Notes: This table displays the RD results for of being slightly above a population threshold on the asset growth of the elected mayor. The column "Mean" reports the estimated mean at the cutoff of the declared value of the assets for the mayors within that category before getting elected. I split the asset growth of the mayors into five different categories. Properties are defined as all immobile properties, such as real estate. The category vehicles are all property used for transport such as cars, boats, and similar. Shareholdings, investments, and funds are all funds, shares and other financial assets owned by the candidate. Deposits, credits and savings include the deposits in the bank, cash and all credits and savings in the candidates name. This includes any business loans made in the candidates name. Other goods are all assets that cannot be put into the above four categories, and include assets such as patent rights and licenses. I use the CCT estimator by fitting a linear regression for the estimate and a local quadratic regression for the bias-correction. However, I have specified the bandwidth to be equal to the optimal bandwidth in the main RD result. The running variable is the proximity to the closest population threshold. There are no control variables included, and the standard errors are clustered at the municipality-level. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Similar arguments are also valid when tying the estimates to a theory of corruption. According to the theory of corruption explained above, we should mainly expect the effects to be strongest for assets declared as "Properties", "Shareholdings, Investments, Funds" and "Deposits, Credits, Savings". It seems that the main source of the point estimate is coming from assets declared within the category "Deposits, Credits, Savings." Mayors slightly above the population threshold increase the value of their assets declared within this category by 51,511 Reais more than their counterparts slightly below the population threshold. The estimate is statistically significant at a 5 percent level with a p-value of 0.018. The estimate is even more eye-catching when we take into consideration that the mean value of the declared assets within this category pre-election is equal to 52,521 Reais. The coefficient on assets declared as "Other Goods" is very interesting as it is negative and very large in absolute value compared to the mean value of assets declared in this category before getting elected. However, it is difficult to conclude anything specific because of the few observations within this category.

In this section, I have tried to tie the point estimate to theories of informational advantage and corruption. I decompose the assets into five broadly defined categories to find the source of the variation in asset growth between treated and untreated units. In contrary to a theory of both informational advantage and corruption, I find no evidence of a treatment effect on assets declared as ”Properties” or ”Shareholdings, Investments, Funds.” It seems that the main source of the variation in asset accumulation is stemming from assets declared within the category ”Deposits, Credits, Savings.” This could be consistent with theories of informational advantage or corruption, but could also be explained by other, legitimate sources.

8 Conclusion

The relationship between government resources and private returns to holding public office is still widely unexplored. In this paper, I try to fill this gap by estimating the causal effect of intergovernmental transfers on the asset growth of elected mayors in Brazil. My main finding suggests that an increase in federal transfers leads to a higher asset growth for the elected mayors. This result highlights that an increase in government resources might adversely affect the incentives and performance of elected politicians.

A few words of caveats are in order before interpreting the findings. First, I am only able to observe the asset growth of mayors in their first term. An adverse effect of term-limits in Brazil is that mayors in their final term are more corrupt than mayors that still can be re-elected (Ferraz and Finan, 2011). Second, the results in this paper only account for publicly disclosed assets. Brollo et al. (2013) find that an increase in federal transfers leads to an increase in observed corruption. Hence, a somewhat uncontroversial assumption of a positive correlation between corruption and asset accumulation would make the case that the estimates in this paper represent a lower bound.

I see several ways in which to take this line of research further. I have been unable to identify the mechanisms driving the estimate. Specifically to this context, it would be interesting to see if the effect is stronger in municipalities where corruption is detected at a later point. This can be done by employing the data from the random corruption audits from the Comptroller General of Brazil (Controladoria-Geral da União(CGU)). By including the mayoral wages in the analysis, we might exclude "wage heterogeneity" as a mechanism.

More generally, the estimates in this paper are only robust to some specifications. Although difficult, it would be interesting to replicate this kind of analysis in other developing countries to see if the results are more robust or even generalizable to different settings. A more in-depth analysis should also aim at identifying the mechanisms.

9 References

Asher, S., & Novosad, P. (2018). Rent-Seeking and Criminal Politicians: Evidence From Mining Booms. *Mimeo.*

Brollo, F., Nannicini, T., Perotti, R., & Tabellini, G. (2013). The Political Resource Curse. *American Economic Review*, *103*(5), 1759–96.

Calonico, S., Cattaneo, M. D., Farrell, M. H., & Titiunik, R. (2017). rdrobust: Software for Regression-Discontinuity Designs. *The Stata Journal*, *17*(2), 372–404.

Calonico, S., Cattaneo, M. D., Farrell, M. H., & Titiunik, R. (2019). Regression Discontinuity Designs Using Covariates. *Review of Economics and Statistics*, *101*(3), 442–451.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2015). Optimal Data-Driven Regression Discontinuity Plots. *Journal of the American Statistical Association*, *110*(512), 1753–1769.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014). Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica*, *82*(6), 2295–2326.

Cattaneo, M. D., Titiunik, R., Vazquez-Bare, G., et al. (2019). The Regression Discontinuity Design. *Handbook of Research Methods in Political Science and International Relations, eds. L. Curini and RJ Franzese, Sage Publications.*

Cunha, G. (2019). Financial Gains From Legislative Office: Evidence From Brazilian Municipal Councilors. *Economic Research Initiatives at Duke (ERID) Working Paper Forthcoming.*

De la Cuesta, B., & Imai, K. (2016). Misunderstandings About the Regression Discontinuity Design in the Study of Close Elections. *Annual Review of Political Science*, *19*, 375–396.

Ferraz, C., & Finan, F. (2011). Electoral Accountability and Corruption: Evidence From the Audits of Local Governments. *American Economic Review*, *101*(4), 1274–1311.

Ferraz, C., & Finan, F. (2009). *Motivating Politicians: The Impacts of Monetary Incentives on Quality and Performance* (tech. rep.). National Bureau of Economic Research.

Firjan - Federação das Indústrias do Estado do Rio de Janeiro. (2017). *IFGF 2017 Índice Firjan de Gestão Fiscal.* https://www.firjan.com.br/data/files/4A/B0/A3/B1/

C4CCD51063C6AAD5A8A809C2/IFGF%202017%20-%20An_lise%20Especial%20-%20Vers_o%20Completa.pdf

Firjan - Federação das Indústrias do Estado do Rio de Janeiro. (2019). *IFGF 2019 Índice Firjan de Gestão Fiscal*. https://www.firjan.com.br/data/files/8F/50/19/81/B2E1E610B71B21E6A8A809C2/IFGF-2019_estudo-completo.pdf

Fisman, R., Harmon, N. A., Kamenica, E., & Munk, I. (2015). Labor Supply of Politicians. *Journal of the European Economic Association*, *13*(5), 871–905.

Fisman, R., Schulz, F., & Vig, V. (2014). The Private Returns to Public Office. *Journal of Political Economy*, *122*(4), 806–862.

Gagliarducci, S., & Nannicini, T. (2013). Do Better Paid Politicians Perform Better? Disentangling Incentives From Selection. *Journal of the European Economic Association*, *11*(2), 369–398.

Hahn, J., Todd, P., & Van der Klaauw, W. (2001). Identification and Estimation of Treatment Effects With a Regression-Discontinuity Design. *Econometrica*, *69*(1), 201–209.

Holland, P. W. (1986). Statistics and Causal Inference. *Journal of the American statistical Association*, *81*(396), 945–960.

Imbens, G., & Kalyanaraman, K. (2012). Optimal Bandwidth Choice for the Regression Discontinuity Estimator. *The Review of Economic Studies*, *79*(3), 933–959.

Kotakorpi, K., & Poutvaara, P. (2011). Pay for Politicians and Candidate Selection: An Empirical Analysis. *Journal of Public Economics*, *95*(7-8), 877–885.

Lee, D. S., & Lemieux, T. (2010). Regression Discontinuity Designs in Economics. *Journal of Economic Literature*, *48*(2), 281–355.

Litschig, S. (2012). Are Rules-Based Government Programs Shielded From Special-Interest Politics? Evidence From Revenue-Sharing Transfers in Brazil. *Journal of Public Economics*, *96*(11-12), 1047–1060.

Litschig, S., & Morrison, K. M. (2013). The Impact of Intergovernmental Transfers on Education Outcomes and Poverty Reduction. *American Economic Journal: Applied Economics*, *5*(4), 206–40.

McCrary, J. (2008). Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test. *Journal of Econometrics*, *142*(2), 698–714.

Mehlum, H., Moene, K., & Torvik, R. (2006). Institutions and the Resource Curse. *The Economic Journal*, *116*(508), 1–20.

Nickson, R. A. (1995). *Local Government in Latin America*. L. Rienner Publishers Boulder.

Pei, Z., Lee, D. S., Card, D., & Weber, A. (2021). Local Polynomial Order in Regression Discontinuity Designs. *Journal of Business & Economic Statistics*, (just-accepted), 1–18.

Robinson, J. A., Torvik, R., & Verdier, T. (2006). Political Foundations of the Resource Curse. *Journal of development Economics*, *79*(2), 447–468.

Sachs, J. D., & Warner, A. M. (1995). *Natural Resource Abundance and Economic Growth* (tech. rep.). National Bureau of Economic Research.

Ziobrowski, A. J., Cheng, P., Boyd, J. W., & Ziobrowski, B. J. (2004). Abnormal Returns From the Common Stock Investments of the US Senate. *Journal of financial and quantitative analysis*, 661–676.

A Appendix

A.1 Manipulation of Population Estimates

In Table 10 I report the p-values from the McCrary sorting test on the population estimates performed separately at all the thresholds in the years 2007-2019 for the final sample used in this paper. Formally, I test the null hypothesis of continuity in the density of the running variable at the cutoff against the alternative hypothesis of sorting around the threshold. A p-value below 0.05 in Table 10 will therefore imply that I can reject the null hypothesis of continuity in the density of the running variable at a 5 percent significance level. Along several of the thresholds for the years 2007, 2010, 2011, and 2012, there seem to be small corresponding p-values, meaning that there is enough evidence to reject the null hypothesis. I have also performed the McCrary sorting test on a larger sample of Brazilian municipalities for the same years, and I find compelling evidence for manipulation of the running variable in the years 2007, 2010, 2011, and 2012. I find no evidence of a discontinuity in the running variable for the years 2013-2019 in the large sample, a result similar to what we observe in Table 10.

Table 10: McCrary Sorting Test P-Values

	1st threshold	2nd threshold	3rd threshold	4th threshold	5th threshold	6th threshold
2007	0.000***	0.000***	0.002**	0.013**	0.016**	0.541
2008	0.274	0.404	0.816	0.311	0.466	0.486
2009	0.063*	0.549	0.892	0.319	0.886	0.447
2010	0.000***	0.038**	0.054*	0.592	0.029**	–
2011	0.000***	0.038**	0.054*	0.592	0.029**	–
2012	0.039**	0.288	0.265	0.172	0.178	0.085*
2013	0.891	0.604	0.256	0.723	0.404	0.250
2014	0.164	0.171	0.365	0.568	0.055*	0.082*
2015	0.179	0.928	0.164	0.645	0.104	0.262
2016	0.638	0.945	0.952	0.639	0.140	0.795
2017	0.913	0.530	0.764	0.219	0.234	0.727
2018	0.358	0.841	0.642	0.667	0.090*	0.336
2019	0.769	0.765	0.722	0.365	0.803	0.166

Notes: This table displays the p-values from the McCrary sorting test. I test the null hypothesis of continuity in the running variable at the cutoff against the alternative hypothesis of sorting around the cutoff. A p-value below 0.05 means that I can reject the null hypothesis of continuity in the density of the running variable at a 5 percent significance level. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

A.2 Further Robustness Checks for Main RD Results

In Table 11 I explore how robust the main RD result is to the inclusion of different control variables. For further explanation of the control variables included, see below Table 11. The main result is not entirely robust to the inclusion of control variables in terms of significance level. But the estimated effect is very similar regardless.

In Table 12 I estimate the effect of being slightly above a population threshold on the asset growth of the mayors, using the CCT estimator. In Column (1) I use a local linear regression for the estimate and a local quadratic regression for the bias correction. In Column (2) I use a local quadratic regression for the estimate and a local cubic regression for the bias correction. In Column (3) I use a local cubic regression for the estimate and a local quartic regression for the bias correction. Similarly, in Column (4), I use a quartic regression for the estimate. Most important, Table 12 shows that the main RD result is robust to different functional specifications, thereby providing further robustness to the main RD results.

Table 11: RD Results With Control Variables

	(1)	(2)	(3)
Above Threshold	$87,093.870^{**}$	68,527.930	68,202.590
(SE)	42,843.710	43,008.390	42,816.010
P-value	0.042	0.111	0.111
Observations	7,595.000	7,595.000	7,595.000
Bandwidth	406.098	404.586	410.527
Mean (T=0)	175,522.500	−14,378.930	−44,922.550
Control Politician	*Yes*	*No*	*Yes*
Control Municipality	*No*	*Yes*	*Yes*

Notes: This table displays the estimated effect of being above a population threshold while controlling several pre-treatment covariates. Column (1) controls for the politician characteristics, such as education, political party, marital status, gender, age, and skin color. Column (2) controls for municipality characteristics such as GDP per capita, the share of GDP related to industry, service, and agriculture, mean per capita share of GDP related to industry, service, and agriculture, mean per capita income, and share of the population receiving bolsa familiar. Column (3) includes both municipality controls and politicians controls. I use the CCT estimator by fitting a linear regression for the estimate and a local quadratic regression for the bias correction. The running variable is the proximity to the closest population threshold. The standard errors are clustered at the municipality level. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 12: RD Results With Different Polynomials

	(1)	(2)	(3)	(4)
Above Threshold	$73,099.200^{*}$	$102,119.000^{**}$	$130,733.800^{**}$	$138,678.600^{**}$
(SE)	43,684.620	51,128.110	61,624.230	65,482.700
P-Value	0.094	0.046	0.034	0.034
Observations	7,595.000	7,595.000	7,595.000	7,595.000
Bandwidth	400.805	567.000	689.089	909.085
Mean (T=0)	24,237.380	6,703.650	−10,543.930	−17,054.790
Polynomial	1	2	3	4

Notes: This table displays the robustness of the main RD results to different functional specifications. The coefficients are estimated using a local linear regression for the estimate and a local quadratic regression for the bias correction in Column (1). In Column (2) I instead use a local regression of 2nd-degree polynomial (p=2) for the estimate and a local regression of 3rd degree (p+1) for the bias correction. Similarly, I use a local regression of the 3rd polynomial for the estimate and of the 4th polynomial for the bias correction. The row "Polynomial" specifies the polynomial used for the local regression for the estimate, the corresponding polynomial for the bias-correction will be p+1. The running variable is the proximity to the closest population threshold, and the standard errors are clustered at the municipality level. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

In Table 13 I present the regression results for the unrestricted sample with no restriction on the declared value of assets pre-election. Columns (1) and (2) of Table 13 present the treatment effect of being above a population threshold for the elected mayors and the placebo group respectively. The results for both the baseline group and the placebo group lack statistical significance. Column (3) and (4) shows the same regression results with asset growth rate as the outcome variable. The results, especially from Column (1) highlight the fact that the coefficient on asset growth measured in Reais is very sensitive to the removal of the richest outliers. The coefficient on "Above Threshold" indicates that crossing a population threshold is associated with an increase in asset growth of 1,352,965 Reais, an estimate that seems a bit too high. The estimate is, however, not significant. To some extent, the result does, however, justify the removal of the richest outliers.

Before I ran the main regression discontinuity in this paper, I removed outliers in terms of the declared pre-election assets. All candidates with assets exceeding a value of 2,000,000 Reais, an amount roughly equal to 1,000,000 USD at the 2012 exchange rate, were removed. This restriction means that I go from 8,115 to 7,595 observations, roughly removing about 6.4 percent of the observations. There are some arguments in favor of removing these outliers. First, because the main RD result presented in this paper is measured in absolute terms, the richest candidates are likely to influence the estimates a lot. This can be seen in Table 13 or Table 14 where the estimated effect of being above a population threshold in the unrestricted sample is very large. Second, I have already discussed possible errors in the data entry. I have identified cases where mayoral candidates have misreported the value of their assets by accidentally adding an extra 0. There is no way of knowing with certainty which candidates have misreported their assets by accident. However, by removing the richest outliers, I remove the most extreme cases of misreporting that are likely to affect the estimates the most.

Table 14 reports the coefficient on asset growth measured in Reais for different maximum values of reported assets pre-election. As more of the richest outliers are removed, the coefficient on absolute asset growth seems to stabilize and the p-value reduces. In the baseline where I restrict the maximum value of assets pre-election to be 900,000 Reais, corresponding to about 462,330 USD, mayors slightly above the population threshold seem to increase their assets by 93,100 Reais more than mayors slightly below the threshold. The estimate is significant at a 5 percent level with a p-value of 0.025. On average in the restricted sample, a municipality crossing the population threshold implies an increase in FPM transfers of 1,525,560 Reais. This means that, on average, 6.1 % of the increased FPM transfers ends up in the mayor's pocket. This is a similar result to the main RD results presented in Table 5, giving some robustness to the estimates. It seems that by removing the richest outliers, the estimated treatment effect of being above a population threshold is stabilizing around 70,000-100,000 Reais with a low corresponding p-value, indicating a clear treatment effect of higher FPM transfers for mayors below the restricted maximum value of assets.

Table 13: RD Results With no Restrictions

	Baseline (1)	Placebo (2)	Baseline (3)	Placebo (4)
Above Threshold	1,352,965.000	−60,349.000	0.337	−0.008
(SE)	1,174,163.000	93,318.030	0.354	0.170
P-value	0.249	0.518	0.341	0.964
Observations	8,115.000	11,032.000	8,115.000	11,032.000
Bandwidth	754.126	919.067	405.000	789.802
Mean (T=0)	−1,448,816.000	37,231.490	0.362	0.501

Notes: This table displays the estimated effect of being above a population threshold on the asset growth of the mayor for the unrestricted sample. Mean (T=0) reports the mean for the mayors that fell slightly below the population threshold. I use the CCT estimator by fitting a linear regression for the estimate and a local quadratic regression for the bias correction. The running variable is the proximity to the closest population threshold. There are no control variables included, and the standard errors are clustered at the municipality level. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

According to Litschig and Morrison (2013), the increase in FPM transfers at the cutoff in more populous municipalities is too small to affect the overall budget. This could imply that there is no "first stage" in terms of the budget size for the municipality. To show that this is less of a problem in the context studied in this paper, I show that the estimates are less sensitive to the inclusion of municipalities within the 4th, 5th and 6th population bracket. This is shown in Table 15. The column names indicate how many population brackets are included in the sample. Although not entirely robust, the coefficient seems to be pretty stable regardless of the inclusion of more populous municipalities, indicating that

Table 14: RD Results for Different Restrictions

Max Value of Assets	Coefficient	(SE)	P-value	Observations
None	1,352,965.000	1,174,163.000	0.249	8,115
<9,000,000	1,108,697.000	1,038,089.000	0.286	8,055
<3,000,000	1,233,622.000	1,142,441.000	0.280	7,819
<2,000,000	73,099.200*	43,684.620	0.094	7,595
<1,000,000	76,501.140*	41,605.780	0.066	6,847
<900,000	93,100.360**	41,417.550	0.025	6,716
<800,000	103,170.600**	41,666.260	0.013	6,527
<700,000	74,473.910*	38,320.220	0.052	6,249
<600,000	99,664.380**	40,252.360	0.013	5,982
<500,000	100,985.500**	40,407.900	0.012	5,553
<400,000	83,003.280**	39,377.340	0.035	5,037
<300,000	76,549.810*	43,578.080	0.079	4,265
<200,000	59,866.930	48,413.320	0.216	3,128
<100,000	34,031.920	40,114.210	0.396	1,604

Notes: This table displays the regression discontinuity results for different restrictions on the declared assets prior to the election. The column "Max Value of Assets" indicates the restriction set on the declared assets by the mayor prior to getting elected. Mean (T=0) reports the mean for the mayors that fell slightly below the population threshold. I use the CCT estimator by fitting a linear regression for the estimate and a local quadratic regression for the bias correction. The running variable is the proximity to the closest population threshold. There are no control variables included, and the standard errors are clustered at the municipality level. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

the argument made by Litschig and Morrison (2013) might be less relevant in this context.

Table 15: RD Results for Different Population Groups

	1-3 (1)	1-4 (2)	1-5 (3)	1-6 (4)
Above Threshold	85,508.460	65,230.720	79,959.350*	73,099.200*
(SE)	53,385.730	46,692.980	45,185.230	43,684.620
P-Value	0.109	0.162	0.077	0.094
Observations	5,243.000	6,382.000	7,134.000	7,595.000
Bandwidth	357.989	384.341	392.429	400.805
Mean (T=0)	12,472.000	26,381.680	20,055.290	24,237.380

Notes: This table displays the regression discontinuity estimates for the inclusion of different population thresholds. Column (1) runs the regression for municipalities within the first four population brackets, while Column (2) includes municipalities within population group 5, and so on. I use the CCT estimator by fitting a linear regression for the estimate and a local quadratic regression for the bias correction. The running variable is the proximity to the closest population threshold. There are no control variables included, and the standard errors are clustered at the municipality level. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

A.3 Robustness for Placebo Group

In Figure 5 I present the estimated effect of being above a population threshold on the asset growth measured in Reais for the placebo group of unelected mayoral candidates for different bandwidth selections. The red color represents the CCT optimal bandwidth selection. In sharp contrast to the same plot presented in Figure 4 for the elected mayors, the coefficient for the placebo group seems to be negative for bandwidth selections closer to the cutoff, and close to zero for bandwidth selections including municipalities further away from the cutoff.

In Table 16 I present the estimated effect of being above a population threshold on the asset growth for the placebo group of unelected mayoral candidates for different specifications. The column "Max Value of Assets" specifies the restriction set on the value of the declared assets pre-election by the mayoral candidate. In sharp contrast to Table 14 where I presented the same table with the same specifications for the baseline of elected mayors, the coefficient for the placebo group seems to be changing a lot depending on the upper bound of the value of assets. For most of the specifications, the coefficient is negative and with relatively high p-values. Both of the results presented in Figure 5 and Table 16 indicate that

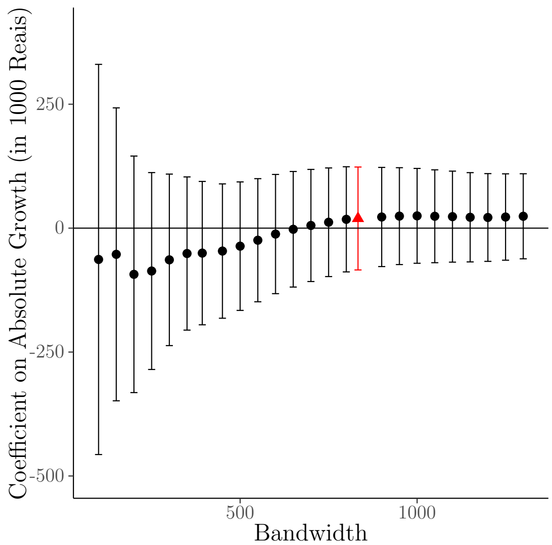


Figure 5: Coefficient on absolute asset growth in 1000's Reais for different bandwidth selections with the associated 95 percent confidence intervals. The figure displays the coefficient for different bandwidths for the placebo group of unelected mayoral candidates. I keep the ratio between the main bandwidth and the bias bandwidth constant and equal to the ratio between main and bias bandwidth for the optimal bandwidth selection. The red triangle represents the optimal CCT bandwidth selection.

there is little evidence of a treatment effect for the placebo group. This is to some extent a reassuring result, considering that none of the mayoral candidates in the placebo group received the treatment. It does, however, also suggest that any possible effect of increased economic activity associated with the increase in FPM transfers might have a smaller effect on the asset growth of the placebo group than on the elected mayors.

Table 16: RD Results for Different Restrictions for the Placebo Group

Max Value of Assets	Coefficient	SE	P-value	Observations
None	−60, 349.000	93, 318.030	0.518	11, 032
<9,000,000	−63, 567.920	79, 355.910	0.423	10, 965
<3,000,000	36.109	55, 849.410	0.999	10, 785
<2,000,000	19, 479.560	52, 975.450	0.713	10, 625
<1,000,000	−33, 280.430	40, 914.010	0.416	9, 901
<900,000	−8, 946.845	28, 893.040	0.757	9, 736
<800,000	−3, 905.264	29, 393.480	0.894	9, 535
<700,000	−8, 247.839	29, 947.410	0.783	9, 210
<600,000	−4, 777.574	29, 990.800	0.873	8, 851
<500,000	−4, 281.352	31, 957.430	0.893	8, 358
<400,000	−12, 182.660	36, 420.830	0.738	7, 590
<300,000	−8, 554.066	35, 745.050	0.811	6, 655
<200,000	−32, 850.670	39, 375.100	0.404	5, 008
<100,000	2, 864.979	17, 755.760	0.872	2, 733

Notes: This table displays the regression discontinuity results for different restrictions on the declared assets prior to the election for the placebo group of unelected candidates. The column "Max Value of Assets" indicates the restriction set on then declared by the mayor prior to getting elected. Mean (T=0) reports the mean for the mayoral candidates in municipalities that fell slightly below the population threshold. I use the CCT estimator by fitting a linear regression for the estimate and a local quadratic regression for the bias correction. The running variable is the proximity to the closest population threshold. There are no control variables included, and the standard errors are clustered at the municipality level. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.