Master Thesis

Are mayors with business experience better politicians? Evidence from close races in Brazil

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Abstract

This paper examines whether policy outcomes differ under politicians with business experience. Using a regression discontinuity analysis for mayoral elections in Brazilian municipalities in the period 2004-2012, I find that business experience per se does not affect public finance, public employment, the number of subsidized sectors or the level of corruption. However, a separate analysis for entrepreneurs and merchants, the two main business-groups in the sample, shows that only relatively "high level" businesspeople with proper management expertise can significantly impact policy outcomes. In light of this, I find that entrepreneurs are likely to attract more transfers from higher government levels and therefore, increase the municipality's total revenue. This in turn allows them to raise total expenditure and hire more direct active administration employees. On the contrary, I do not observe any significant effect when a merchant becomes mayor.

JEL classification: R5, H71, H72, D72.

Keywords: Political selection, Professional background, Local government, Elections, Business experience, Developing Country, Regression discontinuity.

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1 Introduction

During a campaign rally in 2012, former Republican nominee and Massachusetts Governor Mitt Romney attracted attention when he floated the idea that it should be a requirement for future presidents to "spend at least three years working in business before he could become president of the United States." With reference to his own tenure at investment firm Bain Capital, Romney went on to claim that a president with private sector experience would better understand which policies are necessary to stimulate business growth (The Washington Post, 2009). Four years later, Donald Trump, a businessman who has never held a public office before, gets voted to be the 45th President of the United States. Like Romney, he repeatedly praised his business acumen during the electoral campaign. For instance, Trump said during the Second Republican Debate in September 2015: "What I am [...] is a businessman, and that's the kind of mindset this country needs to bring it back" (Time, 2015). In a speech in Charlotte, North Carolina he added: "As you know, I am not a politician. I have worked in business, creating jobs and rebuilding neighborhoods my entire adult life" (The Washington Post, 2016). In this perspective, it would be interesting to know whether these claims are only campaign promises or whether businesspersons are actually more qualified to run a government, foster economic growth and improve public finance.

Concerning this, one should start by asking whether is it reasonable to assume that personal and biographic characteristics might matter for policy outcomes in the first place. Despite an ongoing debate in the literature, a predominant portion of studies comes to the conclusion that personal properties affect a legislator's behaviour and his/her ability to govern. For instance, Brollo and Troiano (2016) show that female mayors in Brazil are less likely to engage in corruption and hire fewer temporary public employees than male mayors. Moreover, in the case of India, having a woman in the village council, influences both adolescent girls' career aspirations and educational attainment (Chattopadhyay and Duflo, 2004) as well as the types of public goods provided (Beaman et al., 2012). With respect to differences in education, Besley et al. (2011) show that high educated political leaders generate higher economic growth. Additionally, Alesina et al. (2015) state that also the politician's age matters. In their study for Italian local governments they show that due to stronger career concerns, younger politicians increase public spending and obtain more transfers from higher levels of government in pre-election years.

In view of this rich literature on gender, education and age, it is notable that the effect of a candidate's professional background has received relatively little attention so far. This is even more surprising when considering that there are many examples for politicians like Romney and Trump who explicitly promote their professional background to signal voters their competence for office.[1]

With my paper, I want to contribute to investigate whether policy outcomes actually differ under legislators with business experience. For this purpose, I examine data on 4,813 mayoral elections in Brazilian municipalities during 2004, 2008 and 2012. In order to rule out any endogeneity

[1] Certainly, Trump and Romney were not the first candidates to make a run for a high political office with experience in the business sector. For example, in Italy media tycoon Silvio Berlusconi was Prime Minister for nine years and in Britain, a former boss of Standard Chartered bank became minister of trade while a former adviser to the investment bank UBS Warburg got the position as competitiveness minister (The Economist, 2009).

issues in the selection of the mayor, I use a regression discontinuity approach to estimate the effect of business experience on public finance, public employment and corruption.
One of the main challenges in this paper is the exact definition of the business-group. Although a businessperson is defined as someone with practical experience is the business sector, it remains unclear to what extent this also requires an appropriate education in a business-related subject or management skills that go beyond pure purchasing and selling processes. As an attempt to deal with this issue, I conduct my analysis for three different business-groups. In my baseline case, I aggregate all candidates with a business profession to one group called "businesspersons". Subsequently, I repeat my study for entrepreneurs and merchants, the two main sub-groups within "businesspersons", separately. Even though all candidates in these two groups have practical experience in the business sector, their areas of responsibility and working conditions are very diverse. Regarding this, merchants are usually specialised in trade and selling goods while entrepreneurs are defined as persons who organize and manage a business, usually with considerable initiative and risk.
The empirical results from the baseline estimation turn out to be mostly insignificant and non-robust. This supports my assumption that the relatively broad definition of a businessperson does not account for heterogeneity within the business-group. Repeating the analysis for entrepreneurs and merchants eventually confirms the presumption that the estimated treatment effect in the baseline case is inaccurate. From the first sub-sample I obtain robust evidence that electing an entrepreneur leads to 7-10% higher total revenue, 8-10% higher total expenditure, 7-12% higher current transfers and between three and five additional active direct administration employees per 1,000 inhabitants. On the contrary, electing a merchant does not affect any of the outcome variables.
Overall, I find three main results. First, I reject the hypothesis that there is an effect of business experience per se. Second, having only practical experience in trade does not affect policy outcomes either. Third, the election of a relatively "high level" businessperson with a relevant management experience does significantly improve public finance. Thus, it can be assumed that due to their advanced negotiating skills and their better understanding of economic interrelations, entrepreneurs attract more transfers from higher government levels. Therefore, the municipality's total revenue increases, which in turn leads to higher expenditure and the hiring of more public servants.
The paper is structured as follows: In the Section 2, I give an overview over the existing literature and explain why policy outcomes might differ when a businesspersons is in charge. In Section 3, I describe the data I use and show how I prepared the dataset for my estimations. After that, I present my empirical strategy in Section 4 and tests the validity of my regression discontinuity design in the baseline case. Section 5 reports and discusses the results for businesspersons in general, as well as for entrepreneurs and merchants. Finally, Section 6 concludes.

2 Literature Review

In the contemporary political debate there appears to be a believe that some personal characteristics qualify certain politicians better for office than others. For instance, it is assumed that professional, social and age groups have different skill sets and heterogeneous personal prefer-

ences. As a consequence, some politicians are expected to be more able to govern, implement reforms or represent certain electoral groups.

Nevertheless, the existing literature has been divided on this topic. On the one side, the classical models of electoral competition, such as the Downsian model (Downs, 1957), suggest that personal characteristics and preferences should not matter. Under these models, all candidates will eventually support the policy preferred by the median voter in an attempt to maximize their vote-shares. Aldrich and Rohde (2000) add that also a party's political agenda or unforeseen events such as natural catastrophes or terror attacks might take precedence over a politician's personal interest. Moreover, Matthews (1984) and Besley (2005) argue that during the time in office the impact of institutional socialization might override past experiences.

However, as stated in the previous section, many studies do find evidence that politicians' biographic and personal characteristics matter. Under the assumption that this is actually the case, the election of a businessperson might impact policy in two ways. First, businesspeople could have different preferences and thus, vote differently on issues specifically related to their professional background. In theory, the "citizen-candidate" models (Osborne and Slivinski (1996); Besley and Coate (1997)) adopt this idea and assume that politicians only run for office if their expected benefit from winning outweighs the costs of running. As these costs are expected to to different for each politician, candidates will not approach and eventually implement the same policy as in the before mentioned median voter models. Poole and Rosenthal (1996) show a direct comparison between a "principal-agent" model, where the legislator (agent) *must* serve the interests of a voter (principal), and an "ideological" approach that accounts for personal preferences and ideology of the principal. Based on an empirical tests for the United States Senate the authors reject the first theory and conclude that legislators take their voting decisions due to a "dash of the principal's preferences, a sprinkle of party discipline, and a pinch of the legislator's personal ideology". Witko and Friedman (2008) find that this is also true for the concrete case of businesspersons. Their results suggest that Congress members with business background have closer relationships with business interests and thus, they are both more likely to receive larger contributions from corporate Political Action Committees (PACs) and demonstrate more pro-business roll call voting.[2]

Second, as Beach and Jones (2016) point out, the probably most frequently named benefits of electing a businessperson is his/her alleged better understanding of economic interrelations, good negotiating skills and a higher ability to improve public finance.[3] In fact, an *AP-GfK Poll* from October 2016 found that Republican voters preferred private sector leadership experience over experience holding elected office 76% to 22%. In contrast, only 33% of Democrats, saw private sector experience as more important.

[2]Note, that this correlation between professional backgrounds and the way how politicians vote on certain issues has been proven for a number of other groups. For example, Lupton (2017) shows that congresspersons with military experience are significantly more likely to vote to increase congressional oversight over war operations, Bellemare and Carnes (2015) conclude that politicians with farming experience tend to vote in favour of farm subsidies and finally, lawyers vote differently on no-fault insurance (Dyer, 1976) and tort reforms (Matter and Stutzer, 2015).

[3]Regarding a background in economics, Rajan (2004) state that "the gains from reform[s] are never as clear to the wider public as they are to economists". Dreher et al. (2009) add that those politicians might also be more likely to detect bad advice and "resist the pressure of lobbying groups preferring the status quo". Nevertheless, one should keep in mind that there is a distinct difference between economics and business and thus, not every businessperson might also have had an education in economics.

However, other authors strongly oppose the idea that the quality or ability of a candidate might vary due to business experience. For example, Nobel laureate Paul Krugman wrote in a Harvard Business Review column:

> "What people learn from running a business won't help them formulate economic policy. A country is not a big corporation. The habits of mind that make a great business leader are not, in general, those that make a great economic analyst [...]." (Krugman, 1996)

Another often repeated argument against an effect of business experience is that goals of running a business vary greatly from those of running a country, state or municipality. In the private sector, the main objective is always profit maximization. This is in stark contrast to the far more complex goals for government success, which is measured in terms of prosperity, social security and many other partially "unmeasurable" things. (Forbes, 2016)
With respect to concrete empirical proof, Beach and Jones (2016) study California city councils to check whether policy outcomes and public finance are different under politicians with business experience. Using a regression discontinuity design they find no evidence that the election of a business-candidate has an impact on city expenditure, revenues or the unemployment rate. In addition, they check for effects that might be observable by the voter but not in the data. However, as future vote-shares for candidates with business experience are unaffected, the authors also reject this hypothesis.
Overall, it can be said that the literature has been divided over the question whether business knowledge has any impact on a politician's later performance. Although some authors argue that governing is not comparable to running a business, others suggest that policy outcomes might vary due to personal preferences or business-specific skills, like leadership expertise and a higher ability to deal with business-related issues such as public finance.
With this paper I want to contribute to the literature in two ways. First, to the best of my knowledge, I am the first one ever to find a significant effect of business experience on public finance and second, I am also the first one to analyse this effect on the municipality level in a developing country.

3 Data on candidates, municipalities and federal units

The emphasis of this paper lies on mayoral elections in Brazilian municipalities during 2004, 2008 and 2012. Today, Brazil consists of 5,570 different municipalities in which the mayor gets directly elected by citizens to a four-year mandate.[4] For my empirical analysis of the alleged effect of electing a candidate with business experience, I combine information from five different sources.
First, I use electoral data from the Superior Electoral Court (*Tribunal Superior Eleitoral*), the highest judicial body of the Brazilian Electoral Justice.[5] From this source, I get two datasets. The first one, *Candidatos*, provides detailed personal information for all candidates that ran

[4]Note, that the number of municipalities is slightly different in each period: 5,560 (2004); 5,564 (2008); 5,565 (2012). For more information see *IBGE: Instituto Brasileiro de Geografia e Estatística*.
[5]The dataset can be downloaded from the official website of the *Tribunal Superior Eleitoral*.

in the mayoral elections. Regarding this, I obtain the profession (*Descrição da ocupação do candidato*), the civil status (*Descrição do estado civil do candidato*) and the name of the municipality where each candidate was born (*Nome do município de nascimento do candidato*). The second dataset, *Resultados*, adds the total amount of votes that each candidate received (*Quantidade de votos nominais totalizados para aquele candidato naquele município e zona*), the electoral zone (*Número da Zona Eleitoral*) and the number of the electoral turn (*Número do turno*).[6]

For financial data, I use the *FINBRA* dataset which is available on the website of the National Treasury (*Tesouro Nacional - Ministério da Fazenda*).[7] This source contains the total population (*População*) as well as a number of financial indicators that I use as potential outcome variables. As the structure of these yearly databases changed in 2013, I only consider the years between 2004 and 2012 for my study. For this period I obtain annual data about total revenue (*Receita total*), total expenditure (*Despesa Total*), current transfers (*Transferências correntes*) and the total dept-to-asset-ratio, which is computed as total liabilities (*Passivo real*) divided by total assets (*Ativo*).[8] Further, I follow an approach by Wang et al. (2007) and Rivenbark et al. (2010) and define the ratio between revenues and expenditure as an additional measure for the government's financial condition.[9] All financial variables are initially measured in Brazilian reais (R$) but I will later re-define them in per capita terms and utilize logarithms for the regressions.

Third, I use the survey "Brazilian Municipalities Profile" (*Perfil dos Municípios Brasileiros*), which is conducted on a yearly basis by the Brazilian Institute of Geography and Statistics IBGE (*Instituto Brasileiro de Geografia e Estatística*).[10] From here, I get information about the mayor's age (*Idade*), gender (*Sexo*) and education (*Escolaridade*), as well as the municipalities' total active direct administration employees (*Total de funcionários ativos da administração direta*) and total active indirect administration employees (*Total de funcionários ativos da administração indireta*). Moreover, the IBGE data helps to answer whether or not the municipality paid subsidies to firms in the last 24 months. I get information for five sectors: Industrial (*Industrial*); commerce and services (*Comercial e serviços*); tourism, sport and leisure (*Turismo, esporte e lazer*); agriculture (*Agrário*) and other sectors (*Outros*). Based on this, I include dummy variables for all five sectors as well as another variable *Subsidies* which is the sum of all five sector dummies.

Fourth, I use corruption data from audit reports that have been coded by Brollo et al. (2013). In 2003, the Brazilian government started a federal anti-corruption program where they randomly audited municipalities to develop an objective measure for administrative irregularities in government contracts and purchases. For this purpose, Brollo et al. (2013) conduct a dummy

[6] Given this, I can calculate the total amount of votes per mayoral election as the sum of received votes over all candidates and all electoral zones within each municipality. Eventually, a candidate's vote-share is the number of votes that he/she received divided by the total amount of votes given to all candidates in this municipality.

[7] The dataset can be downloaded from the official website of the *Tesouro Nacional - Ministério da Fazenda*.

[8] The total liabilities are the sum of (1) financial liabilities (*Passivo financeiro*); (2) securities in circulation (*Obrigações em circulação*); (3) outstanding short-term amounts (*Valores pendentes de curto prazo - PNF*); (4) long-term liabilities (*Exigível a longo prazo*) and (5) long-term results (*Resultados de exercícios futuros*).

[9] The ratio serves as an indicator for whether or not the government was able to stay within the budget and avoid new debts. Note, that values above unity indicate that the mayor did *not* exceed the municipality's budget.

[10] The dataset can be downloaded from the official website of the *IBGE: Instituto Brasileiro de Geografia e Estatística*.

variable for *broad* corruption (*Broad*) corruption.[11] *Broad* is equal to one if at least one of the following irregularities has been detected: (1) *illegal procurement practices*; (2) *fraud*; (3) *favoritism* in the good receipt; (4) *over-invoicing*, occurring when there is evidence that public goods or services are purchased for a value above the market price; (5) *diversion of funds*; or (6) *paid but not proven* occurring when expenses are not proven.[12] Overall, the federal anti-corruption program allows to analyse corruption for 757 municipalities in the first period (2004-2007). As the assignment to audit has been decided by lottery, some municipalities have been audited twice during the period. In these cases, I take the average of the two observations. Finally, I obtain data on the 27 Brazilian federal units (*Unidades Federativas, UF*) from the Institute of Applied Economic Research (*Instituto de Pesquisa Econômica Aplicada (IPEA)*).[13] Here I focus on five measures: average household income in Brazilian reais (R$) per capita (*Renda domiciliar per capita - média*), the GINI coefficient (*Renda - desigualdade - coeficiente de Gini*), the unemployment rate (*Taxa de desemprego* - (%)) and the average years of education for people over 25 (*Anos de estudo - média - pessoas 25 anos e mais*).

After combining all five sources, I apply three steps that are necessary to prepare the data for the regression discontinuity analysis. First, I restrict the dataset to elections of mayors (*Prefeito*) and elections on the municipality level (*Eleições 2004, Eleições 2008, Eleições Municipal 2012*). Second, the Brazilian law distinguishes between two types of mayoral elections: in municipalities with more than 200,000 voters the mayor gets elected by a majority run-off rule, while in municipalities below 200,000 voters a plurality rule is applied. This implies that some elections in large municipalities were decided in a second round if no candidate got more than 50% of the total votes in the first round. In these cases, I drop the first round results and only consider the second round where the two best candidates faced each other.[14] Third, I take the average of each variable over each legislative period and divide total revenue, total expenditure and current transfers by the average total population in each period. In addition, I express all variables related to public employment in terms of per 1,000 inhabitants.

Table A1 in the Appendix provides an overview over all variables in the final dataset and Table A2 shows which variables are available in each year. It might be important to state here that not all variables are available for each municipality within each year and thus, the sample sizes vary between outcome variables.

[11] The authors also derived a variable for and *narrow* corruption which is equal to one if at least one of the following irregularities described in the audit reports is: (1) *severe illegal procurement practices*; (2) *fraud*; (3) *favoritism*; and (4) *over-invoicing*. However, every *narrow* corruption case is also included in the *broad* definition.

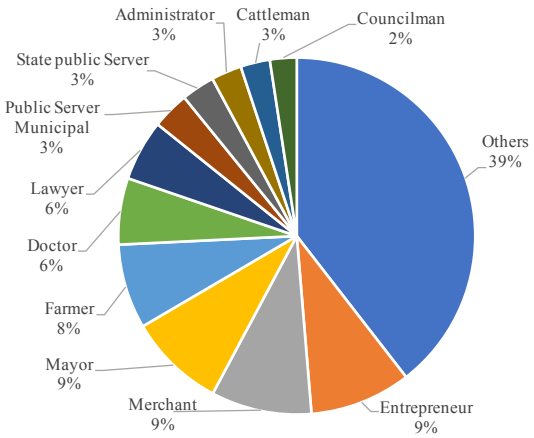
[12] Note, that (1) *severe illegal procurement practices*, occur when any of these issues are reported: (a) competition has been limited, (b) manipulation of the bid value, (c) an irregular firm wins the bid process, (d) the minimum number of bids is not attained, or (e) the required procurement procedure is not executed. For a more detailed definition of the corruption measures see Brollo et al. (2013).

[13] Note, that I only use information about the 26 states within the 27 federal units (UF). For this reason, I will continue to refer to *states* instead of *UFs*. The dataset can be downloaded from the official website of the *Instituto de Pesquisa Econômica Aplicada (IPEA)*.

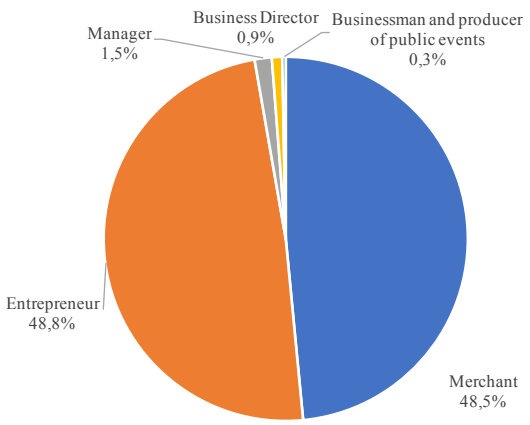
[14] However, in the final dataset I only have 16 out of 4,813 elections that were decided in the second round. For a more detailed description of the electoral system in Brazil see Colomer (2016). Note, that Brollo and Troiano (2016) also use the margin of victory for their analysis of the effect of electing a female mayor in Brazilian municipalities.

Figure 1: Candidates by profession

(a) Distribution all candidates' professions



(b) Distribution of the business-professions



(a) I have assigned all professions that occur less than 1,000 times in the dataset to *Others*. The full dataset contains 255 different professions.
(b) The observations are distributed as follows: 4,077 merchants; 4,102 entrepreneurs; 124 managers; 77 business directors; 26 businessman and producers of public events.

4 Empirical Strategy

4.1 Defining business-professions and descriptive statistics

As previously stated, the main goal of this paper is to examine whether policy outcomes differ due to the mayor's professional background. Figure 1a shows the distribution of occupations over all candidates running in the three elections. It is worth mentioning that the professional backgrounds are very heterogeneous and range from car washer (*Lavador De Veículos*) over gold miner (*Garimpeiro*) to senator (*Senador*). However, the biggest share of candidates are entrepreneur (*Empresário*), merchant (*Comerciante*) and mayor (*Prefeito*) with 9%, farmer (*Agricultor*) with 8% or lawyer (*Advogado*) and doctor (*Médico*) with 6% of the total variety of professions.

One crucial point for my analysis is to define which of these professions are business-related and which ones are not. At first sight, this seems to be relatively straightforward as one could assume that a businessperson is simply somebody who has working experience in the business-sector. However, as I pointed out in Section 2, it is not only general management and negotiating experience but also a better understanding of economic interrelations that allegedly qualifies a businessperson better for office. In light of this, I repeat my analysis for three different definitions of a business-profession. For my baseline study, I use a relatively broad definition and assign all candidates to the business-group if they are either entrepreneur (*Empresário*), merchant (*Comerciante*), business director (*Diretor De Empresas*), businessman and producer of public events (*Empresário E Produtor De Espetáculos Públicos*), or manager (*Gerente*). Figure 1b shows the distribution of candidates within the baseline business-group. It is notable, that 97.3% of all business-candidates are either entrepreneur (*Empresário*) or merchant (*Comerciante*). As candidates from these two professional groups might be different, I complement my baseline study with two additional analysis for these entrepreneurs and merchants in Section 5.2.

Table 1: Summary statistics: Municipalities where the mayor is a businessperson vs municipalities where the mayor has another professional background.

Variable	$B_{it} = 1$	Obs.	$B_{it} = 0$	Obs.	ΔB_{it}	p-Value
Columns	(1)	(2)	(3)	(4)	(5)	(6)
Mayor						
Age	48.164	2,369	48.733	2,437	−0.569	0.041
Education	12.816	2,369	15.160	2,440	−2.344	0.000
Gender	0.052	2,369	0.102	2,440	−0.051	0.000
Married	0.801	2,372	0.778	2,441	0.023	0.054
Origin	0.435	2,372	0.415	2,441	0.020	0.168
Municipality and state						
Population	23,730.94	2,320	24,071.10	2,396	−340.159	0.823
Income	865.748	2,372	859.675	2,441	6.074	0.451
GINI	0.516	2,372	0.517	2,441	−0.001	0.244
Unemployment	0.912	2,372	0.909	2,441	0.003	0.275
Av.Study	6.898	2,372	6.892	2,441	0.006	0.815
Public finance						
Revenue	1,667.285	2,342	1,735.199	2,423	−67.914	0.053
Expenditure	1,641.23	2,342	1,698.144	2,423	−56.914	0.087
Transfers	1,525.191	2,342	1,571.269	2,423	−46.078	0.132
REratio	1.019	2,343	1.023	2,423	0.004	0.384
DAratio	0.493	2,343	0.506	2,423	−0.013	0.476
Subsidies to firms						
Subsidies	1.684	1,257	1.712	1,329	−0.028	0.446
Public employment						
StDirect	44.693	2,341	44.992	2,422	−0.299	0.575
StIndirect	1.764	571	1.851	602	−0.086	0.677
Corruption						
Broad	0.881	118	0.904	104	−0.022	0.592

Note, that column (2) implies that the mayor is a businessperson and column (4) that the mayor has a profession that I did not define as businessperson. Column (6) is the difference between column (2) and (4): $\Delta B_{it} = (B_{it} = 1) - (B_{it} = 0)$. Column (2) and (4) report the average values in the respective samples; *Obs.* is the number of observations; *p-Value* refers to the statistical significance of (5); Standard errors are in parentheses. See Table A1 for the definition of the variables.

The aim of my baseline study is to analyse the effect of electing a mayor who had some kind of business experience in his/her previous job in comparison to electing a mayor without this kind of experience. For this purpose, I focus my analysis on elections where the two candidates with the highest votes-shares were exactly one businessperson and one non-businessperson. Ignoring all other candidates from these elections as well as all other elections that do not fulfil this criteria reduces the total number of candidates from 44,687 to 9,626.[15] As the dataset still includes two candidates per election, it is useful to drop all non-business-candidates and infer that they won the two-candidates-races if the business-candidates lost. However, as I stated in Section 3, mayors in municipalities with less than 200,000 voters get elected by a plurality rule and thus, do not need more than 50% of the total votes to win. Hence, there is no clear threshold value for the vote-share after which a candidate wins the election. One simple solution for this problem is to define the margin of victory (MV_{it}) as the difference between the business-candidate's vote-share and the one of the non-business-candidate. Now, I can conclude that the business-candidate won the election if the margin of victory was non-negative ($MV_{it} \geq 0$) and otherwise, the non-business-candidate became mayor. Eventually, my dataset consists of 4,813 elections of which the business-candidate could win 2,372 (49.28%).[16]

Table 1 shows a summary of the main variables according to the profession of the mayor. The first two panels of the table show control variables for mayor (gender, age, education, civil status, municipality of origin), municipality characteristics (total population in the year before the election) and state characteristics (average household income per capita, GINI coefficient, unemployment rate, average years of study for persons aged 25 or older).[17] The third panel includes variables related to public finance (total revenue, total expenditure, current transfers, revenue-to-expenditure-ratio, debt-to-asset-ratio), the fourth includes a dummy for whether or not the municipality government provided subsidies to firms, the fifth panel shows public employment (total direct and indirect administration employees), and the last one presents a broad measure for corruption.

The dataset is divided into two sub-samples, in column (1) the mayor is a businessperson ($B_{it} = 1$) and column (3) the mayor is a non-businessperson ($B_{it} = 0$). Column (5 and 6) show the difference between column (1) and (3) and the according p-value.[18]

In the first panel, we see that 95% of all business-mayors are male ($Gender_{it} = 1$ if the mayor is female) and the average years of schooling of a businessperson is around 13 years, which equals a completed high school degree (*Ensino médio (2º grau) completo*) and on average over two years shorter than in the sub-sample of non-business mayors. As Figure 2 shows, the share of high-educated mayors in the non-business group clearly exceeds the one of the business-group. The reason for this is the large amount of academics (i.e. lawyers, doctors, teachers) and scientists (i.e. pharmacists, engineers) in the first group. In addition, the share of women in the non-business group is more than five percentage points higher. In both groups, the average

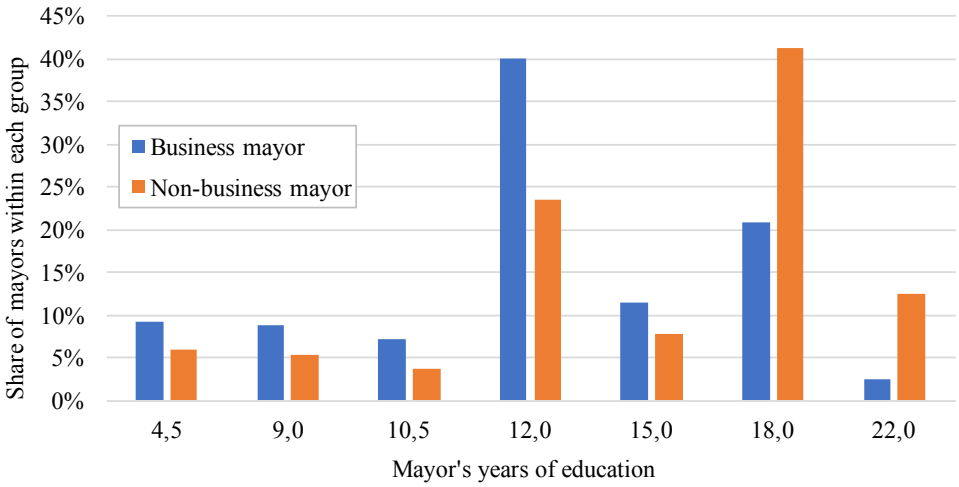
[15] Before that, I dropped 302 elections (0.68% of all elections) where I did not have information about public finance, public employment nor subsidies.

[16] In the unrestricted sample (which includes all 44,687 candidates) businesspersons won 18.38% of the elections.

[17] In order to rule out the possibility of a direct influence of the mayor's profession on the control variable ("bad controls"), I need to use the lagged total population for the years 2003, 2007 and 2011. For the other variables this is not necessary as they are not likely to be directly influenced by the treatment.

[18] Note, that Table 1 only shows the average effect over sub-populations rather than unit-level effects as it is not possible to observe both $B_{it} = 1$ and $B_{it} = 0$ in the same period in one municipality.

Figure 2: Mayors' years of education by profession



The years of education belong to the following education levels: Primary education (incomplete 4.5; complete 9), secondary education (incomplete 10.5; complete 12), higher education (incomplete 15; complete 18) and post-graduation (22).

age of the mayor is 48 years, around 80% are married and approximately 42% of the mayors govern in the same municipality where they were born. The average municipality population in both samples is around 24,000. Further, the state characteristics seem to be balanced between groups in terms of wealth (see *Income* and *Unemployment*), inequality (see *GINI*), as well as education of the population (see *Av.Study*).

Although the findings from the first panel suggest that mayors differ according to their profession, none of the outcome variables in the last four panels seems to support this view. In contrast, this early evidence from an average comparison indicates that electing a businessperson does not have any effect on the municipality's public finance, public employment, the likelihood of providing subsidies to firms or corruption. Nevertheless, it is important to understand that these results do not have a causal interpretation and are most likely biased as I will explain in the following chapter.

4.2 Identification: Regression Discontinuity Design

The underlying hypothesis of this paper is that candidates with a background in business differ from non-business-candidates and thus, we should observe a different performance when they become mayor. Let me define $Y_{it}(1)$ as a potential outcome variable for municipality i during legislative period $t \in \{2004, 2008, 2012\}$ if the mayor's profession has been defined as business-related. Alternatively, $Y_{it}(0)$ defines a potential outcome variable for municipality i during legislative period t if the mayor's profession has not been defined as business-related. The coefficient of interest in my estimation will be the difference between these two potential outcomes when the election has been a race between exactly one businessperson and one non-businessperson, i.e. $E(Y_{it}(1) - Y_{it}(0)|i \in \Omega)$. The main concern is that I cannot observe both cases simultaneously within one municipality at a given time. Further, a simple comparison between the average outcomes (as in Table 1) would fail to identify the true causal effect because

the estimation might be biased due to endogeneity issues.[19] For instance, the decision of electing a businessperson might be correlated with municipality-specific characteristics such as previous experience with businesspeople in politics or relative economic importance of the candidate's business.[20] Hence, it will be necessary to find elections where on the one side both candidates as well as municipality characteristics are similar but on the other side, the electoral outcome is different.

One possible way to deal with these endogeneity issues is using a regression discontinuity design (RDD). The RDD exploits precise knowledge of rules determining the assignment of a certain treatment to identify its causal effect on the outcome of interest. Given that the rule is arbitrary and agents are not able to precisely sort across the selection threshold, the RDD provides a randomized quasi-experiment to rule out selectivity and omitted variable bias in estimating the treatment effect. Lee and Lemieux (2010) explain that the main idea behind this research design is that individuals who score just below a certain cut-off (and thus, did not get the treatment) and those just above the cut-off (who receive the treatment) are a good comparison. In each RDD there is a so-called running variable, which is continuous around the threshold and characterizes the assignment rule. If the treatment is a deterministic function of the running variable, we are in the case of a *sharp* RDD. Otherwise, the RDD is *fuzzy* and there is only a significant discontinuity in the probability of receiving the treatment conditional on the running variable.

For this paper, I use the margin of victory (MV_{it}) as the running variable and define zero as the cut-off value after which the candidate gets the treatment of winning the election.[21] Further, I assume that an election where the businessperson receives a vote-share just below the threshold provides a good counterfactual for another election where the businessperson gets just a few more votes than his/her competitor and wins the election. In order to indicate the treatment status, I define the dummy variable B_{it} which is equal to one if the business-candidate won the election in municipality i at time t and zero otherwise. As the treatment is a deterministic function of MV_{it}, my approach can be categorized as a *sharp* RDD. Given the two potential outcomes, the observed outcome can be expressed as $Y_{it} = B_{it} * Y_{it}(1) + (1 - B_{it}) * Y_{it}(0)$ and I define the *treatment group* as those municipalities which voted for a mayor with business experience by a narrow margin in an election where the two candidates with the highest vote-shares were one businessperson and one non-businessperson. Assignment to treatment can be formalized as:

$$B_{it} = 1[MV_{it} \geq 0] \tag{1}$$

[19] In other words, a comparison of means is only possible under the condition that all unobserved factors are continuous in the *whole* sample. However, it is clear that this is usually not the case in reality.

[20] This could be the case when many voters are working for one large company and one of its managers runs for mayor. Then, it would be likely that he/she understands the needs of both the industry and the employees better than a business outsider. Another example comes from Beach and Jones (2016) who state that businesspeople might be more likely to be elected in cities with economic distress as they are assumed to be better suited to solve those issues.

[21] Recall that $MV_{it} = VoteShare_{it}(Business_{it} = 1) - VoteShare_{it}(Business_{it} = 0)$ where $Business_{it}$ is a dummy variable that is equal to one if the candidate has a business-related profession and zero otherwise. Thus, the candidate with the highest vote-share wins the election.

In general, I can specify the following outcome equation for both sides of the threshold:

$$Y_{it} = \alpha + f(MV_{it}) + \beta B_{it} + \delta X_{it} + (U_{it} + e_{it}) \tag{2}$$

where $f(\cdot)$ is a function of the running-variable, X_{it} is a vector of control-variables, U_{it} is a vector of unobservable factors that might "directly" affect Y_{it} and e_{it} is the stochastic error term.[22] Subtracting the expected value of (2) for the control group from the expected value for the treatment group, I can express the expected causal effect of the treatment around the cut-off as follows:

$$\begin{aligned} E[Y_{it}|MV_{it} = c + \Delta] - E[Y_{it}|MV_{it} = c - \Delta] = \\ \beta + \{E[\delta X_{it}|MV_{it} = c + \Delta] - E[\delta X_{it}|MV_{it} = c - \Delta]\} \\ +\{E[U_{it} + e_{it}|MV_{it} = c + \Delta] - E[U_{it} + e_{it}|MV_{it} = c - \Delta]\} \end{aligned} \tag{3}$$

where c is the cut-off value (here $c = 0$) and $c \pm \Delta$ defines a value just below or above the threshold.

4.3 Validity of the regression discontinuity design

For a causal interpretation of the treatment effect (β) in equation (3), the RDD must satisfy two main assumptions that allow me to neglect the last two terms on the equation's right-hand side. If both requirements are satisfied, I can interpret the difference in outcome variables as the "average treatment effect" (ATE) at the cut-off c and simplify equation (3) as follows:

$$\hat{\beta}_{SRD} = \lim_{\Delta \to 0} E[Y_{it}|MV_{it} = c + \Delta] - \lim_{\Delta \to 0} E[Y_{it}|MV_{it} = c - \Delta] = ATE \tag{4}$$

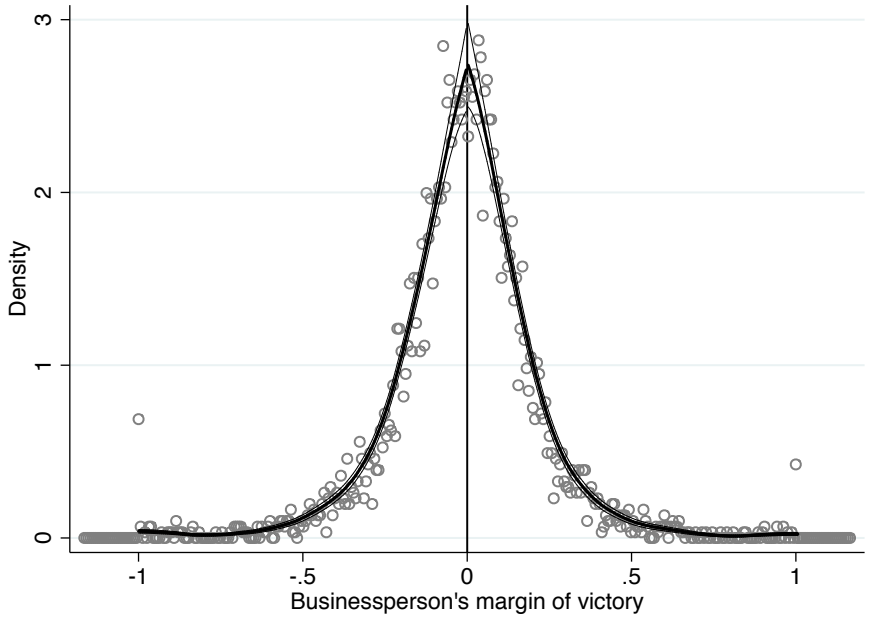
where $\hat{\beta}_{SRD}$ is the estimator for the sharp RDD.[23]

The first condition, requires that the assignment around the threshold is exogenous. In other words, if there is continuity in the joint distribution of the error term $(U_{it} + e_{it})$ and MV_{it}, $\{E[U_{it} + e_{it}|MV_{it} = c + \Delta] - E[U_{it} + e_{it}|MV_{it} = c - \Delta]\}$, the last part in (3) becomes negligible. The problem with this assumption is that I cannot test it directly. However, if I assume this condition is violated, I must observe some sort of skewness or unusual jumps in the distribution of the running-variable because candidates would be able to precisely manipulate their assignment to the control or treatment group. Figure 3 shows a McCrary test (see McCrary (2008)) which treats the frequency counts of the running variable (MV_{it}) as a dependent variable in a local linear regression. As I do not observe any significant jumps around the threshold, I can rule out that businesspeople can directly determine their electoral results and assume that the variation in treatment close to the threshold is as good as random. In other words, every business-candidate will approximately have the same probability of having a MV_{it} that is just below or just above c. A more analytical way to check this assumption is with placebo experiments where I assign a different cut-off value c' at which I should not observe any unusually

[22] Note, that it is possible that B_{it} and U_{it} are correlated.

[23] Note, that the ATE is defined as the mean effect of the treatment B_{it} averaging over all elections with a particular set of observed characteristics (here $MV_{it} = c \pm \Delta$). Moreover, the effect has to be regarded as a (locally) *weighted* ATE because I give more weight to observations close to the threshold.

Figure 3: McCrary test on the business-candidate's margin of victory



The variable on the x-axis indicates the margin of victory of the candidate with a business-related profession. It is calculated as the difference between this business-candidate's vote-share and non-business-candidate's vote-share. Thus, the businessperson won the election if his/her margin of victory is bigger than zero. Otherwise, the non-businessperson won.

large jumps. In Table A3 in the Appendix I show regressions with two alternative thresholds where $c' \neq 0$.[24] The coefficients are not statistically significant and thus, support the graphical evidence from the McCrary test.

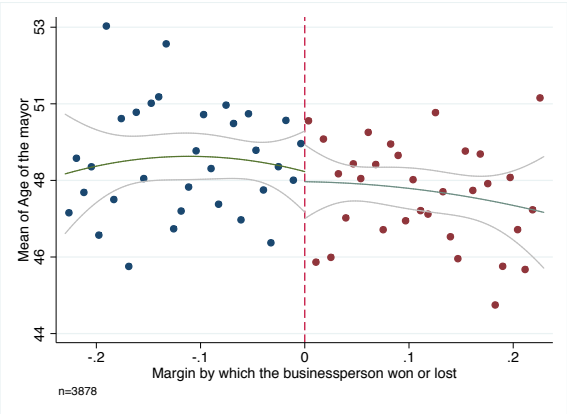
Second, as explained by Hahn et al. (2001), a causal interpretation of the RDD estimation requires that the joint distribution of the vector of control-variables X_{it} and the running-variable MV_{it} is continuous and thus, also the part second part on the right-hand side in (3), $E[MV_{it} = \delta X_{it}|c + \Delta] - E[\delta X_{it}|MV_{it} = c - \Delta]$, can be ignored. An easy way to check this assumption is to test if all covariates that might have a direct effect on the outcome, are the same just below and above the threshold. Figure 4 presents scatterplots of the mean of mayoral, municipality and state characteristics. The horizontal axis is the business-candidate's margin of victory and the observations are averaged within bins. As Table 1 has already predicted, there is a major jump in the years of education and the gender of the mayor. All other covariates seem to be balanced.

Nonetheless, as Lee and Lemieux (2010) point out, it is important to not fully rely on this graphical evidence since its interpretation often depends the viewers perception and on the way the graphs have been constructed. Therefore, Table 2 reports two non-parametric balance tests for all control variables. The results are in line with the graphical evidence and I find that *Education* and *Gender* are statistical significant on the 1%-level. Their coefficients indicate that around the threshold business-mayors are about two years less educated and the share of female mayors with business experience is eight percentage points lower than in the non-business group.

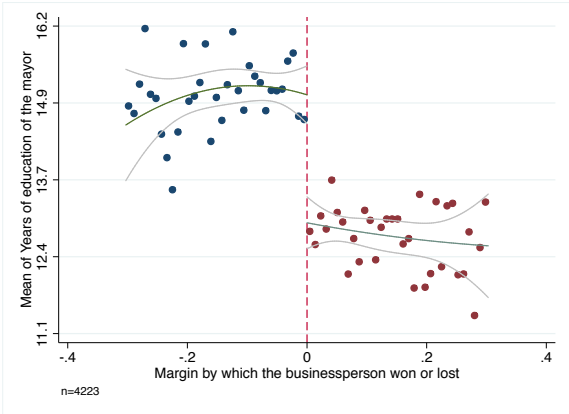
[24] For the regressions, I used a local linear and a local second order-polynomial and the two thresholds $c' = -0.1$ and $c'' = 0.1$.

Figure 4: Balance tests: Control variables for mayoral **(a-e)** municipality **(f)** and state characteristics **(g-h)**.

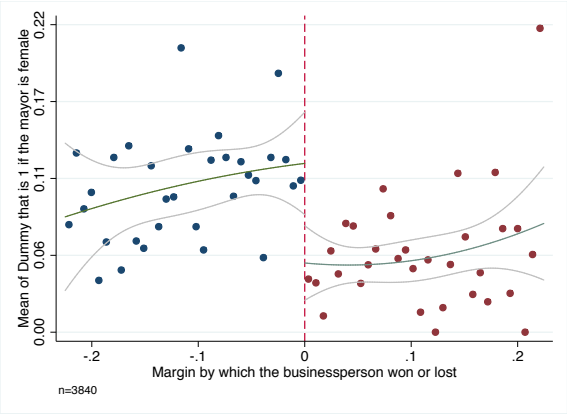
(a) Age of the mayor



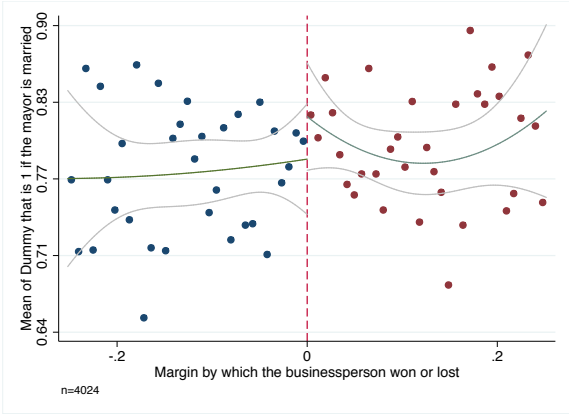
(b) Years of education of the mayor



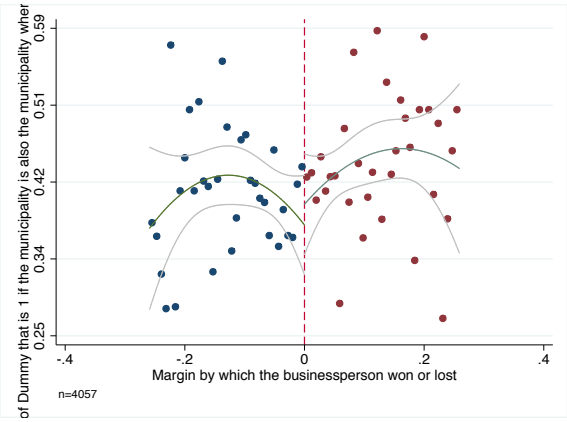
(c) Gender of the mayor



(d) Is the mayor married?



(e) Was the mayor also born in this municipality?



(f) Lagged total population

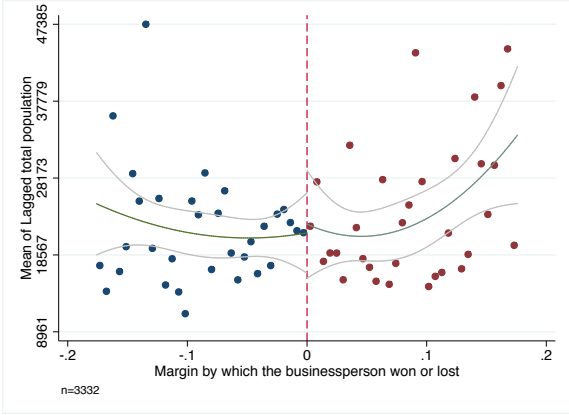
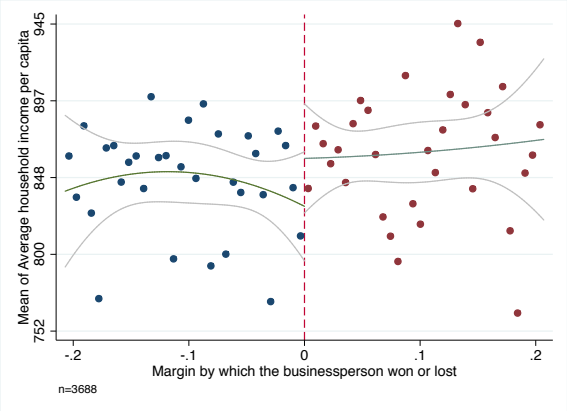


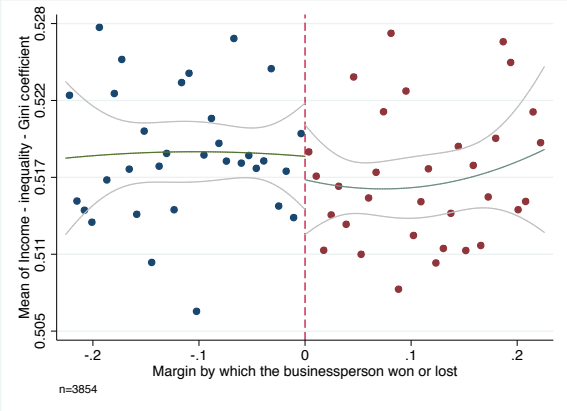
Figure 4 continues on the next page.

Figure 4: Balance tests: Control variables for mayoral **(a-e)** municipality **(f)** and state characteristics **(g-h)**. (continued)

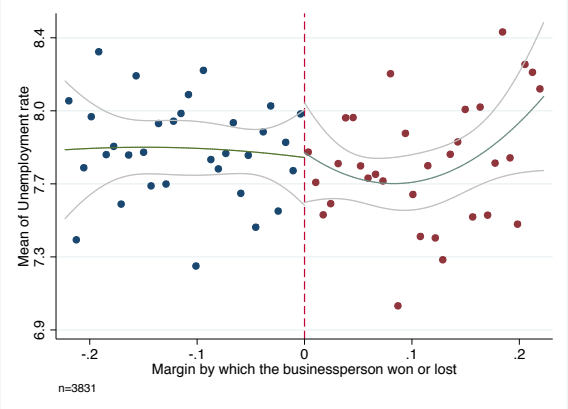
(g) Average household income per capita



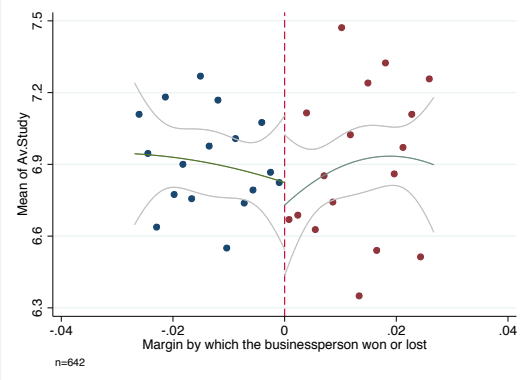
(h) GINI coefficient



(i) Unemployment rate



(j) Average years of study - persons 25 years and over



The green line is a best fit second-order polynomial in the business-candidate's margin of victory for those observations which lie within the optimal bandwidth computed with the Calonico et al. (2014) method. The polynomial is split by a red dashed line which separates the two groups at the threshold $MV_{it} = 0$. Thus, the red points symbolise a business-mayor and the blue ones a non-business-mayor. The thin grey lines are the 95% confidence interval of the polynomial. $Population_{it}$ is the population in the *previous* year of each period (thus, 2003, 2007 and 2011). See Table A1 for the definition of the variables.

On the one side, this is reasonable and confirms the assumption that businesspeople differ from other candidates. However, on the other side, the significant results violate the requirement that all control variables are locally balanced between the two groups. Given these discontinuities, the estimation of the causal effect of electing a businessperson might be biased.

In view of this concern, I have three options. First, I can run my regressions assuming that business-candidates are less educated *because* of their professional background. In other words, a business profession requires fewer years of schooling than for example academic professions and thus, being less educated is a part of the alleged effect of being a businessperson.[25] Unfortunately, I cannot assume the same direct causal relationship between being a businessperson

[25] In the existing literature on RDDs, several authors, such as Becker et al. (2016); Campa et al. (2015); Gagliarducci and Paserman (2016) or Daniele and Vertier (2016), faced similar problems and interpreted the results in a similar fashion.

and the candidate's gender. Nonetheless, I can indirectly suppose that business-professions are either less appealing to women due to heterogeneous preferences or that there are other obstacles that make it more difficult for women to reach higher management positions or become entrepreneurs.[26] For example, Oakley (2000) explains that among others inadequate career opportunities, gender-based stereotypes and "old boy networks at the top" often prevent women from reaching upper management positions.[27]

Second, I could restrict my dataset to a sub-sample in which all covariates evolve smoothly around the cut-off. I repeated my estimation for "male" mayors with at most 12 years of education (completed secondary education) and in doing so, I solved the issue with the two non-continuous covariates. Nevertheless, analysing the McCrary test for this sub-sample raised concerns about the randomness of the assignment around the threshold. In contrast to before, I find a significant positive jump after the cut-off value, which suggests that it is more likely for the businessperson to get a positive margin of victory. This should come as no surprise, if I consider that my restriction reduces the sample to 2,413 elections, of which the businessperson won 1,477 (61.21%). Moreover, a closer look at Figure 2 reveals, that restricting the sample to a certain educational level will generate imbalances between the treatment and the control group. Thus, as it is not possible to eliminate the discontinuities in *Education* and *Gender* while keeping a balanced sample, I will not further consider this option.[28]

Finally, a third way to reduce the observed imbalances around the cut-off would be a matching procedure as used by Daniele and Vertier (2016). However, I will also leave this option to future studies and focus my attention on the first option.

4.4 Estimations

In the previous section, I discussed two conditions that must be fulfilled to validate the use of a regression discontinuity design. Nonetheless, ensuring the estimation of an unbiased treatment effect also requires a correct specification of the empirical model. Concerning this, the recent literature on RDDs distinguishes between two approaches: a non-parametric procedure (i.e. a local linear regression) and a parametric estimation (i.e. a low-order polynomial). Both methods have their strengths and weaknesses and thus, as Lee and Lemieux (2010) point out, they should be regarded as complements rather than as substitutes. For one thing, focusing on data points close to the threshold guaranties a relatively unbiased estimator because control and treatment group are expected to be very similar in all characteristics except for the treatment status. Therefore, it is reasonable to estimate a local regression with a bandwidth h that restricts the sample to elections close to the threshold. However, for another thing, this estimation

[26] See Van Klaveren et al. (2009) for an overview of women in the Brazilian labour market and the difference of female participation between sectors.

[27] Other examples are Langowitz and Minniti (2007) who analyse female entrepreneurship and find evidence for gender differences in self-perception, Shinnar et al. (2012) who show significant gender differences in perceptions of barriers to entrepreneurship or Barbulescu and Bidwell (2013) who point out that different preferences, lower identification with stereotypically masculine jobs as well as lower expectations of job offer success account for gender differences in applications for managerial jobs.

[28] Note, that I select 12 years of schooling because the average education in the group of businesspeople is 14 years and the closest (completed) education is secondary education (after 12 years). However, I also tried a variety of different sub-samples but in every case, I either came to the same result or ended up with very small sample sizes.

Table 2: Baseline results: The impact of electing a businessperson on covariates. RDD estimates.

	(a) Mayoral controls						**(b)** Municipality and state controls				
	Effect	Std.	Poly.	CCT	Obs.		Effect	Std.	Poly.	CCT	Obs.
Column	(1)	(2)	(3)	(4)	(5)		(6)	(7)	(8)	(9)	(10)
Age	−0.585	(0.872)	1	0.163	3,234	*Popu - lation*	−1,080.7	(3,293.9)	1	0.142	2,904
	−0.908	(1.029)	2	0.230	3,881		−125.29	(4,202.4)	2	0.176	3,332
Edu - cation	−2.032***	(0.357)	1	0.169	3,317	*Income*	30.792	(23.128)	1	0.160	3,199
	−1.985***	(0.386)	2	0.303	4,809		43.439	(27.962)	2	0.207	3,688
Gender	−0.078***	(0.023)	1	0.159	3,172	*GINI*	−0.002	(0.003)	1	0.162	3,221
	−0.085***	(0.027)	2	0.225	3,845		−0.002	(0.003)	2	0.226	3,856
Married	0.037	(0.035)	1	0.147	3,033	*Unem - ployment*	−0.043	(0.182)	1	0.163	3,237
	0.042	(0.038)	2	0.252	4,028		−0.120	(0.216)	2	0.223	3,836
Origin	0.022	(0.042)	1	0.171	3,334	*Av.Study*	0.006	(0.069)	1	0.159	3,175
	0.027	(0.048)	2	0.259	4,058		0.027	(0.084)	2	0.027	3,690

The table shows the results from two non-parametric regressions. *Effect* is the treatment effect, *Std.* is the standard deviation, *Poly.* is the order of the polynom, *CCT* is the bandwidth calculated with the Calonico et al. (2014) method and *Obs.* is the number of observations. For a description of all variables, see Table A1. Robust standard errors are in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

might suffer from three shortcomings. First, the estimator could be imprecise due to limited availability of observations close to the threshold. Second, as a local regression might not correctly approximate the functional form of the running variable $f(\cdot)$ in equation (2) it is probably not very informative for candidates more distant to the threshold and thus, does not guarantee external validity. Third, wrong assumptions about the functional form might generate an "artificial" small discontinuity at the threshold and thus, lead to false conclusions. For these reasons, it is useful to also adopt a parametric approach and test for alternative specifications. In my analysis of the alleged effect of electing a businessperson as mayor, I compare the results from seven different models.[29] I start by following Imbens and Lemieux (2008) and adopt both a local linear (5) and a local polynomial regression (6) without covariates:

$$Y_{it} = \beta_0 + \beta_1 MV_{it} + \beta_2 B_{it} + \beta_3 MV_{it} B_{it} + \varepsilon_{it} \tag{5}$$

$$Y_{it} = \beta_0 + \beta_1 MV_{it} + \beta_2 B_{it} + \beta_3 MV_{it} B_{it} + \beta_4 MV_{it}^2 + \beta_5 B_{it} MV_{it}^2 + \varepsilon_{it} \tag{6}$$

where Y_{it} is the outcome variable of interest for municipality i during legislative period t, B_{it} is a dummy that is one when the winner of the election is a businessperson and zero otherwise and ε_{it} is the error term.[30] Please also note, that the inclusion of an interaction terms allows the function $f(\cdot)$ to be different on each side of the cut-off.

[29] Usually, regression lines close to the cutoff are close to linear. However, I decided to also adopt a local second-order polynomial regression because many of the bandwidths that I will use for my estimation in Section 5 are very large and it might be possible that a polynomial fits the data better.

[30] Note, that ε_{it} includes both the vector of unobservable factors that might "directly" affect Y_{it} and the stochastic error term. Using the notation from equation (2), I can express the error term as: $\varepsilon_{it} = U_{it} + e_{it}$.

As stated above, a local regression restricts the sample to municipalities in the interval $MV_{it} \in \{-h, +h\}$. The selection of the bandwidth is a key issue for a local analysis as it has to find a balance between small standard errors and an accurate estimation of the treatment effect. For this reason, I use a method by Calonico et al. (2014) (in the following referred to as CCT) to compute the optimal bandwidth h.[31]

After that, I repeat the two non-parametric estimations (5) and (6) with mayor, municipality and state covariates:

$$Y_{it} = \beta_0 + \beta_1 MV_{it} + \beta_2 B_{it} + \beta_3 MV_{it} B_{it} + \delta X_{it} + \varepsilon_{it} \tag{7}$$

$$Y_{it} = \beta_0 + \beta_1 MV_{it} + \beta_2 B_{it} + \beta_3 MV_{it} B_{it} + \beta_4 MV_{it}^2 + \beta_5 B_{it} MV_{it}^2 + \delta X_{it} + \varepsilon_{it} \tag{8}$$

where X_{it} is a vector of covariates.

Additionally, I complement my analysis with a linear, second- and third-order polynomial regression. In general, a model that fits a p-order polynomial on either side of the threshold can be formalized as follows:

$$Y_{it} = \sum_{k=0}^{p} (\alpha_k MV_{it}^k) + B_{it} \sum_{k=0}^{p} (\beta_k MV_{it}^k) + \varepsilon_{it} \tag{9}$$

For the three models I choose $p = 1$ in the linear case, $p = 2$ for the second-polynomial and $p = 3$ for the third-order polynomial model. Note, I cluster the standard errors at the municipality-level.

Finally, I want to point out that the coefficient of interest in all seven models is the estimator $\hat{\beta}$ that stands in front of the treatment status (B_{it}) and identifies the ATE around the threshold.

5 Empirical Results

In this chapter I present the empirical results of the regression discontinuity analysis. For my estimations, I explore the following potential outcome variables that are measured as the average in each legislative period: the logarithm of total revenue per capita ($Revenue$), the logarithm of total expenditure per capita ($Expenditure$), the logarithm of current transfers ($Transfers$), the ratio between revenue and expenditure ($REratio$), the ratio between total debt and total assets ($DAratio$), the sum of sectors that have been subsidized in the last two years ($Subsidies$, min 0, max 5), the average number of total active direct administration employees per 1,000 inhabitants ($StDirect$), the total number of total active indirect administration employees per 1,000 inhabitants ($StIndirect$) and finally, a dummy variable for broad corruption ($Broad$).

My empirical analysis basically consists of three main parts. First, I examine the results for the baseline case, where I aggregated all candidates with a business-related profession. After that, I also examine the possibility of heterogeneity within the business-group and repeat my estimation for the sub-sample of entrepreneurs ($Empresário$) and merchants ($Comerciante$). In all three cases, I begin with a graphical analysis and then show formal evidence from the four

[31] Calonico et al. (2014) use a uniform bandwidth and controls for an order-polynomial of the business-candidates margin of victory.

local regressions and three global models as described in Section 4.4.
Note, that I previously observed significant discontinuities for two of my covariates. In light of this concern I will interpret all of my results using the first of the three mentioned options and thus, assume a direct causal relationship between being a businessperson and having a lower educational level. Moreover, I suppose that gender-specific obstacles and different preferences have a negative effect on women's labour market participation in the business sector.

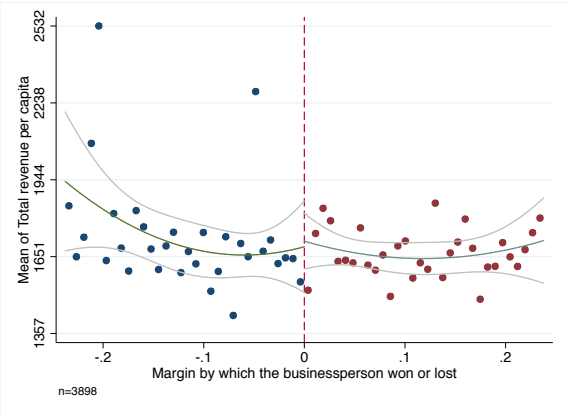
5.1 Baseline regression discontinuity results

The main purpose of this paper is to find out whether or not electing a mayor with business experience has any impact on policy outcomes. I begin by graphically assessing the causal impact of electing a businessperson using scatterplots of the outcome variables. Nonetheless, one should keep in mind that this graphical presentation of "raw data" only gives a first glimpse of whether the jump in the outcome variable around the cut-off is unusually large. Moreover, it is a first step to identify the correct functional form and visualize why different models may lead to different results.
Figure 5 plots the binned averages of the nine variables of interest around the cut-off (when $MV_{it} = 0$). Most outcome variables evolve smoothly through the threshold, suggesting that electing a businessperson does not affect any of those variables.[32] Further, the graphs include a best fit second-order polynomial (green line) and a 95% confidence interval. These lines already give a first impression on the best functional form and also illustrate why a wrong specification might generate small artificial jumps around the cut-off and thus, lead to a false estimation of the treatment effect. However, despite different global functional forms, all graphs become approximately linear or slightly quadratic when approaching the threshold.
Table 3, presents the baseline results for the seven RDD models explained in Section 4.4. The first two columns report the results of a local linear and a local second-order polynomial regression without covariates. As previously stated, I only consider observations close to the threshold using a CCT bandwidth. Additionally, I weight those observations with a triangular kernel, which gives observations close to the cut-off a higher impact on the estimation. Columns (3) and (4) repeat the estimation with covariates for mayoral, municipality and state characteristics. Finally, the last three columns report the results for three parametric models using polynomials of order 1,2 and 3.
With respect to the interpretation of the results of Table 3, it is important to understand the information value of the three kinds of models. Note, that a valid RDD provides a locally randomized experiment and thus, rules out any bias in estimating the treatment effect due to omitted variables. For this reason, I principally only need to consider the non-parametric models in the first two columns to conclude whether or not there is any treatment effect. However, I follow the advice of Lee and Lemieux (2010) and also show non-parametric models with covariates to reduce the sampling variability and increase the precision of my estimation. The purpose of the last three columns is to repeat the results on a global level to also get information about candidates more distant from the threshold and further reduce the standard errors.

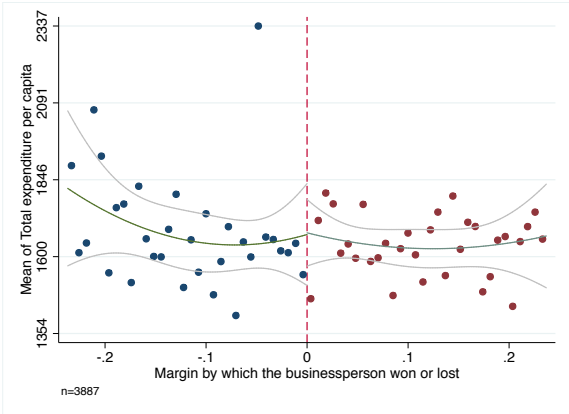
[32] The only exception from this is the debt-to-asset ratio (Figure 5e) which seems to have a slightly negative jump at the cut-off.

Figure 5: Baseline results: The effect of electing a businessperson. Graphical analysis.

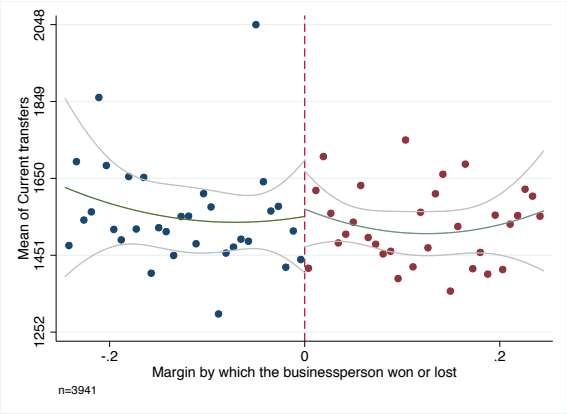
(a) Total revenue



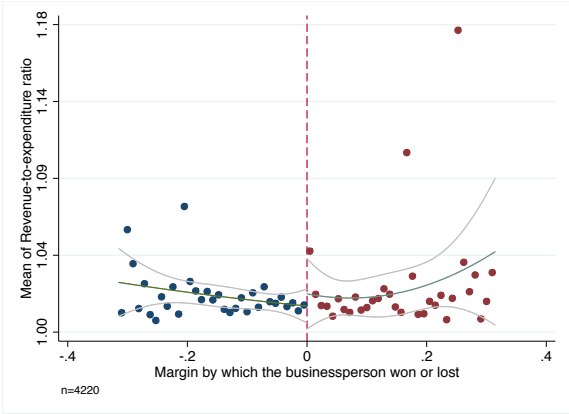
(b) Total expenditure



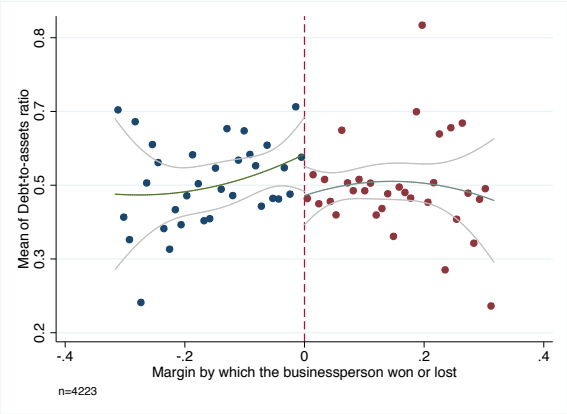
(c) Current transfers



(d) Ratio between revenue and expenditure



(e) Debt-to-asset ratio



(f) Sum of subsidized sectors within the last two years

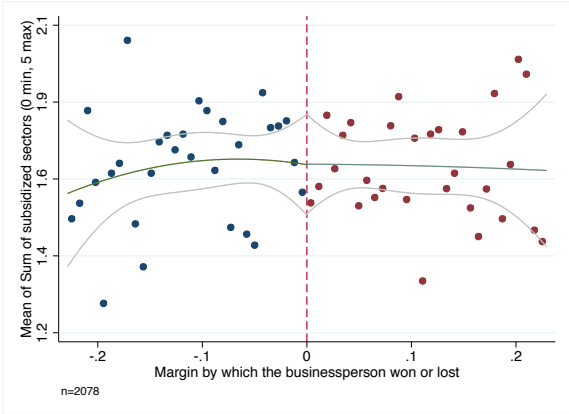
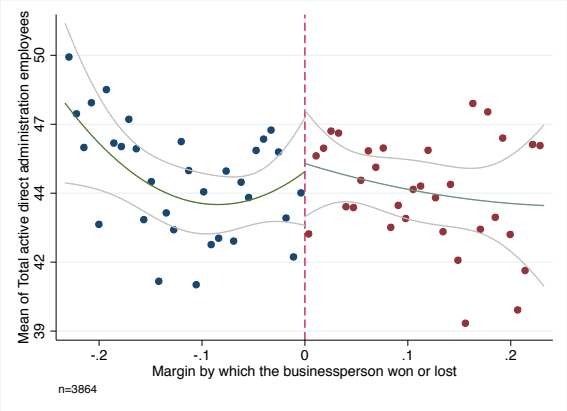


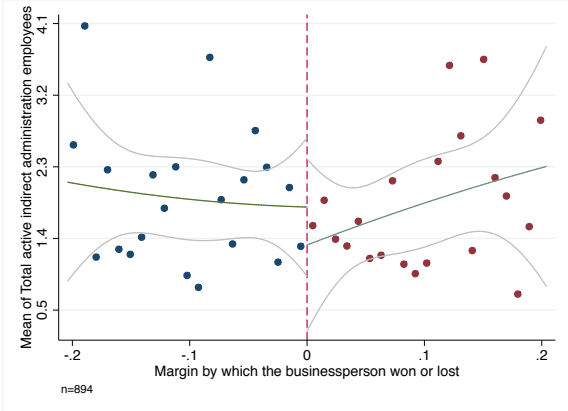
Figure 6 continues on the next page.

Figure 6: Baseline results: The effect of electing a businessperson. Graphical analysis. (continued)

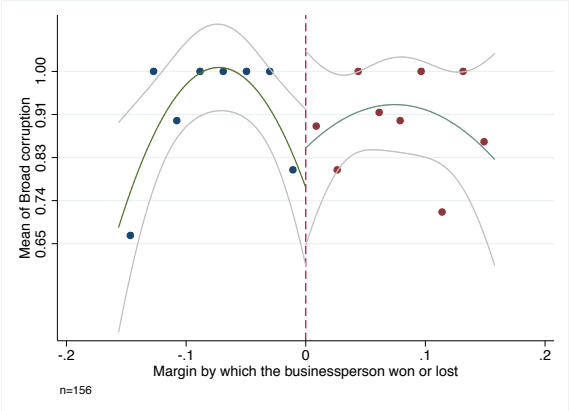
(g) Total active direct administration employees



(h) Total active indirect administration employees



(i) Broad corruption



The green line is a best fit second-order polynomial in the business-candidate's margin of victory for those observations which lie within the optimal bandwidth computed with the Calonico et al. (2014) method. The polynomial is split by a red dashed line which separates the two groups at the threshold $MV_{it} = 0$. Thus, the red points symbolise a business-mayor and the blue ones a non-business-mayor. The thin grey lines are the 95% confidence interval of the polynomial. See Table A1 for the definition of the variables.

Overall, the results from Table 3 support my conjecture that there is no unusually large discontinuity around the cut-off for most outcome variables. It is notable that none of the coefficients from the first two non-parametric models is significant. These results are robust to the inclusion of covariates in columns (3) and (4) and as anticipated, all standard errors shrink.[33] Despite everything, I do find significant coefficients for an impact on public finance when I use a second-order polynomial model. In this case, revenue per capita, expenditure per capita and current transfers per capita are estimated to increase by around 6% each while the debt-to-asset ratio is expected to decrease by 6.1%. Thus, these results from column (6) propose an effect on public finance when electing a businessperson. Nevertheless, it is important to understand that this evidence is very weak because the four results are not robust to different specifications, changes

[33]Further, I repeated the estimation with additional dummy variables for membership of a left, right or center political party. However, the significance levels did not change and also the coefficients' magnitudes and standard deviations have barely been affected. The information about the candidate's party (*Nome do partido*) come from the IBGE.

in the bandwidth nor to the inclusion of covariates. Moreover, one should keep in mind that the parametric models are less likely to identify an accurate treatment effect. Thus, without further investigation, I cannot conclude that electing a businessperson as mayor has a causal effect on any of the nine outcome variables.[34]

In the following I examine three possible reasons for this lack of robustness. First, the bandwidths that I computed with the CCT method have been partially very large and thus, contradict the basic idea of an RDD. For this reason, Table A5 in the Appendix reports results for both local regressions using three very small bandwidths: $h = 0.1$; $h = 0.05$ and $h = 0.02$. The results support my previous findings and indicate that the non-parametric estimations are robust to different bandwidths.

Second, another possible reason why *Revenue*, *Expenditure*, $Transfers$ and $DAratio$ only become significant when I use a second-order polynomial could be a misspecified functional form that generates an artificial small jump around the threshold and thus, leads to biased results. Table A6 in the Appendix reports the Akaike information criterion (AIC) for the whole sample. The results indicate that the best functional form for *Revenue*, *Expenditure* and $Transfers$ is a second-order polynomial, while $DAratio$ should be estimated using a linear model as in column (3).[35] Thus, I can also rule out this possible reason because at least the global functional form of the first three variables is correctly specified. Unfortunately, a comparison between a local and a global model using the AIC is not possible as the optimal form strongly depends on the bandwidth selection.[36]

Third, it is conceivable that the effect of electing a businessperson depends on the mayor's exact profession. Put differently, it might be that aggregating all business-professions to one group leads to inaccurate results because it ignores the possibility that business-professions might impact policy outcomes with different magnitudes and potentially also in opposed directions. In light of this concern, I repeat the RDD analysis separately for entrepreneurs (*Empresário*) and merchants (*Comerciante*) in the next section.[37]

5.2 Regression discontinuity results for entrepreneurs (*Empresário*) and merchants (*Comerciante*)

In Section 4.1 I explained that I aggregate candidates from five different business-related professions to the baseline business-group. However, I also noted, that this basically sums up *Empresários* and *Comerciantes*, which together account for 97.3% of all businesspersons. According

[34] I also considered the possibility that the variable for subsidies to firms might be a bad measure for the effect of electing a businessperson on subsidies as it only neglects an effect on the total number of subsidized sectors. Though, it might be that business-mayors only affect the *distribution* of subsidies between sectors while the total number subsidized sectors remains unchanged. This would go in line with the assumption that businesspersons have different preferences and closer ties the business sector. However, as Table A4 shows, this is not the case.

[35] Note, that the lowest value for the AIC always indicates the best functional form.

[36] As Lee and Lemieux (2010) explain, one possibility to compare these two kinds of models is by model saturation with dummies for bins and a test for the null-hypothesis that the corresponding coefficients are equal to zero. In this test, I would include higher order polynomials until the dummies for bins are jointly insignificant.

[37] An alternative way to improve the determination of the business-group would be to use the *ILO's International Standard Classification of Occupations*. However, as the information about the candidate's profession that I get from the *IBGE: Instituto Brasileiro de Geografia e Estatística* are in a very general manner and unfortunately, do not contain the ILO occupation codes (ISCO-88, ISCO-08), it would be very difficult to use this classification scheme for my study.

Table 3: Baseline results: The impact of electing a businessperson. RDD estimates.

	Non-parametric				Parametric		
Covariates	no	no	yes	yes	no	no	no
Polynomial order	1	2	1	2	1	2	3
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log *Revenue*	0.034	0.043	0.023	0.021	0.013	0.062**	0.042
	(0.038)	(0.048)	(0.024)	(0.033)	(0.021)	(0.026)	(0.033)
Bandwidth (h)	0.203	0.238	0.203	0.238	1	1	1
Observations	3,611	3,895	3,554	3,834	4,765	4,765	4,765
Log *Expenditure*	0.034	0.035	0.025	0.017	0.014	0.061**	0.041
	(0.037)	(0.049)	(0.023)	(0.033)	(0.021)	(0.026)	(0.033)
Bandwidth (h)	0.221	0.237	0.221	0.237	1	1	1
Observations	3,777	3,885	3,716	3,823	4,765	4,765	4,765
Log *Transfers*	0.034	0.043	0.017	0.025	0.008	0.058**	0.035
	(0.040)	(0.048)	(0.026)	(0.034)	(0.021)	(0.026)	(0.033)
Bandwidth (h)	0.192	0.246	0.192	0.246	1	1	1
Observations	3,522	3,943	3,463	3,875	4,765	4,765	4,765
RERatio	0.009	0.018	0.007	0.013	0.002	0.004	0.003
	(0.015)	(0.020)	(0.009)	(0.014)	(0.005)	(0.007)	(0.011)
Bandwidth (h)	0.236	0.315	0.236	0.315	1	1	1
Observations	3,879	4,220	3,816	4,151	4,766	4,766	4,766
DAratio	−0.059	−0.066	−0.036	−0.062	−0.026	−0.061*	−0.075
	(0.058)	(0.062)	(0.045)	(0.054)	(0.026)	(0.035)	(0.046)
Bandwidth (h)	0.186	0.317	0.186	0.317	1	1	1
Observations	3,456	4,223	3,404	4,154	4,766	4,766	4,766
Subsidies	−0.007	−0.012	−0.020	0.030	0.020	−0.000	0.021
	(0.109)	(0.130)	(0.084)	(0.106)	(0.049)	(0.066)	(0.084)
Bandwidth (h)	0.163	0.229	0.163	0.229	1	1	1
Observations	1,739	2,080	1,706	2,034	2,586	2,586	2,586
StDirect	0.582	0.487	0.660	−0.109	0.163	1.587	1.425
	(1.202)	(1.803)	(1.109)	(1.456)	(0.843)	(1.002)	(1.202)
Bandwidth (h)	0.180	0.233	0.180	0.233	1	1	1
Observations	3,408	3,864	3,354	3,803	4,763	4,763	4,763
StIndirect	−0.221	0.257	−0.723	−0.514	−0.305	−0.243	−0.310
	(0.507)	(0.569)	(0.446)	(0.545)	(0.277)	(0.338)	(0.396)
Bandwidth (h)	0.151	0.204	0.151	0.204	1	1	1
Observations	755	884	748	886	1,173	1,173	1,173
Broad	0.049	0.148	−0.011	0.133	−0.062	−0.030	0.124
	(0.175)	(0.235)	(0.114)	(0.154)	(0.057)	(0.086)	(0.123)
Bandwidth (h)	0.138	0.158	0.138	0.158	1	1	1
Observations	143	156	135	149	222	222	222

The bandwidths are calculated with the CCT method and observations are weighted with a triangular kernel. For a description of all variables, see table A1. In columns (1) and (2) I used robust standard errors and in columns (3)-(7) standard errors are clustered standard errors at the municipality level. Standard errors are in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

to the Portuguese dictionary Ferreira (2009), an *Empresário* can be defined as businessman or entrepreneur, while *Comerciante* corresponds to a merchant, dealer or business owner. Hence, one can picture an *Empresário* as "a man [or woman] who works in business, especially one who has a high position in a company", while a *Comerciante* is someone who trades goods on a rather small scale.[38]

In Section 2 I pointed out that businesspersons are assumed to have an advantage due to their managing experience and their alleged better understanding of economic interrelations. With regard to the second point, it might be questionable if the group of merchants really fits this characterization. Even though, they may have strong bargaining skills, owning a business or shop does not necessarily require any specific formal education.[39] Moreover, the daily operations of a merchant are associated with the purchase and sale of goods rather than with corporate finance or complex management decisions. For this reason, it is save to assume that the two groups are quite different and thus, also their effect on policy outcomes might be differing.

In the following, I repeat my baseline estimation for both business-groups separately. First, I restrict my dataset to 2,557 mayoral elections where the two candidates with the highest vote-shares were exactly one entrepreneur and one non-businessperson (which now also includes merchants (*Comerciante*), business directors (*Diretor De Empresas*), businessmen and producers of public events (*Empresário E Produtor De Espetáculos Públicos*) and managers (*Gerente*)). As in the baseline case, I first check the two main requirements that validate the use of an RDD to analyse the alleged effect of electing an entrepreneur. Figure A1a in the Appendix visualises the McCrary test and Table A7 reports balance tests for the control variables. Similar to the baseline case, the distribution of the running variable (MV_{it}) seems to be continuous and except for *Education* and *Gender* all covariates behave smoothly around the threshold.

As before, I begin with my RD analysis with a graphical representation. Figure A2 in the Appendix shows scatterplots of the relationship between the outcome variables and the entrepreneur's margin of victory around the threshold. This time, I observe a relatively clear jump in the total number of active direct administration employees (*StDirect*) as well as several little jumps (i.e. in *Expenditure*, $Transfers$ or *DAratio*). However, it is not clear whether these smaller discontinuities are "real jumps" or only due to a higher degree of variation in the data-points.[40]

To investigate this issue, Table 4 reports results from four non-parametric and three parametric estimations. This time, I observe strong evidence for a discontinuity in the logarithm of total revenue, total expenditure, current transfers, and the total active direct administration employees. The results are robust to different bandwidths, different specifications of the functional form, the inclusion of covariates and a global estimation. Hence, I can conclude that electing an entrepreneur leads to 7-11% higher total revenue, 8-11% higher total expenditure, 7-13% higher current transfers and between three and five additional active direct administration employees per 1,000 inhabitants. However, it is important to keep in mind that I previously ignored the

[38] This characterization is mainly based on the definition of "businessman" and "merchant" from the *Cambridge Dictionary*.

[39] In fact, in my sample, merchants have an average education of 13.48 years, which is 1.03 years shorter than the average for entrepreneurs.

[40] Please note, that I excluded the broad measure for corruption (*Broad*) in this estimation as it would only be based on at most 49 observations in the two local regressions.

Table 4: Sub-sample: The impact of electing an entrepreneur. RDD estimates.

	Non-parametric				Parametric		
Covariates	no	no	yes	yes	no	no	no
Polynomial order	1	2	1	2	1	2	3
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log *Revenue*	0.109**	0.088	0.070**	0.082*	0.029	0.093**	0.105**
	(0.055)	(0.067)	(0.035)	(0.045)	(0.028)	(0.037)	(0.046)
Bandwidth (h)	0.186	0.235	0.186	0.235	1	1	1
Observations	1,819	2,052	1,800	2,031	2,526	2,526	2,526
Log *Expenditure*	0.109**	0.098	0.086**	0.083*	0.035	0.095***	0.108**
	(0.053)	(0.068)	(0.034)	(0.045)	(0.028)	(0.037)	(0.046)
Bandwidth (h)	0.214	0.246	0.214	0.246	1	1	1
Observations	1,962	2,097	1,941	2,074	2,526	2,526	2,526
Log *Transfers*	0.119**	0.127**	0.070**	0.104**	0.027	0.086**	0.113**
	(0.054)	(0.064)	(0.035)	(0.045)	(0.028)	(0.037)	(0.047)
Bandwidth (h)	0.207	0.288	0.207	0.288	1	1	1
Observations	1,921	2,182	1,899	2,159	2,526	2,526	2,526
RERatio	−0.006	−0.009	−0.003	−0.005	−0.009*	−0.003	−0.002
	(0.009)	(0.011)	(0.007)	(0.008)	(0.005)	(0.005)	(0.007)
Bandwidth (h)	0.129	0.185	0.129	0.185	1	1	1
Observations	1,432	1,817	1,413	1,798	2,527	2,527	2,527
DAratio	−0.095	−0.100	−0.064	−0.089	−0.026	−0.060	−0.100
	(0.104)	(0.112)	(0.075)	(0.091)	(0.039)	(0.056)	(0.077)
Bandwidth (h)	0.191	0.316	0.191	0.316	1	1	1
Observations	1,849	2,233	1,827	2,209	2,527	2,527	2,527
Subsidies	0.076	0.132	0.024	0.067	0.039	0.014	0.057
	(0.141)	(0.159)	(0.108)	(0.133)	(0.064)	(0.087)	(0.109)
Bandwidth (h)	0.171	0.252	0.171	0.252	1	1	1
Observations	1,080	1,304	1,062	1,283	1,560	1,560	1,560
StDirect	4.500**	4.348*	3.260**	3.665*	1.311	2.657*	4.507***
	(2.025)	(2.423)	(1.516)	(1.919)	(1.144)	(1.414)	(1.688)
Bandwidth (h)	0.174	0.240	0.174	0.240	1	1	1
Observations	1,752	2,077	1,732	2,054	2,526	2,526	2,526
StIndirect	0.374	0.397	−0.095	0.043	−0.269	0.141	0.254
	(0.506)	(0.554)	(0.434)	(0.455)	(0.305)	(0.365)	(0.394)
Bandwidth (h)	0.150	0.254	0.150	0.254	1	1	1
Observations	454	607	453	603	726	726	726

The bandwidths are calculated with the CCT method and observations are weighted with a triangular kernel. For a description of all variables, see Table A1. In columns (1) and (2) I used robust standard errors and in columns (3)-(7) standard errors are clustered standard errors at the municipality level. Standard errors are in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

two negative jumps in the covariates for *Education* and *Gender* and thus, the estimated magnitudes might not reflect the "pure" effect of business experience. In fact, as both *Education* and *Gender* are assumed to benefit policy outcomes (see Brollo and Troiano (2016) and Besley and Coate (1997)), the results should be regarded rather as the lower bound of the "pure" business-effect.

Moreover, it is notable, that these findings strongly differ from those in the baseline case suggesting that there might be a heterogeneous treatment effect of electing a businessperson. In order to provide additional evidence for this presumption, I also repeat the estimation for the second sub-group. Now, I examine the 2,568 mayoral elections where the two candidates with the highest vote-shares were exactly one merchant and one non-merchant. Again, I can confirm the validity of the RDD because first, the McCrary test in Figure A1b in the Appendix does not show any evidence for a non-random assignment and second, Table A7 indicates that (as before) *Education* and *Gender* are the only discontinuous covariates.

Once more, I start with a graphical analysis of the relationship between the outcome variables and the running variable. Regarding this, Figure A3 in the Appendix does not give cause to assume a non-continuous behaviour for any of the eight outcome variables. As before, the best-fit second-order polynomial suggests some smaller jumps but as there is a wide dispersion in data-points it is not save to conclude that one of them is a "real discontinuity".[41]

On account of this, it is again advisable to focus on outcomes from a regression analysis. Table 5 presents seven RDD models. The results are robust to different specifications of the functional form, the inclusion of covariates and do not change when using a parametric approach. Nevertheless, the most notable aspect here is that I do not observe any evidence for an effect on public finance or public employment when a merchant gets elected. This stands in sharp contrast to the previous findings from the entrepreneurs-sample and thus, indicates that the effect of business experience in general is heterogeneous and strongly depends on the legislator's exact profession. In view of this, I infer that only those businesspersons who have the ability to carry out management tasks in bigger organisations are able to impact a municipality's policy outcomes.

5.3 Discussion of the results

In this last section, I want to interpret these results in view of the findings from the previous literature. As pointed out in Section 2, businesspersons are expected to differ from other politicians in two dimensions. First, they are assumed to have a higher ability to lead a government and improve public finance. In my study, I find that this is only true for the sub-group of entrepreneurs who are able to attract more transfers from higher levels of the government and therefore, boost revenues.[42] Considering the before mentioned main differences between merchants and entrepreneurs, I conclude that the main reason for this are the advanced negotiating

[41] Note, that also in this second sub-sample, I dropped *Broad* due to an insufficient amount of observations.

[42] Regarding this, it should be stated that the municipalities' total revenues originates to 92.19% from transfers, 6.43% from tax revenues and to 1.38% from other income sources. Using data from the *FINBRA* dataset I also tested whether electing an entrepreneur affects tax revenues (*Receita Tributária*) per capita but my results did not suggest a significant effect. The estimated coefficients were again robust to different specifications of the functional form, the inclusion of covariates and under parametric models.

Table 5: Sub-sample: The impact of electing a merchant. RDD estimates.

	Non-parametric				Parametric		
Covariates	no	no	yes	yes	no	no	no
Polynomial order	1	2	1	2	1	2	3
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log *Revenue*	−0.030	−0.019	−0.015	−0.015	−0.000	0.028	−0.019
	(0.050)	(0.062)	(0.031)	(0.040)	(0.029)	(0.035)	(0.044)
Bandwidth (h)	0.204	0.286	0.204	0.286	1	1	1
Observations	1,935	2,214	1,898	2,172	2,549	2,549	2,549
Log *Expenditure*	−0.036	−0.040	−0.023	−0.018	−0.006	0.023	−0.022
	(0.049)	(0.058)	(0.031)	(0.039)	(0.029)	(0.035)	(0.044)
Bandwidth (h)	0.201	0.304	0.201	0.304	1	1	1
Observations	1,926	2,247	1,889	2,204	2,549	2,549	2,549
Log $Transfers$	−0.046	−0.032	−0.024	−0.024	−0.008	0.024	−0.038
	(0.050)	(0.061)	(0.033)	(0.042)	(0.029)	(0.035)	(0.044)
Bandwidth (h)	0.192	0.276	0.192	0.276	1	1	1
Observations	1,878	2,188	1,842	2,146	2,549	2,549	2,549
RERatio	0.012	0.038	0.006	0.024	0.014	0.012	0.008
	(0.028)	(0.042)	(0.015)	(0.025)	(0.009)	(0.013)	(0.020)
Bandwidth (h)	0.267	0.308	0.267	0.308	1	1	1
Observations	2,166	2,250	2,123	2,207	2,549	2,549	2,549
DAratio	−0.058	−0.071	−0.010	0.004	−0.036	−0.063	−0.049
	(0.057)	(0.066)	(0.045)	(0.057)	(0.035)	(0.039)	(0.048)
Bandwidth (h)	0.144	0.230	0.144	0.230	1	1	1
Observations	1,588	2,051	1,556	2,012	2,549	2,549	2,549
Subsidies	−0.032	−0.156	0.038	0.020	0.015	0.036	0.042
	(0.139)	(0.180)	(0.112)	(0.156)	(0.068)	(0.087)	(0.107)
Bandwidth (h)	0.156	0.190	0.156	0.190	1	1	1
Observations	777	877	760	858	1,201	1,201	1,201
StDirect	−3.185	−3.434	−2.281	−2.241	−0.943	−0.360	−1.380
	(2.206)	(2.462)	(1.651)	(1.918)	(1.098)	(1.279)	(1.599)
Bandwidth (h)	0.142	0.247	0.142	0.247	1	1	1
Observations	1,568	2,098	1,542	2,059	2,547	2,547	2,547
StIndirect	−0.564	0.201	−1.031	0.162	−0.096	−0.565	−0.897
	(0.716)	(0.799)	(0.881)	(0.947)	(0.418)	(0.453)	(0.629)
Bandwidth (h)	0.132	0.159	0.132	0.159	1	1	1
Observations	326	363	322	360	541	541	541

The bandwidths are calculated with the CCT method and observations are weighted with a triangular kernel. For a description of all variables, see Table A1. In columns (1) and (2) I used robust standard errors and in columns (3)-(7) standard errors are clustered standard errors at the municipality level. Standard errors are in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

and bargaining skills of entrepreneurs. Moreover, it is notable that neither the ratio between revenue and expenditure nor the debt-to-asset ratio changes. This in turn suggests that entrepreneurs dispose of distinctive economic knowledge that helps them to find an effective way to first raise public revenue and then increase expenditure with a reasonable magnitude and without accumulating additional debts.
Second, it is assumed that businesspersons have specific preferences and thus, they could be more willing to support business-related issues. Unfortunately, I do not have information about voting decisions or concrete policies that the mayors implemented during their legislation. Therefore, I cannot answer this question confidently. Notwithstanding, I tested whether businesspersons, entrepreneurs or merchants are more willing to pay subsidies to firms. Regarding this, I did not find any evidence that would suggest that business-mayors are more receptive to business-interests. Moreover, also my analysis of broad corruption did not suggest that mayors with a business-background are more or less willing to take bribes.

6 Conclusion

It is widely believed that personal characteristics, such as age, gender, education or the profession impact a legislator's preferences and his/her general ability to govern. Although most bibliographic characteristics have already been extensively investigated by previous studies, the effect of professional business-experience has received relatively little attention so far. The purpose of this paper is to contribute to closing this gap and therefore, study whether the election of a candidate with a background in business has an effect on policy outcomes. The group of businesspersons is one of the best represented professional groups in policies and it seems to be a common assumption that businesspersons have a series of beneficial skills, such as negotiation experience, managing expertise or a higher ability to improve public finance.
In order to test whether these assumptions are true, I examined 4,813 mayoral elections in Brazilian municipalities in 2004, 2008 and 2012 and studied changes in public finance, public employment, subsidized firms and corruption. For my baseline analysis, I applied a relatively broad definition of a businessperson and used a regression discontinuity design to estimate the average treatment effect of electing a business-mayor. In light of this, I assumed that candidates are very similar in sufficiently close races and thus, the selection of the mayor is as good as random. I provided both a graphical analysis and additional formal evidence from four non-parametric and three parametric regressions. However, the results turned out to be mostly insignificant and non-robust. Therefore, I decided to divide the baseline business-group into the two sub-samples entrepreneurs and merchants, which together account for 97.3% of all previously defined business-candidates. Repeating the analysis for these two groups generated robust results, suggesting that the baseline estimation has been biased due to heterogeneity in the treatment effect.
All in all, I draw three conclusions from my study. First, I do not find any evidence for the existence of a general effect of business experience and thus, my results oppose the common assumption that business experience per se matters for a legislator's ability to govern. Second, having specific knowledge related to trade does not impact policy outcomes either. Third, only the election of a politician who has relevant managing expertise eventually leads to an

improvement of the municipality's financial condition. To be more precise, better negotiating and bargaining skills allow this kind of politicians to attract 7-13% more transfers per capita from higher government levels, which in turn leads to 7-11% higher total revenues per capita. As a consequence, the mayor is able to hire more public servants and increase total expenditure per capita by 8-11% while keeping both the revenue-expenditure-ratio and the debt-to-assets ratio constant.

Overall, my findings raise a number of additional questions for future research. First, all my results rely on the assumption that there is a direct relationship between a candidate's profession and his/her educational level and gender, albeit the later cannot be regarded as causal. Although I provided some arguments that support this view, it would be interesting to formally test this assumption (i.e. with a matching procedure) and thereby examine if my results really reflect the effect of business experience or if they are mostly driven by educational differences and potentially a lower share of women.

Second, I do not have any information about the extent of the candidates' business expertise and thus, I cannot distinguish between candidates who have a decade-long experience in this sector and those, who maybe only had a brief insight into the business-world.

Third, and related to the previous point, I do not control for previous political experience. Concerning this, Witko and Friedman (2008) found a high degree of heterogeneity within the group of businesspeople in their study on Congress members. For example, they show that especially "members making a direct transition from a business career to the House sponsor more business-focused legislation".

Fourth, I assume that entrepreneurs have both a better economic understanding as well as practical management experience. This assumption should also be investigated as it could be that some candidates categorized themselves as entrepreneurs although they actually do not match this characterization. In other words, it would be useful to also gather information about the focus of the candidates' education (i.e. specialization in school, subject of university studies, etc.) and maybe also the exact position(s) in their professional career.

Fifth, it is important to keep in mind that there are significant differences between responsibilities or power of a mayor and those of a senator, minister or president. Therefore, I recommend future studies to test to what extend my findings change on other government levels.

Finally, it is unclear whether my results would persist in other countries. As The Economist (2009) pointed out, different countries tend to favour different professions. Due to their historical background, cultural preferences or stage of development, they seem to like particular qualities and these qualities are provided disproportionately by only a few professions. Thus, it could be that the effect might decrease or increase depending on the country's general perception of a businessperson.

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A Appendix

Table A1: Overview over all variables.

1. Running Variable	
MV	Margin of victory (= share of votes that the candidate received minus the vote-share of the winning candidate).[1]
2. Mayoral characteristics	
Gender	Dummy which is 1 if the mayor is *female* and zero if she is *male*.[2]
Age	Age of the mayor (in 2005, 2009 or 2013).[2]
Education	Years of education of the mayor: 4.5 (*Ensino fundamental (1º grau) incompleto*), 9 (*Ensino fundamental (1º grau) completo*), 10.5 (*Ensino médio (2º grau) incompleto*), 12 (*Ensino médio (2º grau) completo*), 15 (*Ensino superior incompleto*), 18 (*Ensino superior completo*), 22 (*Pós-graduação*) or empty (*não informado*).[2,4]
Married	Dummy which is 1 if the mayor is married (*casado(a)*) and zero otherwise.[1]
Origin	Dummy which is 1 if the mayor is born in the same municipality and zero otherwise.[1]
3. Municipality and state characteristics	
Population	Total population in the previous year in each legislative period (2003, 2007, 2011).[3]
Income	Average household income per capita in each state (in R$).[4]
Unemployment	Unemployment rate in each state.[4]
GINI	Gini coefficient (measure for income-inequality) in each state.[4]
Av.Study	Average years of study (persons 25 years and older) in each state.[4]
4. Public finance	
Revenue	Total revenue per capita (in R$).[3]
Expenditure	Total expenditure per capita (in R$).[3]
Transfers	Current transfers per capita (in R$).[3]
REratio	Ratio between revenue and expenditure.[3]
DAratio	Ratio between total debts and total assets.[3]
5. Subsidies to firms	
Subsidies	Sum of sectors that have been subsidized in the last two years (0 min, 5 max).[2]
SubIndustrial	Dummy which is equal to 1 if the municipality provided subsidies to firms in the "Industrial sector" in the last two years.[2]
SubServices	Dummy which is equal to 1 if the municipality provided subsidies to firms in the "Commercial and Services sector" in the last two years.[2]
SubTSL	Dummy which is equal to 1 if the municipality provided subsidies to firms in the "Tourism, sport and leisure sector" in the last two years.[2]
SubAgriculture	Dummy which is equal to 1 if the municipality provided subsidies to firms in the "Agriculture sector" in the last two years.[2]
SubOthers	Dummy which is equal to 1 if the municipality provided subsidies to firms in other sectors in the last two years.[2]

Table A1 continues on the next page.

Table A1: Overview over all variables (continued).

6. Public employment	
StDirect	Total active direct administration employees per 1,000 inhabitants.[2]
StIndirect	Total active indirect administration employees per 1,000 inhabitants.[2]
7. Corruption	
Broad	Dummy which is equal to 1 if at least one (broad) corruption episode is reported.[6]
8. Identification variables	
B	Dummy which is equal to 1 if the mayor is a businessperson.[1]
Business	Dummy which is equal to 1 if the candidate is a businessperson.[1]

Note, that except for the state characteristics, all information refers to municipalities. Further, all variables referring to "Municipality and state characteristics", "Public finance", "Subsidies to firms", "Public employment" and "Corruption" are averaged over each period.

[1] *Source*: Tribunal Superior Eleitoral.
[2] *Source*: IBGE : Instituto Brasileiro de Geografia e Estatística.
[3] *Source*: Tesouro Nacional - Ministério da Fazenda.
[4] *Source*: IPEA: Instituto de Pesquisa Econômica Aplicada.
[5] For the number of years per educational level I used information from Stanek (2013). If the degree was not completed, I accounted for 50% of the years that are required. For the higher education, I took the average for a Bachelor and Master degree.
[6] *Source*: Brollo et al. (2013).

Table A2: Available control and outcome variables per year.

Variables	Period 2004				Period 2008				Period 2012			
	04	05	06	07	08	09	10	11	12	13	14	15
Mayor												
Gender	-	yes	-	-	-	yes	-	-	-	yes	-	-
Age	-	yes	-	-	-	yes	-	-	-	yes	-	-
Education	-	yes	-	-	-	yes	-	-	-	yes	-	-
Married	yes	-	-	-	yes	-	-	-	yes	-	-	-
Origin	yes	-	-	-	yes	-	-	-	yes	-	-	-
Municipality and state												
Population[1]	yes	yes	yes	yes	yes	yes	yes	yes	yes	-	-	-
Income	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	-
Unemployment	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	-
GINI	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	-
Av.Study	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	-
Public Finance												
Revenue	yes	yes	yes	yes	yes	yes	yes	yes	yes	-	-	-
Expenditure	yes	yes	yes	yes	yes	yes	yes	yes	yes	-	-	-
Transfers	yes	yes	yes	yes	yes	yes	yes	yes	yes	-	-	-
REratio	yes	yes	yes	yes	yes	yes	yes	yes	yes	-	-	-
DAratio	yes	yes	yes	yes	yes	yes	yes	yes	yes	-	-	-
Subsidies to firms												
Subsidies	-	-	yes	-	-	yes	-	-	-	-	-	yes
Subsidies by sector[2]	-	-	yes	-	-	yes	-	-	-	-	-	yes
Public employment												
StDirect	yes	yes	yes	-	yes	yes	-	yes	yes	yes	yes	yes
StIndirect	yes	yes	yes	-	yes	yes	-	yes	yes	yes	yes	yes
Corruption												
Broad	yes				-				-			

[1] Note, that I also have information about *Population* for 2003.
[2] The five sectors are: (1) Industrial; (2) commercial services; (3) tourism, sport and leisure, (4) agriculture and (5) others.

Table A3: RDD estimates with two placebo thresholds **(a)** $c' = -0.1$ and **(b)** $c'' = 0.1$.

		(a) Placebo threshold $c' = -0.1$				**(b)** Placebo threshold $c'' = 0.1$			
	Poly.	B_{it}	Std.	CCT	Obs.	B_{it}	Std.	CCT	Obs.
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log *Revenue*	1	−0.011	(0.044)	0.220	3,437	0.041	(0.049)	0.169	2,830
	2	−0.045	(0.058)	0.237	3,611	0.080	(0.060)	0.224	3,434
Log *Expenditure*	1	−0.024	(0.047)	0.194	3,177	0.034	(0.050)	0.169	2,833
	2	−0.049	(0.058)	0.239	3,627	0.065	(0.059)	0.237	3,555
Log *Transfers*	1	−0.042	(0.049)	0.174	2,945	0.023	(0.047)	0.189	3,086
	2	−0.073	(0.061)	0.220	3,432	0.040	(0.054)	0.289	3,946
REratio	1	0.005	(0.008)	0.107	1,898	0.005	(0.008)	0.128	2,241
	2	0.005	(0.009)	0.194	3,183	−0.007	(0.016)	0.135	2,362
DAratio	1	−0.048	(0.075)	0.280	3,937	−0.053	(0.056)	0.120	2,120
	2	−0.139	(0.125)	0.263	3,811	−0.056	(0.062)	0.222	3,418
Subsidies	1	−0.091	(0.134)	0.142	1,349	−0.092	(0.120)	0.169	1,511
	2	−0.076	(0.167)	0.203	1,772	−0.114	(0.151)	0.221	1,828
StDirect	1	0.323	(1.688)	0.182	3,043	0.004	(1.578)	0.177	2,955
	2	−0.556	(2.242)	0.200	3,246	1.500	(2.023)	0.195	3,154
StIndirect	1	0.449	(0.694)	0.202	789	1.631*	(0.961)	0.181	738
	2	0.419	(0.898)	0.248	908	1.691*	(1.003)	0.304	984
Broad	1	0.206	(0.138)	0.135	110	−0.067	(0.131)	0.141	124
	2	0.123	(0.217)	0.122	96	−0.071	(0.144)	0.219	171

The table presents the results of two local polynomial regressions without covariates. For my estimations I calculated the optimal bandwidth with the Calonico et al. (2014) method and where observations are weighted with a triangular kernel. *Poly.* is the order of the polynom, B_{it} is the treatment effect, *Std.* is the standard deviation, *CCT* is the bandwidth calculated with the Calonico et al. (2014) method and *Obs.* is the number of observations. For a description of all variables, see Table A1. Robust standard errors in parenthesis. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A4: Baseline results: RDD results for subsidies to firms in five sectors.

	Non-parametric				Parametric		
Covariates	no	no	yes	yes	no	no	no
Polynomial order	1	2	1	2	1	2	3
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SubIndustrial	0.085	0.093	0.026	0.042	−0.005	0.012	0.029
	(0.054)	(0.060)	(0.041)	(0.049)	(0.025)	(0.031)	(0.040)
Bandwidth (h)	0.141	0.233	0.141	0.233	1	1	1
Observations	1,579	2,096	1,543	2,052	2,531	2,531	2,531
SubServices	0.031	0.026	0.041	0.056	0.016	0.003	0.054
	(0.057)	(0.064)	(0.047)	(0.057)	(0.027)	(0.035)	(0.045)
Bandwidth (h)	0.158	0.249	0.158	0.249	1	1	1
Observations	1,706	2,142	1,670	2,097	2,586	2,586	2,586
SubTSL	−0.017	−0.003	−0.005	−0.008	0.009	0.001	−0.011
	(0.040)	(0.051)	(0.031)	(0.043)	(0.019)	(0.026)	(0.033)
Bandwidth (h)	0.191	0.233	0.191	0.233	1	1	1
Observations	1,908	2,094	1,865	2,052	2,586	2,586	2,586
SubAgriculture	−0.043	−0.055	−0.025	−0.028	−0.017	−0.012	−0.018
	(0.045)	(0.053)	(0.037)	(0.045)	(0.020)	(0.027)	(0.035)
Bandwidth (h)	0.160	0.228	0.160	0.228	1	1	1
Observations	1,721	2,076	1,682	2,032	2,586	2,586	2,586
SubOthers	−0.060	−0.059	−0.037	−0.039	0.017	−0.005	−0.032
	(0.042)	(0.046)	(0.035)	(0.039)	(0.020)	(0.025)	(0.032)
Bandwidth (h)	0.139	0.241	0.139	0.241	1	1	1
Observations	1,567	2,124	1,531	2,080	2,586	2,586	2,586

The bandwidths are calculated with the CCT method and observations are weighted with a triangular kernel. For a description of all variables, see table A1. In columns (1) and (2) I used robust standard errors and in columns (3)-(7) standard errors are clustered standard errors at the municipality level. Standard errors are in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A5: Baseline results: RDD results with different bandwidths.

	Local linear			Local 2^{nd}-order polynomial		
Bandwidth (h)	$h = 0.1$	$h = 0.05$	$h = 0.02$	$h = 0.1$	$h = 0.05$	$h = 0.02$
Column	(1)	(2)	(3)	(4)	(5)	(6)
Log *Revenue*	0.067	0.006	0.053	0.048	0.020	0.236
	(0.064)	(0.089)	(0.139)	(0.083)	(0.116)	(0.193)
Observations	2,283	1,197	482	2,283	1,197	482
Log *Expenditure*	0.049	−0.024	−0.024	0.021	−0.03	0.109
	(0.065)	(0.089)	(0.137)	(0.083)	(0.115)	0.189
Observations	2,283	1,197	482	2,283	1,197	482
Log *Transfers*	0.070	−0.013	0.033	0.042	−0.019	0.199
	(0.064)	(0.089)	(0.138)	(0.083)	(0.117)	(0.191)
Observations	2,283	1,197	482	2,283	1,197	482
REratio	0.043	0.083	0.212	0.069	0.142	0.326
	(0.045)	(0.088)	(0.214)	(0.073)	(0.148)	(0.315)
Observations	2,283	1,197	482	2,283	1,197	482
DAratio	−0.132	−0.105	−0.096	−0.116	−0.063	−0.072
	(0.081)	(0.087)	(0.133)	(0.084)	(0.137)	(0.165)
Observations	2,284	1,198	482	2,284	1,198	482
Subsidies	−0.044	−0.046	−0.062	−0.023	−0.126	−0.005
	(0.165)	(0.239)	(0.402)	(0.220)	(0.323)	(0.558)
Observations	1,235	440	265	1,235	440	265
StDirect	1.461	−1.423	1.799	0.506	−2.253	8.635
	(2.412)	(3.610)	(6.787)	(3.283)	(5.238)	(10.094)
Observations	2,283	1,197	482	2,283	1,197	482
StIndirect	0.679	0.286	−1.965*	0.750	−0.241	−1.184
	(0.646)	(0.713)	(1.125)	(0.719)	(0.883)	(1.244)
Observations	554	289	118	554	289	118
Broad	0.148	0.273	0.397	0.217	0.092	0.809
	(0.270)	(0.414)	(0.809)	(0.385)	(0.535)	(1.475)
Observations	116	61	25	116	61	25

The table presents results for two non-parametric models without covariates. The observations are weighted with a triangular kernel. For a description of all variables, see Table A1. Robust standard errors are in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A6: Baseline results: Akaike information criterion (AIC).

	Linear	2^{nd}-order polynomial	3^{rd}-order polynomial	Best polynoial
Column	(1)	(2)	(3)	(4)
Log *Revenue*	7,093.267	7,085.399	7,088.029	2
Log *Expenditure*	7,095.281	7,087.318	7,090.107	2
Log $Transfers$	7,078.124	7,067.493	7,068.668	2
REratio	$-4,743.219$	$-4,740.462$	$-4,736.78$	1
DAratio	9,097.597	9,097.707	9,100.753	1
Subsidies	6,922.312	6,925.273	6,929.082	1
StDirect	41,248.94	41,231.88	41,232.72	2
StIndirect	6,301.6	6,305.309	6,308.65	1
Broad	117.6911	120.4543	118.8257	1

The table presents the Akaike information criterion (AIC) for three different parametric specifications: linear, second-order polynomial and third-order polynomial regression. For a description of all variables, see Table A1.

Table A7: Sub-sample: The impact of electing an entrepreneur and a merchant on covariates. RDD estimates.

		(a) Entrepreneurs				**(b)** Merchants			
	Poly.	Effect	Std.	CCT	Obs.	Effect	Std.	CCT	Obs.
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Age	1	0.125	(1.224)	0.161	1,694	−1.000	(1.004)	0.169	1,766
	2	−0.388	(1.547)	0.197	1,897	−1.125	(1.263)	0.220	2,013
Education	1	−1.454***	(0.474)	0.164	1,712	−2.447***	(0.415)	0.237	2,083
	2	−1.458***	(0.505)	0.298	2,227	−2.152***	(0.580)	0.233	2,073
Gender	1	−0.063**	(0.027)	0.158	1,675	−0.094***	(0.032)	0.166	1,740
	2	−0.074**	(0.031)	0.218	2,007	−0.097**	(0.038)	0.237	2,086
Married	1	0.030	(0.048)	0.171	1,763	0.056	(0.045)	0.143	1,591
	2	0.030	(0.052)	0.300	2,233	0.059	(0.047)	0.268	2,183
Origin	1	0.008	(0.049)	0.239	2,100	−0.030	(0.056)	0.151	1,637
	2	0.102	(0.076)	0.185	1,841	−0.058	(0.073)	0.189	1,873
Population	1	−9032.7	(6266.4)	0.133	1,452	6491*	3305.6	0.173	1,747
	2	−6067.7	(7167.4)	0.200	1,883	5979.1	3788.3	0.243	2,052
Income	1	32.848	(33.74)	0.167	1,733	16.003	26.547	0.174	1,793
	2	28.188	(40.511)	0.226	2,042	37.786	32.227	0.242	2,096
GINI	1	−0.004	(0.003)	0.248	2,134	−0.001	(0.004)	0.159	1,692
	2	−0.000	(0.005)	0.221	2,020	−0.002	(0.004)	0.258	2,154
Unemployment	1	0.043	(0.263)	0.159	1,685	−0.001	(0.224)	0.187	1,860
	2	0.208	(0.332)	0.192	1,878	−0.542*	(0.307)	0.174	1,795
Av.Study	1	0.002	(0.093)	0.173	1,769	0.024	(0.088)	0.158	1,691
	2	−0.010	(0.116)	0.215	1,994	0.034	(0.099)	0.257	2,152

The table shows the results from two non-parametric regressions. *Poly.* is the order of the polynom, *Effect* is the treatment effect, *Std.* is the standard deviation, *CCT* is the bandwidth calculated with the Calonico et al. (2014) method and *Obs.* is the number of observations. For a description of all variables, see Table A1. Robust standard errors in parenthesis. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Figure A1: Sub-sample: McCrary test for entrepreneurs and merchants.

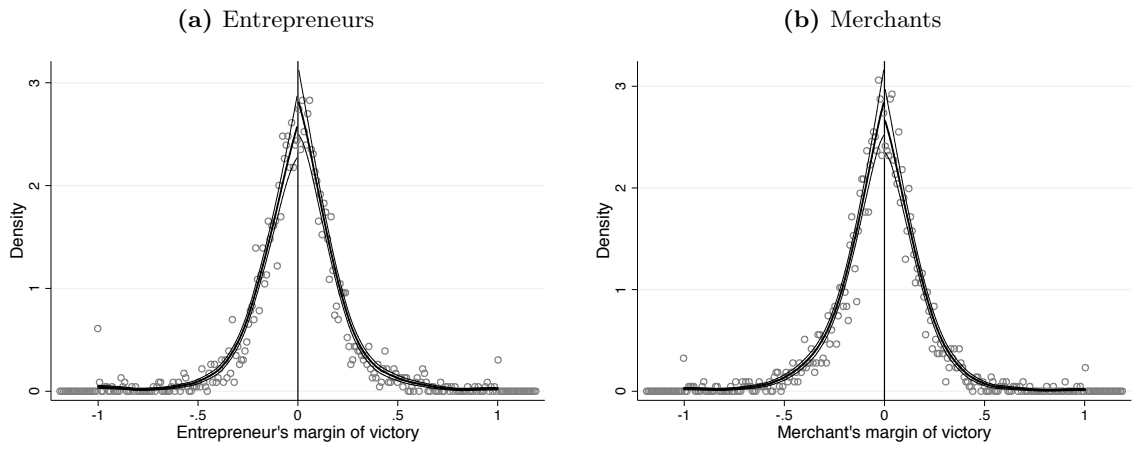
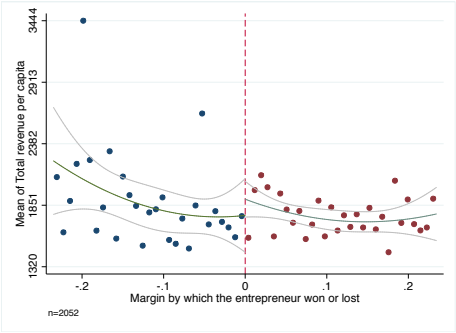
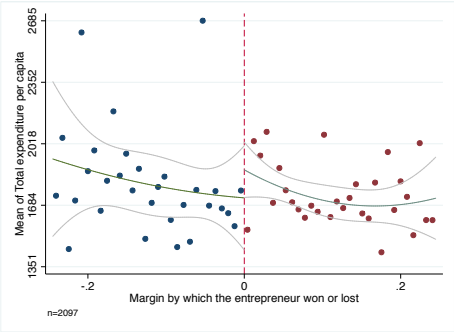


Figure A2: Sub-sample: The impact of electing an entrepreneur. Graphical analysis.

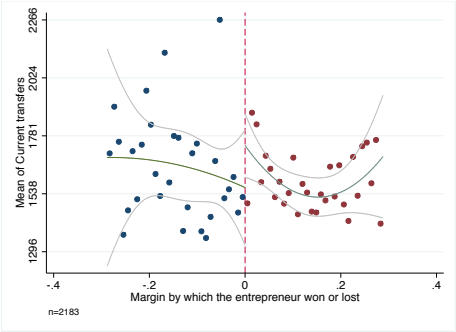
(a) Total revenue



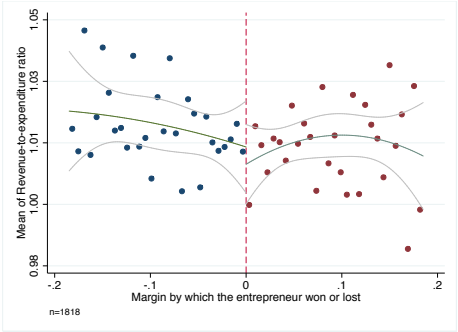
(b) Total expenditure



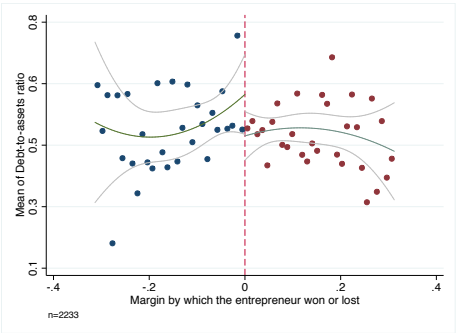
(c) Current transfers



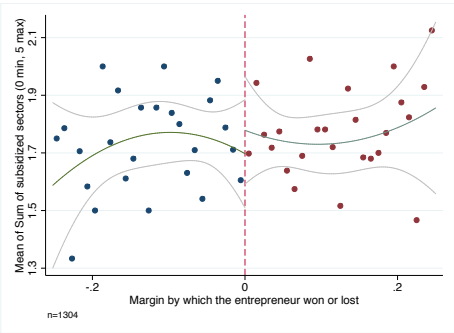
(d) Ratio between revenue and expenditure



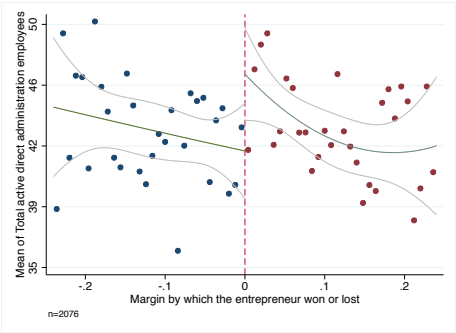
(e) Debt-to-asset ratio



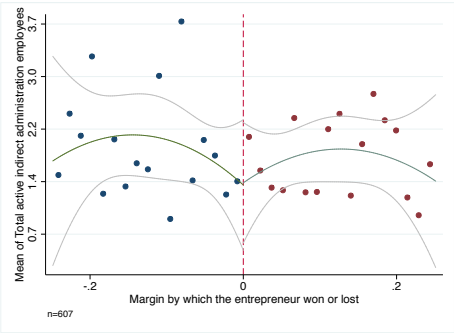
(f) Sum of subsidized sectors within the last two years (0 min, 5 max)



(g) Total active direct administration employees



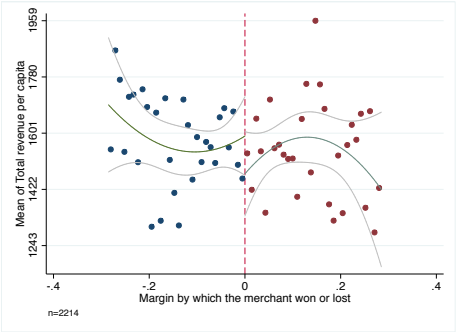
(h) Total active indirect administration employees



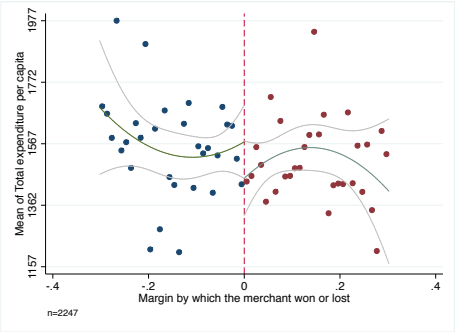
The green line is a best fit second-order polynomial in the entrepreneur's margin of victory for those observations which lie within the optimal bandwidth computed with the Calonico et al. (2014) method. The polynomial is split by a red dashed line which separates the two groups at the threshold $MV_{it} = 0$. Thus, the red points symbolise a entrepreneur-mayor and the blue ones a non-entrepreneur-mayor. The thin grey lines are the 95% confidence interval of the polynomial. Note, that I dropped *Broad* due to a low number of observations. See Table A1 for the definition of the variables.

Figure A3: Sub-sample: The impact of electing a merchant. Graphical analysis.

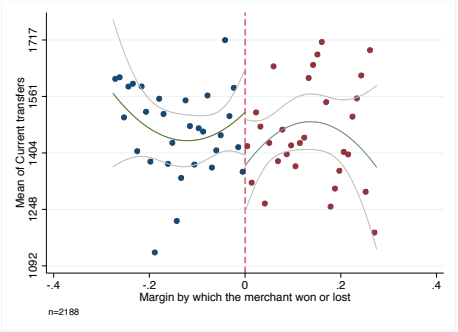
(a) Total revenue



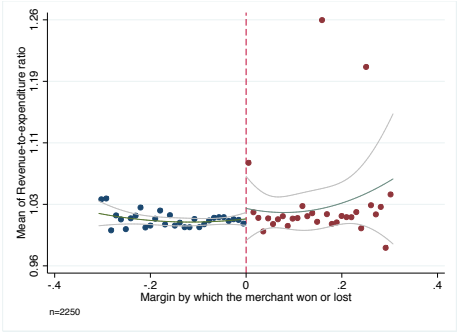
(b) Total expenditure



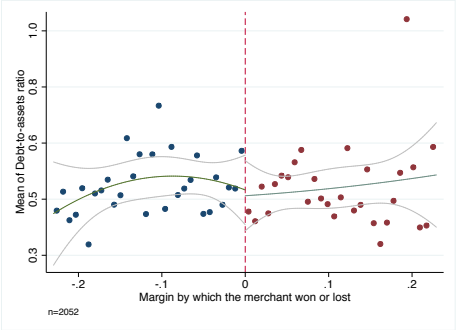
(c) Current transfers



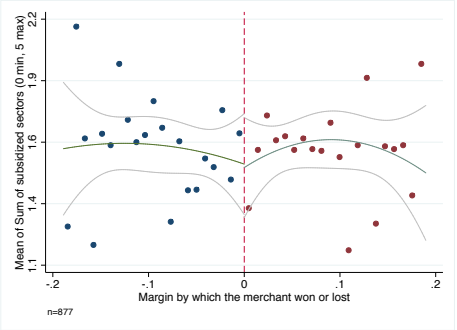
(d) Ratio between revenue and expenditure



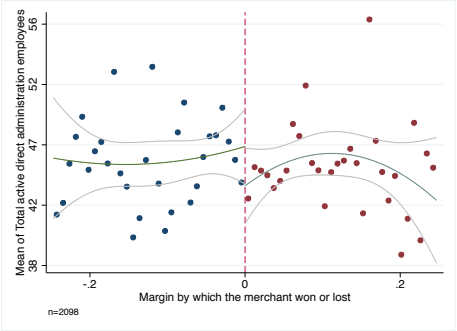
(e) Debt-to-asset ratio



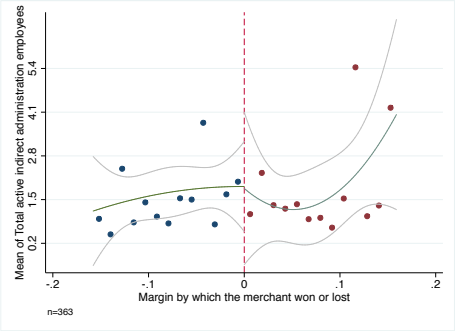
(f) Sum of subsidized sectors within the last two years (0 min, 5 max)



(g) Total active direct administration employees



(h) Total active indirect administration employees



The green line is a best fit second-order polynomial in the merchant's margin of victory for those observations which lie within the optimal bandwidth computed with the Calonico et al. (2014) method. The polynomial is split by a red dashed line which separates the two groups at the threshold $MV_{it} = 0$. Thus, the red points symbolise a merchant-mayor and the blue ones a non-merchant-mayor. The thin grey lines are the 95% confidence interval of the polynomial. Note, that I dropped *Broad* due to a low number of observations. See Table A1 for the definition of the variables.