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A Comissão Examinadora composta pelos professores abaixo, sob a presidência do primeiro, considera o Candidato Vitor Gonçalves Cavalcanti **APROVADO.**

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Aos meus familiares e amigos.

Resumo

Esta tese é dividida em três capítulos separados. No primeiro capítulo, estimo o efeito da política de desvalorização fiscal implementada na Alemanha em 2007. No segundo capítulo, reavalio o estudo conduzido por Arvate (2013), que avaliou o efeito da competição eleitoral sobre a provisão de bens públicos locais em municípios brasileiros. No último capítulo, apresento evidências sobre o efeito do alinhamento político entre os poderes governamentais sobre a composição da despesa pública local nos municípios brasileiros. Nos próximos três parágrafos, eu forneço o resumo desses capítulos.

O objetivo deste capítulo é analisar os efeitos causais da política de desvalorização fiscal sobre a competitividade econômica. Para atingir esse objetivo, estudo a política de desvalorização fiscal implementada na economia alemã em 2007. Esta política foi implementada com o objetivo principal de reduzir a carga tributária e melhorar a situação fiscal da Alemanha. Os resultados são consistentes em ambas as metodologias e mostram que a política aumentou o saldo de exportação e o número de pessoas empregadas na economia, mas não afetou fortemente o PIB per capita. Além disso, o aumento do saldo das exportações parece ter favorecido apenas os setores de produtos manufaturados relacionados a equipamentos de transporte e produtos automotivos.

Este capítulo reavalia os efeitos da competição eleitoral sobre a provisão de bens públicos no Brasil estimados por Arvate (2013). Depois de uma revisão abrangente das mudanças recentes que ocorreram nos critérios que determinam o número de vereadores nos municípios brasileiros, argumento que a estratégia de estimação de Arvate (2013) não é adequada, provavelmente resultante de uma interpretação errônea das mudanças das leis brasileiras. Pro-

ponho novos resultados para esta mesma pergunta avaliada pelo autor, utilizando uma abordagem fundamentada em um desenho metodológico baseado de regressão descontínua. Os resultados encontrados em minhas análises diferem muito dos apresentados no artigo de Arvate. Alguns destes resultados estão alinhados com a evidência de ausência de efeito da competição política sobre o fornecimento de bens públicos em outros países. Este último capítulo analisa em que medida o alinhamento político entre os poderes executivo e legislativo locais afeta a despesa local e a provisão de bens e serviços públicos nos municípios brasileiros. Por um lado, o alinhamento político entre poderes municipais pode aumentar o bem-estar a partir de uma diminuição da barganha política entre coalizões rivais voltadas a atender apenas os interesses desses grupos políticos. Por outro lado, a consequente redução do tamanho da oposição pode ter efeitos perversos por meio da diminuição da representatividade de diferentes grupos sociais e de uma menor fiscalização do poder legislativo sobre o executivo. Explorando a variação em ter a maioria legislativa mínima (em comparação com ter a minoria, marginalmente diferente) na mesma coalizão alinhada ao prefeito, utilizamos uma metodologia baseada em uma regressão descontínua para fornecer evidências de que o alinhamento político pode causar redução de gastos em bens públicos cruciais, como educação, resultando na redução do número de alunos matriculados em escolas públicas locais e aumento do gasto do Poder Executivo que é apoiado pela maioria dos vereadores.

Palavras-chave: Política tributária - avaliação. Finanças municipais - Brasil. Orçamento - Brasil. Eleições municipais - efeitos.

Abstract

This thesis is divided into three distinct chapters. In the first chapter, I estimate the effect of the fiscal devaluation policy implemented in Germany in 2007. In the second chapter, I re-assess a study conducted by Arvate (2013) which evaluated the effect of electoral competition on the supply of local public goods in Brazilian municipalities. In the last chapter, I provide evidence on the effect of the political alignment between government branches on the composition of the local public expenditure in Brazilian municipalities. In the next three paragraphs, I provide the summary of these chapters. The objective of this chapter is to analyze the causal effects of the fiscal devaluation policy on economic competitiveness. To achieve this goal, I studied the policy of fiscal devaluation implemented in the German economy in 2007. This policy was implemented with the primary objective of reducing the tax wedge and improving Germany's fiscal position. My results are consistent in both methodologies and show that the policy increased the export balance and the number of people employed in the economy, but did not strongly affected the GDP per Capita. Also, the increase in the export balance appears to have favored only those sectors of manufactured products related to transport equipment and automotive products. This chapter revisits the effects of electoral competition on the supply of public goods presented in Arvate (2013) for Brazil. After a comprehensive review of recent changes that occurred in the criteria that determine the number local legislators in Brazilian municipalities, I argue that Arvate's (2013) exclusion restriction is weak probably resulting from a misinterpretation of the Brazilian laws. I propose to use a regression discontinuity design to better estimate the effect of interest. Results differ much from the ones presented in Arvate's paper. Some of these results are aligned with evidence of no effect of the political competition

over the supply of public goods in other countries. This last chapter analyzes the extent to which political alignment between executive and legislative government branches impacts the local expenditure and provision of public goods and services in Brazilian municipalities. On one hand, political alignment between branches in the same state level may increase welfare through diminished non-socially-oriented political bargaining among rival coalitions. On the other hand, the resulting reduction in the size of opposition may have perverse consequences through diminished representativeness of different social groups and less thorough checks and balances. Using variation in having the marginal legislative majority (as compared to having the marginal minority) in the same coalition as the mayor's, I exploit a close elections regression discontinuity design to provide evidence that political alignment may cause reduced expenditure in crucial public goods, such as education, resulting in the reduction of the number of students enrolled in local public schools, and increased expenditure of the executive branch supported by the majority of local councilors.

Keywords: Tax policy - evaluation. Municipal finance - Brazil. Budget - Brazil. Municipal elections - effects.

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CHAPTER 1

The Impact of Germany's Fiscal Devaluation Policy

1.1 Introduction

The complex work of elaborating and implementing economic policies turns out to be even more difficult when formulators and executors are faced with restrictions that limit actions. Such restrictions could occur in any politico-economic field for different reasons. For example, member countries of a monetary union zone or countries that have their own currency and adopt fixed exchange rate regimes naturally face serious restrictions on policy implementation, where the objective is to increase the competition of domestic products, starting with the devaluation of the nominal exchange rate. It is important to go around this and other restrictions, finding alternative actions that generate equivalent effects to those of restrictive policies. Such alternatives amplify the capacity of political action and could result in increased levels of social well-being. One example is the Eurozone, where most European countries are restricted from devaluing their nominal exchange rate to stimulate their economies.

In these very restricted scenarios, one may argue that a different fiscal policy could be implemented to promote more competitiveness in the economy and overcome the impossibility of doing so through a nominal exchange rate devaluation. The so-called fiscal devaluation policy has been pointed out as an alternative to that. This policy has the objective of emulating the effects of a nominal exchange rate devaluation by reducing employers' social security contributions, thus reducing wage costs and making exports cheaper, and increasing the value added

tax, thus making goods sold in the country more expensive.

According to Koske (2013), Denmark was the first country to implement a policy of fiscal devaluation in 1987. More recent examples include Germany and Hungary, which adopted the policy in 2007 and 2009, respectively. No conclusive evidence about the effectiveness of such policy has been provided despite some studies that have analyzed the effects of fiscal devaluations through simulations or time series models. Finding the real effects of this policy is an important objective to economies of the world, especially those belonging to the Eurozone.

The main objective of this paper is to provide an accurate estimate of the impact of a fiscal devaluation on economic competitiveness. Our approach will be to use an identification strategy in order to vouch for causal interpretation. Despite the difficulties in identifying the causal impact of this policy, I argue that the fiscal devaluation policy implemented in Germany provides us with a natural experiment to conduct an impact evaluation.

At the beginning of 2007, the German authorities announced a cut in the social contribution of work in 2.3 p.p. (from 6.5% to 4.3%) and an increase of 3.0 p.p. (from 16.0% to 19.0%) to the tax value added in the German economy. According to Lusinyan et al. (2012), the primary objective of this policy was not to restore competitiveness, but to reduce the tax wedge, and improve Germany's fiscal position. Before the implementation of the fiscal devaluation policy in 2007, the German economy presented a dichotomous situation. According to Sinn (2006), at that period of time German exporting companies were performing very well and the country was the second largest world export, but the economic growth was stagnated and the labor market was in difficult situation presenting record levels of unemployment. Additionally, because of international low-wages competition and the rigidity of Germany's wages, many German firms were declaring bankruptcy - close to 40,000 cases per year, achieving the post-war record level in 2005. As reported by the author, there was a consensus among a relevant number of economists that the labor market rigidity, defended by unions and the ideals behind the welfare state, were imposing to German companies high wages for unskilled workers. In

2004, Germany had the second highest wage cost of industrial workers in the world. This explained the need for the tax wedge reduction, according to Sinn (2006). As this policy was not designed primarily to boast the exports level, I may argue that, based on the methodological approach chosen for this paper, I was able to conduct an impact evaluation of this policy on the interest variables.

I argue that to overcome the causal inference challenges, I apply quasi-experimental designs to find the empirical evidences of the causal effects of this policy. To do that, I focus on the policy evaluation on the following variables: i) Exports Balance of goods and services (% of GDP), ii) number of people occupied (in millions) and iii) GDP per capita in Germany. Based on the results achieved, I also conducted a deep analysis to evaluate the impacts on the detailed export sectors. Our research design makes use of the Synthetic Control Method (SCM), proposed and developed by Abadie and Gardeazabal (2003) and extended by Abadie et al. (2010). This method proposes the appropriate causal inference of political events or interventions in aggregated entities, such as cities, states, countries and regions. Given the details of the affected unit (the treated country, in this case) by the "shock" studied and of similar units that do not suffer such events or politics, this method is capable of generating a counter-factual element for the treated unit, of the causal inference. Additionally, I also make use of the Difference-in-Differences model to provide a robustness check of the results and expand the analysis, as is explained later.

The countries that composed the control group for the construction of the counter-factuals proposed in this paper did not suffer policies similar to that suffered in Germany in 2007. The construction of the counter-factual for Germany before the policy is favored by the scenario of economic stability of the period that preceded the world crisis of 2008. Until then, the world economy did not show any signs of difficulty that demanded a need for policies in countries that could change relative distortions in the economic scenery of the considered countries.

Although some studies have tried to estimate the impact of this policy in certain countries,

they did not apply the methods of impact identification, which propose to deal with all of the issues of causal inference. Besides evaluating the impact of this policy, this paper has the additional purpose of adding empirical evidences consistent with the recent econometric literature of identification to the literature related to the fiscal devaluation policy.

The results found in the paper showed that the policy of fiscal devaluation implemented in Germany in 2007 caused positive impacts in the exports balance and the number of people occupied. The results also showed that there is no strong evidence that the GDP per Capita was affected by the policy. Additionally, I find that only two of thirteen export sectors were benefited by this policy. They were evaluated in the short term, as argued by the difficulty of isolating the shock of the policy and the crisis of 2008, which caused Germany and the other countries around the world (here used as controls) to implement different policies. In this case, these policies seem to add up to the effect of the fiscal devaluation policy already started in 2008, which would have made it impossible for evaluations in the long term.

In addition to this introductory section, this paper has five sections. In the following section, a brief review of the literature of prior studies that addressed the issue of fiscal devaluation will be discussed. In the third section I discuss the description of the econometric model used for the analysis of the impact of this work. In the forth section, the data used in this paper is presented. In the fifth section, the results found are shown and in the sixth section I present our conclusion of the work done.

1.2 Literature Review

Several papers have attempted to analyze the economic impact of the fiscal devaluation on aggregate outcomes. Such studies use different methodologies for different analytical purposes. Some use simulation models to estimate results that would be achieved with the implementation

of that policy in countries.

Correia (2011) shows, with regards to three different theoretical models, with varying degrees of complexity, that a fiscal devaluation policy exists, based on the adjustment of the social contribution of work and the value-added tax that could effect changes similar to a reduction of the nominal exchange rate.

Farhi et al. (2013) evaluated that a fiscal devaluation can be implemented by a country that is unilaterally using conventional fiscal instruments. A fiscal devaluation that is not anticipated can be implemented with only a few adjustments to the value-added tax and social contribution of work. In addition, the results of the economic impact of this policy are robust for different economic scenarios and do not have an effect on government revenues.

In addition to these works, Mooij and Keen (2012) empirically analyzed the implementation of a fiscal devaluation policy for the Eurozone countries. The authors concluded that this policy can be capable of causing an improvement in the commercial balance of these countries in the short term. Performing the estimations of the VAR model equations, the studies try to simulate scenarios and evaluate that the positive results of the policy are consistent with the empirical findings. Cenare and Danninger (2008) analyzed the effect of the fiscal devaluation that occurred in Germany in 2007 in the core of German inflation. The evidence shows an adjustment in the core of the inflation.

Koske (2003) explained the fiscal devaluation and cites the example of countries that implemented policies that could be characterized as fiscal devaluations. Furthermore, the author presents possible results of works that evaluate the impact of the fiscal devaluations based on simulation models calibrated with empirical information of countries.

In these works evaluated by the author, different economic dimensions were evaluated. The works show that the effect on the commercial balance is moderate and that this effect dissipates in the long term. With regards to the level of employment, the studies indicate the occurrence of positive effects, as well as the effects on the GDP, but the time horizon in which the effect

remains is different. In general, the author concludes that a positively moderate effect exists in this policy over the economy in the short term and that the fiscal devaluation should not be used as a substitute for reforms in the labor market. The intent of increasing the productivity of work is only a part of a set of actions.

Given the analyses already performed on this topic, this paper intends to present robust empirical evidences, based on studies of profound cases, with respect to potential results of implementation and fiscal devaluation. Such results will be achieved based on the econometric methodology of identification, and can be compared with the results already found in simulation models and empirical works that used methodologies considered less adequate to evaluate what is proposed.

1.3 Empirical Strategy

The main objective of this paper is to evaluate the impact of the fiscal devaluation policy implemented in Germany in 2007. Clearly, as this policy was not randomly assigned to Germany in 2007, its impact evaluation is not straightforward. To do that, I need to estimate economic outcomes for the German economy under a scenario with no devaluation policy. I propose therefore the use of two different empirical strategies to construct these counterfactuals: the synthetic control method and a difference-in-differences model. There are different advantages and disadvantages regarding these two empirical strategies, which I discuss in further detail below. I note, however, that results across these estimation procedures were quite similar, adding credibility to our results and strengthening our causal interpretation.

1.3.1 Synthetic Control Method

In this section, I describe the synthetic control method (SCM) developed by Abadie and Gardeazabal (2003) and extended by Abadie et al. (2010). I also discuss its advantages and limitations when compared to other methodologies used in the literature and why I believe the SCM is an adequate method to help us overcome the challenges of estimating our cause-and-effect relationship of interest.

Suppose there are $J+1$ countries and that only the first country (Germany) is exposed to the policy change (the country experiencing a fiscal devaluation policy), so that there are J remaining countries as potential controls (all other countries not experiencing an equivalent policy). Intuitively speaking, using the J potential controls, the SCM proposes the construction of a synthetic counterfactual to emulate outcomes for the treated country in the hypothetical situation of the policy not having been implemented in the treated country.

Let Y_{ct}^N be the outcome that would be observed for country c at time t in the absence of the policy, for units $c = 1,\ldots,J+1$, and time periods $t = 1,\ldots,T$. Let Y_{ct}^I be the outcome that would be observed for unit c at time t if unit c is exposed to the policy in periods T_0+1 to T, where T_0 is the number of pre-intervention periods such that $1 \leq T_0 < T$. It is assumed that the policy has no effect on the outcome of interest before the implementation period, such that for $t \in 1,\ldots,T_0$ and all $c \in 1,\ldots,N$ I have that $Y_{ct}^I = Y_{ct}^N$.

Our objective is to estimate $(\alpha_{1T_0+1},\ldots,\alpha_{1T})$, which is given by $\alpha_{1t} = Y_{1t}^I - Y_{1t}^N = Y_{1t} - Y_{1t}^N$, such that for $t \in T_0,\ldots,T$. The problem with estimating α's in this case is that Y_{1t}^N is never observed for the treated country once $t > T_0$. To overcome this obstacle, the SCM proposes the estimation of Y_{ct}^N for the treated region when $t > T_0$. To see how a control group might be obtained from the set of control countries, suppose as in Abadie et al. (2010) that Y_{ct}^N is given by the following model

$$Y_{ct}^{N} = \delta_t + Z_c\theta_t + \lambda_t\mu_c + \varepsilon_{ct} \tag{1.1}$$

where δ_t is an unknown common factor with constant factor loadings across units, Z_c is a vector of observed covariates (not affected by the intervention), θ_t is a vector of unknown parameters, λ_t is a vector of unobserved common factors, μ_c is a vector of unknown factor loadings, and the error terms ε_{ct} are unobserved transitory shocks at the region level with zero mean.

Now consider a $(J \times 1)$ vector of weights $W = (w_2, \ldots, w_{J+1})'$ such that $w_j \geq 0$ for $j = 2, \ldots, J+1$ and $w_2 + \cdots + w_{J+1} = 1$. Each value that W might take represents a synthetic control group for country one. For example, if $w_2 = 1$ and $w_j = 0$ for $j = 3, \ldots, J+1$, then country 2 works as control for country one (the treated one). If, on the other hand, one sets a subset $J' \subset J$ to have equal weights, such that $w_{j'} = 1/J'$ for $j' \in J'$ and 0 otherwise, the comparison would be between the treated country and the average of all other countries that belong to the group J'.

Abadie et al. (2010) showed that based on an optimal vector of weights $(w_2^*, \ldots, w_{J+1}^*)$ such that the following holds, $\sum_{j=2}^{J+1} w_j^* Y_{j1} = Y_{11}, \ldots, \sum_{j=2}^{J+1} w_j^* Y_{jT_0} = Y_{1T_0}$ and $\sum_{j=2}^{J+1} w_j^* Z_j = Z_1$ than it implies that $Y_{1t}^N = \sum_{j=2}^{J+1} w_j^* Y_{jt}$, which suggests the following estimator for the α vector:

$$\widehat{\alpha_{1t}} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}. \tag{1.2}$$

To obtain the vector of optimal weights W, let $X_1 = (Z_1', Y_{11}, \ldots, Y_{1T_0})'$ be a vector of pre-intervention characteristics for the treated country and X_0 be a matrix that contains the same variables for the untreated countries, such that the jth column of X_0 is $(Z_j', Y_{ij}, \ldots, Y_{jT_0})'$. Then, W^* is chosen to minimize the distance, $\|X_1 - X_0W\|_V = \sqrt{(X_1 - X_0W)'V(X_1 - X_0W)}$, between X_1 and X_0W subject to $w_j \geq 0$ for $j = 2, \ldots, J+1$ and $w_2 + \cdots + w_{J+1} = 1$, where V is a symmetric and positive semi-definite matrix chosen in a way that the resulting synthetic control country approximates the trajectory of the outcome variable of the affected country in the pre-

intervention periods.

On the other hand, this approach has the limitation that it does not allow one to assess the significance of the results using standard inferential techniques, given that the number of untreated regions and the number of periods considered are small. Abadie et al. (2010) suggest that inference should be carried out by implementing placebo experiments. In this case, inference is based on comparisons between the magnitude of the gaps generated by the placebo studies and the magnitude of the gap generated for the treated state. Thus, if the gap estimated for the treated state is large compared to the gap estimated for the placebo experiments, then the analysis would suggest that the treatment had an effect on the outcome of interest and is not driven by chance.

The model described above has a series of advantages when compared to other approaches used in the literature. As highlighted by Nanncini and Ricciuti (2010), the model is transparent, given the weights $(w_2^*, \ldots, w_{J+1}^*)$ that identify the countries used to build the counterfactual of the treated country and the flexibility of the model, since the group of candidate countries participating in the synthetic control can be appropriately restricted to make the comparisons justifiable (for example, only consider countries of Europe with possible controls for one European treated country). Additionally, the model relaxes the supposition that the factors are not variants in time (fixed effect) or share one, same tendency (difference-in-differences), given that the effects of perturbing factors not observed are flexible and can vary in time.

On the other hand, this approach has a limitation that consists of the impossibility of access to the significance of the results using techniques of standard inferences, given that the number of countries not treated and that number of analyzed periods is generally small. Abadie et al. (2010) suggest that the inference must be performed via the implementation of the Placebo experiments, also known as a "falsification test".

This has the intention of evaluating if the estimated effect for the treated unit is significant with relation to the distribution of effects in the control units that were not exposed to

the treatment during the considered period of the whole analysis. The inference is based on comparisons between the magnitude of differences generated by the Placebo studies and the magnitude of the difference generated in the treated state. In other words, the Placebo test tries to provide us with evidence regarding the probability of a difference between the synthetic unit and the treated unit being a chance result.

In case the generated results in the Placebo tests corroborate the differences in amplitude equivalent to the analyzed unit, one can conclude that the estimated results do not present significant evidences. Therefore, if the estimated difference for the treated state is big, compared to the estimated differences from the Placebo experiments, then the analysis would suggest that the treatment had an effect on the outcome of interest and is not caused by chance.

The comparison between the difference in the treatment unit and the respective differences of the other control units can be done through the graphic analysis of the generated values. Besides the graphic analysis, based on Abadie et al. (2010) and Keele et al. (2013), it is also possible to calculate a *pseudo P-value for the result of each estimates model*. For this, the proportion of the Mean Square Prediction Error (MSPE) is calculated through the period post-policy with relation to the MSPE of the period pre-policy. This statistic is obtained for the treated country and all other controls, based on the generated information of the Placebo test.

It is possible, through the distribution inspection of these statistics, to analyze the difference (between the data observed and the estimated model) of the unit that the treatment suffered and the other control units. If the proportion of the MSPE of the treated unit is big, when compared to the generated proportions by the Placebo test, the probability of having an effect on the evaluated policy over the treated unit is also big.

The estimation of *pseudo P-value* based on the generated empirical distribution is thus feasible. Despite the lack of randomness of the distribution, as argued, the *pseudo P-value* still has interpretation probability of obtaining a ratio of one of the control units with a higher value than the proportion of the treated unit. Still in agreement with Keele et al. (2013), given the

small sample size, a *threshold* more liberal than 0.10, instead of the standard 0.05, to reject the null hypothesis, seems more adequate.

1.3.2 Difference-in-Differences

In this paper, I also follow a more restrictive approach and, in addition to the SCM, I estimate the effects of the fiscal devaluation policy though the difference-in-differences method.

As it was mentioned before, I also estimated the impacts of the fiscal devaluation policy over the variables of interest based on the Difference-in-Differences (DD) methodology. This method is quite similar to the SCM, where I propose an estimate of the policy's impact based on the comparison of the treated unit and the control group, before and after the policy implementation.

However, DD has one additional assumption that the SCM does not have: The "equal trend" assumption. This assumption requires that the confounding factors have a common trend for treated and control units. In some cases, this assumption is not needed to capture group level omitted variables.

Consider a model where d_t is a dummy variable indicating the period of fiscal devaluation policy implementation (the treatment year), $Germany_s$ is a dummy variable indicating the German unit (treated unit) and $(Germany_s.d_t)$ indicating the interaction between them, as described in the equation 1.3. It includes two main effects for country and time and the interaction term which marks the observations of Germany after the policy implementation.

$$Y_{ist} = \alpha + \gamma Germany_s + \lambda d_t + \delta(Germany_s.d_t) + \varepsilon_{ist} \tag{1.3}$$

This regression specification of the DD method allow us to estimate the parameters, standard errors and include information to control units. Moreover, this type of specification allow us to include fixed effects of the countries, year and add extra data that must be considered in

the estimates. This estimate has been widely used in impact evaluations and it was used in this study as an additional methodological source to complement the SCM.

1.4 Data

In addition to the German information, data over three countries was used. The purpose of using these additional countries is the formation of the control group, as required by the methodology used. This control group is formed of thirty-three member countries of the Organization for Economic Cooperation and Development (OECD) [1], except Denmark and Sweden. These two were excluded from the potential control groups because they experienced similar situations as that of fiscal devaluation.

Because this paper also proposes the impact evaluation of the policy on exports from Germany and it is one of the three biggest exporters of the world, according to the World Trade Organization[2], I have also included information on China, the biggest exporter of goods in the world[3]. The importance of this inclusion is due to the fact that the SCM proposes a weighted average of the controls to generate a synthetic control. In this way, the importance of the inclusion of China is due to the need to present all the countries with a larger volume of exports than Germany in the group of potential controls and in that way facilitate the balancing of weights. This inclusion also generated significant improvement in the creation of synthetic controls.

The objective of the pre-selection of these thirty-three countries that composed the group of potential controls was the simplification of the process of optimization of the weights. Instead

[1]OECD country members: Australia, Austria, Belgium Canada, Chile, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Israel, Italy, Japan, South Korea, Luxembourg, Mexico, Holland, New Zealand, Norway, Poland, Portugal, Slovakia, Slovenia, Spain, Sweden, Switzerland, Turkey, United Kingdom and United States.

[2]`http://stat.wto.org/CountryProfile/WSDBCountryPFView.aspx?Language=E\&Country=DE`

[3]`http://stat.wto.org/CountryProfile/WSDBCountryPFView.aspx?Country=CN\&`

of using a larger quantity of possible controls, which would make the process of optimization even more complex, the control selection was made from a group of countries that at the time were similar in a wide range of political areas, countries with a certain degree of economic heterogeneity (the OECD is not composed only of developed countries) that can replicate the properly weighted German economy.

For the proposed objective of evaluating the impact of fiscal devaluation at the level of national income, level of employment and balance of exports of the German economy, only one variable was chosen for each of these dimensions. The World Development Indicators 2014 (WDI 2014), elaborated by the World Bank, was used to extract the "GDP per Capita" variables, which is the Gross Domestic Product in American Dollars in 2005 divided by the average population of the respective year, and "External balance on goods and services (% of GDP)", which is the difference between the quantity of the goods and services exported and the quantity imported. The variable "Employment", which is the number of persons engaged in millions, was extracted from Penn World Table 8.0 (PWT 8.0).

Besides the three outcomes presented, another five indicators were used as covariates. The Commercial Volume (% of GDP) variable is the sum of exports and imports of goods and services divided by the value of the GDP (variables in current American Dollars), and was extracted from WDI 2014.

The PWT 8.0 used four variables: i) Consumption Component of GDP is the share of household consumption to the current Purchasing Power Consumption; ii) Government Spending Component of GDP is the share of government consumption to the current Purchasing Power Consumption; iii) Population, represented in millions; and iv) The Level of human capital is the index of human capital per person, based on the years of schooling and education returns.

Charts 1.1, 1.2 and 1.3 in the first column present, the respective covariates for each of the three synthetic controls estimated in the paper. In addition to the covariates, the charts show

the periods in which each covariate was introduced in the model. As shown, in most cases, the pre-treatment period is divided in two so that the estimated weights can better capture the variations of the period.

The additional estimates, based on the Difference-in-Differences methodology, used different sources of data. I used the Quarterly National Accounts of the OECD to extract three indicators for the interest outcomes: i) Employment, which is measured by the number of persons in thousands; ii) External balance, which is the seasonally adjusted difference of exports and imports in US dollars; and iii) GDP per Capita, which is the seasonally adjusted US dollar amount Per Head.

1.5 Results

Also according to the results of tables 1.1, 1.2 and 1.3, the SCM was able to generate a counterfactual similar to that of Germany in the period of treatment. The proximity of the values of the average predictors of the synthetic control (third column in the charts) with the respective values for the treated country (second column in the respective charts) is an indicator of good adjustment of the model.

When looking at the last column of these tables, one can observe the average values of the predictors for each average of all the potential control countries. For practically all covariates on all models, these values were very different when compared to the values of the countries weighted by the SCM. This is more of a hint that the weight proposed by the method is efficient in the attempt to construct a counter-factual and that a possible use of the combination of control countries with equal weights could generate a possible inadequate counter-factual.

The results of the policy were evaluated only for 2007 (the year of the policy implementation, which became effective at the beginning of the year). This provoked the understanding

Table 1.1 Predictors Average - External Balance (% of GDP)

Germany	Treated	Synthetic	Average Mean
Government Expenditure Share of the GDP	0.148	0.149	0.180
Human Capital (2002 - 2003)	3.118	3.158	2.980
Human Capital (2004 - 2006)	3.296	3.253	3.021
External Balance - (% of GDP) (2002 - 2003)	4.210	4.213	1.365
External Balance - (% of GDP) (2002 - 2003)	5.290	5.300	1.171
GDP per Capita (2002 - 2003)	32977.650	32587.124	27280.029
GDP per Capita (2004 - 2006)	33887.233	34317.791	29060.414
Trade (% of GDP) (2004 - 2006)	67.180	72.196	83.976
Trade (% of GDP) (2004 - 2006)	78.293	72.287	90.711

Source: Prepared by the author.

Table 1.2 Predictors Average - Occupied People

Germany	Treated	Synthetic	Average Mean
GDP per Capita	33523.400	33523.864	28924.584
Population * Human Capital	266.071	251.540	207.024
Consume Share of the GDP (2002 - 2004)	0.604	0.562	0.573
Consume Share of the GDP (2005 - 2006)	0.580	0.555	0.566
Trade (% do PIB) (2002)	66.840	67.040	83.178
Trade (% do PIB) (2006)	85.410	80.975	92.522
Occupied People (2002)	39.119	39.019	38.863
Occupied People (2006)	39.157	39.244	40.521

Source: Prepared by the author.

Table 1.3 Predictors Average - GDP per Capita

Germany	Treated	Synthetic	Average Mean
GDP per Capita (2002 - 2004)	33083.700	33032.877	27387.237
GDP per Capita (2005)	33542.800	33699.792	28819.975
GDP per Capita (2005 - 2006)	34182.950	34125.384	29236.577
Trade (% do PIB) (2002 - 2004)	68.803	73.780	87.213
Trade (% do PIB) (2005 - 2006)	81.415	81.260	94.289
Government Expenditure Share of the GDP (2002 - 2004)	0.151	0.160	0.186
Government Expenditure Share of the GDP (2005 - 2006)	0.145	0.146	0.171
Consume Share of the GDP (2002 - 2004)	0.604	0.578	0.575
Consume Share of the GDP (2005 - 2006)	0.580	0.567	0.567

Source: Prepared by the author.

that such occurrence could affect the quality of evaluation of results of the policy for the years after 2007. The reason for this is due to a possible commitment of basic presupposition of the adopted method.

In 2008 the crisis also spread to countries, particularly in Europe (most members of the OECD). The consequences of the crisis required different actions in each country, taking in accordance with the politico-economic understanding of each of them into account. These actions led Germany and the rest of the control countries to possess new parameters for comparison.

Thus, the extrapolation, which is based on estimated weights in the pre-fiscal devaluation period, would not construct an adequate counterfactual starting in 2008, since there was a modification in the comparison among the countries. In other words, the observed data of Germany, when compared to Synthetic Germany, would present differences not only due to the political impact of fiscal devaluation that I want to identify, but would also include an unknown bias, arising from the inappropriate use of weights that are no longer valid.

Due to this empirical limitation, this paper does not discuss the effect of the policy conditioned to the temporary horizon, raised earlier by theoretical studies. Instead of evaluating the short term versus the long term, this paper only evaluates the immediate impact of the policy (only in the year of implementation).

The estimated results show that the fiscal devaluation policy caused significant impacts in the exports balance and the number of people occupied. The results did not show a conclusive effect on the GDP per Capita. The results presented in table 1.4 show the difference between synthetic Germany, generated through the SCM, and the data observed of Germany in the year that the evaluation was conducted.

Graphs 1.1, 1.2 and 1.3 present the German data for each one of the three outcomes analyzed and their respective synthetic controls (counter-factual). The strong similarity between the observed variables and their synthetic controls in the pre-treatment period shows the adequacy of the SCM in estimating weights for the respective countries that create a unit that is

Table 1.4 Fiscal Devaluation Impact

	External Balance (% of GDP)	Occupied People	GDP per Capita
Policy Effect	+ 2,71%	+ 1,02%	+ 1,87%
Pseudo P-Value	0.0303	0.0909	0.46875

Source: Prepared by the author.

satisfactorily similar to the treatment.

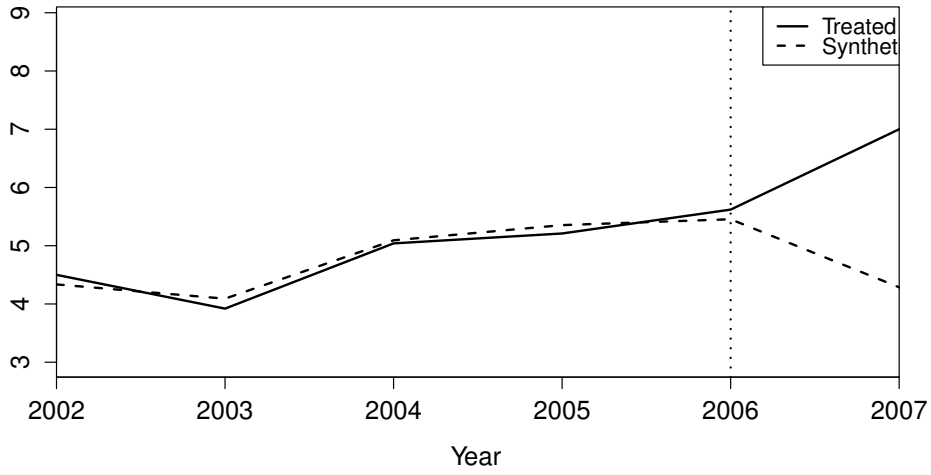
The dotted line in 2006 was placed to mark the last year before the fiscal devaluation policy (last year of the pre-treatment period and the limit for the estimate of synthetic control weights). The relationships of the German variables analyzed with the synthetic control in 2007 show the magnitude of the impact of the policy on the outcomes, equivalent to the value in chart 4. For each estimated synthetic control, the weights of the control countries that compose this counterfactual are different. Table 5 presents the weights of each country for each synthetic control, when these values are larger than 1%. The models are composed of countries with relatively equal weights.

1.5.1 Synthetic Control Method Robustness Checks

The limitation of the SCM in performing classical hypothesis testing is contoured for conducting the Placebo tests and the pseudo P-value calculation. Figures 1.4, 1.5 and 1.6 present graphic results of the Placebo tests. More than one Placebo test is presented for the respective outcomes. Each graph has different levels of control cuts that present a low quality of adjustment that, consequently, are not good comparative references in the test.

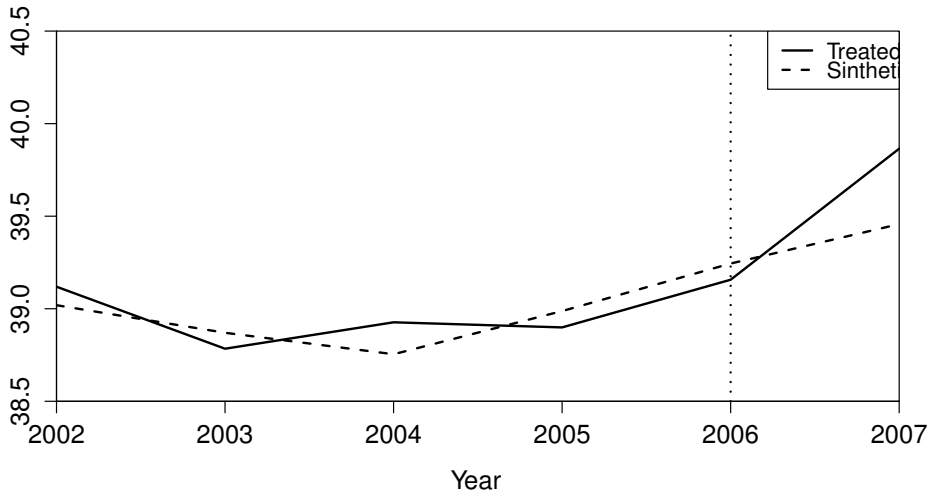
These cuts are made discarding the controls with a mean square prediction error (MSPE): (a) Twice as large as the exports balance of Germany in the pre-treatment period; (b) five times larger than Germany's export balance in the pre-treatment period; and (c) twenty times larger than Germany's export balance in the pre-treatment period.

Figure 1.1 Germany External Balance: Treated X Synthetic



Source: Prepared by the author.

Figure 1.2 Occupied People in Germany: Treated X Synthetic



Source: Prepared by the author.

Table 1.5 Control Weights (larger than 1%)

Country	External Balance	Occupied People	GDP per Capita
Austria	-	-	3.0%
Canada	54.1%	-	-
Chile	10.3%	-	-
Czech Republic	4.0%	1.1%	-
Greece	3.5%	-	-
Hungary	-	-	1.3%
Italy	-	-	49.7%
Japan	-	54.3%	-
Luxembourg	-	11.9%	-
Mexico	-	-	1.1%
Netherlands	-	-	16.1%
Norway	18.2%	-	-
Poland	-	2.6%	-
Portugal	-	-	2.0%
Slovakia	-	2.2%	1.6%
Slovenia	-	2.9%	1.5%
Switzerland	-	-	17.4%
Turkey	-	14.3%	-

Source: Prepared by the author.

The strategy of presenting more than one Placebo test for each model was proposed by Abadie et al. (2010). The intent of this selection is to provide the possibility of carrying out checks on the magnitude of the impact on Germany with control countries that possess mean square errors of prediction, in the pre-treatment period, that are similar (not more than two times the error of Germany in the pre-treatment period) to Germany in different proportions.

In the graphics of figures 1.4, 1.5 e 1.6, the dotted lines represent the differences between the observed variables of the treatment unit and their respective proposed synthetic controls. The other gray continuous lines represent the same difference for each of the potential control countries. The results show that the differences for the Export Balance variable are superior in all cuts, showing strong hints of the significance of the positive impact.

The Occupied People and the GDP per Capita variables present similar results as the graphic test. The differences of the respective values of these variables are larger than the great majority of control units. Since the magnitude of the differences of these two variables is always larger

than the majority of the controls, even more so when the considered controls are those with errors close to Germany's, it is possible to conclude that this suggests that the impacts identified are significant.

The P-Value values listed in table 1.4 were calculated based on Abadie et al. (2010), according to what is described in the methodology section. Figure 1.7 presents a group of three graphics with the distributions of the EQM proportions of the post-policy period in relation to the EQM of the pre-policy period for each variable. Graphic (a) of figure 1.7 shows that the proportion of the German Export Balance variable is well above the proportions of control countries. If the fiscal devaluation policy were implemented before in another country, the probability of the proportion being as large as Germany's is 1/33 = 0.0303.

The distribution of the proportions of the Occupied People variable shows that only two control countries possess proportions of the EQMs larger than Germany's. Even then, the P-Value of this result shows the rejection of the null hypothesis in the proposed threshold (0.10). Although the graphic analysis of the Placebo test suggests the significance of a positive impact, the distribution of the proportions (which originates the P-Value) suggests the contrary. Firpo and Possebom (2016) addressed the problem of lack of inference procedures for SCM estimations coming up with a strategy described in their paper. I implement this methodology in order to have another significance check of the estimated effects. Using the significance level at 0.05 the graphs 1 and 2 in the Appendix of this paper presented significant effects for the external balance and the number of occupied people, respectively[4]. The grey areas depicts the confidence sets (described in the graphs descriptions). The chart 3 presented the inference result for the GDP per Capita at this same significance level. Although this result does not show evidence of a significant effect, an equivalent result using a significance level of 0.10 presented in figure 4 showed evidence of significance.

I also conducted another placebo test. This test is similar to the one conducted in Abadie

[4]The results were estimated using a *precision* equal to 30 and confidence subset *linear*.

et al. (2015) where they estimated the effect of interest; reassigning it in the data to a year other than the real one. In our approach, since I only analyzed the policy effect one year after the policy, I reassigned the year of the policy to one year before to the real one and estimated the policy's effect. The graphic results are shown in the figures 1.8 and 1.9. This placebo test showed that the effects over the external balance and occupied people for this hypothetical year are not even positive. These results were estimated with the same models as those estimated to achieve the original results, except the GDP per Capita, that already presented enough findings to show that it did not have significance.

Additionally, to check if the positive effects suggested by the SCM estimation in 2007 I conducted the effect estimation in 2008 as well. Although, as explained previously, 2008 was a year characterized by the international crises and disregarded in the impact analysis, I estimated the effect in this year to as another robustness check. This check aims to verify if the positive effect found in 2007 was due to any atypical influence on the data. The results in 2008 showed an equivalent positive impact, indicating that the policy effect remained at the second year after the policy implementation.

1.5.2 Additional Approach to Estimate the Policy's Effect

Besides the SCM estimates, I also estimated the effects of the fiscal devaluation policy over the variables of interest with a different approach. Similarly to the SCM, the Difference-in-Differences method uses the information of the treated unit and the control group to estimate the effect of a policy, for example, over the treated unit. The basic difference between the two methodologies is that the Difference-in-Differences requires that the confounding factors have a common trend for treated and control units, while the SCM does not.

As the DD is the basis for the SCM, I also estimated the effect of the policy based on this method. The control group of this additional estimate was chosen from those countries which

were most likely to have a similar policy implementation. This was conducted by the Propensity Score Matching (PSM) method, which indicates the control units that have more similarity with Germany to have such a policy implemented and allowed us to discard the countries from the control group which were not comparable to the treated one. I estimated one control group for each variable.

In this additional analysis I estimated the policy's effect over the three equivalent variables already estimated by the SCM, but now, instead of annually, they are distributed quarterly. With this approach, I also evaluated the impacts of the fiscal devaluation policy until right before the 2008 crisis, which occurred in the last quarter of 2008. The impact is estimated considering all four quarters of 2007 and the first three quarters of 2008.

Table 1.6 shows the effect of the policy over the quarterly distributed External Balance, Employment and GDP per Capita extracted from Quarterly National Accounts of the OECD. These results were obtained through the Difference-in-Differences estimates with fixed effects on countries and considering specific control groups, composed of five countries, chosen for each outcome based on the PSM method. For the External Balance, Austria, Finland, Belgium, The Netherlands and Denmark were considered. The control group for the Employment variable was France, United Kingdom, Italy, South Korea and Mexico. Finally, the control group for the indicator of the GDP per Capita indicator was France, Australia, Canada, Japan and Belgium. Consistent with the SCM results, I identified that the fiscal devaluation policy positively affected the External Balance and the Employment level. It did not affect the GDP per Capita.

Table 1.6 Difference-in-Differences

	External Balance	Employment	GDP per Capita
Parameter	54306.52	474	465.1213
P-Value	0.000	0.034	0.318

Source: Prepared by the author.

The results consistently show that the Employment and the External Balance variables were positively affected by this policy. I conducted a deeper evaluation to identify how this positive affect was distributed by each export sector and sub-sector. The data regarding external balance by sectors and sub-sectors was extracted from the World Trade Organization website.

The estimate was also based on the Difference-in Differences estimate, and these were conducted for all of the sectors and sub-sectors, considering the same control group as the one for the External Balance DD estimation. I decided to estimate all of the sectors and sub-sector separately, instead of running just one regression with fixed effects to sectors and sub-sectors, because there are unspecified products that do not add up to overall totals of the sectors and external balance.

This data was distributed annually and was divided into three main sectors: i) Manufactures ; ii) Fuels and Mining Products; and iii) Agricultural Products. In terms of exportation volume, during the years of 2000 Germany's bulk of exports come from the manufacturing sector, as described by Danninger and Joutz (2007) and evidenced by the data. The manufacturing sector is composed of different sub-sectors, which were defined by the Standard International Trade Classification. The names of these sub-sectors are: 1.1) Iron and steel (MAIS); 1.2) Machinery and transport equipment (MAMT); 1.2.1) Automotive products (MAMTAU); 1.2.2) Office and telecom equipment (MAMTOF); 1.2.2.1) Electronic data processing and office equipment (MAMTOTEP); 1.2.2.2) Integrated circuits and electronic components (MAMTOTIC); 1.2.2.3) Telecommunications equipment (MAMTOTTL); 1.2.3) Transport equipment (MAMTTE); 1.3) Textiles (MATE); 1.4) Clothing (MACL); 1.5) Chemicals (MACH); 1.5.1) Pharmaceuticals (MACHPH). The results showed no significant positive impact over of the exportation of Fuels and Mining products and Agricultural products. The increase of the German's exportation in 2007 was led by the increase in manufactures, as one could expected. The results of all manufactures sub-sectors are presented in table 1.7. The Manufacture sub-sector showed mainly positive significant impacts.

Table 1.7 Difference-in-Differences: Manufactures Sub-Sectors

				Parameter	P-Value
MA				1.35E+11	0.000
	MAIS			-4.20e+09	0.003
	MAMT			9.80E+10	0.000
		MAMTAU		4.59E+10	0.000
		MAMTOF		-5.77E+09	0.027
			MAMTOTEP	-8.06e+08	0.463
			MAMTOTIC	-1.08e+09	0.278
			MAMTOTTL	-3.89e+09	0.004
		MAMTTE		5.04E+10	0.000
	MATE			7.47e+07	0.845
	MACL			-1.17e+09	0.007
	MACH			1.40e+10	0.015
		MACHPH		7.47e+09	0.002

Source: Prepared by the author.

1.6 Conclusions

The SCM showed that it is capable of offering a satisfactory construction of counter-factual elements for the three analyzed outcomes, since the adjustments of the observed values and the respective synthetic controls in the pre-treatment period present a large similarity, as shown on graphics 1.3, 1.2 and 1.1. Besides this, tables 1.3, 1.2 and 1.1 show that the values of the covariates estimated for the synthetic control are close to the values of the same covariates of the treated unit.

The same charts show further relevant evidence. The proposal of using a counter-factual composed of the pondering of the weights of the control countries, when compared to the use of the combination of the group of countries with the same weights, proved to be more appropriate. These charts show that in the pre-treatment period, the SCM is capable of reaching adjustment values for the reality of Germany that are a lot closer than the similarity of the sample mean.

With relation to the impact of the fiscal devaluation policy on the German economy, the results show that the Export Balance and the Number of Occupied People grew significantly

due to the fiscal devaluation policy. The two different placebo tests, where the treatment of interest is assigned to each of the control units one year before the real one, showed that these results are significant.

The effects of these impacts are immediate, since the results were evaluated in 2007, when the policy was implemented. The results of the Exports Balance variables and the Number of Occupied People are significant according to the evaluations conducted. The result of the GDP per Capita was not significant.

The additional estimates based on the Difference-in-Differences methodology showed similar results; significant positive impacts over the External Balance and the Employment levels. These results used quarterly data, which gave us time to expand our evaluation. Following an equivalent approach, I analyzed how the positive impact of the policy over the External Balance was divided by sectors and sub-sector. The results showed that the Fiscal Devaluation Policy affected positively the external balance led by the increase of manufacture sector exports. The sub-sectors which caused this increase were Machinery and Transport Equipment (Automotive products and Transport Equipment) and Chemicals (Pharmaceuticals).

It shows that the fiscal devaluation policy caused the increase of the export volume of each of the sectors which were already more important in the total volume of exports. According to Sinn (2006), since 1960's, when Japan started a worldwide low-wage competition, followed over time by the Asian Tigers and most recently by Poland, China and India, countries such as Germany, where unions and the welfare state play an important role against the international low-wage competition, had to struggle to maintain their competitiveness.

Based on the World Trade Organization data, Sinn (2006) showed that in 2004 West Germany had the second highest labor cost. This scenario led some German companies to promote the replacement of people with machines, outsourcing and off-shoring to try to survive in the international competition. However, it was not enough to avoid the high number of firms that bankrupted, where in 2005 the number of bankruptcies achieved the post-war record level.

The results show that the combination of this background scenario and the policy did not benefit all of the German export sectors. It seems that this new fiscal set up only affected the exports of those products with high levels of capital intensive usage and that the fiscal devaluation policy was a very important tool for the German government to improve competitiveness.

Therefore, the analyses show that, given the impossibility of using the devaluation of the nominal exchange rate in order to stimulate the economic activity, the policy in question was effective in increasing the competitiveness of the German goods and services in the world markets. There has been an increase in the exports balance and in the levels of employment.

This paper does not intend to evaluate the long term impacts of the policy of fiscal devaluation. In this case study, the proposal of constructing a counter-factual is based on the hypothesis that, in the pre-treatment, the economic characteristics of the countries that compose the synthetic control remain similar after the treatment. For this reason, the evaluation of the policy is made only in 2007, the only year that this hypothesis seems to be supported.

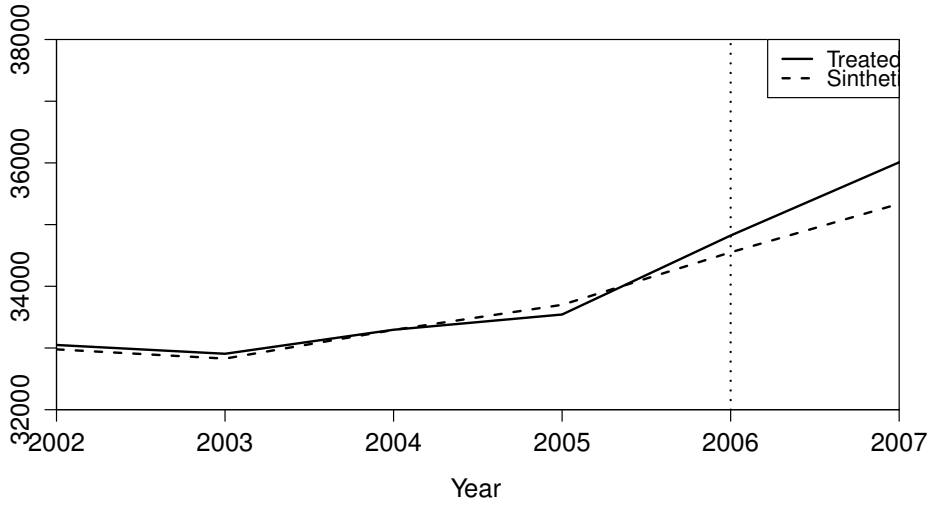
It is difficult to argue that no coordination of the economic policies facing the 2008 crisis provoked relative changes between the control countries and how this was treated before and after the crisis. For this reason, this empirical analysis does not propose the long term impact analysis of the policy, nor the comparison between the possible effects in these different temporary horizons. In the short term, the results are coherent with prior studies that use simulations to try to understand the effects of economic fiscal devaluation.

Even though the policy implemented in Germany showed efficiency during the analyzed period, this same policy can have its implementation limited or impeded in other economies. As cited by Koske (2013), countries such as Hungary, Norway, Sweden, Denmark and Iceland possess value-added tax levels in the 25% range. A possible policy of fiscal devaluation in these countries, which would need to elevate these percentages, does not seem feasible from an economic or political point of view.

Furthermore, the combination of elevated value-added tax and the reduction of the employer

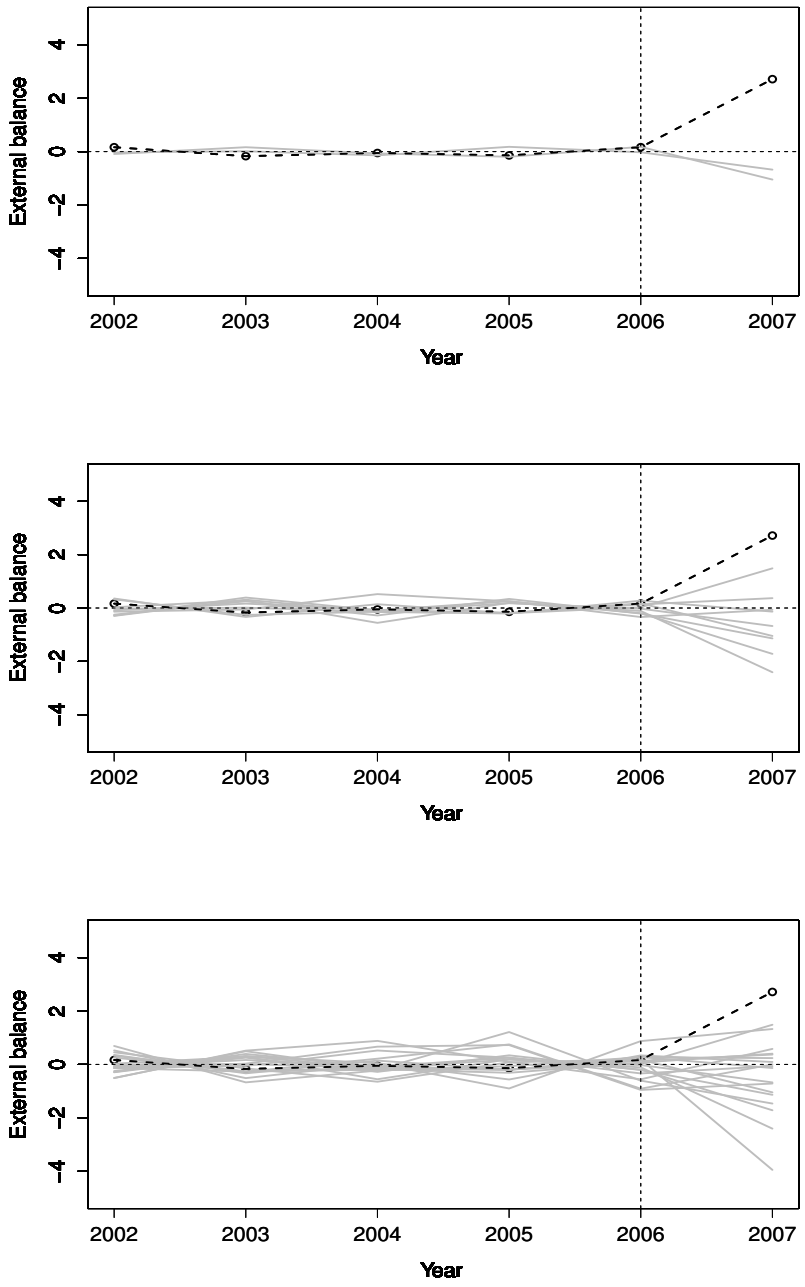
social contributions seems to be adequate for the economic stimulus even in countries where the realization of the devaluation of the nominal exchange rate is possible. The implementation of this devaluation, combined with the appropriate changes of the fiscal devaluation, could enhance the impact of the increased domestic product competitiveness in international markets.

Figure 1.3 Germany GDP per Capita: Treated X Synthetic



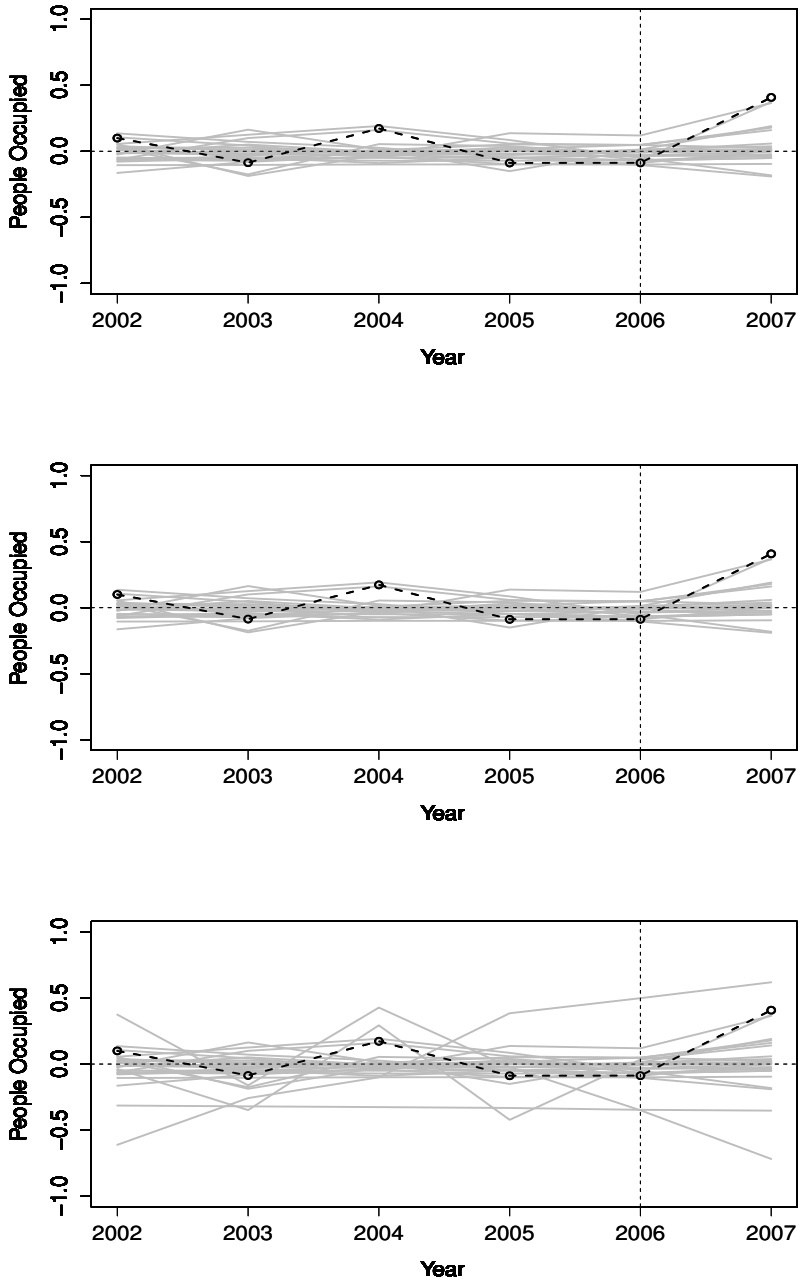
Source: Prepared by the author.

Figure 1.4 Placebo tests for the variable External Balance discards control units with pre-Policy MSPE: (a) twice as high as than Germany's; (b) five times as high as than Germany's; and (c) twenty times as high as than Germany's.



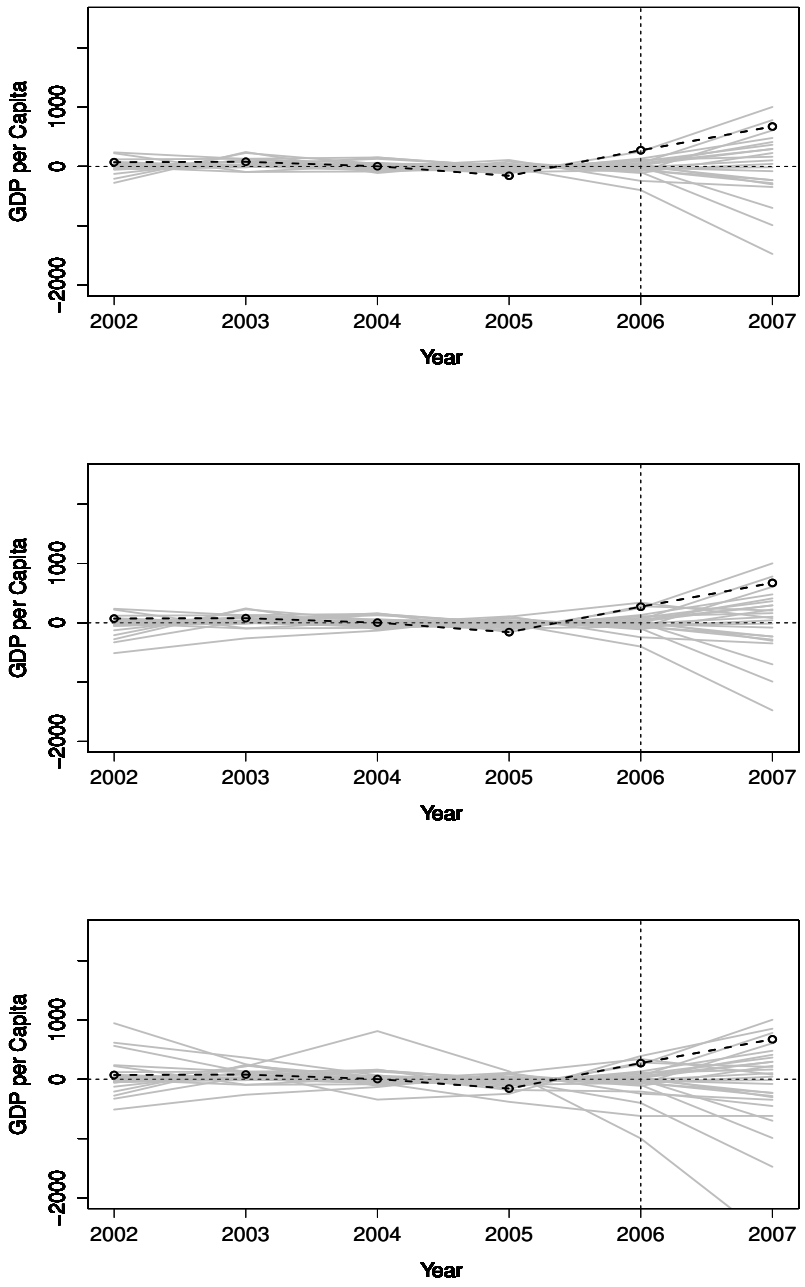
Source: Prepared by the author.

Figure 1.5 Placebo tests for the variable Occupied People discards control units with pre-Policy MSPE: (a) twice as high as than Germany's; (b) five times as high as than Germany's; and (c) twenty times as high as than Germany's.



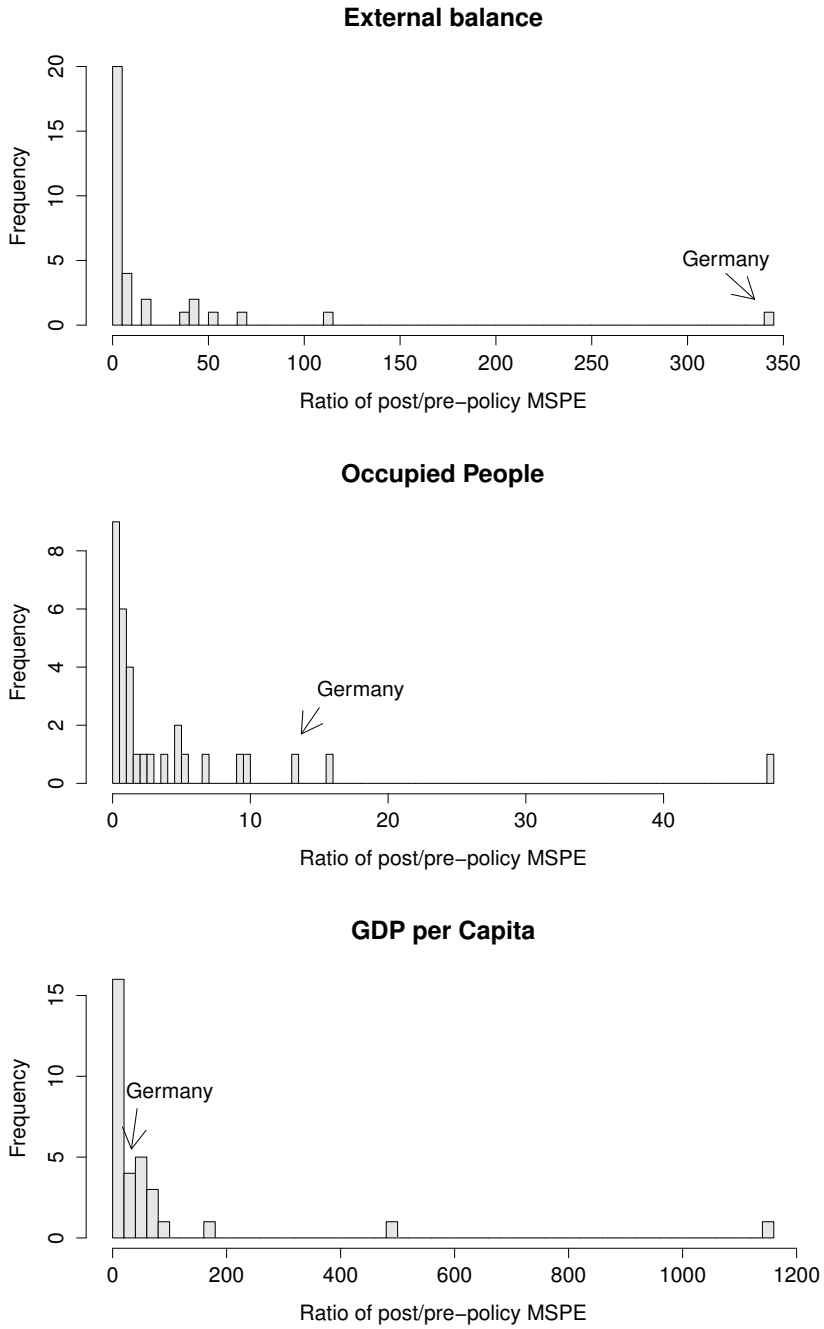
Source: Prepared by the author.

Figure 1.6 Placebo tests for the variable GDP per Capita discards control units with pre-Policy MSPE: (a) twice as high as than Germany's; (b) five times as high as than Germany's; and (c) twenty times as high asthan Germany's.



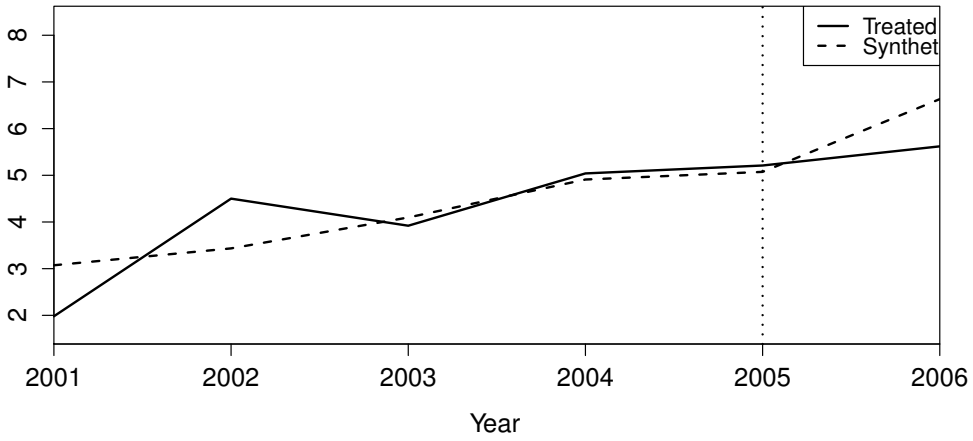
Source: Prepared by the author.

Figure 1.7 Distribution of the pre-policy MSPE under post-policy MSPE ratio of Germany and control countries: (a) External Balance; (b) Occupied People; and (c) GDP per Capita.



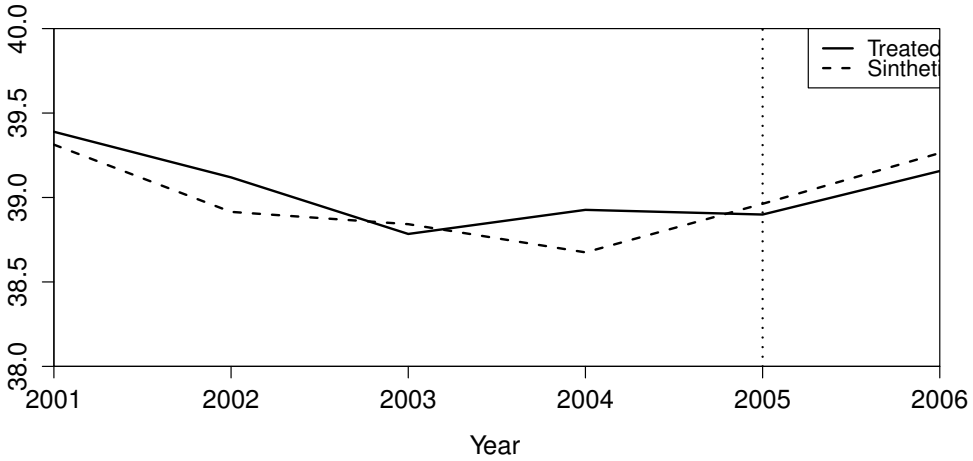
Source: Prepared by the author.

Figure 1.8 Placebo Test: External Balance



Source: Prepared by the author.

Figure 1.9 Placebo Test: Occupied People



Source: Prepared by the author.

CHAPTER 2

Electoral Competition and Local Government Responsiveness in Brazil: A Reassessment

2.1 Introduction

This paper reopens the discussion conducted by Arvate (2013) about electoral competition and government responsiveness in Brazilian municipalities. In his analysis, the author wanted to find evidence on the common sense idea that electoral competition leads to the growth of public goods supply, an idea shared by economists and political scientists. After considering the possible challenges involved in conducting an empirical analysis to find evidence about this topic, the author sets an empirical strategy based on an Instrumental Variable (IV) candidate and estimated by two stage least squares (TSLS). According to the results revealed in this paper, the electoral competition (number of candidates for mayor) increases the supply of public goods in the Brazilian municipalities.

I argue that the study conducted by Arvate (2013) based its analytical approach on a misinterpretation of the Brazilian law. He used an electoral rule (which sets the number of members of the local legislative branch proportionally to the population of the municipality) as an IV candidate to conduct the empirical estimation. However, this electoral rule came into effect in 2009, and the period of analysis considered the municipal elections of the years of 1996, 2000 and 2004. This misunderstanding compromises the results and conclusion of the paper, which were achieved based on an inadequate choice of an Instrumental Variable.

In order to understand Arvate's (2013) misinterpretation of the electoral rule and suggest

its correct use as an IV candidate, it is necessary to understand the recent changes of the rule that fix the number of local legislators in Brazilian municipalities. During the past years, the number of seat councilors in the municipal legislative branch in Brazil has been in discussion in the different government institutions and has been modified by the National Congress and Supreme Electoral Court (Tribunal Superior Eleitoral - TSE), following a decision of the Supreme Court (STF - Supremo Tribunal Federal) about the same issue. Up to the election of 2000, the Brazilian federal constitution fixed three ranges of local councilors' numbers proportionally to the size of the population.[1] In 2004, before the municipal elections of that year, the STF judged the case of the municipality of Mira Estrela, in the state of São Paulo, that fixed the number of councilors in its organic law disregarding the proportionality criteria of the federal constitution (the municipality had only 2,700 inhabitants and had fixed 11 councilors, two above the minimum number, which should have been applied). After identifying that 44% of the municipalities were not respecting the proportionality norms of the constitution, the STF determined in 2004 a specific rule composed of an arithmetic proportionality based on what was determined in the constitution.[2] Since the constitution establishes three ranges of the number of councilors by ranges of population, the STF determined the arithmetic proportionality based on these proportions. Therefore, municipalities with up to 1 million inhabitants that, according to the original text of the constitution, should have from 9 to 21 councilors, would have the addition of one councilor per $\frac{1{,}000{,}000}{21} \approx 47619$ inhabitants. The same idea was used to calculate the proportion of the number of inhabitants per councilor in the municipalities that have 1 to 5 million and more than 5 million inhabitants (subsection IV of the article 29 of the constitution). Table 20 in the appendix shows all of the ranges of population and the respective number of councilors.

[1]The subsection IV of the article 29 of the Brazilian federal constitution in 2000 read: "the number of councilors is proportional to the population of the municipality, taking into consideration the following: a) from 9 to 21 in municipalities with up to 1 million inhabitants; b) from 33 to 41 in municipalities with 1 to 5 million inhabitants; c) from 42 to 55 in municipalities with more than 5 million inhabitants".

[2]The complete decision of the STF can be found on - `http://www.stf.jus.br/arquivo/informativo/documento/informativo341.htm`

Following the decision made by the STF, the TSE decided in the same year (2004) that the municipal election of that year would elect the number of councilors established in this new proportion (table 20).[3] The main effect of this intervention was the decrease in the number of local councilors. In 2000, there were 60,317 councilors seats in Brazilian municipalities. Because of what was determined by the TSE, in 2004 there were only 51,838 councilors seats (a decrease of more than 14%). The same rule applied in the election of 2004, which was adopted by the TSE in the following municipal election of 2008.[4] In this election, about the same number of local councilors seats were maintained in comparison to the previous election. It is important to highlight that the proportion of councilors according to the population of the municipality is an upper limit. It means that municipalities, through their organic laws, can fix a lower number of councilor in their municipalities, but they cannot extrapolate this limit.

Up to the election of 2008, the electoral rule referred to by Arvate (2013) was not in effect. He argues that the Brazilian constitution determined different numbers of councilors for different bands of inhabitants, from 15,000 to 8 million, varying from 9 to 55 councilors. As I presented previously, this is not true and the bands in force for the elections of 2000 and 2004 were different from the one mentioned in Arvate's paper. The electoral rule he mentions only came into effect in 2009 due to a constitutional amendment.[5]

This review of the recent changes of the electoral rule that defines the number of local legislative members of the municipalities in Brazil is central in the decision of the empirical approach. As I pointed out, the evidence on the electoral competition and government responsiveness is not convincing. In the period of the elections of 2000, Arvate (2013) argues that there is a stable relationship between the number of seats and the local population. I argue that this is not correct because, once the STF and STE, in 2004, enforced the reduction of seats of the legislative members in municipalities in Brazil because they were not respecting the federal

[3] Resolução TSE n.21.702/2004. http://www.tre-sc.jus.br/site/legislacao/resolucoes/tse/2004/resolucao-tse-n-217022004/.

[4] http://anexos.datalegis.inf.br/tm/RES22823_08_TSE.pdf

[5] https://www.planalto.gov.br/ccivil_03/constituicao/emendas/emc/emc58.htm

law, and it is possible to consider that there is a stable relationship starting in 2004. This is very important because Arvate uses two dummy variables as an IV in his empirical approach to separate municipalities in three groups: i) small district size, when a municipality has less than 10 seats available for council candidate disputes; ii) medium size district for municipalities with between 11 and 30 seats available and iii) large district size, when a municipality has 31 or more seats available for disputes. The small district size dummy variable is not considered in the estimation process. Based on the scenario characterized by breaking the law that defined the relationship between the number of inhabitants and the number of local legislative members in 2000, I argue that there is no stable distinction between the three groups of districts represented by the two dummy variables. Moreover, in his paper the author does not shows evidence that municipalities adopted the correct number of seats determined by the law. Comparing the population and the number of seats, I find that many municipalities did not adopt any electoral rule, but just implemented the number of seats as was decided up to the year 2000.

According to Arvate (2013), more seats in the legislative municipal branch are a good fit for future candidates for mayor. He argues that to be a candidate for mayor is a natural career path for councilors (at least for those who wish to advance in their political careers). This means that the number of seats in a municipality in the previous election influences the number of mayoral candidates in the following election. Arvate (2013) used the number of seats in the municipal election of 1996 as the key variable to determine the number of candidates for mayor in the municipal election of 2000. In that sense, beginning in the elections of 2004, when the municipalities adopted the number of councilor seats in proportion to their population, I look for a new strategy to validate Arvate's idea that more councilor seats influence the number of mayoral candidates (more electoral competition) and that it causes an increase in public goods.

After the identification of the misinterpretation of the legislation and the lack of evidence on the paper's main question, I propose an appropriate estimation approach to answer this question. First of all, I propose an analysis based on the data of the elections of 2004 and

2008, when municipalities practiced the legal determination of the number of councilors. This means that the number of councilor seats in the legislative branch in 2004, according to the idea presented in the original paper, would influence the number of mayoral candidates in the elections of 2008. I suggest an IV approach aligned with the idea presented by Arvate (2013), but with a different empirical approach, which I consider to be more relevant.

Instead of separate municipalities in three different groups (small-, medium- and large-size districts) and instead of running the TSLS estimation (using control variables of the census of 2000, which would not be possible for the period of elections of 2004 and 2008) as Arvate did, I suggest a sharp Regression-Discontinuity (RD) design. In this context, the RD approach takes advantage of the arbitrary cutoffs created by the electoral rule. The law determines that, in many cases, municipalities with a similar number of inhabitants (and overall characteristics) are assigned to different numbers of councilors. This is true when municipalities are lying closely in either side of the cutoff. By normalizing every cutoff to zero, the left side (negative values) is composed by a group of municipalities with less councilors than the group of municipalities lying on the right side. Thus, I can compare municipalities with similar characteristics as if their number of councilors were randomly assigned, once these municipalities cannot control their population size and, consequently, the number of local legislative members, when they are located around the cutoffs. Therefore, it is possible to estimate the local average treatment effect (LATE) through the comparison of these two groups, of the number of councilors over the number of candidates for mayor (electoral competition) and the supply of public goods.

In his paper, Arvate conducts a brief literature review about studies which analyzed the relationship of the electoral competition and increase of the supply of public goods. As he stated, at the local level, there are not conclusive empirical evidences about this relationship. The evidence brought to the literature by other studies are divergent to each other. For example, Chhibber and Noorudin (2004) presented evidences about local government responsiveness and political competition in Indian states. In this studies, the authors based their analysis on

macroeconomics and postelection voter surveys data. They concluded that in localities where the two-party competition system provides more public goods than the multiparty system. In some sense, this result is contrary to the results finding by Arvate, once, in this case, less political competition leads to more public goods. Besides this study, Cleary (2007) concluded that in Mexican municipalities electoral competition does not increase the supply of public goods. He argues that the supply of public goods is affected by the cooperation between political leaders and their constituents, and the engagement citizenry. Besley, Persson, and Sturm (2007) did not find evidences about the impact of the electoral competition over the government efficiency in the south of the United States and, finally, Ashworth, Geys, Heyndels, and Wille (2010) concluded the the electoral competition reduces government inefficiency in a local government in Belgium.

After the mention of these previous results in different contexts, it is possible to conclude that the effects of the electoral competitions over the supply of public goods at the local level are highly dependent of the political scenario and the socioeconomics conditions of each locality. So, the results of my analysis are not dedicate to make generalization and are only intended to understand the Brazilian case. The results were estimated based on two different RD approaches: local-polynomial and local-randomization. The results showed evidences that the conclusion of the Arvate's (2013) study are not correct. There is no evidence of an increase in the number of effective candidates in municipalities with more councilor seats. Contrary to that, some findings in this analysis showed evidence that municipalities with more seats in the legislative branch have less mayoral candidates. This is possibly true because of the party coalition system in Brazil, which can encourage parties to form more coalitions composed by as many parties as possible in larger municipalities. The results showed no evidence of increase in public goods supply in municipalities with larger legislative bodies.

Besides this introductory section, this paper is structured in four different sections. In the next section, I present the empirical strategy that I suggest to conduct the empirical analysis to

achieve meaningful findings for Brazilian municipalities. In the third section, I describe the data used to estimate the results. In the fourth section, I show the the empirical findings generated in my estimations. In the last section, I present my conclusions and final considerations about my analysis.

2.2 Empirical Strategy

As mentioned in the introductory section, I use a sharp RD design to estimate the effect of the number of councilor seats to dispute in a municipality (an indicator of the electoral competition for mayor) over the supply of public goods (and the effective number of candidates for mayor). In this context, I use the number of inhabitants of the municipality and the decision of the TSE, which defined specific numbers of councilor seats for different ranges of number of inhabitants, as an instrumental variable. I want to compare the different groups of municipalities, which have different numbers of councilor seats.

Thus, I consider as the cutoff the minimum number of inhabitants to a municipality that have an additional seat disputed in the legislation branch (all cutoff values are presented in table 20). The approach can either estimate the effect in each cutoff or homogenize the treatment when I normalize the score variable. In this case, all of cutoff values are normalized in a single one and assume the value of zero. Negative values are the representation of municipalities that have one seat less than the number of seats of the closest cutoff and positive values are the representation of municipalities with the same number of seats as the one attributed by the cutoff.

This means that the assignment status of the additional seat is a discontinuity function of the population (x_i) of the municipality i. Let D_i be this function, which assumes a value equal to 1 when an additional seat is assigned to a municipality and 0 when no additional seats are

assigned. This can be represented by

$$D_i = \begin{cases} 1 \text{ if } x_i \geq x_0 \\ 0 \text{ if } x_i < x_0 \end{cases} \tag{2.1}$$

where x_0 is the cutoff (minimal number of inhabitants to have an addition seat in the legislative branch).

As described by Angrist and Pischke (2008), the sharp RD idea can be formalized by the model below. Apart from the assignment function described in equation 3.3, the relationship between the level of the public good (Y_i) and the number of inhabitants (x_i)

$$E[Y_i|x_i] = \alpha + f(x_i). \tag{2.2}$$

The average treatment effect that I want to estimate is denoted by $\rho = Y_{1i} - Y_{0i}$. Combining this equation with equation 2.2, the amount of public goods (or the number of effective candidates for mayor) of a municipality i in this context can be described as

$$Y_i = f(x_i) + \rho D_i + \eta_i. \tag{2.3}$$

It is worth highlighting that $D_i = 1(x_i \geq x_0)$, the regressor of interest, is a discontinuous function around the cutoffs (the number of inhabitants established by the TSE in which a municipality can have an increase in one councilor seat in the legislative branch) which, besides being correlated to x_i, is a deterministic function of x_i (population of the municipality i). Under the assumption that the function $f(x_i)$ is continuous around the cutoffs (x_0), the discontinuous jump of it in x_0 can be used to measure the treatment effect (ρ). This local-polynomial approach is suitable to fit the relationship between these variables for different orders of polynomial.

The validity of the RD estimate relies on possibility that the polynomial model will offer a good fit of $E[Y_i|x_i]$. To increase the likelihood that the polynomial model will provide a good fit

for it, the RD design considers only the information around the cutoff (discontinuity). Consider the cutoff x_0 and a small number of inhabitants δ, the interval that the RD uses to estimate this fit is $[x_0 - \delta, x_0 + \delta]$. So, the effect that I want to identify can be described by

$$\lim_{\delta \to \infty} E[Y_i | x_0 < x_i < x_0 + \delta] - E[Y_i | x_0 - \delta < x_i < x_0] = E[Y_{1i} - Y_{0i} | x_1 = x_0]. \quad (2.4)$$

Although some studies using the local-polynomial RD approach argue that their results can be interpreted as a local randomized experiment, this interpretation is not correct. Cattaneo, Frandsen and Titiunik (2015) present conditions under which the local randomized experiment interpretation in the context of the RD approach is justified. In addition, in that study, the authors develop inference procedures based on these conditions. This RD estimation method is known as the RD local-randomization approach and it can be used as a robustness check for the local-polynomial approach.

Two important problems when considering the local-polynomial approach as an experiment are that population (score variable in this context) is not a uniform random variable across municipalities and the cutoff cannot be manipulated to ensure a given probability of treatment (it is set by the determination of the TSE, in proportion with the original text of the constitution). Basically, the local-randomization approach has two steps: i) choose a small window around the original cutoff; and ii) use randomization inference tools for a hypothesized treatment assignment.

I consider this quasi-experiment approach more suitable than the methodology applied by Arvate (2013) to generate evidence on the main question of this study. Besides the fact that I use the correct electoral rule, the RD design is intuitively more precise when generating evidence if $\rho \neq 0$. Moreover, I use two different approaches of RD design to ensure the robustness of the findings.

2.3 Data

In the elections of 2000, 44% of the municipalities in Brazil broke the electoral law and elected more councilors than determined by law. As a matter of fact, the elections of 2000 was characterized by the municipal election in Brazil as having the most councilor seats ever disputed in the history of Brazil[6]. This distortion, as explained in the introductory section, was corrected by the TSE, following the decision of the Supreme Court on that issue. These facts are key in the determination of the period of the analysis that I suggest. Because of this problem related to the elections of 2000 and the data available so far, I use the information of the elections of 2004 and 2008 to conduct the analysis of this paper.

I use information about the municipalities from different sources of the Brazilian government. The first source is the TSE, which provides access to information of the elections for 2000 and 2004, such as the number of councilor seats being disputed and the number of effective candidates for mayor in each municipality. The information on population by municipality is provided by the IBGE (*Instituto Brasileiro de Geografia e Estatística*), the main institution in charge of statistics in the country. This information was collected in the municipal census of 2000 and 2010, population count in 2007 and population estimate elaborated by the IBGE for the remaining years.

Arvate conducts a discussion in his paper about the choice of the outcomes used as information of the supply of public goods in municipalities. He presents evidence that education and health are the most important areas in Brazilian municipalities, in terms of the amount of resources spent. In reference to this information, Arvate selects two outcomes in the education and one in the health area. The outcomes in education are: i) the number of student enrollments in municipal elementary schools, and ii) the number of teachers in municipal elementary

[6] http://www12.senado.leg.br/jornal/edicoes/2012/11/13/numero-de-vereadores-cresce-na-eleicao-mais-disputada-da-historia

schools per 10,000 inhabitants. This information is made available by the Ministry of Education of Brazil (*Inep - Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira*) and it is gathered by the elementary school census. The outcome in the health area is the number of free immunizations per 100,000 inhabitants (as highlighted by Arvate, this is "one of the measures of health service coverage established by the World Health Organization"). This information is made available by the Ministry of Health (*SUS - Sistema Único de Saúde*).

According to Brazilian law, the elementary education system is managed by the municipalities. Thus, it makes sense to consider the two education indicators as proxies of the municipal supply of public goods. However, an additional information on the health indicator that I consider worth highlighting is regarding the effective participation of the municipality government in the supply of the free immunizations. These immunizations are provided in each municipality through the joint effort of the federal union states and the municipalities. It is called a tripartite system, which means that the financial resources come from other levels of government, besides the municipal. However, the efforts conducted by the municipality government to ensure that the inhabitants receive free immunization are key in this process.

2.4 Results

As recommended by Cattaneo, Frandsen and Titiunik (2015), I use the RD design based on the local-polynomial approach to achieve the main empirical findings in this paper and the local-randomization approach as a robustness check. For estimation, I normalize all of the scores (number of inhabitants) so that the cutoff is zero for all municipalities. Then, I homogenize all of the information as a single running variable and run estimations. In addition to that, an estimation check around the cutoff where municipalities are assigned to 9 seats of councilors in municipalities (values below zero) and 10 seats (values above zero), showing consistency

with the results of the estimation with the homogenized cutoffs. This is a key cutoff for the information of the whole country because the Brazilian municipalities are mostly characterized by a small number of inhabitants. In the elections of 2004, more than 95% of the municipalities were assigned 9 or 10 seats to dispute in the legislative branch.

The first step when using the RD approach is to test if there is any evidence of systematic manipulation of the running variable. In this case, the test checks, for example, if hypothetically the local representatives manipulated the information of inhabitants to achieve a higher number of seats in the legislative branch. I do not expect any evidence manipulation of the population because this variable is not self-reported by the local representatives. The number of inhabitants is accounted for by the IBGE. Moreover, the decision of the TSE in 2004, when this Court determined the stricter rule which established the number of seats for exact ranges of inhabitants, it also determined that the number of inhabitants would be in reference to the population estimate of the IBGE of 2003. This estimation was the result of a clear methodology adopted by the TSE based on the information of the Census of 2000. In spite of the way the number of seats in the legislative branch was determined from 2004 onwards, it does not seem to allow any manipulation, I still test this hypothesis. To conduct the manipulation test, I use the test suggested in Cattaneo, Jansson, and Ma (2016). According to the author, this test exhibits better size properties than conventional approaches used in the literature. Table 2.1 shows the results of this test. As expected, no evidence of any systematic manipulation is found.

In his analysis, Arvate found, in his first stage of results, evidence of positive significant relationships between the number of councilor seats in a municipality and the number of effective candidates for mayor in the following election. This means that he found evidence that more seats in the legislative branch tend to increase the electoral competition (in the executive branch). To check that finding, I first estimate the correlation between the effective number of candidates (in the election of 2008) and the effective number of councilor seats of the municipality in the prior election of 2004. These results are controlled by the population and they

are shown in table 21 in the Appendix. The results were estimated via quantile regression estimation because this method is more robust against outliers, which are highly expected in an analysis of all of the Brazilian municipalities with poor controls than the least squares method. The results showed a negative correlation between the number of candidates and the number of seats in the local legislative body, contradicting what is expected and found by Arvate (2013). The results were also estimated for the combination of the number of effective candidates in the elections of 2004 and 2008 and the results are robust. Table 22 in the Appendix shows the same analysis for municipalities with less than 40 seats in the legislative branch (more than 99% of the municipalities) using an OLS model and the results showed the same negative correlation. The same OLS regression using all of the municipalities showed a positive correlation, indicating a bias caused by the few municipalities with a high number of inhabitants(table 23).

As these results suggests that, on average, municipalities with more local-councilors seats (consequently, municipalities with more inhabitants) have less mayoral candidates running for elections, they may suggest that in larger municipalities parties tend to have predilection of being part of political coalitions formed by as many political parties as possible. This is due to the fact that Brazil is characterized by a political system composed of many political parties and because of the higher potential electoral benefits associated with coalitions composed by a higher number of parties, such us more time of free elections campaign broadcasts at radio and TV and higher agglomeration of financial resources in coalitions with more parties. So, this setup may suggest that municipalities with more councilors seats have also less political diversity in terms of more elected mayors aligned with more elected local-councilors in larger municipalities. To check this hypothesis, I estimated the relationship between the number of elected local-councilors aligned with the elected mayor versus the number of local councilors seats by municipality. The table 24 presents the results of the estimation of this data for the mandate of 2009-2012. This result showed a positive elasticity between the percentage variation between these variables.

Using the RD approach to analyze the effect of the number of seats in the prior election over the number of effective mayoral candidates in the following election, I find some evidence that there was a negative relationship between these variables. The results based on the local-randomization approach showed significance for polynomial or orders 2 and 3. These results are shown in table 26. However, the results based on the local-polynomial approach did not present the same evidence (table 25).

To characterize the results of each political mandate, I use the average of the four-year mandates (2009-2011) of the outcomes (number of students, number of professors and the number of free immunizations) that use the information of the supply of pubic goods to conduct the estimations. Tables 2.2, 2.3 and 3.13 present the results of the estimation for all of these three outcomes. The results were estimated based on the local-polynomial approach using different polynomial orders and different types of Kernel functions.

The estimations based on the local-randomization approach adopt a different bandwidth selection as described in Cattaneo, Jansson, and Ma (2016). In my analysis, different variables were considered for the estimation of the bandwidth and they showed consistency for the interval of $(-0.28, 0.28)$. The results for the three variables that are used as proxies for the supply of public goods are presented in tables 2.5, 2.6 and 3.14. They showed consistency with the results based on the local polynomial approach, showing no significant impact at the confidence level at 95%.

2.5 Conclusion

First of all, after the revision of the recent changes in the Brazilian legislation, I conclude that the evidence presented by Arvate (2013) is not precise. I presented the facts showing that in the period that he conducted his analysis there was no stable relationship between the number of

Table 2.1 RD Manipulation Test

Method	T	P>|T|
Conventional	0.1775	0.8591
Undersmoothed	-0.7191	0.4721
Robust Bias-Corrected	-0.5635	0.5731

Source: Prepared by the author.
Note: The total numbers of observation used in the test are 469 and 350, on the left and right side, respectively. The effective numbers are 227 and 193, on the left and right side, respectively.

Table 2.2 Free Immunizations per 100,000 inhabitants: Local-Polynomial Approach

	P = 1	P = 2	P = 3
FI_{LATE}	2035.7	2880.8	3555.7
Robust P-Value	0.401	0.295	0.347
Left Bandwidth	-0.098	-0.123	-0.139
Right Bandwidth	0.098	0.123	0.139
Kernel	Triangular	Triangular	Triangular
FI_{LATE}	1911.5	4296.8	3084.2
Robust P-Value	0.516	0.167	0.388
Left Bandwidth	-0.093	-0.103	-0.116
Right Bandwidth	0.093	0.103	0.116
Kernel	Uniform	Uniform	Uniform
FI_{LATE}	2315.5	3128.4	3372
Robust P-Value	0.385	0.263	0.389
Left Bandwidth	-0.083	-0.117	-0.130
Right Bandwidth	0.083	0.117	0.130
Kernel	Epanechnikov	Epanechnikov	Epanechnikov

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 469 and right side 350.

Table 2.3 Number of Teachers per 10,000 inhabitants: Local-Polynomial Approach

	P = 1	P = 2	P = 3
NT_{LATE}	9.2743	-14.38	-25.674
Robust P-Value	0.924	0.529	0.489
Left Bandwidth	-0.109	-0.116	-0.169
Right Bandwidth	0.109	0.116	0.169
Kernel	Triangular	Triangular	Triangular
NT_{LATE}	10.563	-9.7189	-20.124
Robust P-Value	0.738	0.600	0.637
Left Bandwidth	-0.087	-0.101	-0.126
Right Bandwidth	0.087	0.101	0.126
Kernel	Uniform	Uniform	Uniform
NT_{LATE}	-1.6156	-13.847	-27.486
Robust P-Value	0.705	0.550	0.465
Left Bandwidth	-0.081	-0.105	-0.156
Right Bandwidth	0.081	0.105	0.156
Kernel	Epanechnikov	Epanechnikov	Epanechnikov

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 469 and right side 350.

Table 2.4 Number of Students per 10,000 inhabitants: Local-Polynomial Approach

	P = 1	P = 2	P = 3
NS_{LATE}	52.979	-71.145	-120.21
Robust P-Value	0.780	0.656	0.625
Left Bandwidth	-0.130	-0.124	-0.180
Right Bandwidth	0.130	0.124	0.180
Kernel	Triangular	Triangular	Triangular
NS_{LATE}	16.922	47.728	-61.215
Robust P-Value	0.966	0.943	0.875
Left Bandwidth	-0.089	-0.155	-0.130
Right Bandwidth	0.089	0.155	0.130
Kernel	Uniform	Uniform	Uniform
NS_{LATE}	26.498	-69.459	-149.17
Robust P-Value	0.970	0.678	0.480
Left Bandwidth	-0.112	-0.113	-0.179
Right Bandwidth	0.112	0.113	0.179
Kernel	Epanechnikov	Epanechnikov	Epanechnikov

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 469 and right side 350.

Table 2.5 Free Immunizations: Local-Randomization Approach

	P = 1	P = 2	P = 3
FI_{LATE}	3359.550	5230.612	-4832.920
P-Value (Large Sample)	0.423	0.414	0.510
Kernel	Triangular	Triangular	Triangular

Source: Prepared by the author.

Note: The window selected is equal to (-0.028, 0.028). These values were estimated through the `rdbwselect` method. The numbers of observation on the left side and the right side are 469 and 350, respectively.

Table 2.6 Number of Teachers: Local-Randomization Approach

	P = 1	P = 2	P = 3
NT_{LATE}	-5.400	28.502	161.973
P-Value (Large Sample)	0.919	0.739	0.069
Kernel	Triangular	Triangular	Triangular

Source: Prepared by the author.

Note: The window selected is equal to (-0.028, 0.028). These values were estimated through the `rdbwselect` method. The numbers of observation on the left side and the right side are 469 and 350, respectively.

Table 2.7 Number of Students: Local-Randomization Approach

	P = 1	P = 2	P = 3
NS_{LATE}	135.796	418.944	817.691
P-Value (Large Sample)	0.692	0.417	0.141
Kernel	Triangular	Triangular	Triangular

Source: Prepared by the author.

Note: The window selected is equal to (-0.028, 0.028). These values were estimated through the `rdbwselect` method. The numbers of observation on the left side and the right side are 469 and 350, respectively.

inhabitants and the number of councilor seats in dispute. This is crucial, as the author argued that in order to conduct an IV analysis and make conclusions about municipalities in Brazil.

Based on this misread of the Brazilian law, Arvate concluded that there is evidence that more seats in dispute in the local executive branch leads to an increase in the number of effective mayoral candidates in the following election and the increase of the supply of public goods (represented by the number of elementary students, professors and free immunizations in a municipality). Precisely, Arvate concluded that one effective mayoral candidate increased the number of student enrollment by 18.24%, the number of teachers by 0.9% (both per 10,000 inhabitants), and the number of free immunizations by 20.47% per 100,000 inhabitants.

However, based on the methodology that I suggest, using the period of analysis in which the electoral rule could be used as an IV, I find different conclusions. First of all, there is evidence that the number of councilor seats is negatively related to the number of effective candidates for mayor in the following election (less electoral competition). This is contrary to what Arvate found. In addition to that, I do not find any evidence that municipalities with more seats are associated with municipalities with more supply of public goods. These findings are aligned with other studies refereed to by Arvate and mentioned in the introductory section of this paper, which showed no evidence of an increase in government responsiveness because of the electoral competition. These findings were achieved based on the RD methodology with the local-polynomial and local-randomization approaches that provide reliable estimations with results that are easily interpreted. Moreover, another relevant is presented on table 24, as presented on previous section. This result shows more evidence on that in larger municipalities political parties tend to group in larger political coalitions, in term of number of political parties. This fact has clear implications on political governance, as government branches tend to align in larger municipalities.

Another point mentioned in the analysis of Arvate is regarding the Brazilian political scenario. He says that there are few barriers to the entry of competitors in elections, which would

indicate that individuals could easily make decisions on their participation in the electoral processes. I argue that this is not right. Brazil is characterized by a multiparty system with many local political coalitions. This means that the decision of who the mayoral candidates will be is strategically decided among the coalition parties, which tend to form coalitions with as many parties as possible (for example, coalitions with more parties have more free time for political propaganda broadcast in radio and transmitted on television in Brazil). Further evidence on the argument that there are few political barriers in Brazil is brought up by Carvalho da Rocha et al. (2013). They presented evidence on how expensive mayoral elections are in Brazil. Using the information made available by the TSE, the authors estimated that, on average, a mayoral campaign costs R$ 147,248.30 with a standard deviation of 716,033.41. In some sense, this evidence is also aligned with some findings of Bragança, Ferraz and Rios (2015). They found evidence that the self-perpetuation of relatives in Brazil's local government and that winning a close election in Brazil increases in almost 60% the likelihood of having a relative in office in the future. This shows that few groups and families dominate some local elections in Brazil. So, there are barriers in these elections, which can affect the electoral competition and the number of mayoral candidates.

CHAPTER 3

Effects of Political Alignment Between Government Branches: Evidence From Brazilian Municipalities

3.1 Introduction

In this paper, I provide evidence of the consequences of the political alignment between local government branches in Brazilian municipalities over the provision of public goods. Municipalities in Brazil are entities of the federation structured by their own organic law and with autonomy to make decisions on how to allocate public budget. Just like the Federal government and its states, the local government is divided into three branches, although decisions on public expenditure is made by the executive and legislative branches.[1]

One of the key questions regarding government's role is how its representatives allocate public resources. This allocation decision is key in politics, once parties look for electoral support distributing benefits to voters. In this context, the notion of benefits goes beyond the allocation of public goods and services taking different forms, such as public job offers, concession of public contracts, buying of votes, creation of laws targeting specific groups of interest and tax exemption.

In general, politicians allocate public resources in search of political support. Political in-

[1] According to the Brazilian Constitution, the executive branch is in charge of the elaboration of the budget plan (*Plano Plurianual*, *Lei de Diretrizes Orçamentárias* and *Lei Orçamentária Anual*) and the legislative representatives are responsible for approving it. The federal constitution also states that this approval must be determined by the absolute majority of votes of the official number of representatives in the legislative power.

stitutions seek to bind politicians' behavior to ensure political accountability on this allocation. These institutions try to avoid the incident of representatives' misuse of public allocation. However, there are some undesirable situations in which the public agent targets the allocation of benefits for specific groups of political supporters and/or potential political supporters only. In this situation, the public interest is set aside in favor of the personal or group political interests.

Some undesirable allocations are easily classified as illegal, such as the buying of votes and explicit corruption. Other types of allocation are difficult to identify as such, although they are undesirable. For example, the provision of pork barrel goods (public goods targeted to benefit a small group of individuals, while the costs are spread out to the society as a whole), clientelism and patronage. As described in the review by Golden and Min (2003), different papers provide evidence on the literature about the use of pork barrel goods, clientelism and patronage in different ways across countries.

In the context of our analysis, the study conducted by Samuels (2002) about distributive politics in Brazil is key. He provides evidence on how deputies in Brazil act in order to gain political support. The author finds evidence that shows that pork barrel policies are not effective in terms of electoral return to incumbents in Brazil. According to him, deputies do not trade pork barrel goods for votes. Instead, they trade money for votes and the way they raise money is through contributions from private companies that profit from government contracts. Complementary to this finding, Boas, Hidalgo and Richardson (2014) provide further evidence on the relationship between politicians and private companies in Brazil. They found evidence of financial return through the concession of public contracts for private companies that support the political campaign of elected deputies in Brazil. According to their study, companies specializing in public-works projects can expect an increase in the number of public contracts in 14 times the amount donated to deputies for political campaigns.

Corruption has considerable costs to society when it comes to policy implementation. Ferraz, Finan and Moreira (2012) provide evidence on the costs of corruption in Brazil with regard

to the provision of public goods. In their paper, the authors found that the reduction of federal education transfer funds, motivated as punishments due to the identification of irregular use of prior federal transfers.

Other relevant findings on the relationship between political performance and corruption in Brazil are brought up by Brollo and Troiano (2016). In their study, the authors show that, in Brazilian municipalities, the mayor's gender is linked to corruption. They found that male mayors are associated with more corruption than female ones. In the context of my analysis, the most relevant finding of their analysis is that mayors linked to more corruption events are those that perform better in the following election. Considering the previous studies, these findings highlight more evidence on electoral performance and corruption.

The institutional setup is key to bind politicians' acts and ensure accountability. Hence, it is relevant to carry out analyses that show evidence of the influences of the institutions on government types of policies and activities. In this same line, Ferraz and Finan (2011) find additional evidence on the effects of political institutions on corruption events. They found evidence that the mayors with reelection incentives misuse 27% fewer public resources than mayors without reelection incentives.

There is evidence in the literature about the importance of political alignment across federation units. According to Brollo and Nannicini (2012), there is evidence that the federal government representatives used federal transfers funds to support aligned mayors (from the same political parties and political coalitions) than mayors from different political groups. The authors explained that this effect comes from higher level of discretionary money transfers to municipalities aligned with the federal government. According to the authors, once the municipalities aligned to this coalition used to receive more infrastructure transfers by about 40% in pre-election years, these transfers could be understood as a political instrument to strengthen the president's coalition.

More evidence brought up in the literature by Brollo, Kaufmann and La Ferrara (2015)

supports the idea that political coalitions act in collusion across federation units. In this paper, the authors indicate the practice of strategic enforcement manipulation in the information of students' attendance in the municipal education system, which is key in keeping the eligibility to continue receiving the social benefit of *Bolsa Família*. The authors also provide empirical evidence on the loss of electoral support for mayors from the same coalition as the president's party.

In summary, Brollo and Nannicini (2012), and Brollo, Kaufmann and La Ferrara (2015), offer information on the costs related to the political collusion in the federal system of Brazil, while Ferraz and Finan (2011) bring to the literature findings on the influence of institutions on the political behavior. In this sense, our analysis seeks to explain what the influences of power coalitions and institutions are in relation to the local public finance in Brazil. Since the government power is divided into three different branches, I want to evaluate what are the effects of the political alignment between the executive and legislative branches. Ultimately, our analysis provides evidence on the importance of the oversight function of the legislative branch over the executive one.

In general, one could expect that the effects of government branches alignment can affect the provision of public goods in different ways. On the one hand, one can expect that this alignment is positive to the country because branches having similar ideologies could implement responsive policies. In addition, the alignment between government branches could avoid the economic costs of political bargain caused by the tendency of political groups to provide unconditional support to multiple political agendas with the purpose of bargaining support to their own interests. On the other hand, the alignment can affect the political accountability with a clear lower commitment of the legislative branch to oversee the executive branch.

To identify the effects of the alignment between the legislative and executive branches on fiscal policy, I propose a quasi-experimental approach based on the regression discontinuity (RD) design. Considering this methodology and the Brazilian electoral rules, I construct an

indicator that represents the margin of surplus votes for local councilors (the local legislative branch) in municipalities where the majority of local councilors are aligned with the elected mayor and the margin of lacking votes to have the majority of the local councilors aligned with the elected mayor (in municipalities where the mayor is not aligned with the majority of local councilors). This indicator scale shows how close a municipality in Brazil is aligned with the executive and legislative branches and how crucial it is to the RD design. In order to identify the exact effect of the political alignment on the municipal outcomes, I select in the estimation process municipalities that had different combinations of government branches in their previous mandates.

Because of the legal relationship between the executive and legislative branches, my analysis is focused on the evaluation of the public expenditure by function of municipalities in Brazil. I also look for evidence besides the expenditure. The results show that municipalities with alignment between government branches have lower expenditure in education, an important social expenditure that has a direct effect on the provision of public goods, particularly for low income individuals. The results also show that this reduction has impacts on the number of students' enrollment and teachers in municipal public schools. In addition, government alignment led to an increase in the number of public employees working in the indirect public local administration, a clear sign of patronage policy. The results also present evidence of increased expenditure in infrastructure functions and more corruption events in the group of municipalities where the mayor had the support of the majority of local councilors in comparison to municipalities where the mayor had the majority of local councilors as opponents.

This paper is divided into six sections, including this introduction. In the next section, I explain how the elections in Brazilian municipalities works. It is important to describe this, as these rules are the basis of the running variable used in the empirical approach. In the third section, I describe the data used in my analysis. In the fourth section, I present the empirical approach I used to conduct the research. I present the results in the fifth section of this paper

and in the last section I highlight the main conclusions.

3.2 Institutional Context

Brazil adopts a federal system that is divided into three levels: The federation, 26 states and a federal district, and 5,564 municipalities in 2005. The municipalities play an important role in the country because, according to the federal constitution of 1988, municipalities are members of the federation with the authority to decide on their policies and budgetary plans. Every 4 years, municipal elections are held to elect a mayor (as the representative of the executive branch) and the local councilors (representatives of the legislative branch). The number of local councilors is determined by the federal legislation in proportion to the number of inhabitants. Although during recent years many municipalities have been breaking the law and setting the number of local councilors higher than the proportion established by the law, since the elections of 2004 the federal court has enforced this law.

The mayor is the political agent that is responsible for the cities' executive functions, the taxes and public expenditure planning, and for ensuring the rule of law. As a complementary power, the local councilors in Brazilian municipalities have the role of supervising the local public administration; suggesting new local laws; debating with the executive branch on the public policies to be implemented through the multi-annual plan, budget guidelines and the yearly budget law; and auditing the financial public resources and the activities of the mayor and local councilors. Thus, the political alignment or non-alignment between the mayor and the local councilors is crucial to the decision-making of the local policies and expenditures.

Although mayors and local councilors run for office during the same election period, the electoral rule in Brazil adopts different voting systems to elect the executive and legislative authority members in municipalities. Both are elected for four-year terms. Four years later,

through the same democratic electoral system, mayors can get reelected for the next subsequent mandate once and local councilors can get reelected indefinitely. Mayoral elections are based on different electoral rules conditional on the number of registered voters in the municipality. The Constitution states that mayoral elections should be run under the single-ballot plurality rule in municipalities with less than 200,000 registered voters, while municipalities with 200,000 voters or more are eligible to dual-ballot plurality rule in case no candidate gets more than half of the valid votes.

The local councilors' elections adopts the proportional election system based on an open-list party.[2] Considering a high number of parties in the Brazilian electoral system, the open-list system is based on the coalition of parties. This systems relies on quotients to define the allocation of seats.[3] Based on the total number of votes for candidates for local councilors and the number of seats in a municipality, the integer part of the electoral quotient (EQ^i) determines the number of votes needed for each coalition to win one seat in the dispute in the municipality i. The EQ^i is defined by:

$$EQ^i = \frac{V^i}{Seats^i}; \tag{3.1}$$

where the EQ^i of the municipality i is the proportion between the total number of valid votes for local councilors in the municipality i (V^i) and the number of seats to dispute in the municipality i ($Seats^i$).

The number of seats that a coalition x in the municipality i wins is determined by the partisan quotient (PQ^i_x) defined by:

$$PQ^i_x = \frac{v^i_x}{EQ^i}; \tag{3.2}$$

[2] http://www.tse.jus.br/imprensa/noticias-tse/2016/Setembro/saiba-como-calcular-os-quocientes-eleitoral-e-partidario-nas-eleicoes-2016

[3] http://www.tse.jus.br/institucional/escola-judiciaria-eleitoral/revistas-da-eje/artigos/revista-eletronica-eje-n.-5-ano-3/como-funciona-o-sistema-proporcional

where v_x^i are the valid votes won by coalition x. The coalitions that have a total number of valid votes lower than the EQ^i win no seats. In the case where the sum of seats distributed to the coalitions is lower than the total number of seats, the remaining seats are allocated according to the quotient for each coalition x in a municipality i (Q_x^i), defined by:

$$Q_x^i = \frac{v_x^i}{V^i} \frac{Seats^i}{SC_x^i + 1}; \tag{3.3}$$

where v_x^i is the number of votes of the party coalition x of municipality i, $Seats^i$ is the total number of seats at municipality i, SC_x^i is the number of seats won by the party coalition x in municipality i and V^i is the total number of votes of municipality i of the local councilors. According to the rule, one seat is allocated at a time for the coalition with a higher quotient. Allocation rounds are repeated successively until all of the remaining seats are distributed. In each allocation round the quotient of the coalition that won a seat in a prior allocation is updated (as its number of seats SC_x^i increases) and a new comparison among the coalitions' quotients is made to determine the subsequent seat allocation.

3.3 Data and Methodology

To characterize the effects of branch alignment on policy implementation, local governance, and mismanagement, I assemble several information from different sources in a comprehensive data set. In this section I summarize our sources, key variables, and sample definitions.

Sources

All data used in this analysis was made available by official agencies of the Brazilian government. The period of analysis was carefully selected due to noncompliance of Brazilian

municipalities with the Federal Constitution that established the number of local councilors in proportion to the number of inhabitants up to 2004. On that year, after the realization that a considerable number of municipalities established higher numbers of local councilors than what was described in the Federal Constitution, a judicial decision by the Supreme Court in Brazil determined a more specific rule to set the number of local councilors in proportion to the number of inhabitants. The result of that was that municipalities in Brazil began to strictly comply with the Federal legislation during the mandate between 2005 and 2008.

Because of that, the period of analyzed considers the mandate from 2009 to 2012 versus the previous mandate (2005-2008), as I compare changes in mandates with different combinations of alignment between executive and legislative. This is important because the unbalancing in the number of local councilors in proportion to the number of inhabitants may affect the relationship between government powers. So far, there is not enough information to analyze the subsequent mandate from 2013 to 2016.

The information on local election results in Brazil is made available by the TSE (*Tribunal Superior Eleitoral*). The TSE is the Superior Electoral Court in charge of election processes. After every election, it provides a rich set of information on the candidates and the electoral results in each municipality. With this information I can identify the number of votes casted to each each candidate for mayor, legislative seats and the votes in each legislative coalition. Using this information I construct the running variable explained in the next section of this paper.

The financial information for each municipality is centralized in the FINBRA data-set, provided by the *Secretaria do Tesouro Nacional*, a secretary of the *Ministério da Fazenda* (equivalent to the Ministry of Finance). This information highlights the expenditure by function in each municipality. There are almost thirty different functions of expenditures and they are reported annually.[4] In addition, it highlights local government revenues and intergovernmental

[4]Legislative, executive and judiciary branches, defense, public security, external relations, social assistance, social security, health, labor, education, culture, rights of citizens, urbanism, housing, sanitation, environmen-

transfers, important resources for Brazilian municipalities.

The information on public employment by municipality is provided by the *Instituto Brasileiro de Geografia e Estatística* (IBGE). This information is also provided by types of employment in the direct and indirect local public administration. The information on the amount of federal transfers to the local public administration in which irregularities were found is generated and provided by the *Controladoria-Geral da União* (CGU). This is a federal institution in charge of audits carried out in municipalities in order to oversee the quality of use of federal transfer of funds and detect possible irregularities. Both sources of information are grouped annually.

The data on education by municipality is provided by the *Censo Escolar*, an annual census of the education in Brazil conducted by the Ministry of Education. Basically, I extract the information on the number of students enrolled, the number of teachers and the number of municipal public schools. I also use the information on the coverage of the local health system and mortality by municipality provided by *DATASUS* from the Ministry of Health.

Measuring the Effects of Inter-branch Alignment

The empirical approach carried out in this paper is based on a regression discontinuity method. In this context, the methodology takes advantage of Brazilian legislation, which determines that the budget plan proposed by the mayor has to be approved by the majority in the local legislative chamber. In that sense, I want to compare municipalities whose legislative chamber members aligned with the party of the elected mayor are barely the majority of the total local councilors with municipalities whose legislative chamber members are barely contrary to the party of the elected mayor. Therefore, as I compare groups of municipalities that are barely the same in terms of the number of local councilors aligned with the elected mayor, the difference in the total number of local councilors aligned with the party of the elected mayor between

tal management, science and technology, agriculture, agriculture organization, industry, commerce and services, communications, energy, transport, leisure and sports, and special expenditure.

these two groups of municipalities is defined by only one seat.

This means that, given the elected mayor, the assignment status of the political alignment or non-alignment in a municipality i between the executive and legislative branches is a discontinuity function of the margin of victory MV^i of the additional seat that defines whether the coalition of local councilors aligned with the party of the mayor is barely assigned. In the case where the coalition aligned with the mayor barely loses the majority of members in the legislative chamber, I call the margin of defeat of the seat that would have given the majority in the chamber as MV^i represented by negative values (the percentage of votes that a coalition should have won to win a seat and achieve the majority of the local councilors. Let D_i be the discontinuous function, which assumes a value equal to 1 when the minimum majority of the legislative chamber members is aligned with the party of the elected mayor and 0 when the number of local councilors aligned with the elected mayor's party is the closest to the majority in the municipality i. This is represented by:

$$D_i = \begin{cases} 1 \text{ if } MV^i \geq MV^0 \\ 0 \text{ if } MV^i < MV^0 \end{cases} \tag{3.4}$$

where MV^0 is the minimal margin of victory that a coalition of local councilors needs to win to have the seat that makes the coalition aligned with the mayor as the majority assigned to it.

Taking into consideration the electoral rules in Brazil, the measure of MV^i is not straightforward like the majority voting system. Since the legislative local elections in Brazil are based on the open-list party proportional system, the parties' coalitions dispute the seats depending on their total number of votes. This total number of votes defines the number of seats that each coalition wins after allocation rounds. As explained in section 3.2, quotient 3.3 determines the allocation of remaining seats. Based on this rule, I measure the margin of victory of the last seat won by the coalition that won the allocation dispute of the last remaining seat. This margin of victory is a comparison between the quotients of the two coalitions with higher quotient num-

bers in the last distribution round. It is measured by the proportion of votes above the margin of victory in comparison with the total number of votes.

According to quotient 3.3, in Brazil, if the local councilors' coalition of parties $x1$ wins the last seat of municipality i allocated in the last round of distribution seats in an election and coalition $x2$ is the one with the second highest quotient value, it means that $Q^i_{x1} > Q^i_{x2}$, which is equivalent to:

$$\frac{v^i_{x1}}{V^i}\frac{Seats^i}{SC^i_{x1}+1} > \frac{v^i_{x2}}{V^i}\frac{Seats^i}{SC^i_{x2}+1}. \tag{3.5}$$

The comparison between the quotients of coalitions in the last allocation round in a given municipality defines which coalition wins the last distributed seat. Moreover, it is possible to check the difference in number of votes that determined that the quotient of the winning coalition ($x1$) was higher than the coalition with the second highest quotient ($x2$). Thus, we can calculate the minimum number of votes that coalition $x2$ should have won to get the seat distributed in the last allocation round. This hypothetical number of votes, which I call α^i, is the minimal number of votes that could change the relationship described by inequality 3.5 to $Q^i_{x1} < Q^i_{x2}$ with other values remaining the same. Therefore, α^i may be identified modifying the inequality to:

$$\frac{v^i_{x1}}{V^i}\frac{Seats^i}{SC^i_{x1}+1} < \frac{(v^i_{x2}+\alpha^i)}{V^i}\frac{Seats^i}{SC^i_{x2}+1};$$

and isolating the value α^i I find that this value is

$$\alpha^i > \frac{v^i_{x1}(SC^i_{x2}+1)}{SC^i_{x1}+1} - v^i_{x2}. \tag{3.6}$$

Based on the electoral rule and using the electoral results of municipal elections in Brazil, I can measure the margin of victory of the last seat allocated won by coalition $x1$ of every Brazilian

municipality. I define this margin of victory (MV^i) as the proportion of α^i in relation to V^i. That is

$$MV^i = \frac{\alpha^i}{V^i}. \tag{3.7}$$

I use this margin of victory in combination with a selection of specific municipalities to measure the margin of alignment or non-alignment between the legislative and executive branches in a municipality. Since I want to compare the effect of the political alignment as compared to non-alignment, I select municipalities considering their status in a prior mandate. This is important to ensure that the estimated effect does not consider an accumulated result of the current and previous mandates. So, municipalities that have political alignment are those which, in the mandate considered at the estimation, the number of local councilors in the coalition of the mayor's party is the minimum number of majority seats and that in a previous mandate these municipalities had the minority of the local councilors aligned with the elected mayor. This is the treatment group and the effect of the political alignment is measured comparing the variation of outcomes of this group with the same outcomes variations of the control group. The control group consist of those municipalities which had the minority of local councilors aligned with the elected mayor during the first mandate and the number of local councilor members of the coalition of the mayor's party is the closest to the maximum number of majority seats in the following mandate (maintained the minority of seats).

Using a practical example, because of the population size of Brazilian municipalities, a large number of them have 9 seats composing the local legislative body. So, for these municipalities I use the margin of victory of the last allocated seat to indicate the margin of victory of the fifth seat (when the mayor's coalition wins the fifth seat in the last allocation round), which is the lowest majority of the legislative body aligned with the mayor's party, when in these municipalities the number of local councilors aligned with the party of the mayor is four or less. Moreover, when the mayor's party coalition loses the last allocation seats and it has already

assigned 4 seats, the MV^i in this case indicates the margin of defeat of the mayor's coalition to have the majority of the legislative body. In this case (control group), the municipalities are selected only if their number of local councilors aligned with the party of the mayor was four or less in a previous mandate. On the other hand, the effect of the political non-alignment can be measured by the comparison of different treatment and control groups. In this case, the treatment group is the one composed by municipalities which changed the alignment status from the majority of local councilors aligned with the mayor in the first mandate to the case when the number of local councilors aligned with the elected mayor is the closest to the maximum number of majority seats. In this estimation, the control group is composed by those municipalities which the majority of the local councilors members are aligned with the elected mayor in both mandates.

In addition to those two causal effect analyzes, I complement these evaluations with the two comparison of groups of municipalities. The first one I compare municipalities that changed the alignment status between government branches in opposite direction. In other words, I compare municipalities that the elected mayor had in the first mandate the minority of local councilor members and the majority of local councilors in the next term versus municipalities where the mayor had the majority of local councilors in the first term and the minority of them aligned in the next one.

Identification: Regression Discontinuity

Therefore, these two specific groups represent different situations of political alignment versus non-alignment, although the coalitions that the mayor's party is a part of have very similar electoral results in terms of votes in both groups. That is, this running variable (MV^i) is a measurement of the margin of political alignment (when the mayor's coalition in the legislative branch barely wins the majority of seats) and political non-alignment (when the mayor's

coalition in the legislative branch barely loses the majority of seats) in terms of votes. This measurement takes into account not only the number of votes of the mayor's coalition, but also the other coalition results, contrary to the measurement of the margin of victory of candidates running for executive offices, as is common in RD literature.

3.4 Results

As explained in previously, I explore the legal arrangement in Brazil, which determines that the executive branch proposes the municipal budgetary plan that has to be approved by the absolute majority of the local chamber. This RD design selects a set of two specific groups of municipalities as the treatment group (municipalities whose majority of local councilors are aligned with the mayor's party) and control group (municipalities that are close to having the majority of local councilors aligned with the mayor's party). In applying this methodology, I would be comparing homogeneous groups of municipalities that have similar characteristics, except the assignation of whether the government branches of a specific municipality are aligned or not with each other and I could estimate the effect of the chosen outcomes.

Sample Selection and Comparability

In order to ensure the homogeneity of the groups used to run the regressions, I select municipalities with similar observable characteristics. First the municipalities have the same number of local councilors. Based on the municipalities pre-selected in the RD design, I only select municipalities with 9 local councilor seats, the most common number of local councilors in Brazilian municipalities. Second, the municipalities were not represented by reelected mayors during the mandate of 2009 to 2012. As discussed, I select municipalities that had the composition of the alignment changed between the mandates of 2005-2008 and 2009-2012 as

the treatment group. According to the Brazilian legislation in the elections of 2004 and 2008, municipalities with up to 47,619 inhabitants could elect up to 9 (the minimum number) local councilors. However, there was a small unbalance of the municipalities with the highest number of inhabitants. Because of that, I selected municipalities with up to 35,000 inhabitants to exclude possible outliers from my analysis, although it did not considerably change the results.

After this selection of municipalities, I check whether these two groups are balanced in terms of observable characteristics. Those were: (*i*) mayors' gender compositions; (*ii*) number of mayors with university degree; (*iii*) municipality GDP; (*iv*) population; (*v*) political alignment between the mayor and state governor; (*vi*) political alignment between mayor and president; (*vii*) number of votes casted to the elected mayor in the 2008 election; (*viii*) number of candidates for mayor in the 2008 election. All of the results showed that the two groups were balanced. Additional balancing checks were conducted and are presented in combination with specific significant results below.

As previously explained, Brazilian legislation defines that the local legislative branch is responsible for approving the budgetary plan proposed by the mayor. Because of the nature of the relationship between these government branches, I focus the analyses on the evaluation of local expenditures by function and then I look for additional findings to enrich the evidence. The RD estimations are carried out using the triangular kernel and polynomial orders one, two and three. I also conduct several falsification tests. For the results with statistical significance I run two new RD regressions for the same outcome of interest using different cutoffs (at -0.025 and 0.025). These falsification tests aim to verify whether the effect found locally at the accurate cutoff is specific to the treatment rather than to a random significant effect present in different values of the running variable.

Before the effect estimations, I conduct the manipulation test suggested in Cattaneo, Jansson, and Ma (2016). This test looks for empirical evidence for the problem of self-selection or non-random sorting of the municipalities around the cutoff. This problem could happen, for

example, if mayors could manipulate the electoral results of the local councilor seats to obtain more political support. Hence, the test evaluates whether there is discontinuity in the density of municipalities around the cutoff. Table 34 in the Appendix shows the results of this test. As expected, no evidence of any systematic manipulation is found.

Effects of Political Alignment

Before the effect estimations, I conduct the manipulation test suggested in Cattaneo, Jansson, and Ma (2016). This test looks for empirical evidence for the problem of self-selection or non-random sorting of the municipalities around the cutoff. This problem could happen, for example, if mayors could manipulate the electoral results of the local councilor seats to obtain more political support. Hence, the test evaluates whether there is discontinuity in the density of municipalities around the cutoff. Table 27 in the Appendix shows the results of this test. As expected, no evidence of any systematic manipulation is found.

Two functions of the public expenditure showed significant changes because of the political alignment between government branches. The first effect is on the executive branch expenditure per capita. The results of this effect presented on table 3.1 consider the difference of average expenditure per capita of the whole term versus the previous term. This result consider different orders of polynomial in RD model and the significance tests are robust. The falsification tests for this result are presented in tables 32 and 33 in the Appendix and they showed no effect in the placebos cutoffs.

Another significant effect of the political alignment is on local public expenditure in education per capita, presented in table 3.2. Contrary to the previous positive effect, this result showed a decrease in the average expenditure in education.The respective results of the falsifications tests are presented in tables 28 and 29. This result is corroborated by another effect. I checked the impact of the reduction on the number of students enrolled in public municipal

schools and I found some evidence of decrease on it. This result is presented on table 3.3 and the falsification teste are presented on tables 30 and 31 in the Appendix.

One could expect that the effect of the political alignment over those outcomes were greater over the years of the term. To test that, I estimated the effects of the political alignment on these outcomes only at the first year of the mandate. The results showed no effect. In addition to that, instead of estimate the effect for the average term, I estimated the effects on these outcomes disregarding the first year of mandate. The results showed that the effect on these outcomes have higher statistical significance in the last three years of mandate. The results are presented on tables 3.4, 3.5 and 3.6, respectively for the expenditure on executive branch per capita, education per capita and the number of students per 10,000 inhabitants. Moreover, there is evidence of the impact on the reduction of local teachers of public schools during the three last year of mandate. This result is resented on table 3.7.

I conducted similar estimations to check whether there was any effect on the number of municipal public schools. However, I did not find any evidence of impact over this number. To check if the reduction in public expenditures in education, and the number of teachers and students in municipalities with alignment between government branches were influenced by a hypothetical unbalancing of federal transfer to municipalities, I estimate the effect of the political alignment on these transfers. *FUNDEB* (*Fundo de Manutenção e Desenvolvimento da Educação Básica e de Valorização dos Profissionais da Educação*) is a federal fund intended to support municipalities on the provision of elementary education. The amount of the transfer is calculated on the basis of the number of students in each municipality. I ran regression tests to see if the amount of resources per student is different between the groups of municipalities. The results showed no evidence of an unbalanced transfer of public resources to any group of municipalities.

In order to check if the significant effects on the expenditure function were caused by an unbalance of the federal transfer for municipalities (*Fundo de Participação dos Municípios*),

Table 3.1 Effect on the Difference of the average Executive Branch Expenditure Per Capita (EA) between 2005-2008 and 2009-2012 mandates

	P = 1	P = 2	P = 3
EA_{LATE}	59.827	98.073	116.39
Robust P-Value	0.073	0.086	0.134
Left Bandwidth	-0.028	-0.031	-0.039
Right Bandwidth	0.028	0.031	0.039

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 111 and right side 113.

Table 3.2 Effect on the Difference of the average Education Expenditure Per Capita (EE) between 2005-2008 and 2009-2012 mandates

	P = 1	P = 2	P = 3
EE_{LATE}	-64.178	-88.56	-97.433
Robust P-Value	0.100	0.094	0.140
Left Bandwidth	-0.027	-0.035	-0.045
Right Bandwidth	0.027	0.035	0.045

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 111 and right side 113.

Table 3.3 Effect on the Difference of the average Number of Students Per 10,000 inhabitants (NS) between 2005-2008 and 2009-2012 mandates

	P = 1	P = 2	P = 3
NS_{LATE}	-400.4	-467.58	-497.18
Robust P-Value	0.069	0.050	0.104
Left Bandwidth	-0.026	-0.044	-0.049
Right Bandwidth	0.026	0.044	0.049

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 111 and right side 114.

Table 3.4 Effect on the Difference of the average Executive Branch Expenditure Per Capita (EA) between the **last 3 years** 2005-2008 and 2009-2012 mandates

	P = 1	P = 2	P = 3
EA_{LATE}	91.966	209.48	221.45
Robust P-Value	0.007	0.000	0.001
Left Bandwidth	-0.027	-0.025	-0.036
Right Bandwidth	0.027	0.025	0.036

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 316 and right side 323.

Table 3.5 Effect on the Difference of the average Education Expenditure Per Capita (EE) between the **last 3 years** 2005-2008 and 2009-2012 mandates

	P = 1	P = 2	P = 3
EE_{LATE}	-65.278	-83.643	-92.207
Robust P-Value	0.036	0.074	0.138
Left Bandwidth	-0.022	-0.032	-0.035
Right Bandwidth	0.022	0.032	0.035

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 316 and right side 323.

Table 3.6 Effect on the Difference of the average Number of Students Per 10,000 inhabitants (NS) between the **last 3 years** of 2005-2008 and 2009-2012 mandates

	P = 1	P = 2	P = 3
NS_{LATE}	-451.38	-539.8	-459.07
Robust P-Value	0.010	0.003	0.088
Left Bandwidth	-0.023	-0.043	-0.041
Right Bandwidth	0.023	0.043	0.041

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 331 and right side 343.

or by the total revenue or the total expenditure of the municipalities, I estimate the effect of the political alignment on these three variables. Again, no evidence of unbalancing between the treatment and control groups was found. I also look for evidence of effect of the political alignment over number of public employment and no effect was found. Additionally, there is no significant effect in those outcomes when I estimate the effect of the political non-alignment (when the treatment group changes the alignment status from majority of the local councilors aligned with the elected mayor to the minority of legislative members aligned with the elected mayor and the control group is represented by municipalities which the majority of local councilors is aligned with the mayor in both terms).

Complementary Comparisons

Complementary to the effect estimations presented in previous subsection, I compare the results of groups of municipalities that did not change the alignment status. That is municipalities which had the majority of local councilors aligned with the mayor in both terms versus municipalities that had the minority of legislative chamber members aligned with the elected mayor in both mandates. No significant difference was found using this comparison.

Finally I compare municipalities which had opposite changes in the alignment of government branches. That is, municipalities that the elected mayor had in the first mandate the minority of local councilor members and the majority of local councilors in the next term versus municipalities where the mayor had the majority of local councilors in the first term and the minority of them aligned in the next one. Although this comparison does not provide evidence of causal effect of neither the political alignment nor political non-alignment, this comparison can show how the expenditure composition change in comparison to each other.

The manipulation test for this comparison is presented on table 34. The first significant result showed evidence of reduction of the education local expenditure per inhabitant. Table

3.8 shows the evidence for estimations of the four-years mandate average of the expenditure per inhabitant in each municipality. Table 3.10 shows the results of the estimation which compares only the first year of the mandates. There is no change of expenditure in education at the start of the mandate. However, table 3.9 presents the results of the estimation by year excluding only the first year of mandate. The results present a higher significance in the decrease of education expenditure. The falsification tests for the results of tables 3.8 and 3.9 are presented on table 35, 36, 37 and 38 in the Appendix. No significant effects are found in these tests.

I also look for compare the difference of these groups on the number of teachers, students, and schools of the municipal public education system. Table 3.11 presents the results for the difference of the number of teachers per 10,000 inhabitants for each year of the mandates. However, the result for the estimation of the difference of the average number of teachers during the mandate presented in table 3.12 did not show the same level of significance.

Another finding is the difference on the sum expenditure per inhabitant of four expenditure functions: Urban infrastructure, housing, sanitation and transportation. Although none of the functions have a significant difference individually, there is significant difference on functions related to infrastructure together. These results are true from the second until the last year of mandate. They are presented in table 3.15 and the falsification tests are presented in tables 39 and 40 in the Appendix. The increase of this expenditure is even more significant in the last year of mandate. This evidence is presented in table 3.16 and the falsification tests in tables 41 and 42.

By definition, a political setup where government branches are aligned by party affiliation is expected to be an environment with less objections between political players. One could imagine that branches led by allied politicians may result in less oversight than in a polarized political environment (and more objections between the players). This situation could facilitate the conditions for corruption events. To test this hypothesis, I look for evidence of difference on the corruption events in Brazilian municipalities comparing groups of municipalities with

Table 3.7 Effect on the Difference of the average Number of Teachers Per 10,000 inhabitants (NS) between the **last 3 years** of 2005-2008 and 2009-2012 mandates

	P = 1	P = 2	P = 3
NS_{LATE}	-41.48	-54.25	-29.467
Robust P-Value	0.108	0.089	0.557
Left Bandwidth	-0.024	-0.037	-0.042
Right Bandwidth	0.024	0.037	0.042

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 331 and right side 343.

Table 3.8 Difference of the average Education Expenditure Per Capita (EE) between 2005-2008 and 2009-2012 mandates

	P = 1	P = 2	P = 3
EE	-67.71	-181.88	-191.91
Robust P-Value	0.112	0.018	0.012
Left Bandwidth	-0.036	-0.030	-0.050
Right Bandwidth	0.036	0.030	0.050

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 119 and right side 113.

Table 3.9 Difference of Education Expenditure Per Capita (EE) in the last three years of mandates (2010-2012 versus 2006-2008)

	P = 1	P = 2	P = 3
EE	-91.128	-204.17	-217.49
Robust P-Value	0.007	0.000	0.001
Left Bandwidth	-0.030	-0.026	-0.039
Right Bandwidth	0.030	0.026	0.039

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 341 and right side 323.

Table 3.10 Difference of Education Expenditure Per Capita (EE) in the first year of mandates (2005 versus 2009)

	P = 1	P = 2	P = 3
EE	-26.943	-96.372	-130.31
Robust P-Value	0.639	0.354	0.439
Left Bandwidth	-0.041	-0.035	-0.043
Right Bandwidth	0.041	0.035	0.043

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 112 and right side 102.

Table 3.11 Difference of Number of Teachers per 10,000 Inhabitants (NT) of mandates (2012-2009 versus 2008-2005)

	P = 1	P = 2	P = 3
NT	-85.574	-99.575	-104.06
Robust P-Value	0.009	0.019	0.030
Left Bandwidth	-0.022	-0.033	-0.051
Right Bandwidth	0.022	0.033	0.051

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 474 and right side 457.

Table 3.12 Average Difference of Number of Teachers per 10,000 Inhabitants (NT) of mandates (2012-2009 versus 2008-2005)

	P = 1	P = 2	P = 3
NT	-73.593	-90.331	-97.459
Robust P-Value	0.060	0.102	0.159
Left Bandwidth	-0.026	-0.039	-0.051
Right Bandwidth	0.026	0.039	0.051

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 120 and right side 114.

Table 3.13 Difference of Number of Students per 10,000 Inhabitants (NS) of mandates (2012-2009 versus 2008-2005)

	P = 1	P = 2	P = 3
NS	-753.73	-894.73	-964.72
Robust P-Value	0.000	0.001	0.001
Left Bandwidth	-0.020	-0.031	-0.050
Right Bandwidth	0.020	0.031	0.050

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 474 and right side 457.

Table 3.14 Difference of Average Number of Students per 10,000 Inhabitants (NS) of mandates (2012-2009 versus 2008-2005)

	P = 1	P = 2	P = 3
NS	-617.22	-811	-947.7
Robust P-Value	0.005	0.013	0.016
Left Bandwidth	-0.026	-0.036	-0.049
Right Bandwidth	0.026	0.036	0.049

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 120 and right side 114.

Table 3.15 Difference of Urban Infrastructure + Housing + Transportation + Sanitation Expenditure Per Capita (EI) in the first year of the mandates (2012+2011+2010 versus 2008+2007+2006)

	P = 1	P = 2	P = 3
EI	25.779	115.18	131.15
Robust P-Value	0.472	0.016	0.020
Left Bandwidth	-0.036	-0.027	-0.038
Right Bandwidth	0.036	0.027	0.038

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 341 and right side 323.

aligned and polarized government branches. I use the information collected in the *CGU* lotteries. Because this information is available only for a few municipalities in our selected sample, there is not enough information exploit the same empirical approach I have been using so far.

Under limited data availability, in attempt to provide meaningful insights regarding alignment and irregularities I use all observations selected in the RD estimation that overlap with information on corruption from CGU and provide OLS estimates. In doing so, I apply the same rationale behind alignment and non-alignment, using all of the municipalities that have 5 out of 9 local councilors aligned with the elected mayor and municipalities where 4 out of 9 local councilors are aligned with the mayor (and have the same conditions as in the RD regression: municipalities with only 9 seats for local councilors, up to 35,000 inhabitants), distinguished by a dummy variable (1 = political alignment and 0 = political non-alignment), to check the difference of types of government branches' alignment and the proportion of federal public transfers that was irregularly used by municipalities identified by the *CGU*. Table 3.17 shows the evidence of more corruption in municipalities where the mayor and the majority of local councilors became aligned in comparison to municipalities which the mayor and local councilors became unaligned. I also run another regression adding the information of federal transfers to control the dummy variable. The result is presented in table 3.18 and it shows equivalent significance.

Another comparison conducted involves the difference of these groups on the number of local public employees. Because of data limitation, I check this comparison on the number of public employees in the first and last year of mandate in comparison to the same years of previous mandate. The results presented in table 3.19 show evidence of significance differences on the number of public employees at the indirect administration institutions in the first year of mandate. The falsification tests presented in the tables 43 and 44 did not show significant results for estimations. The indirect public administration is a set of entities that fulfill government activities through decentralization and these results can be understood as clientelism.

Table 3.16 Difference of Urban Infrastructure + Housing + Transportation + Sanitation Expenditure Per Capita (EI) in the last year of mandate (2012 versus 2008)

	P = 1	P = 2	P = 3
EI	145.88	233.22	240.05
Robust P-Value	0.029	0.007	0.011
Left Bandwidth	-0.023	-0.031	-0.047
Right Bandwidth	0.023	0.031	0.047

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 110 and right side 103.

Table 3.17 Correlation between Corruption (2009): Estimated by Ordinary Least Square (OLS)

Variable	Coefficient	P>|t|
Political Alignment (=1)	0.10893	0.040
Constant	0.7668361	0.000

Source: Prepared by the author.
Note: Number of observations is 141.

Table 3.18 Correlation between Corruption (2009): Estimated by Ordinary Least Square (OLS)

Variable	Coefficient	P>|t|
Political Alignment (=1)	0.1064299	0.053
Federal Transfers	2.03e-09	0.512
Constant	0.7334763	0.000

Source: Prepared by the author.
Note: Number of observations is 136.

No significant difference on the number of public employees in the last year of mandate were found.

3.5 Concluding Remarks

The first meaningful results are about the checks and falsification tests in order to ensure the homogeneity between the treatment and control group. Apart from the selection of specific municipalities (with equivalent population size, same number of local councilors, municipalities that have changed the alignment between legislative and executive government branches from mandate 2005-2008 to 2009-2012), I also conduct several falsification checks to test the homogeneity between groups of municipalities and to validate that the identified significant effects are specific around the cutoff of the running variable and not to any value of it. The results consistently showed evidence of balancing and homogeneity between treatment and control groups, ensuring the comparability of them to identify the effect of the political alignment between the executive and legislative branches.

I bring to the literature more findings regarding the determinants of the public expenditure in Brazilian municipalities and the influence of the institutional setup on the behavior of the local public representatives in Brazil. According to these findings, the political alignment between government branches influences the composition of the public expenditure, affecting mostly expenditure in education to make way for expenditure in the executive branch. As shown, there is also evidence of a decrease in the number of students and teachers of the municipal public education system as a consequence of the political alignment between government branches. Additionally to these evidence of the impact of the political alignment on the public expenditure and the consequence on the provision of public education goods, the comparison of groups of municipalities that had opposite changes in the alignment status between government branches

showed similar results.

The evidence that the government alignment between branches may affect the public expenditure composition is relevant because of the direct effect that this problem has on the provision of public goods. The set of findings on the reduction of the provision of the education public service is worrisome. In Brazil, it is frequently pointed out by the literature that one of the main challengers of development is the low productivity caused, in part, by the low human capital capacities. So, this lack of accountability, represented by less oversight of the legislative over the executive branch, could enhance this problem, as the education expenditure is affected. Ferraz, Finan, Moreira (2012) found that missing resources due to corruption in education cause a reduction in students' performance, as well as higher dropout and failure rates in Brazil. They also found that there is a negative impact on computer labs, teaching supplies, and teacher training in the presence of corruption.

The results found with my estimation strategy are very specific to municipalities with similar degrees of political non-alignment or alignment. However, one could expect that the difference between municipalities with higher alignment of government branches (e.g. all local councilors aligned with the party of the mayor) and municipalities with lower political alignment (e.g. none of the local councilors aligned with the mayor) would be more pronounced. Even though I use a binary classification to distinguish between political non-alignment and alignment, it is worth highlighting that the degree of political alignment is important to define how pronounced the effects are. It can suggest policy implication in line with the audit lotteries conducted by the *Controladoria-Geral da União* in Brazil. For example, it is possible to consider that lotteries conditional to the information of the presence of more or less institutions that caused more or less political accountability, as the political alignment between government branches, may improve the audits outcomes. In summary, the political alignment between the executive and legislative branches is a relevant indicator of political accountability in Brazilian municipalities.

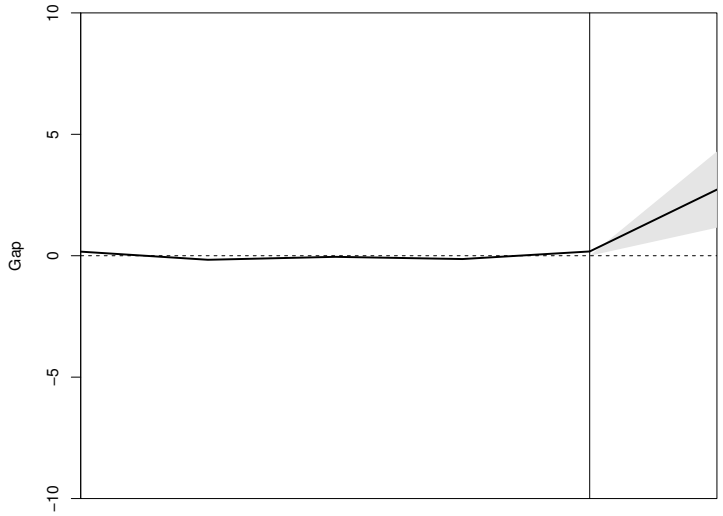
Table 3.19 Difference of Number of Public Employees (PE) in the first year of mandate (2009 versus 2005) working in the indirect administration

	P = 1	P = 2	P = 3
PE	21.841	106.8	194.25
Robust P-Value	0.760	0.045	0.041
Left Bandwidth	-0.030	-0.025	-0.037
Right Bandwidth	0.030	0.025	0.037

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 112 and right side 112.

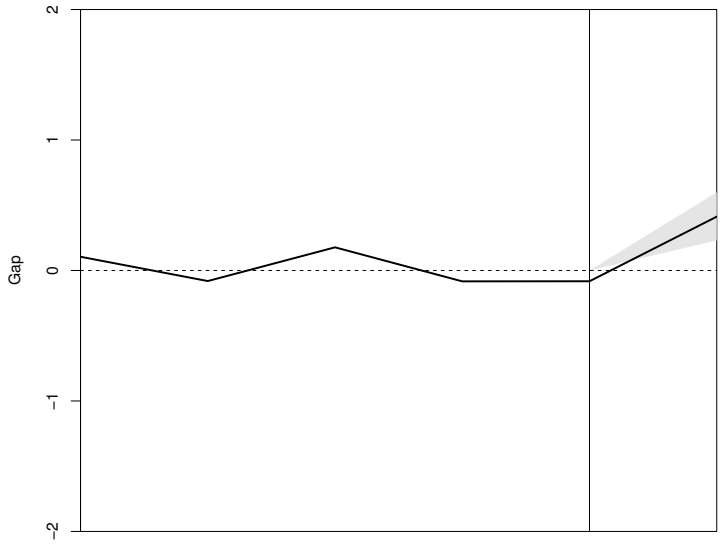
Appendix

Figure 1 Confidence Sets of External Balance - Confidence bounds [1.154854, 4.290649] at a significance level of 0.05.



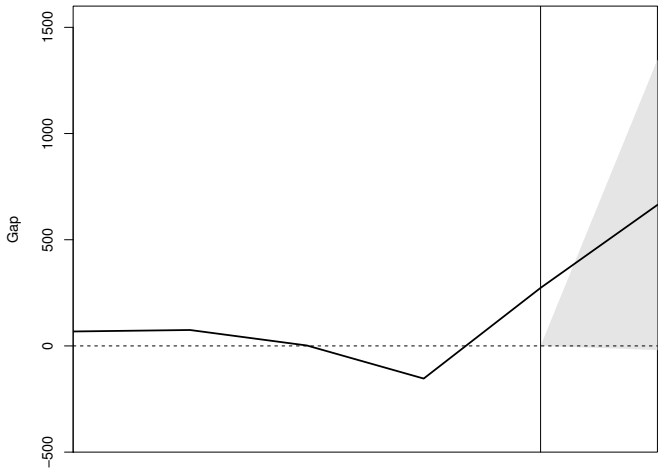
Source: Prepared by the author.

Figure 2 Confidence Sets of Occupied People - Confidence bounds [0.2307064, 0.5989574] at a significance level of 0.05.



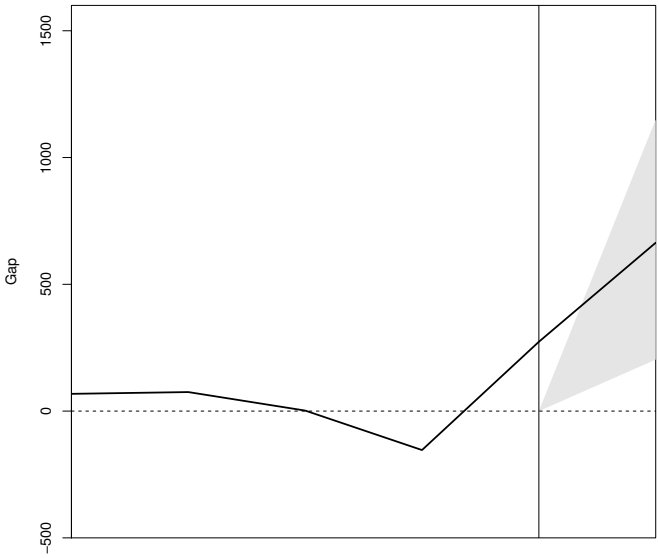
Source: Prepared by the author.

Figure 3 Confidence Sets of GDP per Capita - Confidence bounds [-19.4464, 1347.402] at a significance level of 0.05.



Source: Prepared by the author.

Figure 4 Confidence Sets of GDP per Capita - Confidence bounds [203.0576, 1149.956] at a significance level of 0.10.



Source: Prepared by the author.

Table 20 The bands of inhabitants and the respective number of local councilors in the elections of 2004 and 2008 in Brazil

Number of Inhabitants	Number of Councilors
Up to 47,619	09
From 47,620 to 95,238	10
From 95,239 to 142,857	11
From 142,858 to 190,476	12
From 190,477 to 238,095	13
From 238,096 to 285,714	14
From 285,715 to 333,333	15
From 333,334 to 380,952	16
From 380,953 to 428,571	17
From 428,572 to 476,190	18
From 476,191 to 523,809	19
From 523,810 to 571,428	20
From 571,429 to 1,000,000	21
From 1,000,001 to 1,121,952	33
From 1,121,953 to 1,243,903	34
From 1,243,904 to 1,365,854	35
From 1,365,855 to 1,487,805	36
From 1,487,806 to 1,609,756	37
From 1,609,757 to 1,731,707	38
From 1,731,708 to 1,853,658	39
From 1,853,659 to 1,975,609	40
From 1,975,610 to 4,999,999	41
From 5,000,000 to 5,119,047	42
From 5,119,048 to 5,238,094	43
From 5,238,095 to 5,357,141	44
From 5,357,142 to 5,476,188	45
From 5,476,189 to 5,595,235	46
From 5,595,236 to 5,714,282	47
From 5,714,283 to 5,833,329	48
From 5,833,330 to 5,952,376	49
From 5,952,377 to 6,071,423	50
From 6,071,424 to 6,190,470	51
From 6,190,471 to 6,309,517	52
From 6,309,518 to 6,428,564	53
From 6,428,565 to 6,547,611	54
More than 6,547,612	55

Source: Prepared by the author using data from TSE.
Note: This table was released by the TSE. Access `http://www.tre-sc.jus.br/site/legislacao/resolucoes/tse/2004/resolucao-tse-n-217022004/` for full publication.

Table 21 Correlation between the Effective Number of Mayoral Candidates in the 2008 election (dependent variable) and Number of Legislators in prior election (2004): Estimated by Quantile Regression

Variable	Coefficient	P>|t|
Prior Election Seats	-.2035584	0.000
Population	.0000193	0.000
Constant	8.723979	0.000

Source: Prepared by the author.
Note: Quantile=0.5 (median). Number of observations is 4,354.

Table 22 Correlation between the Effective Number of Mayoral Candidates in the 2008 election (dependent variable) and Number of Legislators in prior election (2004): Estimated by Ordinary Least Square (OLS)

Variable	Coefficient	P>|t|
Prior Election Seats	-1.293975	0.000
Population	.000038	0.000
Constant	18.60805	0.000

Source: Prepared by the author.
Note: Number of observations is 4,275. This regression excludes municipalities with the number of seats larger than 40. The standard deviation is robust.

Table 23 Correlation between the Effective Number of Mayoral Candidates in the 2008 election (dependent variable) and Number of Legislators in prior election (2004): Estimated by Ordinary Least Square (OLS)

Variable	Coefficient	P>|t|
Prior Election Seats	.5361048	0.000
Population	2.28e-06	0.225
Constant	2.646704	0.021

Source: Prepared by the author.
Note: Number of observations is 4,330. The standard deviation is robust.

Table 24 Correlation between the Logarithm Number of Local-Councilors of the elected mayor's party and the Logarithm Number of Local-Councilors Seats (at 2009-2012 mandate): Estimated by Ordinary Least Square (OLS)

Variable	Coefficient	P>|t|
Log(Local-Councilors Seats)	0.9125145	0.000
Number of Mayor Candidates	-0.056151	0.000
Constant	-0.984432	0.000

Source: Prepared by the author.
Note: Number of observations is 2,381. The standard deviation is robust.

Table 25 Number of Candidates for Mayor (NCM): Local-Polynomial Approach

	P = 1	P = 2	P = 3
NCM_{LATE}	113.98	106.31	225.57
Robust P-Value	0.364	0.462	0.232
Left Bandwidth	-0.077	-0.133	-0.138
Right Bandwidth	0.077	0.133	0.138
Kernel	Triangular	Triangular	Triangular

Source: Prepared by the author.
Note: The window selected to estimate these models were calculated by the `rdbwselect` method. Left side 832 and right side 701.

Table 26 Number of Candidates for Mayor (NCM): Local-Randomization Approach

	P = 1	P = 2	P = 3
NCM_{LATE}	-5.507	-8.137	-15.946
P-Value (Large Sample)	0.112	0.042	0.000
Kernel	Triangular	Triangular	Triangular

Source: Prepared by the author.
Note: The window selected is equal to (-0.028, 0.028). These values were estimated through the `rdbwselect` method. The numbers of observation on the left side and the right side are 344 and 267, respectively.

Table 27 RD Manipulation Test

Method	T	P>|T|
Conventional	1.5675	0.1170
Undersmoothed	0.2600	0.7948
Robust Bias-Corrected	0.6925	0.4886

Source: Prepared by the author.
Note: The total numbers of observation used in the test are 111 and 114, on the left and right side, respectively.

Table 28 Placebo: Effect Over the Public Expenditure in Education per inhabitant (PEE) (2005-2008 versus 2009-2012 mandate) at Cutoff=0.025

	P = 1	P = 2	P = 3
PEE_{LATE}	-98.027	-184	-177.37
Robust P-Value	0.439	0.288	0.314
Left Bandwidth	-0.016	-0.016	-0.029
Right Bandwidth	0.016	0.016	0.029

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 65 and right side 77.

Table 29 Placebo: Effect Over the Public Expenditure in Education per inhabitant (PEE) (2005-2008 versus 2009-2012 mandate) at Cutoff=-0.025

	P = 1	P = 2	P = 3
PEE_{LATE}	-98.027	-184	-177.37
Robust P-Value	0.439	0.288	0.314
Left Bandwidth	-0.016	-0.016	-0.029
Right Bandwidth	0.016	0.016	0.029

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 65 and right side 77.

Table 30 Placebo: Effect Over the Number of Students per 10,000 inhabitants (NE) (2005-2008 versus 2009-2012 mandate) at Cutoff=0.025

	P = 1	P = 2	P = 3
NE_{LATE}	-45.253	3.8804	-198.26
Robust P-Value	0.984	0.876	0.880
Left Bandwidth	-0.018	-0.025	-0.017
Right Bandwidth	0.018	0.025	0.017

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 65 and right side 78.

Table 31 Placebo: Effect Over the Number of Students per 10,000 inhabitants (NE) (2005-2008 versus 2009-2012 mandate) at Cutoff=0.025

	P = 1	P = 2	P = 3
NE_{LATE}	-45.253	3.8804	-198.26
Robust P-Value	0.984	0.876	0.880
Left Bandwidth	-0.018	-0.025	-0.017
Right Bandwidth	0.018	0.025	0.017

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 82 and right side 65.

Table 32 Placebo: Effect Over the Public Expenditure in the Executive Branch per inhabitant (PEB) (2005-2008 versus 2009-2012 mandate) at Cutoff=0.025

	P = 1	P = 2	P = 3
PEB_{LATE}	-3.2431	10.09	22.525
Robust P-Value	0.990	0.883	0.944
Left Bandwidth	-0.015	-0.024	-0.024
Right Bandwidth	0.015	0.024	0.024

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 65 and right side 77.

Table 33 Placebo: Effect Over the Public Expenditure in the Executive Branch per inhabitant (PEB) (2005-2008 versus 2009-2012 mandate) at Cutoff=-0.025

	P = 1	P = 2	P = 3
PEB_{LATE}	-88.347	-159.19	-248.89
Robust P-Value	0.386	0.349	0.351
Left Bandwidth	-0.018	-0.024	-0.024
Right Bandwidth	0.018	0.024	0.024

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 82 and right side 65.

Table 34 RD Manipulation Test

Method	T	P>|T|
Conventional	-0.1426	0.8866
Undersmoothed	-0.5015	0.6160
Robust Bias-Corrected	-0.3661	0.7143

Source: Prepared by the author.
Note: The total numbers of observation used in the test are 120 and 114, on the left and right side, respectively.

Table 35 Placebo: Difference of the average Education Expenditure Per Capita (EE) between 2005-2008 and 2009-2012 Mandates - Cutoff=-0.025

	P = 1	P = 2	P = 3
EE	108.08	89.845	150.07
Robust P-Value	0.133	0.193	0.336
Left Bandwidth	-0.013	-0.024	-0.020
Right Bandwidth	0.013	0.024	0.020

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 77 and right side 78.

Table 36 Placebo: Difference of the average Education Expenditure Per Capita (EE) between 2005-2008 and 2009-2012 Mandates - Cutoff=0.025

	P = 1	P = 2	P = 3
EE	-97.772	-185.48	-233.94
Robust P-Value	0.350	0.286	0.228
Left Bandwidth	-0.018	-0.016	-0.023
Right Bandwidth	0.018	0.016	0.023

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 78 and right side 77.

Table 37 Placebo: Difference of Education Expenditure Per Capita (EE) in the last three years of mandates (2010-2012 versus 2006-2008) Cutoff=-0.025

	P = 1	P = 2	P = 3
EE	78.525	41.199	167.09
Robust P-Value	0.266	0.477	0.330
Left Bandwidth	-0.010	-0.023	-0.021
Right Bandwidth	0.010	0.023	0.021

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 222 and right side 220.

Table 38 Placebo: Difference of Education Expenditure Per Capita (EE) in the last three years of mandates (2010-2012 versus 2006-2008) Cutoff=0.025

	P = 1	P = 2	P = 3
EE	-69.009	-254.78	-197.81
Robust P-Value	0.388	0.162	0.243
Left Bandwidth	-0.020	-0.014	-0.022
Right Bandwidth	0.020	0.014	0.022

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 220 and right side 222.

Table 39 Placebo: Difference of Urban Infrastructure + Housing + Transportation + Sanitation Expenditure Per Capita (EI) in the first year of mandates (2012+2011+2010 versus 2008+2007+2006) Cutoff=-0.025

	P = 1	P = 2	P = 3
EI	-38.792	-10.739	384.84
Robust P-Value	0.607	0.849	0.020
Left Bandwidth	-0.013	-0.020	-0.015
Right Bandwidth	0.013	0.020	0.015

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 222 and right side 220.

Table 40 Placebo: Difference of Urban Infrastructure + Housing + Transportation + Sanitation Expenditure Per Capita (EI) in the first year of mandates (2012+2011+2010 versus 2008+2007+2006) Cutoff=0.025

	P = 1	P = 2	P = 3
EI	25.779	-28.825	-108.68
Robust P-Value	0.899	0.016	0.511
Left Bandwidth	-0.036	-0.016	-0.028
Right Bandwidth	0.036	0.016	0.028

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 341 and right side 323.

Table 41 Placebo: Difference of Urban Infrastructure + Housing + Transportation + Sanitation Expenditure Per Capita (EI) in the last year of mandate (2012 versus 2008) Cutoff=-0.025

	P = 1	P = 2	P = 3
EI	-169.26	-188.86	108.2
Robust P-Value	0.039	0.077	0.477
Left Bandwidth	-0.012	-0.017	-0.016
Right Bandwidth	0.012	0.017	0.016

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 72 and right side 71.

Table 42 Difference of Urban Infrastructure + Housing + Transportation + Sanitation Expenditure Per Capita (EI) in the last year of mandate (2012 versus 2008) Cutoff=0.025

	P = 1	P = 2	P = 3
EI	-258.41	-326.91	-379.58
Robust P-Value	0.092	0.007	0.136
Left Bandwidth	-0.012	-0.017	-0.025
Right Bandwidth	0.012	0.017	0.025

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 71 and right side 70.

Table 43 Placebo: Difference of Number of Public Employees (PE) in the first year of mandate (2009 versus 2005) working in the indirect administration at Cutoff=0.025

	P = 1	P = 2	P = 3
PE	306.18	547.89	615.95
Robust P-Value	0.394	0.390	0.488
Left Bandwidth	-0.038	-0.033	-0.037
Right Bandwidth	0.038	0.03	0.037

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 76 and right side 77.

Table 44 Placebo: Difference of Number of Public Employees (PE) in the first year of mandate (2009 versus 2005) working in the indirect administration at Cutoff=-0.025

	P = 1	P = 2	P = 3
PE	37.226	106.8	16.523
Robust P-Value	0.910	0.185	0.376
Left Bandwidth	-0.016	-0.014	-0.017
Right Bandwidth	0.016	0.014	0.017

Source: Prepared by the author.
Note: The windows selected to estimate these models were calculated by the `rdbwselect` method. Left side 71 and right side 76.

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