FUNDAÇÃO GETÚLIO VARGAS
ESCOLA DE ADMINISTRAÇÃO DE EMPRESAS DE SÃO PAULO

ANDRÉ ASSUMPÇÃO

Estimating the effect of discretionary spending on corruption: Evidence from Brazilian municipalities

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Dissertação apresentada à Escola de Administração de Empresas de São Paulo da Fundação Getúlio Vargas como requisito para obtenção do título de Mestre em Administração Pública.

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Banca Examinadora:

_____
Prof. Dr Ciro Biderman (orientador)
FGV-EAESP

_____
Prof. Dr. George Avelino
FGV-EAESP

_____
Prof. Dr. Sergio Firpo
FGV-EESP

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RESUMO

O presente artigo estuda a relação entre corrupção e discricionariedade do gasto público ao responder a seguinte pergunta: regras de licitação mais rígidas, uma *proxy* para discricionariedade, resultam em menor prevalência de corrupção nos municípios brasileiros? A estratégia empírica é uma aproximação de regressões em dois estágios (2SLS) estimadas localmente em cada transição de regras de licitação, cuja fonte de dados de corrupção é o Programa de Fiscalização por Sorteio da CGU e os dados sobre discricionariedade são derivados da Lei 8.666/93, responsável por regular os processos de compras e construção civil em todas as esferas de governo. Os resultados mostram, entretanto, que menor discricionariedade está relacionada com maior corrupção para quase todos os cortes impostos pela lei de licitações.

Palavras-chave: corrupção, discricionariedade do gasto público, licitações, políticas públicas
Classificação JEL: H57; H75; H83.

ABSTRACT:

This paper analyzes the relationship between corruption and discretion of public spending in answering to the following research question: tighter procurement rules, as a *proxy* to discretion in spending, result in lower prevalence of corruption in Brazilian municipalities. The empirical strategy conducted here draws from a two-stage least squares (2SLS) regression approach locally estimated in each cutoff of procurement categories. Corruption data is drawn from the CGU's audit program and discretion categories from Bill 8.666/93, which regulates all procurement processes for purchases of goods/services and public works in all government levels. Results show, however, that lower discretion is associated with higher corruption prevalence to most cutoffs of Bill 8.666/93 in both purchases and works subgroups.

***Keywords*:** economics of corruption, public procurement, discretionary spending, public policy.
JEL Classification: H57; H75; H83.

Summary

1. INTRODUCTION

When the government has assured quality of officials and procedures, their self-judgment in spending decisions is welcome, since they are free from red tape burocracy and can increase efficiency by deciding where to allocate public funds. Thus, in anecdotal evidence, one would agree that freedom of action of government official allows positive discretion in allocating resources. Unfortunately, that is not always the case, especially in poor countries whose institutions share characteristics such as ethno-linguistic heterogeneity, French rule of law or Christian or Muslim religion ground (LA PORTA et al., 1999). Hence, instead of an all-benefit situation in public spending, officials could misuse discretion and misallocate public resources in favor of private rent extraction. In fact, scientific evidence shows that there is strong correlation between bad governments on one side and corruption and mismanagement on the other (MAURO, 1995; ACEMOGLU & VERDIER, 2000; TREISMAN, 2000; OLKEN, 2006).

The purpose of this dissertation is to explore the relationship between corruption and discretion in public spending by using a unique database of Brazilian municipalities introduced by the government monitoring agency, The Comptroller-General Office (CGU), in 2003 and still running today. The proposed research question to be tested is: does tighter procurement legislation result in lower prevalence of corruption? As a matter of fact, this question is based on the fact that inefficiencies in the government subvert the nature of discretion as to make it a cost to public resource allocation. In line with the developments of the corruption literature (DI TELLA & SCHARGRODSKY, 2003; BERTRAND et al., 2007; FISMAN & MIGUEL, 2007; OLKEN, 2007; FERRAZ & FINAN, 2008; 2011), I employ an objective measure of corruption. For the discretion measure, I draw upon Bill 8.666/93, which sets the standards for public procurement in Brazil and regulates all public contracts with private providers. Exogenous variation is guaranteed in a regression discontinuity approach at each cutoff level presented in Bill 8.666/93 and by instrumenting the independent variable of interest.

My results indicate that lower discretion does not result in lower prevalence of corruption; if any, the effect of tighter rules is positive on corruption. They demonstrate, on one hand, that the changes introduced by each group of procurement rules are not sufficient to reduce overall

corruption[1], most likely because public agents do not perceive changes in procurement types as an increase in their costs of engaging in corruption. When accounted for each type of procurement rules and around the cutoffs, I consistently find evidence of increased corruption on upper rules of government purchases. Although the result is contrary to the expected, I believe it might be related to some limitations of the database, such as the lack of data on actual procurements done by each municipality, which is the causal variable of interest and would be instrumented if possible in the correct specification.

The dissertation is organized as follows: section II discusses evidences from previous works, sheds some light on the auditing program by CGU and provides information on the database here employed. Section III presents the empirical strategy of this research design, including its strengths and limitations, while section IV contains the main results and derivations on estimation strategies. Section V concludes the dissertation and provides suggestion for future research in the field.

[1] Defined as use of public office for private gain.

2. BACKGROUND & DATA

The economics of crime and corruption literature was inaugurated by the works of Becker (1968), Stigler (1970) and Rose-Ackerman (1975) in which they set baseline models of crime and corruption. The following works explored the determinants of public sector corruption: market structures (SHLEIFER & VISHNY, 1993), economic growth (MAURO, 1995), market imperfections (ACEMOGLU & VERDIER, 2000), decentralization, democracy, liberal economies, etc. (Treisman, 2000). More recently, Ferraz & Finan (2008);(2011) study the impact of corruption on electoral competition and outcomes and Olken (2007) analyzes the impact of monitoring missing funds in public policies. It is important to point out, however, that the first studies, most notably those from the 1990's, have made use of what is now called subjective indexes of corruption. These indexes were based on surveys to assess the perceived level of corruption from individuals. Although they have been fundamental for the development of the subject, there is currently a wide body of experts who frown upon subjective measures, once they implicitly carry forward cultural backgrounds and question-oriented responses[2]. Latest works in the economics of corruption discipline are already in line with so-called objective measures of corruption. Their strength is that data collection of indexes does not depend on interviewees' opinions or cultural beliefs, as they do not have any interfere in the resulting corruption measure.

An objective measure of corruption is one of the strengths of this work. CGU's audit program was introduced in 2003 as a measure to monitor resource allocation in Brazilian municipalities. The audited resources are grants from the Federal government to municipalities through executive ministries. In this program, municipalities are randomly selected by the Federal Lottery and allocated to audit by local CGU employees. They received service orders (SO) to investigate an earmarked grant to a particular public policy in the municipality. Apart from the public policy, most SOs have a monetary amount and a list of spending irregularities reported by auditors. After collecting evidence of misallocation of funds, auditors then write a report about that municipality which is uploaded to CGU's website, where civil society can download information about their municipality management

[2] There exists an enlightening paper on the bias of subjective data collection. For further reading, please refer to: BERTRAND, M.; MULLAINATHAN, S. Do People Mean What They Say? Implications for Subjective Survey Data. *American Economic Review*, v. 91, n. 2, p. 67-72, 2001.

and about grants from each ministry[3]. One might argue that mayors could anticipate auditing and improve public spending[4] before the CGU team arrives in town for the document analysis. I believe this is not the case for the probability of being audited in any given lottery is 1.1%[5] and also because the resources audited are those from the 3 previous years before the lottery took place. Mayors have four-year mandates with probability 7.92% of having some of their resources audited and 3.96% of having all their resources audited[6]. I assume further that agents are independent and do not discriminate between municipalities, so there is no bias in information from the reports (see Ferraz & Finan (2008)).

After the reports go online, a team of researchers at the Center of Studies of Politics and Economics of the Public Sector (CEPESP) at Fundação Getúlio Vargas (FGV) classifies each SO from the Ministries of Education (MEC), Health (MS) and Social Development (MDS) and match irregularities with corruption and mismanagement codes. The team at CEPESP has created 34 codes to classify each finding at CGU's reports[7]. There are eight codes that account for corruption, while the others are evidences of mismanagement.

Table 2.1: Corruption codes for SOs

Procurement infractions	Private appropriation
Procurement infraction (no publicity) = 4	Private appropriation (overpricing) = 11
Procurement infraction (false receipts) = 5	Private appropriation (false receipts) = 12
Procurement infraction (ghost companies) = 6	Private appropriation (no receipts) = 13
Procurement infraction (false documents) = 8	
Procurement infraction (tender favoring) = 9	

Source: CEPESP-FGV.

So, if a particular SO had one of the above findings, it would be flagged as a corrupt grant. Formally:

$$pcorrup_{i,m} = \begin{cases} 1 & \text{if SO has one of the corruption codes} \\ 0 & \text{otherwise} \end{cases} \quad (1)$$

$$wcorrup_{i,m} = \begin{cases} 1 & \text{if SO has one of the corruption codes} \\ 0 & \text{otherwise} \end{cases} \quad (2)$$

[3] For more information, I recommend taking some time to read CGU's website in general and the auditing program in particular. There are more rules and specifications on the audit that can be found there. For the purpose of this study, the information on the reports suffices to create the corruption measure.
See: http://www.cgu.gov.br/AuditoriaeFiscalizacao/ExecucaoProgramasGoverno/Sorteios/index.asp

[4] As in reallocating to better policies or in reducing momentarily the infractions of that particular grants.

[5] There are currently 5,568 municipalities in Brazil of which 48 are not eligible to be audited, which leaves 5,520 cities in the raffle. There have been 36 lotteries so far, therefore an average of 3.6 raffles every year with 1.1% probability of being selected in each of them.

[6] If municipality is selected in 2012, mayors would have resources from 2011, 2010 and 2009, their first year in office, audited. If municipality were selected in 2013, mayor would not have any resources audited, leaving previous mayor bookings to CGU auditors and so forth.

[7] All codes used to classify auditors' findings are at Appendix B.

Where *pcorrup* and *wcorrup* are the corruption indexes for SO number *i* in municipality *m*. The first corresponds to the purchases group and the latter to the public works group. Both are index functions taking value 1 when the SO has one of the corruption codes. The measure of corruption here employed is consistent with those of Lopes (2011) and Ferraz & Finan (2011), where their indexes are equal to the expected value of this index. The only change I make is that I do not aggregate irregularities and SOs by municipality, because my unit is the SO instead of each municipality. The division between purchases and public works is present in Bill 8.666/93. According to the piece of legislation, whenever the public sector is procuring goods, services or public works, it should abide by Bill 8.666/93 procurement types. The reason why one can analyze the relationship between corruption and discretion in this setting is twofold: (i) there are clearly defined inputs and outputs in the grants from the federal government, so it is possible to know how much money was destined to a municipality and where it should have been spent; (ii) the discretion measure is defined for both groups in Bill 8.666/93 and each grant can be classified according to it.

The main independent variable is the discretion measure. So far, the economic literature has not dealt yet with discretion as a determinant of corruption, in which resides the contribution of this dissertation. Treisman (2000) presents evidence that federalist states, by assumption more decentralized and permissive to discretion, are perceived to be more corrupt than centralized states; Fisman & Gatti (2002) conclude that federal states are less corrupt by analyzing their legal structure as an instrument to decentralization; Fan et al. (2009) confirm the results from Treisman (2000) using governments with many levels of decision making as the main control variable. The discussion is of vital importance to the fiscal decentralization literature as well, since it can be expected that discretion and local autonomy correlate positively[8]; also, one should expect that the more a local government is autonomous, the better should be its public funds allocation, because those would know better local problems and solutions (Akai & Sakata, 2002). However, on the opposite end, Zhang & Zou (1998) and Davoodi & Zou (1998) present evidence in the opposite direction, decentralization reduces local economic growth and efficiency; particularly, Davoodi & Zou (1998) support the hypothesis that the relationship is valid only in developing countries.

[8] It is expected that federalist States are heterogeneous with respect to population; one expects, therefore, that in such States local autonomy is larger than in unitary States – hence discretion in government decisions.

Bill 8.666/93 has three types of procurement rules in both public purchases and public works and one leave of procurement rules. A particular procurement is subject to a set of regulations depending on the amount procured x by the government. In this particular setting, I am assuming that a SO monetary amount is the same as the amount procured; so, amount procured is $x_{i,m}$, where subscript i denotes the i SO and m the municipality to which the SO belongs. Table 2.2 presents procurement types as a function of the amount procured. My hypothesis is that they are decreasing in discretion and, therefore, are decreasing in corruption prevalence.

Table 2.2: Procurement categories in Bill 8.666/93

		Public works	**Goods/Services Purchase**
Category 0	Leave of tender	$x_{i,m} \leq$ R$15,000	$x_{i,m} \leq$ R$8,000
Category 1	Invitation bidding	R$15,000 $< x_{i,m} \leq$ R$150,000	R$8,000 $< x_{i,m} \leq$ R$80,000
Category 2	Price-taking bid	R$150,000 $< x_{i,m} \leq$ R$1,500,000	R$80,000 $< x_{i,m} \leq$ R$650,000
Category 3	Competitive tender submission	$x_{i,m} >$ R$1,500,000	$x_{i,m} >$ R$650,000

Source: Bill 8.666/93.

Category 0[9], leave of public tender, is when the municipality is exempt from conducting a tender offer, in which purchases and public works might be bought directly from private seller. Therefore, it is expected to be the most discretionary public procurement. In category 1, municipalities have to send a tender offer invite to at least three private companies, subject to repetition if not accepted by any three invitees. Companies compete and the one with lowest overall value is the winner of the contract. In category 2, companies should prove infraction free financial and tax records and municipalities should publish edicts of procurement in the local media and establish a procurement commission to analyze bids. It is more restrictive for public agents to allocate public resources. In category 3, tender offers made by companies should abide by general monetary and foreign trade policy to allow for foreign companies to take part and states also that companies ought to maintain bid prices up to one year ahead. All rules are cumulative as we move up Bill 8.666/93. Consequently, procurement rules are increasing in procurement amount while discretion is expected to be decreasing. Bill 8.666/93 strengthens this argument by allowing the public agent to choose from upper sets of regulations but not from lower sets and also by prescribing that upper regulation carries on rules from lower ones.

[9] I leave it as 0, 1, 2 and 3 because it is easier to identify that zero means no tender required.

I compute both the corruption and the discretion variable for a sample of 5,834 SOs from CEPESP's database of audit reports, whose range is 2004-2010[10]. For those observations, I also have a purpose text entry, which serves to identify whether that particular SO belongs to the purchases group or to the public works group[11]; there is also a monetary amount and one or more irregularities. These SOs are divided as follows:

Table 2.3: General Audit Data

	Audit reports	SOs
MEC	731	2272
MS	490	2506
MDS	339	1056
TOTAL	1,560	5,834

Source: CEPESP-FGV.

There are 1,560 reports of all ministries, but only 883 municipalities, which means that for some of the local governments I have audits on two or even all three ministries. So, although sample units are SOs, I compute statistics from complementary data sources to account for the municipal characteristics of SOs. It means that SOs in a given municipality have the same vector of covariates, differing only their amount and the ministry they belong to. Municipal characteristics are drawn from the Instituto Brasileiro de Geografia e Estatística (IBGE), which is the main government statistical office, and the Instituto de Pesquisa Econômica Aplicada (IPEA), the main government economic research institute. The first two variables, GDP per capita (gdp_capita) and population (pop), are taken from IBGE and averaged out for the three years prior to the audit. Judiciary (judiciary) is a dummy for existence of local criminal court in the municipality in years 2004, 2006 and 2009, for which IBGE has collected the municipal database Pesquisa de Informações Básicas Municipais (MUNIC). Urban population (urban_pop), intergovernmental (transfers) and income inequality (theil) are drawn from the Brazilian Census of 2000, the last one before all audits. The first is the share of total municipal population living in urban areas, the second is the municipal share of revenues coming from the Federal and State government and the third is the Theil Index[12] for that particular municipality.

[10] Sample sizes vary much specially because of missing values on covariates.
[11] I run a hermeneutics classification procedure that is described in Appendix A.
[12] The higher is the index higher is the income inequality.

The next set of variables is the political vector. Political results from municipal elections are drawn from the Tribunal Superior Eleitoral (TSE) for elections in the years of 2000, 2004 and 2008. Mayor in second term (reelected_mayor) is captured by the dummy for mayors that have been reelected in the last election prior to the audit. Voter turnout (turnout) is the share of total registered voters in the municipality who voted in the election prior to the audit. Electoral competition (vote_margin) is measured by the difference in percentage points between first place and second place candidates in municipal election prior to the audit. Political parties (party_pres) are also taken into account by analyzing whether the respective mayor is of the same party as the federal government, i.e., the Workers' Party (PT). Although the Partido da Social Democracia Brasileiro (PSDB) was in power in 2002, which would show up in our database, the approximation here does not compromise estimation strategies.
Complementary data sources make up education, health and social development vectors. For the education matrix, I compute illiteracy rates (illiteracy) and average schooling years (schooling) in a given municipality from the Brazilian Census 2000. Health variables are drawn from the Ministry of Health DATAsus, which is the official statistical office of health policy in Brazil. Health transfers (htransfers) is the share of intergovernmental health transfers to total health revenues in municipality *m*, while personnel (personnel), investments (investment), medical supplies (medsupplies) and providers (providers) are the shares of total health revenues spent in payroll, capital accumulation, medicines procurement and services providers in the municipality in the three years prior to the audit. Last, social development covariates are indexes of human development (United Nations' HDI) and IFDM from an association of Brazilian industries (FIRJAN) – and the share of people employed to total workforce in a given municipality (employed).

There are, however, some shortcomings in the database I use. Audit by CGU is assumed to be independent of officials' or municipal's characteristics, which is a strong assumption from an anecdotal point of view. It is likely that local CGU offices might experience heterogeneous skill distribution among officials. But, if we expect subnormal skill distribution among any set of individuals, we would also expect that results would strengthen if all auditors were rigid enough. For the latter case, Ferraz & Finan (2008) have also discussed the issue and it does not seem to be the case. Another problem that might come up is the fact that municipal officials choose to deny access to their books, by reporting documents as missing. Once again, if that is the case, then my results should be augmented by full disclosure of information to auditors.

Table 2.4 presents descriptive statistics for the 883 municipalities in the sample. It is important to notice that all SOs in a given municipality will share the following independent variables. Variability is guaranteed across municipalities, SO amount and ministry. The number of municipalities is lower than the number of audit reports, since there are many municipalities with more than one ministry audited.

Table 2.4: Descriptive statistics for municipal and political vector

Variable	Obs	Mean	StdDev
gdp_capita	871	5989.46	5578.66
pop	882	26099.57	51272.29
judiciary	883	0.296	0.457
urban_pop	876	0.586	0.226
transfers	876	0.171	0.056
theil	876	0.527	0.109
reelected_mayor	882	0.297	0.457
turnout	883	0.875	0.057
vote_margin	882	16.82	19.22
party_pres	883	0.191	0.394

Source: IBGE, IPEA and TSE.

Problems might arise when converting information from the reports to the spreadsheets in the classification process. To minimize potential problems, a double-check method was applied. First, two research assistants classify each municipality. Then, the web-based system confronts each classification and presents the results to a third senior research assistant who decides whether to accept one or the other. It is intuitive to conclude that the process minimizes classification bias; all in all, its existence should not be disregarded completely.

Finally, the problem to be discussed later is that there is no open database of procurements done by Brazilian municipalities, which constrains us to instrument the procurement type by the SO amount in the database and to instrument procurement itself by analyzing the purpose text entry. The results here presented should then be interpreted in one of two ways: (i) first, although I do not have the instrumented variable but only the instrument, I expect that parameter signs are kept even when actual procurement data is available. I expect that correlation between main independent and dependent variable is in the right direction; (ii) not having actual procurements allows me to provide lower bounds of discretion effect on corruption; These issues will be discussed in greater depth in section IV.

3. METHODOLOGY

In this dissertation, I am interested in enlightening the following research question: do tighter procurement regulations result in lower corruption in public spending? The ideal experiment would be a random assignment of procurement rules to an unbiased sample of public purchases or public works in Brazilian municipalities. Auditing should also be randomized so public agents would not change their corrupt behavior *ex ante*; an alternative could be that audits were performed across sample after procurements were made without agents anticipating it[13]. Although such a setting is not feasible for obvious reasons countering social experiments, I believe that CGU audit program and the research design here employed might be a good approximation to this experiment.

The first advantage of this program is that CGU actually randomizes audits across municipalities. Sixty local governments are randomly chosen every lottery to take part in the audit. Every draw is made with replacement of municipalities. It is guaranteed, therefore, that there is not selection bias in municipality selection. Second, procurement types and regulations are mandatory to every public purchase or public work according to their amount. Despite I cannot completely control for public agents manipulating procurement amount to subject it to one or another category, it is possible to find exogenous variation around the cutoffs in which procurement types change to evaluate locally whether being subject to a tighter rule has had any impact on corruption prevalence.

Regression-discontinuity designs (RDD) are most welcomed in the economics of education literature (THISTLETHWAITE & CAMPBELL, 1960; ANGRIST & LAVY, 1999; VAN DER KLAAUW, 2002), although Lee (2008) finds interesting evidence on incumbent advantage in U.S. house elections. In this particular case, one would expect that observations of procurements around the cutoffs determined by Bill 8.666/93 are heterogeneous only on the category they are subject to, but not on other covariates.

Formally, I am estimating the following equation:

[13] For ideal field experiments, though not natural, see: OLKEN, B. A. Monitoring Corruption: Evidence from a Field Experiment in Indonesia. *Journal of Political Economy*, v. 115, n. 2, p. 200-249, 2007.
, BJORKMAN, M.; SVENSSON, J. Power to the People: Evidence from a Randomized Field Experiment on Community-Based Monitoring in Uganda. *Quarterly Journal of Economics*, v. 124, n. 2, p. 735-769, 2009.

$$corrup_{i,m} = \beta_0 + \rho_c z^c_{i,m} + \sum_k \gamma_k \left(x^0_{i,c,m}\right)^k + \beta_j X_{i,m} + \eta_{i,m} \qquad (3)$$

Which derives into (4.1) and (4.2):

$$pcorrup_{i,m} = \beta_0 + \rho_c z^c_{i,m} + \sum_k \gamma_k \left(x^0_{i,c,m}\right)^k + \beta_j X_{i,m} + \eta_{i,m} \qquad (4.1)$$

$$wcorrup_{i,m} = \beta_0 + \rho_c z^c_{i,m} + \sum_k \gamma_k \left(x^0_{i,c,m}\right)^k + \beta_j X_{i,m} + \eta_{i,m} \qquad (4.2)$$

Where $corrup_{i,m}$ is the corruption index for SO *i* in municipality *m*. I estimate equation (3) for the purchases group as $pcorrup_{i,m}$ (4.1) and the public works group as $wcorrup_{i,m}$ (4.2); ρ_c are the treatment parameters for each cutoff c = 1, 2 and 3; $z^c_{i,m}$ is the main independent variable, which is a dummy taking value 1 when the SO *i* in municipality *m* has monetary amount $x_{i,m}$ above cutoff *c*. Formally: $z^c_{i,m} = 1\left[x_{i,m} > c\right]$; $x^0_{i,c,m}$ are the amounts $x_{i,m}$ of the *i*th SO in municipality *m* centered[14] at cutoff c = 1, 2 and 3 and their polynomial parameters γ_k up to k = 4; $X_{i,m}$ is the covariates vector of municipal and political characteristics of SO *i* and municipality *m*; β_0 is a constant, β_j are parameters of covariates vector, and $\eta_{i,m}$ is the normally distributed error term. The hypothesis here is that the parameters of treatment conditions $z^c_{i,m}$ capture the impact of lower discretion on corruption. Also accounting for the differences among ministries, we add a ministry-specific vector to the equation. Their interactions represent binaries for ministries combined with their vector of education, health or social development. Hence:

$$pcorrup_{i,m} = \beta_0 + \rho_c z^c_{i,m} + \sum_k \gamma_k \left(x^0_{i,c,m}\right)^k + \beta_j X_{i,m} + \lambda_j MEC{\times}V_{i,m} + \lambda_j MS{\times}W_{i,m} + \lambda_j MDS{\times}K_{i,m} + \eta_{i,m} \quad (5)$$

$$wcorrup_{i,m} = \beta_0 + \rho_c z^c_{i,m} + \sum_k \gamma_k \left(x^0_{i,c,m}\right)^k + \beta_j X_{i,m} + \lambda_j MEC{\times}V_{i,m} + \lambda_j MS{\times}W_{i,m} + \lambda_j MDS{\times}K_{i,m} + \eta_{i,m} \quad (6)$$

Where *MEC*, *MS* and *MDS* are dummy variables taking value 1 when the SO is respectively from the education, health and social development ministries; vector $V_{i,m}$ is the educational

[14] I am letting x be the SO amount and x^0 the SO amount centered.

characteristics; vector $W_{i,m}$ represents health characteristics and vector $K_{i,m}$ social development characteristics, all with respect to SO i in municipality m.

One of the shortcomings of this research design is the manipulation of procurement amount by the public agent to select out of tighter procurement categories. For example, officials might lower overall contract value in order to procure items with lesser restrictions. This constitutes a fuzzy design because treatment condition is not a deterministic function of the amount procured. The alternative is to use the probabilities of being assigned treatment given that the treatment variable is above the threshold. These probabilities are a function of amount procured x_i minus cutoffs x_c, therefore the centralized measure $x^0_{i,c,m} = x_{i,m} - x_c$ in (3), (4.1), (4.2), (5) and (6). I should also test for the manipulation of the running variable and other covariates before I guarantee a fuzzy RDD is feasible. In addition, I have to test whether the probabilities of treatment differ between cutoffs (ANGRIST & PISCHKE, 2009) to make sure there is actually a cutoff in procurement categories – if not, I would conclude that manipulation is large enough to totally determine treatment.

3.1. RDD FEASIBILITY TESTS

Before I move on to the regressions, some tests are needed to guarantee it is a valid fuzzy RDD design. First, it is necessary to control for the manipulation of procurement amount by public agents. It is intuitive to believe that corrupt agents know that procurement rules are tighter depending on the amount procured and will try to manipulate procurement amount in order to escape from tighter rules. In particular for this research design, probability of treatment (to belong to a certain procurement category) is not a deterministic function of the amount procured x_i. Formally:

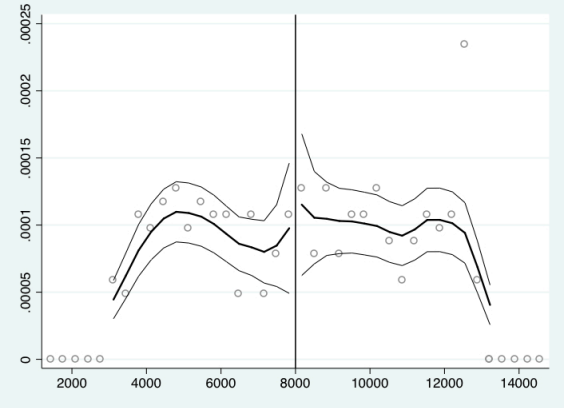
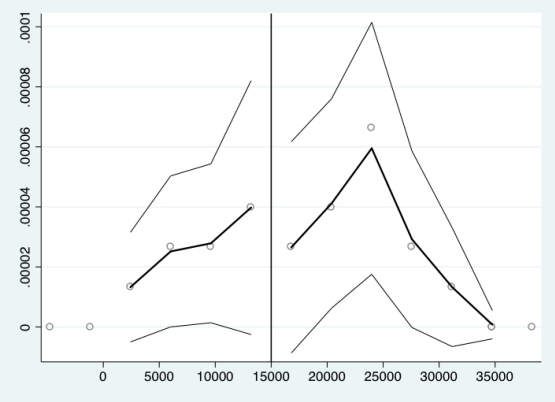
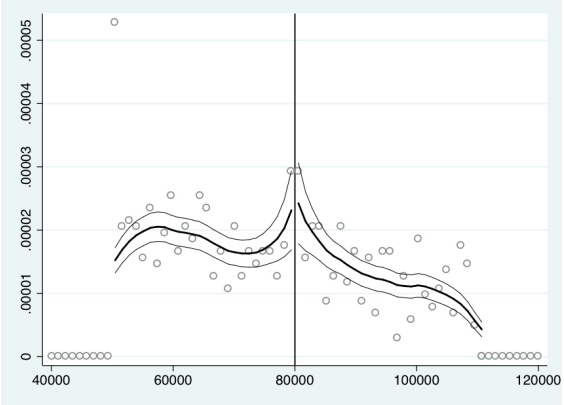
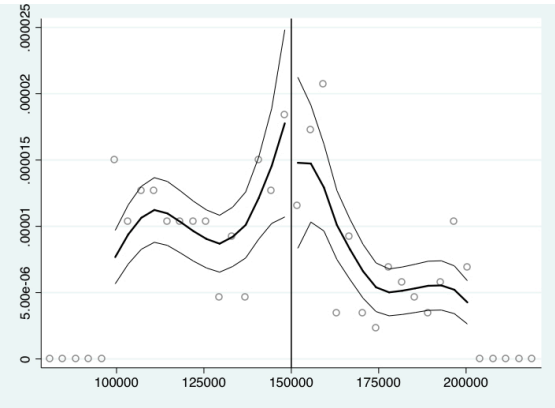
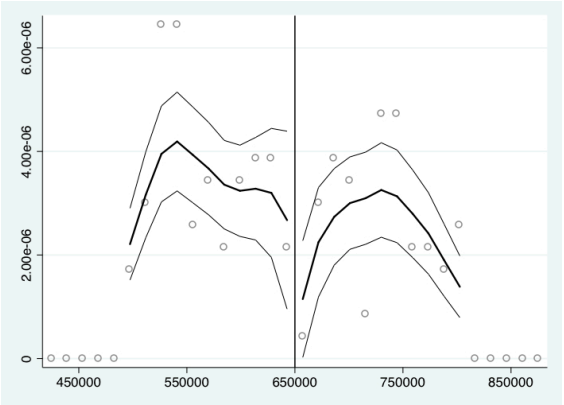
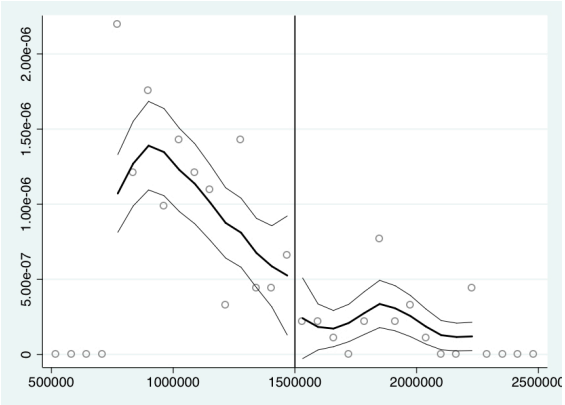
$$P\left(z^c_{i,m}\right) = \left(x_{i,m} - x_c\right) + \theta = x^0_{i,c,m} + \theta \qquad (7)$$

Where the probability of treatment $z^c_{i,m}$ for SO i in municipality m is given by the difference between amount $x_{i,m}$ and cutoff amount x_c, which is again my centralized variable:$x^0_{i,c,m} = \left(x_{i,m} - x_c\right)$. There is also some undetermined manipulation power of

public agent $\theta \leq 0$[15]. By construction, the larger is the right-hand side of equation (7), the higher is the probability of treatment. If $x_{i,c,m}^{0} >> 0$, it means that the amount procured $x_{i,m}$ is much over cutoff x_c and the agent has low manipulation power θ, therefore $P\left(z_{i,m}^{c} | x_{i,c,m}^{0} >> 0\right) \cong 1$. If $x_{i,c,m}^{0} \leq 0$, it means that the amount procured should fall in the category just below c, then $P\left(z_{i,m}^{c}\right) = 0$. Manipulation power does not matter in the latter because my assumption is that public agents never choose upper procurement categories because of tighter rules. I am left to prove that when $x_{i,c,m}^{0} > 0$ manipulation power θ is not large enough to balance $x_{i,c,m}^{0}$ out and turn $P\left(z_{i,m}^{c} | x_{i,c,m}^{0} > 0\right) \cong 1$ into $P\left(z_{i,m}^{c} | x_{i,c,m}^{0} > 0\right) \cong 0$. This means that probabilities just above cutoff c should differ from probabilities just under it (ANGRIST & PISCHKE, 2009). McCrary (2008) proposes a formal test to verify whether manipulation of the running variable is large enough to compromise the analysis around discontinuities. Results from his test are presented in Charts 3.1 to 3.6:

[15] For notation simplicity, I am assuming manipulation power is measured in R$ too. Manipulation power is negative because public agent can only manipulate a given procurement downwards.

Table 3.1.1: McCrary (2008) test for manipulation of running variable

Chart 3.1: First purchases cutoff	**Chart 3.4: First public works cutoff**
	
$N = 304$ and R\$3,000 $\leq x_i \leq$ R\$13,000	$N = 21$ and $x_i \leq$ R\$30,000
Chart 3.2: Second purchases cutoff	**Chart 3.5: Second public works cutoff**
	
$N = 883$ and R\$50,000 $\leq x_i \leq$ R\$110,000	$N = 233$ and R\$100,000 $\leq x_i \leq$ R\$200,000
Chart 3.3: Third purchases cutoff	**Chart 3.6: Third public works cutoff**
	
$N = 160$ and R\$500,000 $\leq x_i \leq$ R\$800,000	$N = 144$ and R\$750,000 $\leq x_i \leq$ R\$2,250,000

Source: CEPESP-FGV.

By analyzing the charts, we can conclude that the observable manipulation of procurements is not a problem here, because there is overlapping between frequency

estimates and confidence intervals[16]. Although they are not completely overlapping, especially in both purchases and public works cutoff three, it is easy to notice that they converge. Had the database being larger, we might have seen smoother densities estimates[17]. We notice that the only compromised sample is public works and yet it is so because there are not enough observations to test for manipulation. In any case, I choose not to estimate the first cutoff for the public works group. Even if it is not the goal here, I also run the discontinuity test for the other independent variables and find there is no discontinuity that could contaminate the results from discretion[18].

If I can drop variable manipulation power θ in all cutoffs without loss of generality, then I am making sure that probability of treatment is:

$$x_{i,c,m}^{0} > 0 \ \wedge \ x_{i,c,m}^{0} \geq |\theta| \Longrightarrow P(z_{i,m}^{c}) \cong 1 \qquad \text{and} \qquad x_{i,c,m}^{0} \leq 0 \Longrightarrow P(z_{i,m}^{c}) = 0 \qquad (8)$$

Equation (8) supports including variable $z_{i,m}^{c}$ in the regression analysis as the main independent variable. There is, however, a major problem with the data from Brazilian municipalities to run the fuzzy RDD. Since we cannot know for sure if a particular SO has generated a procurement of the same value, because the information on procurements held by municipalities is not readily available nor there is legal obligation of disclosure[19], there is no way of knowing if SOs have generated a procurement of the exact same amount[20]. When the Federal government grants local governments with transfers, municipalities might break them down and make more than one procurement with that amount. In theory, it would be possibly also to pack more than one grant into a larger procurement. I do not take that hypothesis into account for two main reasons: (i) in CEPESP's database there are 5,834 SO and none has been packed together with another SO; (ii) legal requirements of a SO are so comprehensive that it makes it unworthy to pack grants together.

[16] There can be an unobserved manipulation due to the fact that I do not have an actual procurement database. Although there is no alternative but having that data, I believe that had manipulation been the case, unobserved manipulation would spill over observed manipulation, for which there is no evidence.
[17] Appendix C presents the Kernel densities for those interested.
[18] I have all manipulation tests available upon request.
[19] Except at the time of procurement.
[20] That is why I instrument the procurement amount using the SO amount.

The lack of information on actual procurement imposes a restriction on the estimation strategy. According to Angrist & Pischke (2009), in the RDD approach local average treatment effect is given by equation (9):

$$\rho = \{E[y_i|z_i = 1] - E[y_i|z_i = 0]\}/\{E[s_i|z_i = 1] - E[s_i|z_i = 0]\} \qquad (9)$$

Where y_i is the dependent variable, z_i is the instrumental variable and s_i is the instrumented variable. If it is a sharp design, $E[s_i|z_i = 1] = 1$ and $E[s_i|z_i = 0] = 0$, then denominator equals 1 and numerator is the parameter of interest ρ. In this particular research design, we lack the database on variable s_i, which makes the denominator of the Wald estimator undetermined. Formally, let equation (9.1) and be my local Wald estimator:

$$\rho_c = \frac{E[corrup_{i,m}|z^c_{i,m}=1]-E[pcorrup_{i,m}|z^c_{i,m}=0]}{E[s^c_{i,m}|z^c_{i,m}=1]-E[s^c_{i,m}|z^c_{i,m}=0]} \qquad (9.1)$$

Where $s^c_{i,m}$ is the dummy taking value 1 when procurement is from category c and 0 otherwise. Although I cannot determine the denominator of (9.1), I know that most likely $E\left[s^c_{i,m}|z^c_{i,m} = 1\right] - E\left[s^c_{i,m}|z^c_{i,m} = 0\right] > 0$, because $E\left[s^c_{i,m}|z^c_{i,m} = 0\right] \approx 0$, if not always[21]. It is so because $E\left[s^c_{i,m}|z^c_{i,m} = 0\right]$ has amount procured $x_{i,m}$ below cutoff x_c by the properties of $P\left(z^c_{i,m} = 0\right) = x^0_{i,c,m} + \theta \leq 0$. This condition allows us to evaluate equation (9.1) with respect to parameter signs, since we know that denominator will not change the numerator sign. As to the magnitude of parameters, because the expected value of the denominator is less than or equal to 1, results will provide a minimal threshold of the effect of discretion on corruption. What I am doing here is estimating the RDD locally as if it were a sharp design. Because I know it is not sharp and also because of the specificity of the denominator being most likely positive, parameter signs from this strategy might provide us with good information – though not precise – on the behavior between lower discretion and corruption. Consequently, the parameter estimates represent a lower bound to the true coefficient.

[21] The only case in which $E\left[s^c_{i,m}|z^c_{i,m} = 0\right] \neq 0$ would be if public agents packed SO together, which I have not found in the sample. Because of the assumption that agents do not choose upper sets of procurement, there are no other cases where it could happen.

The last shortcoming is that we cannot actually determine exactly the group of a SO in Bill 8.666/93, purchases or public works, so I run a hermeneutics procedure to classify the SOs based on their purpose text entry, which is pretty much as close as we can get to the nature of the SO. Although we do have a careful approach to classifying SOs[22], there might arise problems when classifying a SO as purchases when it were actually from the public works group. In Appendix A I describe the procedure and it seems that the hermeneutics works pretty well to separate both groups. Then again, if we find results in a parsimonious approach and accounting for errors, it means that more comprehensive designs would reinforce results here presented.

I proceed to choosing the intervals around the cutoffs for each regression that will be tested. The goal of this approach is to verify whether results are consistent as we move closer to the threshold. If results are significant for the largest interval and keep their signs throughout all three ranges, it means that results are robust and describe well the effect of the exogenous variation on the dependent variable (i.e. lower discretion on corruption findings), even if we end up with very few observations statistically insignificant[23].

Table 3.1.2: Local estimates interval

	Cutoffs	Range 1	Range 2	Range 3
pcutoff1	R$ 8,000	± R$5,000	± R$3,000	± R$1,000
pcutoff2	R$ 80,000	± R$30,000	± R$15,000	± R$5,000
pcutoff3	R$ 650,0000	± R$150,000	± R$100,000	± R$50,000
wcutoff1	R$ 15,000	-	-	-
wcutoff2	R$ 150,000	± R$50,000	± R$20,000	± R$10,000
wcutoff3	R$ 1,500,000	± R$750,000	± R$500,000	± R$300,000

Source: Bill 8.666/93.

The first cutoff in the public works group is left aside because of the few observations in the subsample. The reason underlying this exclusion is that there are only a handful of observations found corrupt around the cutoff, which means that there is not enough variability in the dependent variable. I believe that one of the reasons for this is the fact that there are not many public works possible to be done with such low monetary amount.

[22] Again, it is the object of Appendix A.
[23] A thorough discussion can be found at Angrist & Lavy (1999).

I have tried an alternative in which it would be possible to calculate variable of interest $s_{i,c,m}$ on a subsample of SOs from CEPESP's database. Apart from reporting individual findings, the database also groups similar irregularities into larger sets, of which the most important is the procurement infractions one. In that set, there are two codes, 30 and 31, which flag an SO when certain procurement was required but has not been implemented. Hence, for this particular sample, I can know with certainty when the procurement type corresponded to the prescription of Bill 8.666/93. Formally:

$$s_{i,m} = \begin{cases} 1 \text{ if SO does not have codes 30 or 31} \\ 0 \qquad\qquad \text{if SO has codes 30 or 31} \end{cases} \quad (10)$$

From (10) I can build the Wald estimator (9.1) and (9.2). The central problem with this specification and mainly the reason why I do not report estimates here is that, by making use of such approach, I bias the corruption index because the probability of finding corruption evidence is larger in the procurement infractions set. The corruption index is composed by five procurement infractions codes and three private appropriation codes (see Table 2.1), while the total procurement infractions are nine. It means that flagging corruption almost certainly also flags procurement infractions. For further research, I should get around such limitations to truly report the parameters between discretion and corruption.

4. RESULTS

The first step into analyzing the results presented by the general sample is to verify whether there is correlation between the dependent variable, the corruption index, and independent variables. So I run simple regressions using *pcorrup* or *wcorrup* as the dependent variable (respectively the SO corruption indexes for the purchases and public works groups) against municipal and political characteristics of the SOs. It is very important, however, to state that these are not causal inferences, for endogeneity is not treated and nor the purpose of the analysis. The goal is make sure that the inclusion of these variables is theoretically and actually correct. Table 4.1 presents the results from those regressions.

Table 4.1: Regressions of corruption indexes against covariates matrix

dependent	pcorrup			wcorrup		
	OLS (1)	probit (2)	logit (3)	OLS (1)	probit (2)	logit (3)
gdp_capita	-4.97e-06	-1.46e-05	1.000	-3.37e-06	-9.08e-06	1.000
	[2.49]**	[2.24]**	[2.38]**	[0.98]	[0.95]	[0.95]
pop	3.18e-07	8.71e-07	1.0000	4.31e-07	1.09e-06	1.000
	[2.13]**	[2.17]**	[2.26]**	[1.28]	[1.24]	[1.26]
judiciary	0.0749	0.2174	1.4346	0.0232	0.0631	1.1061
	[4.06]***	[4.12]***	[4.17]***	[0.65]	[0.67]	[0.66]
urban_pop	-0.1236	-0.3491	0.5680	0.0385	0.1091	1.1857
	[3.09]***	[2.96]***	[2.89]***	[0.51]	[0.54]	[0.52]
transfers	0.1580	0.4669	2.0284	0.1721	0.4742	2.1094
	[1.07]	[1.10]	[1.00]	[0.62]	[0.64]	[0.62]
theil	0.0213	0.0769	1.1387	-0.0026	0.0004	0.9954
	[0.30]	[0.38]	[0.39]	[0.02]	[0.00]	[0.01]
reelected_mayor	-0.0327	-0.0907	0.8552	-0.0296	-0.0800	0.8795
	[1.91]*	[1.81]*	[1.87]*	[0.93]	[0.94]	[0.92]
turnout	-0.5848	-1.6598	0.0695	-0.3638	-0.9803	0.2043
	[3.70]***	[3.67]***	[3.58]***	[1.25]	[1.26]	[1.26]
vote_margin	-1.18e-05	-6.1e-05	0.9999	0.0006	0.0016	1.0027
	[0.03]	[0.05]	[0.04]	[0.78]	[0.79]	[0.79]
party_pres	-0.0376	-0.1095	0.8347	-0.0686	-0.1894	0.7367
	[1.93]*	[1.88]*	[1.86]*	[1.76]*	[1.73]*	[1.70]*
_cons	0.8669	1.0640	1.000	0.6387	0.3834	1.000
	[5.69]***	[2.47]**	[2.38]**	[2.25]**	[0.51]	[0.95]
R^2	0.02			0.01		
N	3,509	3,509	3,509	1,102	1,102	1,102

Source: CEPESP-FGV, IPEA, IBGE and TSE.

Note: *For the logit estimates I report odds ratios instead of parameters.*

* $p<0.1$; ** $p<0.05$; *** $p<0.01$.

One observation stands out in this first test, which is that most significance comes from the public purchases group. The first reason for this is the larger database on purchases' SOs,

which is not only because there exists in fact more purchases than public works done by Brazilian municipalities, but also because the hermeneutics procedure was better accurate at the purchases level than on the public works level. Throughout the remaining of this dissertation, the larger sample size helps the analysis of purchases SOs, whose results are better when analyzing the effect of discretion on corruption.

GDP per capita is negatively related to corruption in both groups, albeit in very small magnitudes, and only statistically significant for the purchases group. These results are consistent with Fan et al. (2009) and Fisman & Gatti (2002), who both report statistically significant negative coefficients for GDP per capita. Likewise, parameter for population in a given municipality is statistically significant and positive at the purchases level. Special attention should be devoted to the judiciary variable. It is intuitive to believe in correlation between both variables, therefore the statistical significance at 1% level for the purchases group, but one would expect them to be negatively correlated. One possibility why this is not the case might be the fact that the judicial system in Brazil is somewhat distributed accordingly to places where crime also is more prevalent, leading us to the positive correlation[24]. Since I do not test for this hypothesis nor is the focus here, I leave it aside for further research. Results for urban population differ across groups, although I believe had the data on public works been larger, I would have found the same results as in the purchases group. The supporting hypothesis is that a higher share of urban inhabitants in a given town would lead to better access to information and, therefore, individuals monitoring political activities (Ferraz & Finan, 2008; Bjorkman & Svensson, 2009).

As to the political vector, variable vote margin is not statistically significant for neither groups, and yet intuition tells us that mayors elected with larger margins have greater support from the population, hence would feel more freedom to engage in corrupt activities. Turnout and party of president have the expected results, the former meaning more political activism therefore more political accountability on corruption scandals (CHONG et al. (2011); STOCKEMER et al., 2012) and the latter meaning political alliances that impute both politicians in case of corruption[25], hence lower prevalence of corruption if same party. For this second case, there is another alternative, which is the possibility that auditors were

[24] Provided I do not treat for endogeneity, the causality relationship is unknown.
[25] Li et al. (2008) find results for political ties with private agents. One would expect, therefore, that results be stronger when political agents are on both ends.

softening the auditing of grants in those municipalities from the same party as the Federal government, which casts doubt on whether the auditing program was truly unbiased. I do not believe it is the case because works with similar data (Ferraz & Finan, 2008; 2011) have supported the evidence that the auditing program is unbiased. The negative coefficient on reelected mayor is somewhat counterintuitive, as one would expect from Ferraz & Finan (2008) that corruption scandals reduce probability of reelection. A possible although less likely explanation to this phenomenon might be the timing of both researches. In Ferraz & Finan (2008), CGU audit reports were fresh of the book and totally unanticipated by mayors and authors profit from the first election after the audit program was in place. As time went by, mayors pushed the Federal government and managed to reduce audits every year, therefore lowering expectations of being caught, even in the second term in office[26]. On the other hand, the more likely alternative is that corrupt politicians are punished by their behavior in electoral outcomes, which means that fewer corruption prone mayors would go on to a second term in office, which finds support in Ferraz & Finan (2008)'s results that corrupt mayors have lower probabilities of reelection than non-corrupt ones.

Apart from testing the results of the covariates dealt with in the literature, I run the regressions on *pcorrup* and *wcorrup* adding the amount procured variable x_i[27], which is close to the research question. The goal here is to evaluate whether there is correlation between the amount procured and corruption *per se*, that is, if corruption is condition on the amount procured or the other way around. Results are presented in Table 4.2.

[26] It is quiet premature to attest that the audit program has been efficient in preventing corruption. I should underline that this is not the objective of this paper and will be left out for the obvious reasons.
[27] I do not use the centralized amount yet since this is the whole sample in which I am just drawing conclusions on correlation issues.

Table 4.2: Regressions of corruption indexes against covariates matrix adding amount procured

dependent	pcorrup			wcorrup		
	OLS	probit	logit	OLS	probit	logit
	(1)	(2)	(3)	(4)	(5)	(6)
amount	2.33e-09	5.40e-09	1.0000	1.77e-09	4.35e-09	1.0000
	[0.64]	[0.71]	[0.29]	[0.24]	[0.23]	[0.23]
gdp_capita	-5.01e-16	-1.46e-05	1.0000	-3.37e-06	-9.08e-06	1.0000
	[2.51]**	[2.25]**	[2.40]**	[0.98]	[0.95]	[0.95]
pop	3.09e-07	8.52e-07	1.0000	4.24e-07	1.08e-06	1.0000
	[2.06]**	[2.12]**	[2.12]**	[1.26]	[1.22]	[1.23]
judiciary	0.0736	0.2140	1.4256	0.0226	0.0614	1.1031
	[3.98]***	[4.05]***	[4.07]***	[0.63]	[0.65]	[0.64]
urban_pop	-0.1227	-0.3465	0.5700	0.0373	0.1060	1.1797
	[3.07]***	[2.94]***	[2.87]***	[0.50]	[0.52]	[0.51]
transfers	0.1543	0.4583	2.0013	0.1747	0.4808	2.1326
	[1.05]	[1.08]	[0.98]	[0.63]	[0.65]	[0.63]
theil	0.0189	0.0705	1.1237	-0.0045	-0.0046	0.9876
	[0.27]	[0.35]	[0.35]	[0.03]	[0.01]	[0.02]
reelected_mayor	-0.0327	-0.0908	0.8557	-0.0290	-0.0786	0.8815
	[1.91]*	[1.81]*	[1.85]*	[0.91]	[0.92]	[0.90]
turnout	-0.5794	-1.6470	0.0714	-0.3584	-0.9667	0.2088
	[3.67]***	[3.64]***	[3.53]***	[1.23]	[1.24]	[1.24]
vote_margin	-0.0001	-0.0002	0.9997	0.0006	0.0016	1.0026
	[0.12]	[0.14]	[0.12]	[0.76]	[0.77]	[0.77]
party_pres	-0.0371	-0.1081	0.8371	-0.0684	-0.1887	0.7375
	[1.90]*	[1.86]*	[1.83]*	[1.75]*	[1.72]*	[1.69]*
_cons	0.8640	1.0575	1.0000	0.6346	0.3733	1.0000
	[5.68]***	[2.46]**	[0.29]	[2.23]**	[0.50]	[0.23]
R2	0.02			0.01		
N	3,509	3,509	3,509	1,102	1,102	1,102

Source: CEPESP-FGV, IPEA, IBGE and TSE.
Note: *For the logit estimates I report odds ratios instead of parameters.*
* $p<0.1$; ** $p<0.05$; *** $p<0.01$.

The hypothesis of this research is that there is no relationship between the two variables except for the procurement type that the amount entails. From Table 4.2, we observe that there seems to be no correlation between amounts procured and corruption indexes, because all parameters are statistically insignificant. Even if it were to exist a relationship between the two variables, the regression-discontinuity approach should be able to minimize such effect, which is why I believe the results above suffice for the hypothesis supporting the instrumental variable approach.

I also test for correlation between independent variables to avoid multicollinearity problems. Although multicollinearity would be expected between variables, correlation matrix does not

provide values over |.38| for all variables. Specifically for the amount variable, the correlations are bounded by |.18| with all variables[28].

4.1. LOCAL ESTIMATES

Provided I have defined the correct specifications, I am able to estimate local regressions on the intervals around cutoffs one, two and three in both the purchases and the public works group. The intervals are those from Table 3.1.1. It is important to note once again that I do not have Wald estimators since I cannot estimate the denominator. Consequently the magnitude of the coefficients presented is not correct; they provide lower bounds of corruption effects. For that particular reason, I do not discuss parameter magnitude.

The approach at hand is the best exogenous variation to the research question. I set three ranges of procurement amount to analyze whether there has been a causal impact on corruption[29]. The estimates contain the covariates matrix in order to partial out the effect they might have on the corruption index by themselves and to test for misspecifications of the regression equation. Keeping the covariates matrix adds precision to the parameter of interest ρ_c, provided there is not correlation between the main independent variable and the vector of covariates. In the previous section, I have found evidence to the precision argument and to support keeping all covariates while running local regressions between corruption indexes against centralized amount $x_{i,c,m}^{0}$ and political and municipal characteristics.

Table 4.1.1: Local regressions for the first purchases cutoff (R$ 8,000)

	± R$ 5,000			± R$ 3,000			± R$ 1,000		
	OLS (1)	probit (2)	logit (3)	OLS (1)	probit (2)	logit (3)	OLS (1)	probit (2)	logit (3)
ρ_1	0.169	0.841	1.525	-0.044	0.307	0.263	-0.145	0.784	1.014
	[1.39]	[0.36]	[0.48]	[0.29]	[2.11]**	[1.38]	[0.48]	[0.17]	[0.01]
R^2	0.04			0.07			0.22		
N	301	301	301	174	174	174	57	57	57

Source: CEPESP-FGV, IPEA, IBGE and TSE.

[28] I provide all correlations upon request.

[29] Benefiting from the RDD approach, here I try to infer causal relationships between variables since observations are supposedly homogeneous except for the category of procurement.

Note: *Dependent variable is pcorrup and regressions are controlled by political and municipal characteristics of the SO. Some of the observations are dropped because of missing values in the independent variables vector. I report odds ratio for probit and logit estimates.*
* $p<0.1$; ** $p<0.05$; *** $p<0.01$.

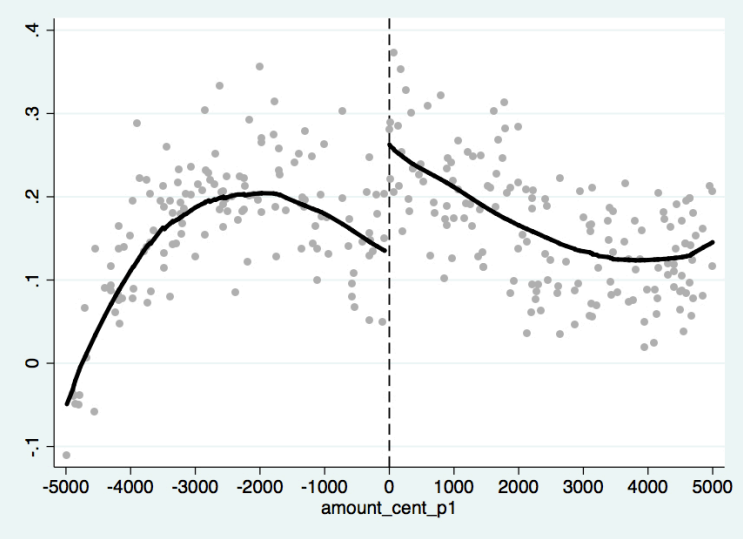
Parameter ρ_1 has mostly positive signs for all intervals, except OLS estimates in medium and small range. One parameter, probit estimate in the medium interval, has significant and positive sign on the relationship between discretion and corruption. One thing that stands out, therefore, is the general positive relationship between discretion and corruption, which is contrary to the hypothesis in this dissertation – lower discretion correlates negatively with corruption. Changing from category zero, leave of public tender, to category one, invitation bidding, there seems to be no decreasing corruption when public agents are faced with tighter procurement rules.

One of the possibilities for the results above is that agents do not perceive such change as an increased cost of corruption (corruption benefits when changing procurement type still marginally outweigh costs). Although only one of the parameters is significant, we will observe the same tendency throughout local estimates, which supports the previous argument on the positive relationship between lower discretion and corruption. Fisman & Gatti (2002), who use decentralization as a proxy to discretion, present similar results, in which decentralized governments have reported less corruption when compared to centralized governments. In this setting, higher discretion would only have positive results, by means of better resource allocation and lower corruption. Treisman (2000) and Fan et al. (2009), however, have found that decentralization increases corruption by lowering controls on how the money is used by local governments.

In particular, another possibility arises if I decide to drop parameters because of their insignificancy, which is the inference that procurement rules in Bill 8.666/93 are imposing a cost to the public sector by means of creating burocratic constrains to purchases or public works without having the desired result of reducing misallocation of funds. If one expects agents to behave rationally, they would raise corruption prices to compensate for the constraints they have to overcome in order to favor any private firm, hence reinforcing the distortions of corruption on efficiency. According to the *rationale* of Acemoglu & Verdier (2000), the attempt by the government to reduce corruption by introducing Bill

8.666/93 might have created a larger inefficiency than accepting a lower level of corruption in purchases and public works without procurement rules and categories.

Chart 4.1.1: Predicted corruption in first purchases cutoff



Source: CEPESP-FGV.
Note: *Points are OLS corruption fitted values and solid line is lowess fit. N =* 304 (largest range).

The distribution of fitted values around the cutoff in the largest interval supports the positive signs from Table 4.1.1, although the supposed discontinuity is not statistically significant. Analyzing both Table 4.1.1 and Chart 4.1.1, I would argue that we should disregard parameters altogether because of their insignificance and also because there could be a continuous regression line on both sides of purchases cutoff one with no jumps of fitted values. One can easily notice that by the dispersion of fitted values in the graph.
As to the first cutoff in the public works group, I choose to leave it out since the small number of observations prevents us from statistical inference. In spite of the hermeneutics procedure, which was less accurate for the group because of CEPESP coding structure, I believe the main reason for only a handful of observations in public works is that there is a very limited number of construction or remodeling activities that can be done with such monetary amount. Even in smaller towns, which is the major part of Brazilian municipalities, my intuition is that school or health clinic construction surpasses by far the R$ 15,000 mark.

Results from the second cutoff in the purchases and public works groups support the evidence that procurement rules have no effect on reducing corruption in public spending.

Again, the most important conclusion is that there is a positive relationship between tighter rules and corruption findings. Parameter ρ_2 is mostly statistically significant at 5% in purchases' intervals.

Table 4.1.2: Second purchases cutoff (R$ 80,000)

	± R$ 30,000			± R$ 15,000			± R$ 5,000		
	OLS (1)	probit (2)	logit (3)	OLS (1)	probit (2)	logit (3)	OLS (1)	probit (2)	logit (3)
ρ_2	0.013	0.387	0.402	0.031	0.400	0.425	-0.033	0.331	0.307
	[0.16]	[4.13]***	[2.34]**	[0.29]	[3.07]***	[1.70]*	[0.20]	[2.33]**	[1.46]
R^2	0.03			0.03			0.07		
N	877	877	877	429	429	429	175	175	175

Source: CEPESP-FGV, IPEA, IBGE and TSE.
Note: *Dependent variable is pcorrup and regressions are controlled by political and municipal characteristics of the SO. Some of the observations are dropped because of missing values in the independent variables vector. I report odds ratio for probit and logit estimates.*
* p<0.1; ** p<0.05; *** p<0.01.

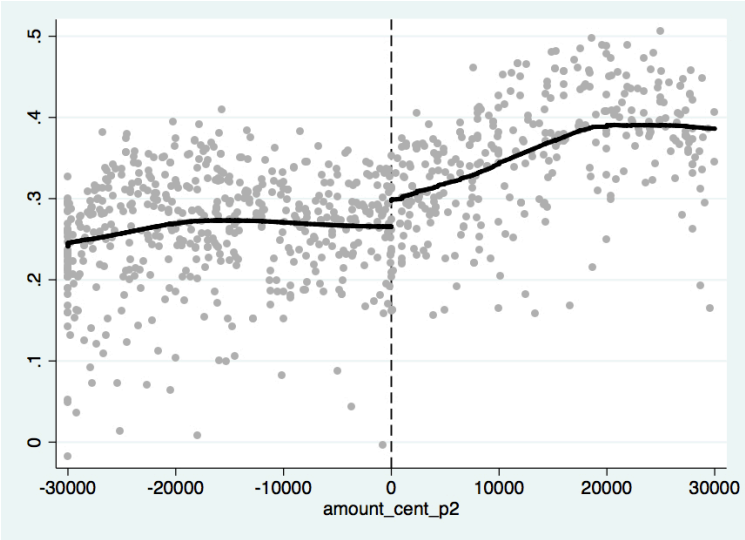
Table 4.1.3: Second public works cutoff (R$ 150,000)

	± R$ 50,000			± R$ 20,000			± R$ 10,000		
	OLS (1)	probit (2)	logit (3)	OLS (1)	probit (2)	logit (3)	OLS (1)	probit (2)	logit (3)
ρ_2	0.217	0.660	0.958	0.315	0.879	1.443	0.428	1.158	2.462
	[1.53]	[1.06]	[0.07]	[1.47]	[0.18]	[0.30]	[1.57]	[0.15]	[0.50]
R^2	0.11			0.33			0.38		
N	233	233	233	107	107	107	76	76	76

Source: CEPESP-FGV, IPEA, IBGE and TSE.
Note: *Dependent variable is pcorrup and regressions are controlled by political and municipal characteristics of the SO. I report odds ratio for probit and logit estimates.*
* p<0.1; ** p<0.05; *** p<0.01.

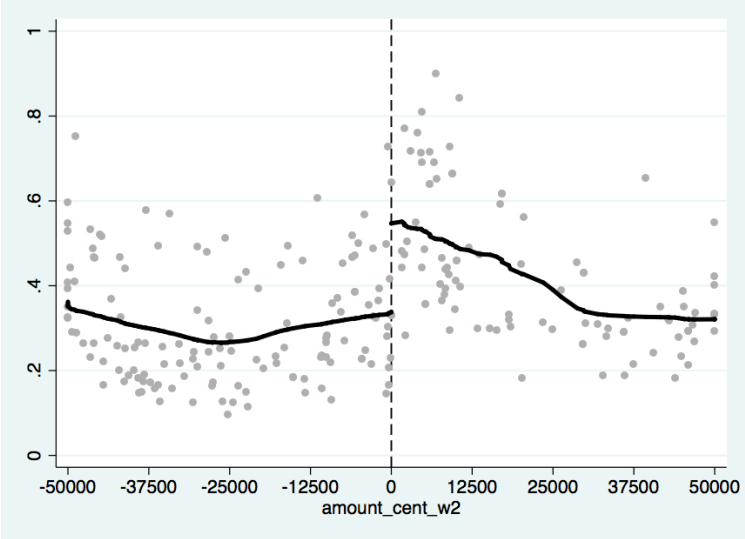
In this scenario, I should reinforce the argument that it seems that public agents do not shift their expected corruption utility to negative figures. I have reason to believe this is the case because of discretion models introduced by Bill 8.666/93. There could be another argument – not discussed in the previous cutoff – that it is easier for auditors to find irregularities as they move up procurement categories. The underlying assumption is that public officials would have to abide by a larger set of prescriptions as the amount procured increased, therefore every SO would be more likely to incur in corruption practices. Although I do not test this hypothesis, leaving it for further research, I believe it is possible to be one of the alternatives, since all cutoffs have positive signs – though not all of them significant – and there seems to be no sign of decreasing corruption. Anyhow, it also supports the hypothesis from Fisman & Gatti (2002).

Chart 4.1.2: Predicted corruption in second purchases cutoff



Source: CEPESP-FGV.
Note: *Points are OLS corruption fitted values and solid line is lowess fit. N =* 883 (largest range).

Chart 4.1.3: Predicted corruption in second public works cutoff



Source: CEPESP-FGV.
Note: *Points are OLS corruption fitted values and solid line is lowess fit. N =* 233 (largest range).

In Chart 4.1.2, I report the non-significant OLS parameter. I decide to keep the non-significant one even when there were many significant others for the sake of comparability between charts. However, supported by the other significant results, I have reason to believe that if there is any causal discontinuity between cutoffs, it is likely to be between categories two and three of the purchases group. For the public works group, two evidences lead to disregarding parameter ρ_2. First, no estimate has significant results.

Second, if we take the discontinuity in Chart 4.1.3 for non-significant, it is clear that a regression fit to all observations would also be drawn with no jumps of fitted values.

For the last cutoff in purchases and public works, we observe results being kept for most, if not all, estimates. In Tables 4.1.4 and 4.1.5, most parameters ρ_3 are positive in the intervals around cutoffs:

Table 4.1.4: Third purchases cutoff (R$ 650,000)

	± R$ 150,000			± R$ 100,000			± R$ 50,000		
	OLS (1)	probit (2)	logit (3)	OLS (1)	probit (2)	logit (3)	OLS (1)	probit (2)	logit (3)
ρ_3	0.525	1.670	4.276	0.126	0.400	0.374	-0.237	0.138	0.071
	[2.04]**	[0.72]	[1.19]	[0.32]	[0.86]	[0.54]	[0.20]	[0.72]	[0.59]
R^2	0.15			0.17			0.24		
N	160	160	160	94	94	94	41	41	41

Source: CEPESP-FGV, IPEA, IBGE and TSE.
Note: *Dependent variable is pcorrup and regressions are controlled by political and municipal characteristics of the SO. I report odds ratio for probit and logit estimates.*
* p<0.1; ** p<0.05; *** p<0.01.

Table 4.1.5: Third public works cutoff (R$ 1,500,000)

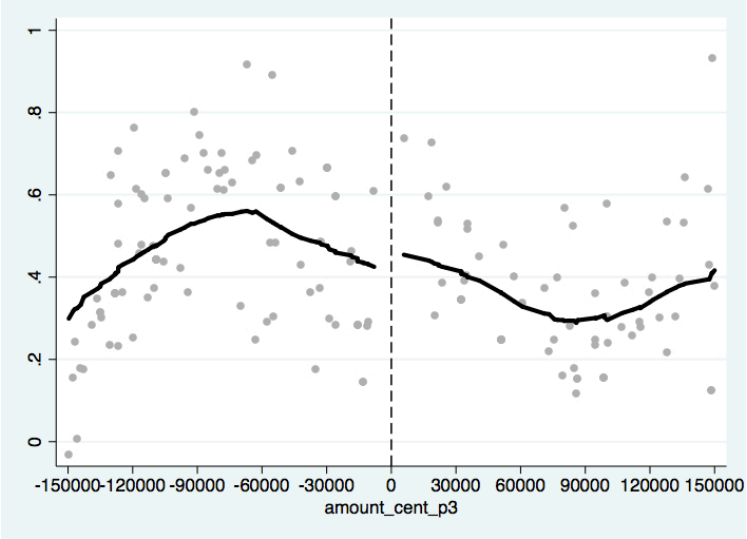
	± R$ 750,000			± R$ 500,000			± R$ 300,000		
	OLS (1)	probit (2)	logit (3)	OLS (1)	probit (2)	logit (3)	OLS (1)	probit (2)	logit (3)
ρ_3	0.120	0.590	0.834	-0.095	0.040	0.012	-	-	-
	[0.34]	[0.53]	[0.10]	[0.28]	[2.46]**	[1.93]*			
R^2	0.18			0.15					
N	144	144	144	77	77	77	35	34	34

Source: CEPESP-FGV, IPEA, IBGE and TSE.
Note: *Dependent variable is pcorrup and regressions are controlled by political and municipal characteristics of the SO. I report odds ratio for probit and logit estimates.*
* p<0.1; ** p<0.05; *** p<0.01.

In Table 4.1.4, I find another significant result in the purchases group, which is the positive sign in the OLS parameter for the largest interval. Consistent with signs from other cutoffs, I would argue that there is likely a positive relationship between corruption and lower discretion, just as such reported in Fisman & Gatti (2002). I would not argue, however, that there is a causal relationship between both variables because all but purchases cutoff two have not showed signs of jumps in regression lines, even when I am pushing it to exist, which is the case from Charts 4.1.1 to 4.1.5[30].

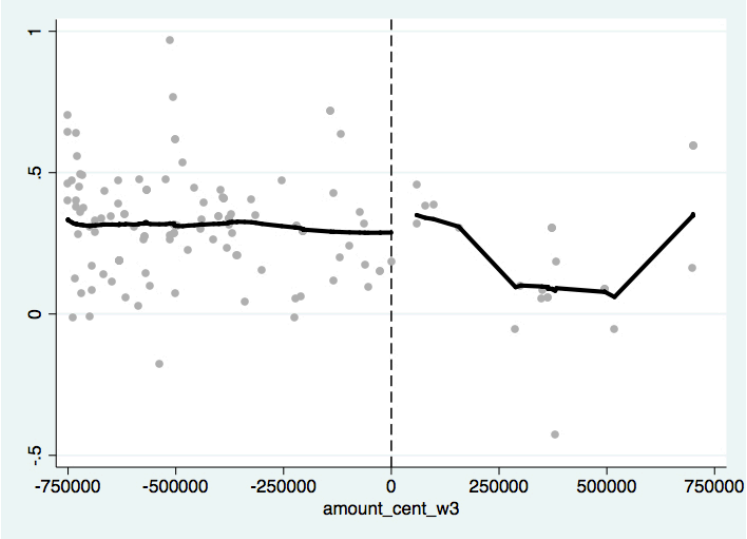
Chart 4.1.4: Predicted corruption in third purchases cutoff

[30] I am estimating lowess fits on both sides of cutoffs.



Source: CEPESP-FGV.
Note: *Points are OLS corruption fitted values and solid line is lowess fit. N = 160* (largest range).

Chart 4.1.5: Predicted corruption in third public works cutoff



Source: CEPESP-FGV.
Note: *Points are OLS corruption fitted values and solid line is lowess fit. N =* 144 (largest range).

Hence the results of the general sample regressions show that most parameters in both groups are generally insignificant when it comes to the relationship of discretion and corruption in Brazilian municipalities. Significant results are fewer and, when present, show positive sign for the discretion parameter. The strongest conclusion I come to is that procurement categories from Bill 8.666/93 are not reducing corruption and might be actually overburdening public agents in their purchases and public works, which would lead to two untested consequences: (i) rising corruption prices and higher inefficiencies in public funds allocation; or (ii) rising corruption in procurement categories because of

higher probability of incurring in irregularities since rules in upper categories are more comprehensive than in lower spending decisions. For the significant cases, I would argue that, if any, the results from the second cutoff show that public agents consider procurement categories two and three as inducing an increase in corruption, for it is the only discontinuity I believe it exists. Finally, once again one notices that I have not discussed magnitude of parameters because I lack instrument denominator.

4.2. LOCAL ESTIMATES SECTIONED BY MINISTRY

One additional test is available in the general sample regressions. Since CEPESP researchers collect data from the Ministries of Health (MS), Education (MEC) and Social Development (MDS), it is possible to partial out the effect of corruption by the characteristics of each ministry in the sample. Although it does not account for all grants from the Federal government to Brazilian municipalities, those three ministries are responsible for two thirds of total transfers, which should provide a good approximation of all grants made available to municipalities. The goal here is to test against particular characteristics of SOs in each group; if I have found that most variability comes from the purchases group, then it is reasonable to think that there might also be heterogeneous results in each ministry. Testing for heterogeneity contributes to parameters accuracy and precision, in addition to reducing omitted variable bias in the former section.

In practice, I am estimating equations (5) and (6) from section II for the middle range intervals around the three cutoffs. I choose such intervals for matters of sample size and amount procured of SO. Although the largest interval in each threshold is the best option with respect to sample size, it is likely to present more noise because of heterogeneous SO amount. The same logic applies to the shortest interval, the best on cutoff distance but with such small samples that could compromise a whole cutoff analysis.

Table 4.1.6: Ministry subsample for purchases

R$ 8,000 ± R$ 3,000			R$ 80,000 ± R$ 15,000			R$ 650,000 ± R$ 100,000		
OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS

	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
ρ_1	0.156	0.140	1.103						
	[0.81]	[0.55]	[3.39]*						
ρ_2				0.102	-0.151	0.341			
				[0.70]	[0.86]	[1.20]			
ρ_3							-0.998	0.125	-
							[0.90]	[0.20]	-
R^2	0.08	0.74	1.00	0.10	0.13	0.21	0.49	0.38	-
N	110	43	21	210	150	69	30	57	7
MEC	Yes	-	-	Yes	-	-	Yes	-	-
MS	-	Yes	-	-	Yes	-	-	Yes	-
MDS	-		Yes	-	-	Yes	-	-	Yes

Source: CEPESP-FGV, IPEA, IBGE and TSE.
Note: *I concentrate on OLS estimates for better table presentation and comparability. Dependent variable is pcorrup and regressions are controlled by political and municipal characteristics of the SO. I do not report estimates with samples under 30 observations.* * p<0.1; ** p<0.05; *** p<0.01.

Most results in the purchases group, though non-significant, maintain their positive signs from the general sample regressions. There is one main change, which is also non-significant: (i) MS parameter ρ_1 changes signs from positive to negative. Since there are almost non-existent discrepancies from the general specification and they are statistically insignificant, I expect the results from the non-controlled sample to be homogeneous to particular SO samples. Once again, I am not treating the instrument denominator problem, so no magnitude concerns. For the public works group, just as in its first cutoff, very few observations for each ministry compromise altogether the analysis in this group.

Therefore, we observe that the general non-controlled sample regressions find evidence of good specification when tested against their ministry-controlled subsamples. Special attention should be devoted to findings in the social development fields in the purchases group – MDS is not estimated in the public works group. I find lesser observations related to corruption because of the characteristics of such ministry. Most programs under their responsibility are management-oriented policies instead of intergovernmental transfers to local implementation. Which is to say that municipalities are responsible for aiding the Ministry of Social Development in their programs instead of receiving money transfers and choosing at will where to spend them; hence, particularly in MDS SOs, discretion is restricted in the first place. The Eradication of Child Labor Program (PETI in Portuguese) is the main public policy under the MDS administration that presents corruption findings, because of its unique characteristics of monetary transfers to take children off labor activities. All variability in the MDS ministry comes from PETI.

5. CONCLUSION

The goal of this dissertation is to test whether tighter procurement rules have reduced corruption prevalence in Brazilian municipalities. By drawing upon the economics literature about the determinants of corruption [a review might be found at Bardhan (1997)], I am formally testing whether discretion could be used to deter corruption in the public realm. A close proxy to discretion has been used in Treisman (2000), Fisman & Gatti (2002) and Fan et al. (2009), where the authors test the impact of decentralization on corruption findings.

Brazilian municipalities are the scope of this dissertation, particularly because of an auditing program introduced by the Brazilian Federal government that aims to deter corruption in Federal grants transferred to municipalities. The responsible office CGU publishes periodically reports in which they describe corruption findings by ministry and by public policy employed. I am then able to build an objective measure of corruption using CEPESP-FGV database. From the analysis of CGU audit reports, I have built an objective corruption, which is based in Lopes (2011) and Ferraz & Finan (2008); (2011), but I do not aggregate corruption findings by municipality. From Bill 8.666/93, I build the discretion variable to verify whether there exists an impact of discretion rules on corruption prevalence in subgroups of public expending in purchases or public works in the municipality.

Broadly speaking, results show that discretion has not had an impact in decreasing corruption. If any, on the same lines as those in Fisman & Gatti (2002), the effect has been positive for all cutoffs analyzed. There are a few hypotheses to the results found in this dissertation: (i) the lack of enough observations in the sample, which would make parameters insignificant or prevent the correct estimation by overweighting the corrupt SOs just above cutoffs. The reason supporting this hypothesis is that I have found a few different parameters from OLS and other specifications; (ii) there is not actually a negative relation between tighter rules (lower discretion) and corruption prevalence. In this setting, I could argue that public agents do not discriminate between any of the procurement categories, so they do not avoid engaging in corrupt activities as the procurement amount increases. In other words, the opportunity cost of corruption does not rise as much as it would be expected. Duggan & Levitt (2002) report a similar setting in which sumo wrestlers in Japan engage in corruption whenever opportunity costs are low enough; (iii) corruption and reduced discretion correlate positively because of

the fact that Bill 8.666/93 might not be doing a good job of preventing corruption, but instead it is increasing corruption cost by means of higher bribes or diverted funds. Therefore, when amount procured raises, public agents would find more room to misallocate funds to compensate for higher costs of corruption in public procurement in municipalities. By trying to correct the inefficient corruption in public procurement in Brazil, Bill 8.666/93 might have introduced a greater distortion by means of resources misallocation when a lower level of corruption would be preferable (ACEMOGLU & VERDIER, 2000).

I have reason to believe that if I manage getting access to the actual procurement database, it would help into finding the precise magnitudes of parameters here presented. For the parameter signs significance and consistency throughout ranges around the intervals, the solution is a broader database of CGU audit reports, which is currently being done by researchers at CEPESP-FGV. In line with those developments, suggestion for further research should take into account the following objectives: (i) first, to construct an accurate instrument in order to locally evaluate corruption with more observations; (ii) second, I have reason to believe that most misallocation might happen in the procurement process itself, which would be correlated to what we have seen here, but not exactly the same research field. It could consist not only on money diversion, but also on favors exchanged between private invitees and procurement commission or municipal officials. For this particular setting, I would recommend the contribution from auction theory mechanism, in order to understand better how agents behave when they have different sorts of discretionary power and how they can contribute to favoring one particular company.

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Appendix A: Hermeneutics

To test for corruption in this research design, it was necessary that I made a classification of the SOs in the database. As mentioned before, an SO is an order given by the central CGU office to its regional agents to audit specific funds from a particular ministry. By looking at its purpose, it is possible to determine whether those funds were used in the first place to either one of the two: (i) public purchases or (ii) public works. These are the two criteria that allow the creation of an objective measure of corruption, because those funds have clear inputs and outputs. i.e., money earmarked for buying medicines but never realized.

I have done so by applying a qualitative analysis of the purpose text entries for each of the SOs in the sample, 5,834 observations. Each text entry in the database is a description of the auditing taking place in that policy. I defined a group of words to flag purchases and works. If it had the words I had chosen, a 1 would come up and that SO would be added to one or the other group. Not only have I used single words, but I have also used combination of words to capture the full extent of SOs. My hermeneutics is described below:

Table A.1: Keywords for defining both groups

#	Purchases	SOs flagged	#	Works	SOs flagged
1	Acquisition(s)	1,886	**1**	Construction(s)	444
2	Execution(s)	1,258	**2**	Infrastructure	377
3	Vehicles	451	**3**	Addition	232
4	Expense(s)	389	**4**	Water Supply	168
5	Medicines+PEAF	305	**5**	Work(s)	163
6	School Transport	130	**6**	Implantation(s)+(¬PSF)	157
7	Purchase(s)	67	**7**	Sanitary+module	148
8	PNATE	48	**8**	Repair(s)	142
9	Acquired	32	**9**	Sewage	127
10	Kit	21	**10**	Physical+improvements	48
11			**11**	Sanitary+improvements	27
12			**12**	Betterment	1
	TOTAL	4,587		**TOTAL**	2,034

Source: CEPESP-FGV

The total of 6,621 does not, however, represent the total number of SOs, because there is overlapping between codes in each group and between groups. When discounted for the overlaps, there are 4,643 SOs, with the purchase group composed by 3,535 SOs and the works

group by 1,108. Substantial research was done to determine whether a word should be included or not, such as: does the word go with the public program and action that is subject to? Does it suffice for a particular category? And, most importantly, do the irregularities found in it correspond to the group in hand? This was one of the major problems in organizing the dataset, once I had to confront every SO and its irregularities to attest for making a type I (false positive) or type II (false negative) errors. Such an issue was fine-tuning my hermeneutics unit so as each of the groups presented the lowest combination of errors. I have done so by conducting the following process: first, I would mark each SO according to codes found by our research assistants. In the database there are 10 codes that suffice for either purchases or works, which totaled 2,876 for both subsamples; then I confronted the classification done by the hermeneutics with those SOs. The rationale is quite simple: the more I found out of those 2,068, the better my method. I have computed errors type I and II for this, which follow:

Table A.2: Error types from hermeneutics

Purchases and public works combined	**#**	
Type I error	1,900	False positive
Type II error	522	False negative

Source: CEPESP-FGV

The most important error in this classification is type II. It means that I have left out 522 of the 2,876 SOs. If, then, my hermeneutics has accounted for 4,643 and missed 522, my sample is underestimated by only 11.24%. Type I error is not important because of the fact that the hermeneutics searches for SOs in the total sample and is confronted against only flagged SOs, which means that those 1,458 might not have been assigned groups' codes simply because they have not had any problems with corruption or mismanagement. In computing type I error, one can find that 1,567 errors were made under the two strongest purchase words, "acquisition" and "execution". That builds confidence that the hermeneutics is a good method to analyze corruption.

Chart A.1: Codes test

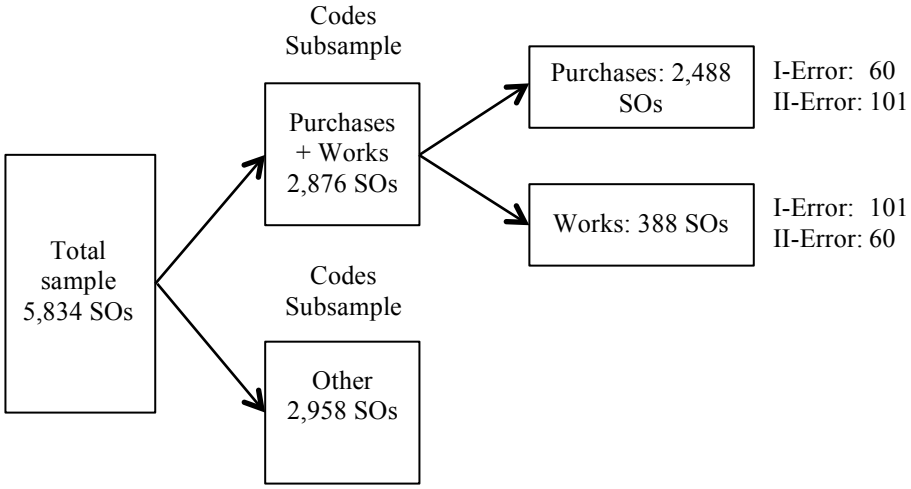
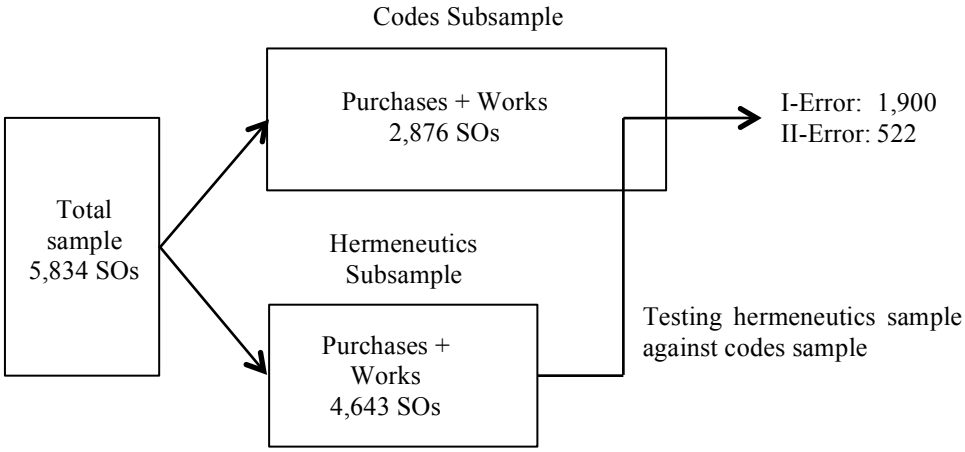


Chart A.2: Hermeneutics test



In order to increase even further the confidence of this process, I have also run type I and type II errors within the SOs selected by their codes. The rationale is also intuitive, as increasing keywords in each group to reduce general type II error might have had the negative impact of flagging SOs firstly assigned to one group to the other. Because the codes subsample is fixed, one can easily note that type I and II errors in one group are the counterfactual of the other.

Table A.3: Subsample errors (2,488 SOs)

Public purchases	**#**	
Type I error	60	False positive
Type II error	101	False negative

Source: CEPESP-FGV

For the public purchases group, type I error was fairly easy to account for, once there was only one irregularity that I could guarantee to belong to the public works group, which was code 19. Therefore, there is only one condition to type I here: SO was flagged as purchase by hermeneutics and presented the works code (19). Type II error had also one dimension: SO was flagged as works by hermeneutics and presented one of the private appropriation codes combined with any procurement code[31].

For the works group, the inverted logic applied. In type I error, SO should be flagged by hermeneutics as works and presented any of the private appropriation codes combined with any procurement code. Type II error, in the other hand, is SO flagged by hermeneutics as purchases but had the works code (19).

Table A.4: Works group (388 SOs)

Public purchases	**#**	
Type I error	101	False positive
Type II error	60	False negative

Source: CEPESP-FGV

An attentive reader should have noticed by now the circularity of the classification and that the problems come from the private appropriation group, once it can refer to purchases but also to other kinds of appropriation which do not enter the measure of corruption. Fortunately, the combination of appropriation and procurement is precise enough to define the purchases group, while in the works group the same logic applies provided there is the works code in spite of the appropriation codes.

Apart of the major problem of not having a specific purchases code, which is to be addressed by changing the coding methodology of the database from now on, there are minor issues regarding SOs that had been classified simultaneously in both groups and SOs that were not exclusive to purchases or works but had one of the two aspects in its purpose. In the former, confidence is built by knowing that the SOs in hand have actually had to be in both groups

[31] That is so because the private appropriation group is not exclusive to public purchases, while the procurement group is either purchases or works. Combining the private appropriation group and the procurement group, one finds only purchases made by the municipalities.

precisely because in order to build facilities there is the need for buying inputs[32]. In the latter, my proof-checking tool was to can the database and relate each such SO to their public policy. Most public policies with like SOs were, amongst many other actions, buying goods or services for the municipality.

Relying on the arguments here presented, it is possible to conclude that my hermeneutics classification is effective enough to set both groups and allow for the creation of corruption measures, in which persists only minor errors to verify public spending on purchase or works.

[32] That is not to say that every works SO had to present codes of purchases, for there are SO with problems related only to the construction aspect (it is a proof by contradiction of works' SO with no purchase problem). There is also the possibility of one SO being assigned to audit just the construction and another SO to audit just the buying of inputs.

Appendix B: CEPESP codes

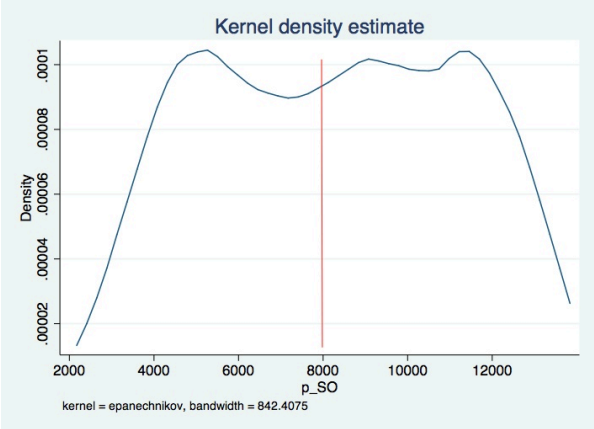
Table B.1: Auditors' findings classification

Irregularity	Irregularity
No irregularity found = 0	Performance (performance deficiencies) = 18
Bottom-up Monitoring (set up) = 1	Performance (public works deficiencies) = 19
Bottom-up Monitoring (no activity) = 2	Infrastructure (precarious work tools) = 20
Bottom-up Monitoring (no structure) = 3	Infrastructure (lack of supplies) = 21
Procurement infraction (no publicity) = 4	Infrastructure (inadequate supplies) = 22
Procurement infraction (false receipts) = 5	Performance (costumer service issues) = 23
Procurement infraction (ghost companies) = 6	Human resources (workload issues) = 24
Procurement infraction (document errors) = 7	Bureaucratic mismanagement = 25
Procurement infraction (false documents) = 8	Infrastructure (inadequate public ads) = 26
Procurement infraction (tender favoring) = 9	Human resources (inadequate training) = 27
Procurement infraction (other problems) = 10	Human resources (inappropriate hiring) = 28
Private appropriation (overpricing) = 11	Infrastructure (conservancy of inputs) = 29
Private appropriation (false receipts) = 12	Procurement (inadequate tender type) = 30
Private appropriation (no funds receipts) = 13	Procurement (leave of tender NA) = 31
Private appropriation (non-public ends) = 14	Human Resources (Payment issues) = 32
Unauthorized use of public funds = 15	Performance (federal funds investments) = 33
Performance (no municipal counterpart) = 17	Inadequate data on beneficiaries = 34

Source: CEPESP-FGV

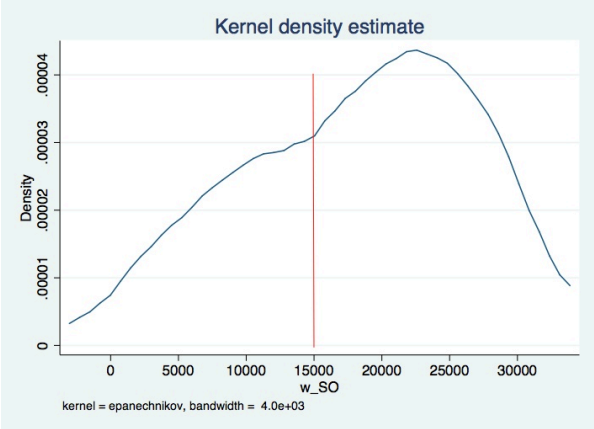
Appendix C: Kernel densities

Chart C.1: First purchases cutoff



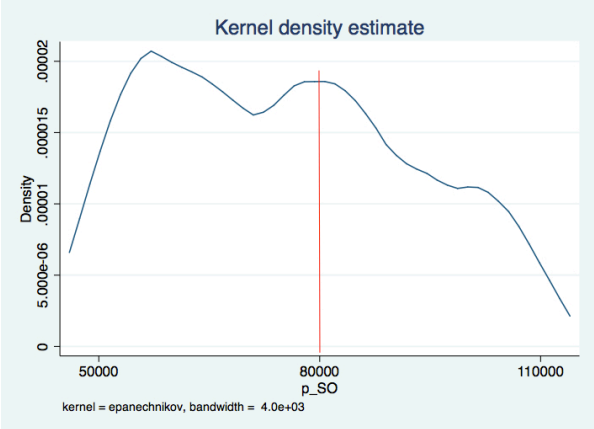
$N = 301$ and R\$3,000 $\leq x_i \leq$ R\$13,000

Chart C.4: First public works cutoff



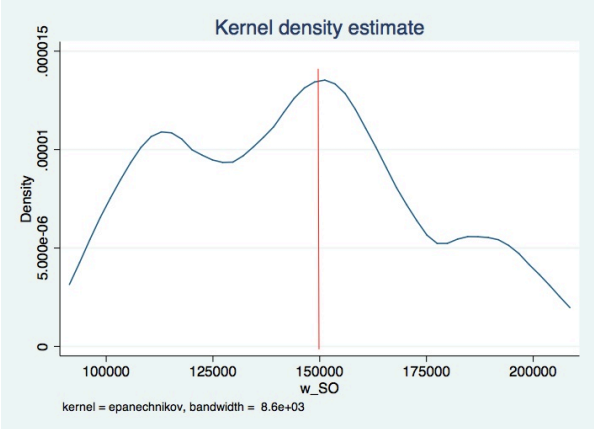
$N = 21$ and R\$0.0 $< x_i \leq$ R\$30,000

Chart C.2: Second purchases cutoff



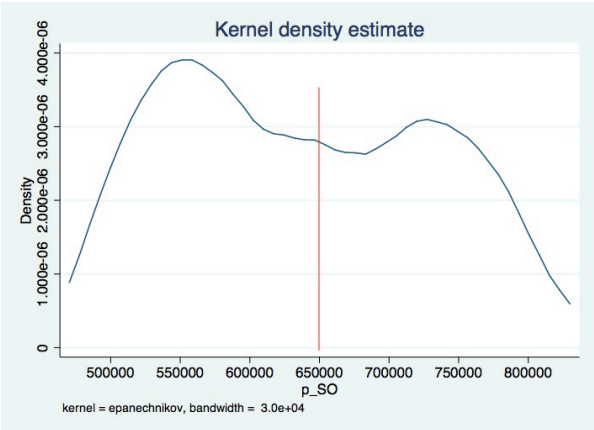
$N = 883$ and R\$50,000 $\leq x_i \leq$ R\$110,000

Chart 3.5: Second public works cutoff



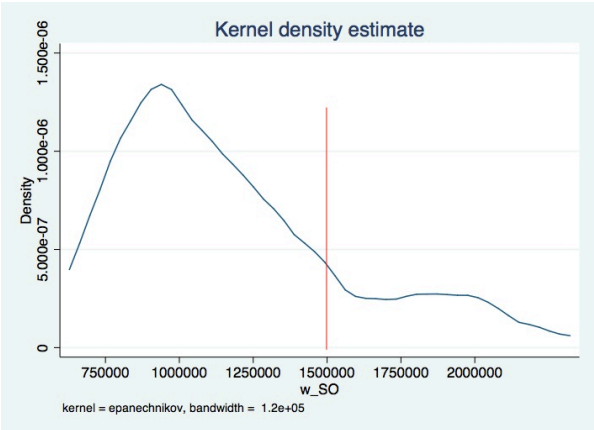
$N = 233$ and R\$100,000 $\leq x_i \leq$ R\$200,000

Chart C.3: Third purchases cutoff



$N = 160$ and R\$500,000 $\leq x_i \leq$ R\$800,000

Chart C.6: Third public works cutoff



$N = 144$ and R\$750,000 $\leq x_i \leq$ R\$2,250,000

Source: CEPESP-FGV.