Don't tie their hands: quasi-experimental evidence on discretion and corruption from Brazil

Andre Assumpcao*

April 28, 2016

Abstract

In this paper, we are interested in identifying if policymakers facing stricter rules in making expenditures incur in less corruption. We lay out a model where corruption is a function of amount procured and procurement rules and test its theoretical predictions by exploring a quasi-experiment in Brazilian municipalities. Our results show, however, that corruption does not decrease with stricter spending rules. In fact, our non-parametric estimates show that the change in procurement rules increases corruption by 12.5 percentage points. When we close in on education expenditures, the effect of stricter rules is even larger at 14.9 percentage points. Besides this effect, we provide a novel interpretation to fuzzy regression discontinuity (RD) coefficients when the researcher cannot directly observe the assignment variable.

*Department of Public Policy, University of North Carolina at Chapel Hill. Paper prepared for ECON881.25 Labor Markets and Development at Duke University, Department of Economics, instructor Dr. Robert Garlick. A preliminary version of this paper can be found here.

1 Introduction

An important issue in developing countries is whether governments attract qualified professionals to carry out public policy and to efficiently allocate scarce resources. Intuition tells us that competent policymakers should have discretion in allocating resources because their expertise is essential to overcome policy implementation problems. However, it is often the case that governments attract a heterogeneous pool of workers, with high and low quality professionals. Some might even be good professionals but have bad intentions. Incompetence and rent-seeking behavior help explain why officials deviate from their duties and why spending controls are called into action (Lambsdorff 2002; Olken 2006).

This paper aims at evaluating whether less discretion in public spending reduces corruption. We exploit discontinuities in Brazilian procurement law to assess whether tighter spending rules make it more difficult for public officials to extract rents (Becker 1968; Rose-Ackerman 1975; Stigler 1970) and to misallocate resources. Exogenous variation comes from Law 8,666, passed in 1993, which determines procurement types for all government levels in Brazil as a function of the amount procured. By looking at municipal purchase orders around monetary cutoffs where procurement rules changes discontinuously, we infer causal relationships between discretion and corruption.

The change to stricter procurement rules increases corruption by 12.5 percentage points. The legislation does a poor job of deterring corruption, most likely because procurement types are not much different from each other. The policy takeaway is the need to improve the design of spending regulations.

Besides looking at an unexplored determinant of corruption, we also contribute to the literature by employing an objective measure of corruption in line with Bertrand et al. (2007); Olken (2009); Fisman and Miguel (2007) and a few others. Our measure is similar to that of Ferraz and Finan (2008, 2011) but we use improvements made by Lopes (2011) to build our corruption index. Finally, we contribute to advance the use of regression discontinuity by suggesting an interpretation of fuzzy RD when the researcher cannot precisely observe the assignment variable.

The remainder of this paper is organized as follows: section II introduces our theoretical model, section III presents previous studies and our data. We run empirical tests in section IV and discuss results in V. Section VI concludes.

2 Theoretical Framework

We are interested in the behavior of public officials when faced with heterogeneous discretion in making expenditures. Let us suppose that a representative policymaker maximizes utility as a function of the income she makes from wages (w) and from kickbacks (k).

$$U(w, k) = w + k \cdot c(x, l) \tag{1}$$

In Brazil, most municipal officials have been approved in civil service exams, and they receive tenure immediately upon approval. This creates a rigid pay schedule (w), so that their chances of increasing own utility is through income shocks in k. Variable k is essentially unobserved, which means that we are not able to compute how much they appropriate (see Olken (2007) for an exercise of precise estimation of resources lost to corruption). We are thus interested in identifying $c(x, l)$, which is the linear probability function that describes corruption decision: if the agent decides to engage in corruption, c turns on and she collects k bribes; otherwise, it remains off and she collects zero. The probability of corruption c is a function of amount procured for private goods and services (x) and the rules officials face to make expenditures (l).

$$c(x, l) = \delta \cdot x + \rho \cdot l + \varepsilon \tag{2}$$

The theoretical prediction of this simple model is straightforward. We expect the probability of corruption to be increasing in the amount procured, i.e., $\delta > 0$. As officials procure larger amounts of goods and services, they are tempted to engage in corruption because the amount they would collect in bribes becomes a smaller part of total procurement – it is more likely that corruption goes unnoticed. Alternatively, the probability of corruption is decreasing in discretion ($\rho < 0$), meaning that, as we move up procurement rules, they face more constraints and are less likely to engage in illegal practices.

What is interesting about this model is the relationship between x and l. They are both endogenous. As amount procured increases, so do spending rules, thus l is a function of x, $l(x)$. However, policymakers can also manipulate x to fall into different categories of public tenders, so that $x(l)$. They might do so because (i) they see a chance to appropriate public funds or because (ii) rules are too big of a red tape to buy essential goods and services. If they can control public tenders completely, there is no way to find out how discretion might drive the probability of corruption and this relationship remains unidentifiable.

The features of our research design allow us to overcome these endogeneity problems. Law 8,666/93 establishes public procurement types as functions of the amount procured. We are able to isolate the effect

of discretion (procurement types l) at procurement orders with amounts in the neighborhoods of cutoffs. It means we hold x constant and tease out the partial effect of l on corruption. Also, the incentives design of Law 8,666/93 makes discretion a monotonic and decreasing function of amount procured, which restricts manipulation unidirectionally. Officials might manipulate purchase orders downwards to avoid stricter rules but never upwards. Thus, we know that the direction of the endogeneity of $x(l)$. This significantly reduces the risk of sorting in fuzzy RD estimations and greatly improves our empirical strategy. In the next section, we present similar contributions of previous studies and go into the details of our data.

3 Background and data

The Brazilian federal government introduced a nationwide anticorruption program in 2003, which was implemented through the National Audit Office, CGU. In this program, municipalities are randomly drawn for expenditure audits. Once drawn, a municipality receives a team of auditors who spends approximately two weeks going through every federal transfer made to that municipality in the three years before the draw. Auditors cover all policy areas, of which the most important are health, education, and social development. They make up two-thirds of all federal transfers to municipalities. These grants are earmarked as purchase orders (henceforth, SO), which constitute our unit of analysis.

Imagine that municipality A has received federal grant i (a six-digit unique identifier) from the Ministry of Health in 2005 to buy tuberculosis vaccine in the amount of R$85,000. If A is drawn in one of the audit lotteries in 2006, CGU auditors will come to A with a spreadsheet with many SO numbers, including i's, and a list of issues they need to check in SOs. For instance, they will look into tender documents, purchase receipts, how many vaccines have been administered and how many are left. More details of the auditing process are very well consolidated in Ferraz and Finan (2008). After their inspection is over, auditors go back to the country's capital and write up an audit report for municipality A containing their findings.

CGU's audit reports are extremely comprehensive and contain all SOs where the auditors have found evidences of corruption and mismanagement. They run through transfers of all ministries and policies. Often reports go over 100 pages. To meet their goal of attracting people's attention to the use of public resources, CGU makes every report available online. They also submit reports to the federal prosecuting-office (MPF) so that they can investigate illegal appropriations of funds. Since its inception in 2003, many corruption schemes have been unveiled by CGU's random auditing program and so it is recognized as an important anticorruption mechanism in Brazil.

The Getúlio Vargas Foundation Center for Politics and Public Economics (CEPESP-FGV) is one of the research centers in Brazil that codifies the information of reports into quantitative data. The most important item are the 'irregularities' reported by auditors. They are text entries where they write down whether they have found evidence of misplacement or corruption of funds. At CEPESP-FGV, we code these text-entries into thirty-four (34) irregularities, where ten (10) are corruption categories and twenty-four (24) are mismanagement issues. We use them to build our corruption index. If the auditor finds one of the ten irregularities in SO documents, our corruption index takes the value of one for that SO; it is zero otherwise. A more detailed explanation of the process is found in Lopes (2011). Lopes' improvement to Ferraz and Finan (2008, 2011) is access to raw data from CGU of all SO in any given municipality, instead of those identified in reports, to compute municipal corruption indexes.

Our discretion variable is built by using the rules in Law 8,666/93. It determines tender rules for any public purchase of goods/services and works under the index function $l_c = 1(x_{i,j} > x_c)$. There are three types of tenders, $c = \{1, 2, 3\}$, and one leave of tender, $c = \{0\}$, which are determined by the amount procured (x_c). Table 1 presents categories and their cutoff amounts.

Table 1: Procurement types (Law 8,666/93)

	Type	**Public Works (w)**	**Goods/services (p)**
Category 0 (l_0)	Leave of tender	$x_0 \leq$ R\$15,000	$x_0 \leq$ R\$8,000
Category 1 (l_1)	Invitation bid	R\$15,000 $< x_1 \leq$ R\$150,000	R\$8,000 $< x_1 \leq$ R\$80,000
Category 2 (l_2)	Price-taking bid	R\$150,000 $< x_2 \leq$ R\$1,500,000	R\$80,000 $< x_2 \leq$ R\$650,000
Category 3 (l_3)	Competitive bid	$x_3 >$ R\$1,500,000	$x_3 >$ R\$650,000

In this paper, we focus on spending under the health (MS) and education (MEC) ministries. Almost half of all federal transfers to Brazilian municipalities fall into one of these two areas. We have 4,778 SO from both ministries, spreading across 883 Brazilian municipalities during the period 2004-2010. We can identify 3,715 as purchases or public works and they make up our final sample.

Table 2: Audit reports [1] and SO in purchases or works

	Audit reports	SOs
MEC (education)	731	1,789
MS (health)	490	1,926
TOTAL	1,221	3,715

We merge our corruption data from CGU audit reports with IBGE (National Statistical Office), IPEA (National Economic Research Office) and TSE (Electoral Court) to create vectors of health, education, socioeconomic and political characteristics [2] .

4 Empirical Strategy

Brazil presents an ideal setting for testing our theoretical model from section II. While the ideal experiment would be the random assignment of procurement rules to an unbiased sample of public purchases or public works, CGU's auditing program allows for a similar quasi-experimental design.

The first key feature in this program is that CGU randomized audits across municipalities. At its peak, sixty municipalities were drawn (with replacement) three times per year. CGU used the National lottery to avoid any political interference with selection. Therefore, unobservable shocks are likely uncorrelated with municipal characteristics. Additionally, the discontinuities of procurement types determined by Law 8,666/93 provide local exogenous variation to test ρ in equation (2). For simplicity, we will be focusing on a single cutoff of a purchase SO ($l_2 = R\$80,000$). Since manipulation [3] of treatment is possible in function (2), we design a fuzzy RD with the following equation:

$$\begin{aligned} c_{i,j} = \alpha_0 + \sum \delta_{j,c} \cdot (x_{i,j} - x_c) + \sum \rho_{j,c} \cdot \hat{s}_{i,j,c}(l_{i,j,c}(x_{i,j})) + \\ + \sum \theta_{j,c} \cdot (x_{i,j} - x_c) \cdot \hat{s}_{i,j,c}(l_{i,j,c}(x_{i,j})) + \sum \gamma_v \cdot M_v \cdot Z_{m,t,v} + \sum \phi_n \cdot Y_{m,t,n} + \tau + \varepsilon_{i,j} \end{aligned} \tag{3}$$

Where $c_{i,j}$ is the (index) corruption function for SO i, type j (purchases p or works w) in municipality m and year t. The second term in equation (3) corresponds to $\delta \cdot x$ in equation (2), where we report the amount

[1] There is double count of the reports in table two. We count the sections on health and education independently for any given municipality audit. We do this because we are not looking at corruption in the municipal level, in which case it would not make sense to double count, but instead we look at corruption in the SO level. Therefore, if any given municipality has SO irregularities in both types of policies, they will be counted twice, one in each row.

[2] Except for CGU audit data coded at CEPESP-FGV, all other data is publicly available at each institution's website. The classification of SOs and the coding processes are available upon request

[3] In order to check whether RD is a feasible method, we run falsification tests using fictional cutoffs, heterogeneous bandwidths and use McCrary (2008) to test excessive manipulation of running variable. We drop the second cutoff in works because manipulation is too problematic. Results available upon request.

procured of SO i, type j, centered at cutoff $c = \{0, 1, 2, 3\}$ as per table 2. The $\rho_{j,c}$ are our coefficients of interest from equation (3) and we include interactions terms of $\hat{s}_{i,j,c}$ and x_c. M_v is a dummy which turns on health and education municipal characteristics $Z_{m,t,v}$ for each ministry's SO, where subscript v is health (h) and education (e). Other municipal social, economic and political characteristics are captured by vector $Y_{m,t,n}$. Time trends and stochastic error are respectively τ and $\varepsilon_{i,j}$.

Besides investigating the causal relationship between discretion in public spending and corruption, the experimental setting allows for an important contribution to the literature in RD. Manipulation of treatment requires the use of instrumented variable $s_{i,j,c}$, which is the actual procurement type j for SO i from Law 8,666/93. Using a fuzzy RD design, we would be interested in the following coefficient:

$$\rho_{p,c} = \frac{E[c_{i,j}|l_{i,j,c}(x) = 1] - E[c_{i,j}|l_{i,j,c}(x) = 0]}{E[s_{i,j,c}(x_{i,j})|l_{i,j,c}(x) = 1] - E[s_{i,j,c}(x_{i,j})|l_{i,j,c}(x) = 0]} \tag{4}$$

While $s_{i,j,c}$ should be predicted by first-stage regressions on $l_{i,j,c}$ (the amount procured), the design of Law 8,666/93 allows for a special case model. It is plausible to assume that officials will not subject themselves to stricter spending rules. They would have a worse time preparing documents, would take longer to procure items and would be under larger scrutiny when auditors came to town. Irrespectively of their quality or intentions, no policymaker will want to use stricter rules when they can have more wiggle room to carry out policy or to extract rents. Incentives built into the law then create only downward manipulation of tender types. It means that even though we cannot directly observe $E[s_{i,j,c}(x_{i,j})|l_{i,j,c}(x) = 1]$ and so are subject to measurement error, we know with certainty the direction of the error, since $E[s_{i,j,c}(x_{i,j})|l_{i,j,c}(x) = 1]$ can only go from 1 to 0 but never from 0 to 1.

Since we also know with certainty that $E[s_{i,j,c}(x_{i,j})|l_{i,j,c}(x) = 0] = 0$ [4] because of the properties of the Brazilian procurement law, the measurement error in assigning $E[s_{i,j,c}(x_{i,j})|l_{i,j,c}(x) = 1] = 1$ (i.e. making it an artificial sharp RD) tells us that $\rho_{p,c}$ is the local minimum treatment effect. We know that the denominator of $\rho_{p,c}$ is always positive and can only go down as we improve measurement of $s_{i,j,c}$. This means that we estimate a sharp RD with measurement error and recover local minimum treatment effects of a fuzzy RD.

An example might be useful to understand the methodological issue here described. Suppose we are looking at SO i from section II, which was a federal grant to buy tuberculosis vaccine in the amount of R$85,000. The policymaker can decide to procure medicine in the latter amount or can manipulate

[4] $E[s_{i,j,c}(x_{i,j})|l_{i,j,c}(x) = 0] \neq 0$ would only be possible if public officials packed SOs into bigger procurements. We do not have a single SO in our sample in which CGU auditors reported this problem. Additionally, it is unlikely that policymakers will seek more restrictions for public spending: (i) if they want to collect bribes, it is just easier when they have less strict rules to follow; (ii) if they want to maximize policy outcomes, it is likely that they will want to carry out tenders in lower amounts to respond quickly to policy demand.

tender to procure R$79,000, just below $c = 2$ (see table 1), in which case Law 8,666/93 determines type $l_{i,p,2} = 1(x_{i,j} = R\$85,000 > x_c = R\$80,000)$ but policymaker carries out $l_{i,p,1}$. If we mistakenly attribute $s_{i,j,2} = 1$ for this and many other similar observations, we deflate $\rho_{p,c}$ by increasing its denominator. In an extreme case where we attribute 1 to all $E[s_{i,j,c}|l_{i,j,c}(x) = 1]$, we estimate the minimum effect of $\rho_{p,c}$ when its denominator would be, in reality, lower. Knowing the type of measurement error here allows for a novel interpretation of fuzzy RD in line with potential developments described in Lee and Lemieux (2010).

5 Results

We follow the guidance in Calonico et al. (2015); Lee (2008); Lee and Lemieux (2010); Hahn et al. (2001); Imbens and Kalyanaraman (2012) to estimate the RD regression (3). To evaluate whether RD is a feasible design, we run two usual tests: (i) McCrary (2008)'s test to check for manipulation of running variable and (ii) the imposition of false cutoffs to check if confounding discontinuities exist. While purchases cutoff 2 fares well (R$80,000), the distributions of SO around cutoff 2 in works (R$150,000) are statistically different from each other and they prevent us to evaluate regression (3) as RD for works SOs. This means that observations are not comparable and manipulation is too strong (see footnote 3). The first exercise is the estimation of unrestricted parametric regressions, which is reported in table 3.

Table 3: Unrestricted parametric regressions

	(1)	(2)	(3)
Dependent variable: corruption index	OLS	Logit	Probit
Amount centered at c=2 ($\delta_{p,2}$)	0.000	0.000	0.000
	[0.000]	[0.000]	[0.000]
Procurement type 2 ($\rho_{p,2}$)	-0.790	-5.602	-3.159
	[1.158]	[7.672]	[4.326]
Amount centered x Procurement type ($\theta_{p,2}$)	-0.000	-0.000	-0.000
	[0.000]	[0.000]	[0.000]
Constant	1.481	6.379	3.665
	[1.168]	[7.703]	[4.346]
Observations	3,509	3,509	3,509
R^2	0.051		

All regressions include the variables and vectors from equation (3). Municipal characteristics are income inequality index, GDP per capita, total population, urban population, presence of judiciary branch; political vector is made up by indicators for reelected mayors, for mayors allied with President at the time, voter turnout and margin of victory in municipal election. Robust standard errors are clustered at the municipality level. We report only the most important independent variables. Presence of judiciary, population and urban population are significant at 10%, all other variables are insignificant.

*** p<0.01, ** p<0.05, * p<0.10

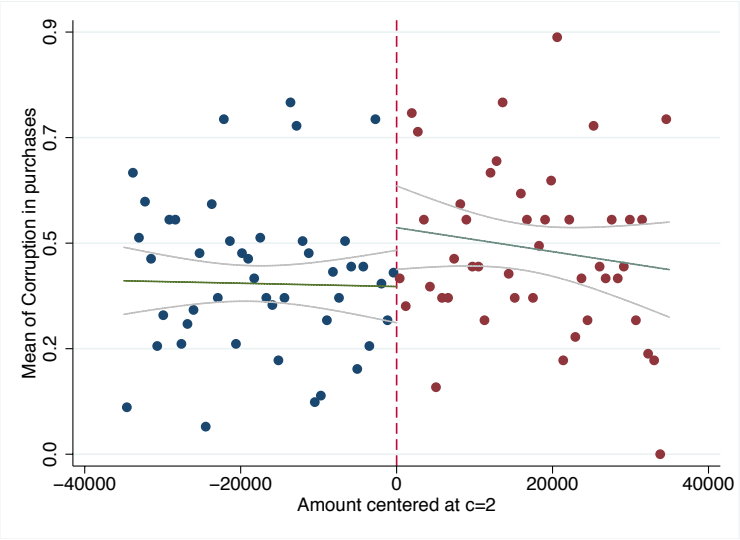
We estimate equation (3) in three specification types to allow for more functional form flexibility, but we are mainly interested in the OLS results. The objective is to identify the correlation between outcome and variables of interest, even if coefficients are biased (Lee and Lemieux 2010). However, we do not identify any statistically significant coefficients in the unrestricted parametric regressions, irrespective of their functional form. There are two potential causes for this result. First, the model might be correctly estimated and in reality variables are not correlated, in which case there is no effect of discretion nor of amount procured on corruption. It does not seem likely that this is the case. In fact, we know with certainty that the model is incorrect. While Law 8,666/93 determines three procurement types as functions of amount procured, we placed no constraints on amounts which would fall under other categories of the Brazilian procurement law, e.g., leave of tender when $x_{i,p} \leq R\$8,000 = x_{i,0}$ or competitive bid when $x_{i,p} > R\$650,000 = x_{i,3}$. We simple ran a pooled regression.

This makes us confident that a second explanation of incorrect model is more plausible. According to Hahn et al. (2001), we should overcome poor parametric specifications with better non-parametric local linear regressions around discontinuities in the assignment variable. These get around essential heterogeneity in observations' covariates and allow that we make local causal inference. We follow Lee and Lemieux (2010) suggestion to selecting bandwidth range by applying visual analysis and the tests in Calonico et al. (2015); Hahn et al. (2001); Ludwig and Miller (2007) which are respectively named CCT, IK and CV.

Visual choice is our preferred method and we start off with an arbitrary amount of ±R$50,000 and move in closer with increments of ±R$5,000 until settling in for ±R$35,000. The results remain unchanged if we use narrower bandwidths, but coefficients turn statistically insignificant[5] . We report figure 1 as the pre-analysis test suggested in Lee and Lemieux (2010), where we split both sides of the cutoff in equally-sized bins where we calculate the average corruption value. So, by analyzing a preliminary test, we should expect a positive effect of procurement type on corruption when we estimate non-parametric regressions in sequence.

[5]Bandwidths by IK and CV are, respectively, larger than ±R$100,000 and close to ±R$80,000, which do not make sense under the rules in Law 8,666/93. CCT selects ±R$38,000, which is very close to our visual choice.

Figure 1: Mean corruption and procurement amount



Bins = 45; Bandwidth = ±R$35,000; N = 1,019 observations.

There is some overlap between corruption indexes on both sides of the cutoff, which is consistent with the results of our unrestricted parametric regressions reported in table 3. In table 4, we present the results of the non-parametric regressions with the selected bandwidth. These are pooled regressions on a single sample for both sides of the cutoff. We use interactions of amount procured and procurement type, even though Hahn et al. (2001) do not, for extra caution. The coefficient on procurement type 2 ($\rho_{p,2}$) is significant at 5% and shows that the change in procurement type increases the linear probability of corruption by 12.5 percentage points. It is a large and unexpected result. We ignore the logit and probit results because the local regressions are linear.

Table 4: Non-parametric regressions (Bandwidth ±R$35,000)

	(1)	(2)	(3)
Dependent variable: corruption index	OLS	Logit	Probit
Amount centered at c=2 ($\delta_{p,2}$)	-0.000	-0.000	-0.000
	[0.000]	[0.000]	[0.000]
Procurement type 2 ($\rho_{p,2}$)	0.125**	0.519**	0.323**
	[0.059]	[0.246]	[0.153]
Amount centered x Procurement type 2 ($\theta_{p,2}$)	-0.000	-0.000	-0.000
	[0.000]	[0.000]	[0.000]
Constant	0.357***	-0.586***	-0.365***
	[0.040]	[0.173]	[0.107]
Observations	1,019	1,019	1,019
R^2	0.007		

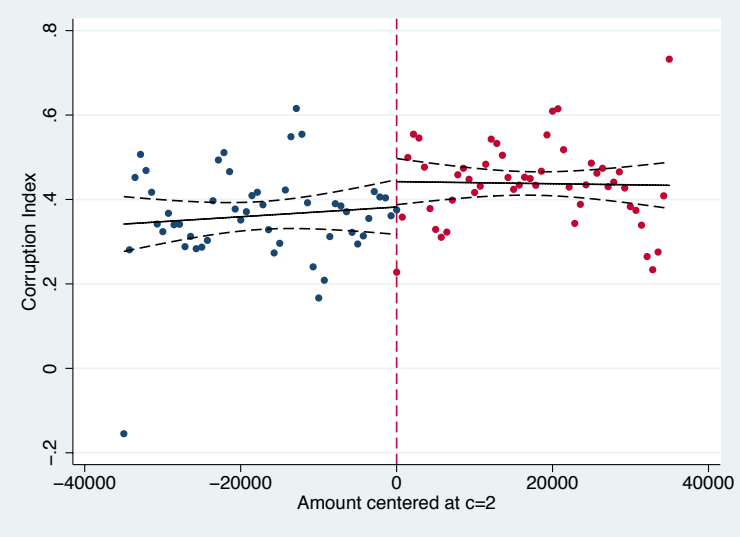
Local linear regressions of equation (3) excluding covariates vectors. Robust standard errors are clustered at the municipality level. Bandwidth = ±R$35,000.

*** p<0.01, ** p<0.05, * p<0.10

An alternative explanation to the positive effect arises. It is likely that the change in procurement rules does not act as a corruption deterrent. Officials might not find too big of a difference in meeting requirements of price-taking bid (type = 2) vs invitation bid (type = 1) of Law 8,666/93. This interpretation is also consistent with little manipulation found in purchases cutoff 2. Had agents been deterred by big discretion changes, we might have seen excessive manipulation of amount procured so as to avoid lower discretion. We would have picked this up with McCrary (2008) tests – which happened for works cutoff 2.

The coefficient on procurement type is estimated under a sharp RD even though we know that treatment is not a deterministic function of the assignment variable. It can then be interpreted as the local minimum average effect of a change in procurement rules on corruption prevalence. The minimum effect is 12.5 percentage points but any improvement on the measurement of the instrumented variable would push $\rho_{p,2}$ up. Since we know from experimental design that treatment is not correlated with error term $\varepsilon_{i,j}$, we only worry about downward bias imposed by assuming sharp RD.

Figure 2: Mean corruption and procurement amount



Bandwidth = ±R$35,000; N = 1,019 observations.

Figure 2 plots the local linear regressions discussed above and shows that coefficients are slightly significant, which is what we have seen in table 4. This graph is similar to figure 1 as it should be. Local linear regressions are just a special case of parametric estimations when we impose the same functional form on smaller subsamples of the data. Next we look into whether the results in table 4 and figure 2 are driven specifically by the variation in one of the subsamples of our data. There could be specific structures of public spending on health or education that would change the impact of discretion on corruption. We do this by breaking down the non-parametric regressions by health and education in table 5.

Table 5: Non-parametric regressions (Bandwidth ±R$40,000)

	Health			Education		
	(1)	(2)	(3)	(1)	(2)	(3)
Dependent variable: corruption index	OLS	Logit	Probit	OLS	Logit	Probit
Amount centered at c=2 ($\delta_{p,2}$)	0.000	0.000	0.000	-0.000	-0.000	-0.000
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Procurement type 2 ($\rho_{p,2}$)	-0.006	-0.029	-0.018	0.149*	0.607*	0.379*
	[0.092]	[0.416]	[0.252]	[0.083]	[0.341]	[0.212]
Constant	0.331***	-0.704***	-0.437***	0.378***	-0.498**	-0.311**
	[0.051]	[0.232]	[0.141]	[0.047]	[0.197]	[0.122]
Observations	388	388	388	584	584	584
R^2	0.000			0.013		

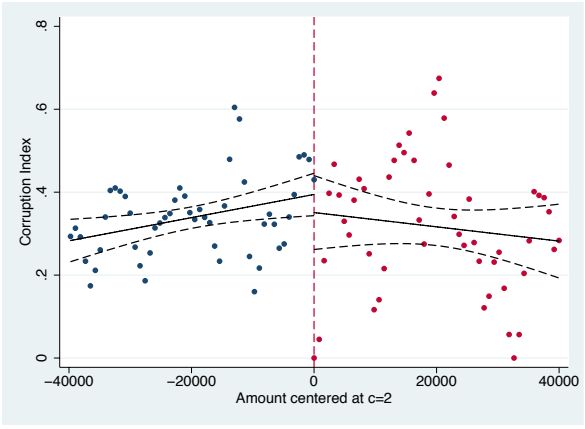
Local linear regressions of equation (3) excluding covariates vectors. Robust standard errors are clustered at the municipality level. Bandwidth = ±R$40,000.

*** p<0.01, ** p<0.05, * p<0.10

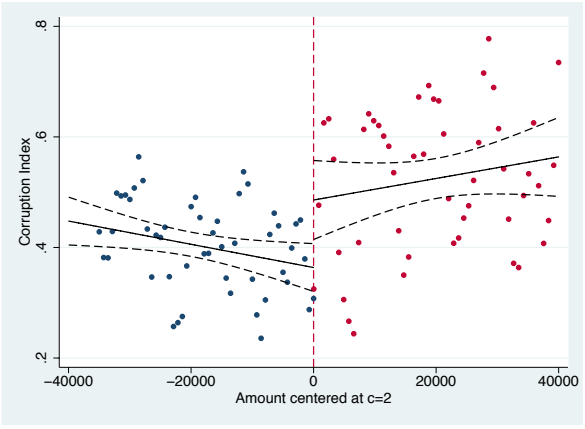
For these subsamples, once again we run the traditional tests for bandwidth choice. Visual analysis results in the same bandwidth for health and education (±R$40,000). Again, we compare our result to CCT, IK and CV methods, where again IK and CV provide non-realistic bandwidths and CCT is very close to our choices (±R$42,000 and ±R$37,000 for each subsample, respectively). What stands out is that the non-parametric results are mostly driven by education expenditures. A change from type 1 (invitation bid) to type 2 (price-taking bid) increases the linear probability of corruption in 14.9 percentage points. Since this is the local minimum effect, we can expect that more accurate measurement of the actual procurement type would increase the magnitude of $\rho_{p,2}$ while preserving its direction.

Figure 3: Non-parametric regressions: health and education subsamples

Panel (A): Health



Panel (B):Education



Bandwidth = ±R$40,000. N = 388 health and N = 584 education.

The evidence here presented seems consistent with a zero, or worse, an increasing, effect of lower discretion on corruption. Whether the effect comes from procurement types which are not heterogeneous or

from small sample sizes around purchases cutoff 2, the policy implication is clear: the procurement Law 8,666/93 does a terrible job to deter corruption, the very reason why it came into being. It does not reduce corruption in purchases of education goods and services and it is likely making policymakers refrain from delivering policy in fear that they will be investigated even for procurement mistakes which are uncorrelated to corruption (Lichand et al. 2016).

6 Conclusion

In this paper, we have investigated if lower discretion in public procurement reduces corruption in Brazilian municipalities. Contrary to what we expected, corruption incidence is actually an increasing function of stricter spending rules. The shift in procurement type between R$45,000 and R$115,000 purchase orders causes an increase of 14.9 percentage points in the linear probability of corruption in education expenditures.

If anything, the current procurement legislation has a perverse effect of increasing corruption in public expenditures. We suggest two possible interpretations to these results. First, we cannot rule out that the sample size might be too small to capture the actual effect of CGU's corruption crackdown program. There are little over 4,500 SOs in our sample while total SOs audited are close to 26,000 for the period 2004-2010. Second, it is possible that the changes in rules are not big enough to deter corruption. Policymakers might even find it easier to cheat or collude in public tenders when procurement amounts are larger.

An anecdotal support to this interpretation comes from the timing of Law 8,666/93. When it came into force, Brazil still experienced high inflation. The last update of amounts determining procurement types was 1998, and since then inflation has accumulated 290%. It means that cutoff 2 should be R$232,000 to preserve the real values of goods/services procured back in 1998. It tells us that agents might not see much difference in the quantity of goods and services they can buy with amounts between ±R$35,000 bandwidth of cutoff 2, so there are other unaccounted causes that make them engage in corruption.

Quantifying how big should discretion changes be to force behavior change is still an interesting avenue for future research in economics and political science. We are unclear on the types of provisions in Law 8,666/93 that make a very difficult and strict procurement type and a more lenient alternative. Any advance in turning discretion into quantitative measures would be very useful to analyze political and economic decisions. This would be an important contribution not only to decisions in public sector but also to decision making more generally.

Our second important contribution is the interpretation of a sharp RD with measurement error as the

local minimum effect of a fuzzy RD. In the quasi-experimental setting in Brazil, we know with certainty the direction of bias. Had there been better measurement of the assignment variable, we would have seen larger procurement effects on corruption. It is a novel interpretation to fuzzy RD.

Finally, governments should not only be interested in how to deter corruption, which is what we address in this paper by analyzing one potential determinant of corruption, but also what are potential adverse effects of cracking down on misallocation of resources. Initial evidence in Brazil suggests that increasing monitoring might reduce the delivery of services because agents become more afraid of making mistakes. Public officials fear that these mistakes might be confounded with corruption and refrain from delivering much needed public goods and services.

References

Becker, G. S. (1968). Crime and punishment: An economic approach. *Journal of Political Economy*, 76(2):169–217.

Bertrand, M., Djankov, S., Hanna, R., and Mullainathan, S. (2007). Obtaining a driver's license in india: an experimental approach to studying corruption. *The Quarterly Journal of Economics*, pages 1639–1676.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2015). Optimal data-driven regression discontinuity plots. *Journal of the American Statistical Association*, 110(512):1753–1769.

Ferraz, C. and Finan, F. (2008). Exposing corrupt politicians: The effects of brazil's publicly released audits on electoral outcomes. *The Quarterly Journal of Economics*, 123(2):703–745.

Ferraz, C. and Finan, F. (2011). Electoral accountability and corruption: Evidence from the audits of local governments. *American Economic Review*, 101(4):1274–1311.

Fisman, R. and Miguel, E. (2007). Corruption, norms, and legal enforcement: Evidence from diplomatic parking tickets. *Journal of Political Economy*, 115(6):1020–1048.

Hahn, J., Todd, P., and van der Klaauw, W. (2001). Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica*, 69(1):201–09.

Imbens, G. and Kalyanaraman, K. (2012). Optimal bandwidth choice for the regression discontinuity estimator. *The Review of Economic Studies*, 79(3):933–959.

Lambsdorff, J. G. (2002). Corruption and rent-seeking. *Public Choice*, 113(1):97–125.

Lee, D. S. (2008). Randomized experiments from non-random selection in u.s. house elections. *Journal of Econometrics*, 142(2):675 – 697. The regression discontinuity design: Theory and applications.

Lee, D. S. and Lemieux, T. (2010). Regression discontinuity designs in economics. *Journal of Economic Literature*, 48(2):281–355.

Lichand, G., Lopes, M. F. M., and Medeiros, M. C. (2016). *Essays in Development Economics*. PhD thesis, Harvard University.

Lopes, M. F. M. (2011). *Corrupção: estudo sobre as formas de mensuração, seus determinantes e perspectivas sobre as formas de combate*. PhD thesis, Getúlio Vargas Foundation.

Ludwig, J. and Miller, D. L. (2007). Does head start improve children's life chances? evidence from a regression discontinuity design. *The Quarterly Journal of Economics*, 122(1):159–208.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, 142(2):698–714.

Olken, B. (2007). Monitoring corruption: Evidence from a field experiment in indonesia. *Journal of Political Economy*, 115(2):200–249.

Olken, B. A. (2006). Corruption and the costs of redistribution: Micro evidence from indonesia. *Journal of Public Economics*, 90(4–5):853 – 870.

Olken, B. A. (2009). Corruption perceptions vs. corruption reality. *Journal of Public Economics*, 93(7):950–964.

Rose-Ackerman, S. (1975). The economics of corruption. *Journal of Public Economics*, 4(2):187–203.

Stigler, G. J. (1970). The optimum enforcement of laws. *Journal of Political Economy*, 78(3):526–536.