Local Government Spending and Employment in Brazil: Evidence from a Natural Regression Discontinuity *

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Abstract

This paper examines the effect of local government spending on labor markets in a developing country context. We use plausibly exogenous variation in the allocation mechanism of federal funds at the municipality level in Brazil to estimate the effect of general spending on formal employment. We estimate that a jobs-spending elasticity of around 0.9%, for a cost of around $4800 per job in current US dollars. This effect is much larger than other employment multipliers estimated in developed country contexts. We also find that most of increase comes from relatively unskilled labor.

JEL Classification: O11, O54, H72

Keywords: Government Multipliers, Local Expenditure, Local Labor Markets

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1 Introduction

Fiscal policy and spending is a primary means by which the government can affect the state of the economy. The efficacy of these channels is of particular interest to macroeconomic policy makers. Accordingly, estimating fiscal multipliers and the effect of government spending on the economy are of great interest; however, isolating the effect of government spending on variables of interest proves challenging due to the lack of existing identifying variation in government spending.

Fiscal policy can entail vastly different outcomes for local economies in developing countries than it might in their developed counterparts. For instance, corruption, differences in the local business environment, and the mistrust of government can all severely limit the extent of the influence of government spending. In spite of these limitations, fiscal policy still plays an important role in a developing country context, and thus estimating its effects remains of great importance. It may very well be the case that fiscal policy can appear *more* effective in creating jobs due to the presumably lower cost of a job in developing countries. The question, then, seems to be an empirical one.

Our paper uses a known discontinuity in the allocation of government transfers in Brazil to estimate the effect of fiscal policy on employment at the local level. Amounts of government transfers to municipalities under the *Fundo de Particpação dos Municípios* (FPM) program vary according to population thresholds such that municipalities in the same population bracket within the same states should all receive the same amount of transfers. These transfers make up a substantial portion of municipality budgets, and as such, revenue at the local level varies discontinuously around the population thresholds. We find that this revenue translates into spending and use this plausibly exogenous variation in spending to estimate the effect of fiscal policy on local labor market variables. We focus mainly on employment using detailed data from an annual administrative survey covering the overwhelming majority of formal labor market, however we also examine other variables of interest, such as monthly earnings.

We consider employment and labor market outcomes for several reasons. First, employment is a

visible indicator of the welfare of an economy. Given the nature of inequality in developing countries like Brazil, it is possible that a glance at the labor market, albeit the formal labor market, could serve as a better tool than other measures such as GDP per capita. Secondly, corruption is a serious concern at the local government level in Brazil. To the extent that corruption can mask the welfare of an economy in that corruption can appear as a part of local economic activity, labor market effects could potentially serve as a better tool for ascertaining social welfare. Finally, formal sector employment as a measure of welfare is interesting unto itself. It is possible that the relationship between employment and economic activity is not as strong as it appears to be in developed countries.

The canon of literature on fiscal spending, including estimates based on military spending (Barro (1981)) and VAR models (Blanchard and Perotti (2002)) has focused on activity indicators such as GDP and has been recently updated by literature attempting to use novel instruments to identify exogenous variation in spending. Several studies have utilized an instrumental variables approach, such as Serrato and Wingender (2011) and Shoag (2010); the former uses variation in census population counts to determine the allocation of government resources, and the latter uses variation in government pension windfalls. Both papers find that taking advantage of instrumental variables yields much larger, positive effects of government spending on local labor market variables, such as employment and income, compared to naive estimates derived from simply regressing labor outcomes on government spending and ignoring endogeneity. Specifically, Serrato and Wingender find estimates in the neighborhood of 1.45, and Shoag finds estimates around 2.12. As for results that are more related to our outcome of interest, Wilson (2012) finds that American Recovery and Reinvestment Act spending yielded about 8 jobs per $ 1 million spent, also using an instrumental variables strategy.

While these results are interesting, the current literature on fiscal multipliers and, more generally speaking, government spending has focused almost exclusively on developed countries. The literature on government spending in developing countries is scant, but existing studies do hint at the notion that developing country (GDP) multipliers are quite small. Kraay (2012, 2013) use variation in World Bank spending projects to gain leverage on identifying the effects of fiscal spending on GDP. Kray (2012) fails to find multipliers significantly different from doer on a

sample of 29 developing (and almost entirely African) countries, and Kray (2013) finds multipliers in the range of 0.3 to 0.5 on a larger sample of 102 developing countries. The author suggests the lack of mechanisms of wealth effects in developing countries as a reason for such low multipliers.

As aforementioned, in order to deal of the endogeneity problem of government spending, our paper uses a known discontinuity in federal transfers to municipalities based on population cutoffs to identify 'shocks' in government spending. A number of studies have used the same discontinuity, however none has focused directly on the effects on labor market outcomes, though some examine a few related outcome variables in supporting analysis. Brollo et al (2013) examines the political resource curse, or the idea that an influx of resources can actually prove detrimental to a state, using the same discontinuity to identify exogenous variation in government resources. The authors indeed find evidence of a political resource curse, and thus of non-negligible corruption at the municipality level. Litschig and Morrison (2012) also use FPM cutoffs to identify the effects of increased government spending on the reelection probabilities of incumbent officials. They find that the increased government spending due to the FPM cutoffs increased the reelection probability by about 10%. The authors explored the effects on wages and income per capita, finding positive effects, however the period they examined was from 1982-1985. Caselli and Michaels (2013) examine a different source of exogenous variation in oil revenue royalties paid to municipalities, finding small effects of oil abundance on government provision of services. They also do not explore labor market outcomes in detail. Finally, Corbi, Surico and Papaioannou (2014) examine the effects of spending on GDP in Brazilian municipalities, as measured primarily with satellite imagery of night-time lights, finding fiscal multipliers on the order of 1.4 to 1.8. Caselli and Michaels (2013) examine a different source of exogenous variation in oil revenue royalties paid to municipalities, finding small effects of oil abundance on government provision of services. They also do not explore labor market outcomes in detail.

To more explicitly state our contribution, we use a relatively novel identification approach to estimate the effects of federal transfers and subsequent government spending on labor market outcomes, namely employment. To our knowledge, an in-depth analysis of the effect these transfers might have on labor markets in Brazil has not been done, and our paper adds more broadly to the

literature on job creation and local government spending in developing counties.

2 Institutional Background and Data Sources

The Brazilian government operates in a highly decentralized manner. The 26 states of Brazil are subdivided into over 5,500 municipalities, or *municípios*, which have an average population of around 35,000 people. These municipalities are the lowest level of governance. Each of the 26 states has, on average, about 200 municipalities. Local political power, including the allocation of government resources, is concentrated within the executive government of these municipalities. Each has an elected mayor, or *prefeito*, that has major influence over the distribution of the municipal budget, along with an elected council, or *Camara dos Vereadores*.

In general, municipalities are heavily dependent on government transfers as a source of revenue. For an average municipality, tax revenues constitute a relatively small percentage of total revenues (around 6%), whereas transfers from the *Fundo de Participação dos Municípios* program constitute a much more sizable fraction (around 40%). The program stipulates that 30% of the funds must be spent on education and health, 70% is unrestricted (Brollo et al (2013)).[1]

The FPM program works by distributing funds first allocated at the state level to municipalities within the states, based on their population. FPM funding is initially collected via income taxes nationwide. The funds are then allocated fractionally. Each of the 26 states receives a different amount of federal transfers, according to state shares which have not changed for some time (Corbi et al (2014)). Within each state, FPM funds to municipalities are distributed according to shares determined by population brackets. To be precise, municipalities, indexed by i within a state j are assigned coefficients λ_i. The amount of federal transfers a municipality receives, or $FT_{i,j}$, is a fraction of the total amount allocated to the state (FT_j):

$$FT_{i,j} = \frac{\lambda_i}{\sum_i \lambda_i} FT_j$$

Table 1 gives a description of the population brackets and their corresponding coefficients. The

[1] All monetary amounts in this paper, unless otherwise specified, are listed in 2000 *reais*.

coefficients, unsurprisingly, are increasing in population, as shown in Figure 1.

This federal transfer framework, then, provides an interesting discontinuity in federal money with population as a running variable. Given the discontinuity in transfers, variation in government spending along this dimension is plausibly exogenous, assuming that the running variable of population is not manipulated by mayors or by other government officials. Non-fungible, general expenditures (*despesas não financeiras*) are subdivided into expenditures on current expenditures and capital spending, and expenditures on labor (*pessoas*); about 45% of general spending goes toward labor expenditures. Of the labor spending, a large majority (around 85%) goes to the existing workforce (i.e. not toward pensions or inactive workers).

2.1 Data Sources

Our data come from three main sources. First, population data are estimated by the Brazilian Institute of Geography and Statistics, or Instituto Brasileiro de Geografia e Estatística (IBGE); the population counts are provided annually to the public on the website of the *Tribunal de Contas da União* (TCU), a federal accountability agency responsible for oversight of federally distributed funds. Public finance data, such as revenues, expenditures, and the distribution of expenditures come from the Finances of Brazil, *Finanças do Brasil* (FINBRA) annual survey of the Ministry of Finance, and employment and wages by sector and education level come from the Annual Report of Social Information, or Relaçao Anual de Informaçoes Sociais (RAIS) of the Ministry of Labor. The RAIS only covers workers in the formal sector. The merged data contain information at the municipality level over the years 2002 to 2010.

2.2 Analytical Sample Construction

For our analysis, we use observations around the first of the seven thresholds. Other papers that use the FPM transfers as identification tend to examine all cutoffs; however, in our data, there does not seem to be a strong link between receipts of FPM funds and spending in cutoffs beyond the first. The program tends to be more important for smaller municipalities, so this result is perhaps unsurprising. We therefore estimate the effect on "jobs" for the smallest municipalities, with an

average population of around 10,000 inhabitants.

Additionally, we examine only the years 2002-2007 – we omit the last three years of data. The reason for this is the threat of (imprecise) manipulation of the population counts of mayors in census years (shown in Monasterio (2013)) which could affect the interpretation of the "jobs" multiplier. More will be explained in the following section as to the extent of the manipulation and the the potential effect it might have on our estimates.

3 Estimation and Identification

3.1 Validity of the Discontinuity

The main concern in estimating the effect of fiscal spending via a "naive" OLS approach is the potential bias of the estimate due to the implausibility of random government spending. For instance, government spending is often a *response* to economic outcomes and usually cannot be seen as random. In the regression discontinuity framework described above, our exogeneity comes from the notion that government funds, and spending, are distributed randomly close to the cutoff. However, even in an RD environment, there can still be threats to this identification. We identify two main sources of potential threats: (1) the exogeneity of the cutoffs, and (2) the manipulation of position around the cutoff.

- **Exogeneity:** Litschig and Morrison (2012) mention that the history of the seemingly arbitrary population bracket cutoff numbers originally come from the establishment of a redistribution program by a military junta in the 1960s aimed at allocating resources to areas by objective measures of need – population happened to be a proxy for this. The original numbers were thought to have been multiples of 2000, however were subsequently updated with population counts and became the arbitrary numbers we see today. Given this history, it is unsurprising that no other known program uses these cutoffs.

- **Manipulation:** If agents are able to precisely change their position around the cutoff in an RD design, the exogeneity of the RD can be compromised (Lee and Lemieux (2009)). Population estimates in non-census years are estimated independently by the IBGE and then verified by Brazil's Federal Court of Audits (the TCU); mayors are never involved in their creation. Litschig (2012) does find evidence of deviations from the estimates in the early 1990s, and while we cannot rule out that the threat of some manipulation of these estimates remains, we find no empirical evidence of manipulation. Specifically, that there do not seem to be discontinuous breaks in the population density, as shown by McCrary (2008) tests, in years in which the population was estimated. We would expect such breaks if mayors were actually attempting to marginally clear the closest population cutoff, which is actually what we *do* see in years in which populations were *not* estimated.

  In 2007, a recount (*Contagem 2007*) was carried out to correct potentially erroneous groupings of municipalities into population brackets. McCrary tests show clear evidence of large breaks in the density of observations around the discontinuity. Monasterio (2013) has shown similar results for Census years. It is clear that agents are somehow manipulating their position around the cutoffs. There are various theories as to how and by whom such manipulation is taking place. Mayors could be engaging in additional hiring in the year of the recount in order to artificially boost population, or be spending on amenities or incentives to attract potential citizens (and workers). In either case, including these years (and those following) could overstate the "effect" of spending on employment. Therefore, to preserve our notion of exogeneity, we omit years 2007 and following from our analysis.

3.2 Specifications

Our specification follows the regression discontinuity literature in the spirit of Lee and Lemieux (2009), Dinardo and Lee (2004), and Hahn, Todd, and van der Klaauw (2001). As such, we estimate the effect of being just above the relevant threshold controlling for a polynomial in the running variable, as well as time and state fixed effects to "soak up" residual variation (Lee and Lemeiux (2009)). It should be noted here that state fixed effects are especially important for estimation,

given that funds are first allocated according to fixed state shares; thus the "size of the pie" that each municipality gets depends on the state in which it is located.

Our main specification is the following:

$$y_{i,s,t} = \alpha + \beta(D_{i,s,t}) + f(pop_{i,s,t}) + \delta_t + \mu_s + \varepsilon_{i,s,t} \quad (1)$$

where $y_{i,s,t}$ represents either the amount of FPM transfers, spending, labor market outcome for a municipality i in state s at year t. $D_{i,s,t}$ is an indicator function taking on the value 1 if the municipality i (in state s at year t) has a population that is greater than 10,189, and 0 otherwise. Year-fixed effects and state-fixed effects are captured by δ_t and μ_t respectively. For precision, we estimate Huber-White standard errors, clustered at the municipality level. We consider two versions of $f(pop_{i,s,t})$: a second degree and third degree polynomial, both allowing for flexibility around the cutoff.

Additionally, we also include time-invariant controls on the status of the labor market (as measured by the 2000 Census) to soak up additional variation. We do not expect the inclusion of these controls to affect the accuracy of our estimates but rather the precision.

We understand that the estimation of regression discontinuities can be a very specification-dependent strategy, and we use the above regression model because it seems easiest to interpret. We want to present the simplest version of our identification strategy possible, and we suspect that were there to be effects, they should show up in this minimalistic specification.

3.3 First-Stage Estimates

As aforementioned, our identification relies on the notion that the receipt of FPM funds increases discontinuously across the cutoff, and that the receipt of these funds translates into discontinuously higher spending. Figures 2 and 3 show the jump in the overall receipts, and overall non-finance government spending [2] (not including state or year fixed effects). Tables 1, 2, and 3 show the

[2] "Non-Finance refers to spending that is not used to pay off loans or debts for, say, past projects. "Finance" spending is a small fraction of overall spending and does not jump at the cutoffs. For our intents, non-finance spending

numerical estimates of the jump. On average, receipts and spending both seem to rise by about 11%-13%.

4 Results

Given our first stage, we show both the reduced form and instrumental variable estimates, which use place above the cutoff to generate plausibly exogenous increases in spending that can be used to determine the effect of spending on labor market outcomes. This approach is in the vein of the 'fuzzy' RD design estimate (Lee and Lemieux (2009)).

4.1 Employment

First stage estimates point to increases in overall jobs by slightly over 210 at the cutoff, seen in Table 4. Instrumental variables estimates (in Table 8) indicate that a 1% increase in spending is associated with an 0.92 % increase in the number jobs for a "spending-employment" elasticity of close to 1. In table 9, we find an "overall" jobs multiplier of about 0.165 jobs per 1000 *reais* spent (in year 2000 *reais*), or a cost of about $4800 per job in current US dollars. For comparison, Wilson (2009) estimates a cost of around $125,000 per job, and Serrato and Wingender (2014) estimate the same to be around $35,000.

This increase in jobs seems to be concentrated in certain skill segments of the labor market, as shown in the reduced form effects of Table 6. Specifically, the employment increases seem to be those without college degrees. Jobs involving college educated workers increase almost negligibly, while jobs involving those without a secondary diploma and those who only have a secondary diploma as their highest education level increase by around 160 and 50 respectively, accounting for nearly all of the increase in overall jobs.

There also seems to be heterogeneity across sectors of the labor market (Table 7). Private

will be henceforth referred to as "general spending."

employment increases significantly, and there seems to be a *reduction* in the number of municipal jobs, though the effect is not precisely estimated. As crowd-out of private sector labor is often a concern in fiscal spending studies, it seems that there is no evidence of this but rather some indication that there may be some "reverse" crowd-out occurring.

4.2 Earnings and Wages

Earnings data is available from the RAIS at the monthly level, however, we so far have not able to identify consistent effects of spending on wages. Point estimates tended to be small and insignificant, as shown in Table 5. Given the rigid nature of wage setting in Brazil, this result is perhaps unsurprising.

5 Discussion

There are several potential explanations for why we might see large job effects concentrated among the lower skilled and the private sector. Firstly, municipalities may be engaging in public works projects that require the use of external, low-skilled (and perhaps temporary) labor. If this is true, then it is debatable to what extent these job numbers can be considered real 'contributions' to the economy. To the extent these workers are taken from the unemployed, it could potentially be hugely beneficial; however, if the workers are coming from outside the municipality and will return to their place of permanent residence, then the long-run effect may be negligible. More work to determine the effects of worker relocation is needed to definitively answer this question.

Secondly, it is known that firing public workers in Brazil is notoriously hard due to legal constraints. Mayors and other employers, conditional on the eventual receipt of government funds, may therefore have an incentive to hire labor on contracts which may be more easily dissolved.

However, aside from these concerns, our results indicate that public spending in Brazil may indeed have some redistributive qualities at the local level. Household income has been linked to

skill-level, and to the extent that jobs are being created among the lower-skilled segment of the labor force, government spending could represent a transfer to lower-income households, which may exist independently of social welfare programs like *Bolsa Familia*.

6 Conclusion

Overall, we find evidence of sizable employment effects from local government spending in Brazil. Using a discontinuity in the allocation of federal transfers according to population brackets, we find that the effect is concentrated at the lower end of the skill distribution – virtually no jobs involving workers with college education were created. Additionally, there appears to be evidence that the effect is also concentrated within the private sector. Surprisingly, public sector jobs did not see an increase as a result of being just above the cutoff.

Our results point to the notion that 'jobs multipliers' in developing countries may be large due to the hiring of low-skilled labor. To the extent that skill level is correlated with household income, our results indicate that government spending may have a potential redistributive effect. More work remains to be done on the effects on wages and on the trajectory of individual workers as they face an economy with higher levels of spending.

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Population interval	FPM coefficient
Below 10,189	0.6
10,189–13,584	0.8
13,585–16,980	1
16,981–23,772	1.2
23,773–30,564	1.4
30,565–37,356	1.6
37,357–44,148	1.8
44,149–50,940	2
Above 50,940	from 2.2 to 4

Figure 1: FPM Population Brackets and Corresponding Coefficients

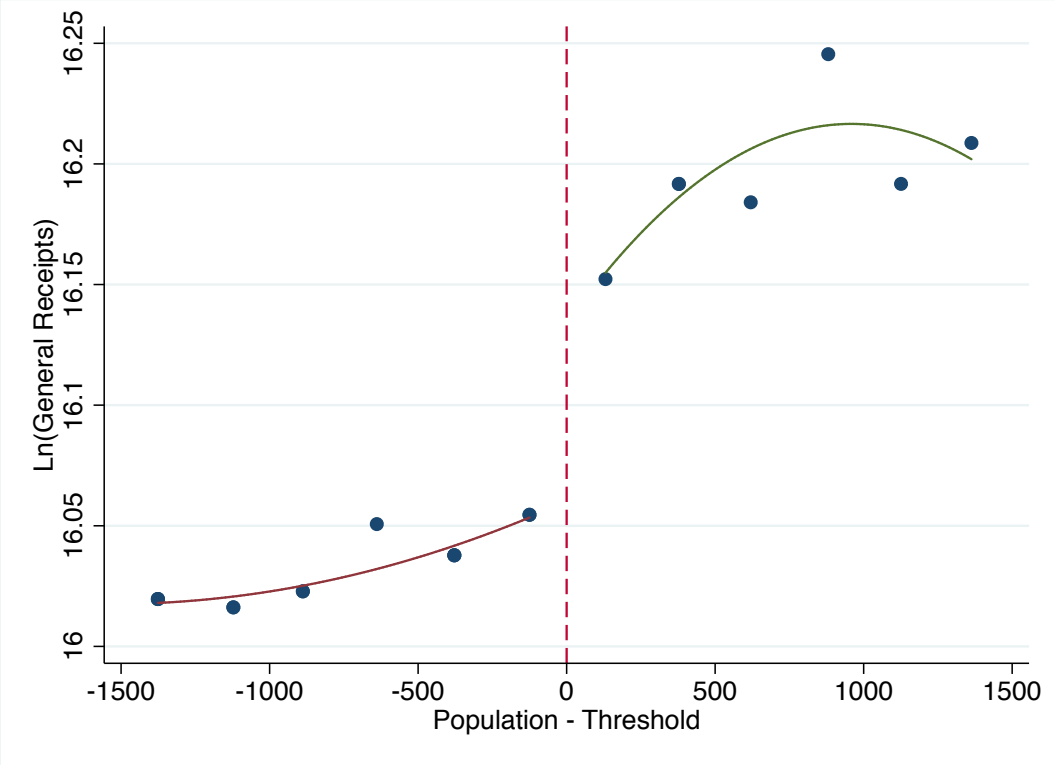

Figure 2: Ln(General Receipts) as a Function of Normalized Population

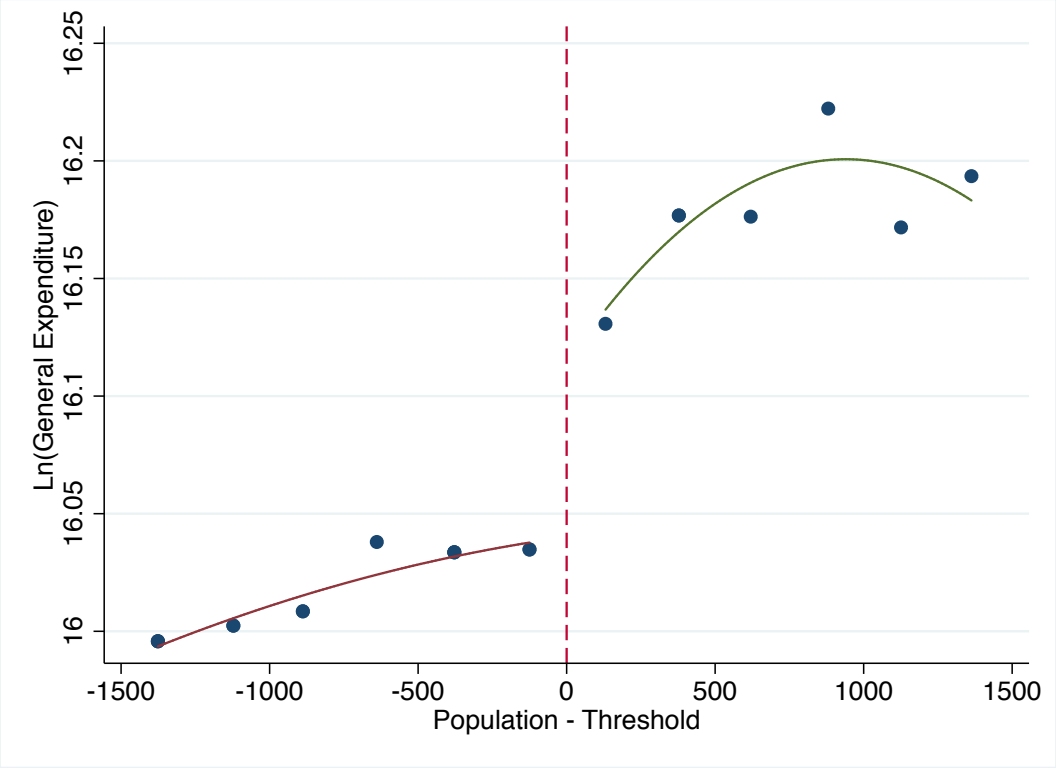

Figure 3: Ln(General Expenditure) as a Function of Normalized Population

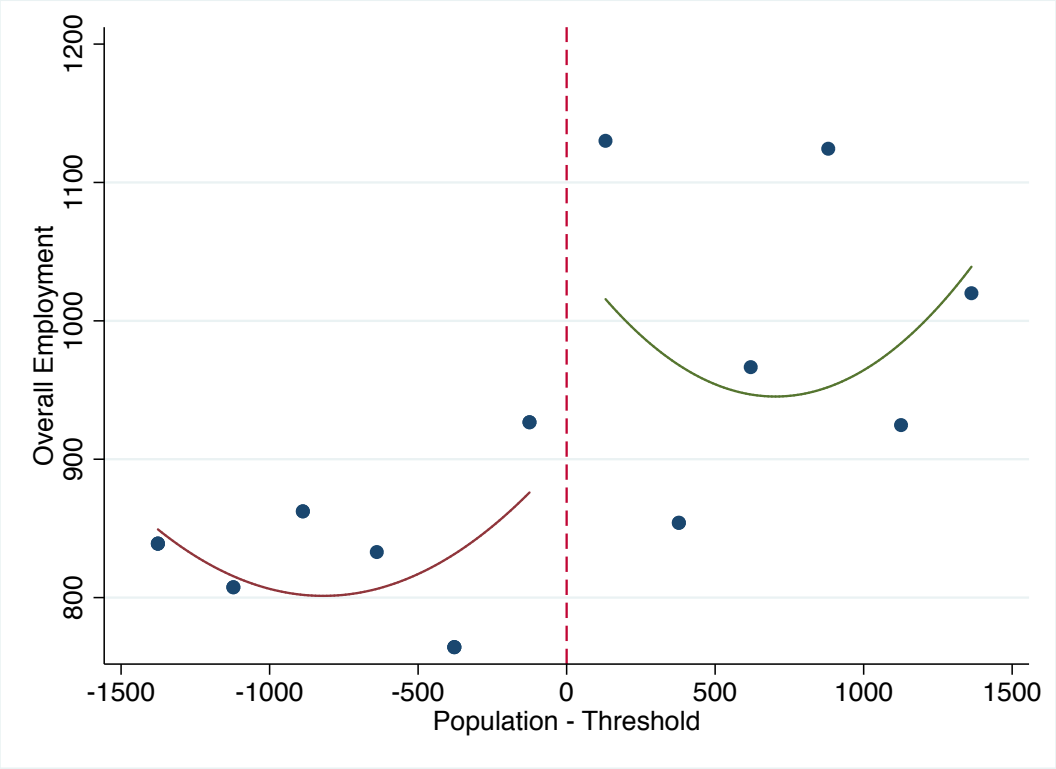

Figure 4: Total Employment as a Function of Normalized Population

	(1)	(2)	(3)
VARIABLES	Ln(FPM Transfers)	Ln(FPM Transfers)	Ln(FPM Transfers)
Above	0.232***	0.224***	0.224***
	(0.0393)	(0.0386)	(0.0389)
Observations	2,818	2,818	2,818
R^2	0.174	0.250	0.251
State FE	NO	YES	YES
Year FE	YES	YES	YES
Poly. Degree	2	2	2
Flex Slopes	YES	YES	YES
Baseline Controls	NO	NO	YES
Clustered SE	Municipality	Municipality	Municipality

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1: Reduced Form Effects on FPM Transfers (in 1000's of year 2000 *reais*)

	(1)	(2)	(3)
VARIABLES	Ln(General Receipts)	Ln(General Receipts)	Ln(General Receipts)
Above	0.123***	0.140***	0.140***
	(0.0470)	(0.0295)	(0.0277)
Observations	2,818	2,818	2,818
R^2	0.208	0.551	0.585
State FE	NO	YES	YES
Year FE	YES	YES	YES
Poly. Degree	2	2	2
Flex Slopes	YES	YES	YES
Baseline Controls	NO	NO	YES
Clustered SE	Municipality	Municipality	Municipality

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2: Reduced Form Effects on Receipts (in 1000's of year 2000 *reais*)

	(1)	(2)	(3)
VARIABLES	Ln(Government Expenditure)	Ln(Government Expenditure)	Ln(Government Expenditure)
Above	0.115**	0.131***	0.130***
	(0.0472)	(0.0306)	(0.0289)
Observations	2,818	2,818	2,818
R^2	0.199	0.518	0.548
State FE	NO	YES	YES
Year FE	YES	YES	YES
Poly. Degree	2	2	2
Flex Slopes	YES	YES	YES
Baseline Controls	NO	NO	YES
Clustered SE	Municipality	Municipality	Municipality

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3: Reduced Form Effects on Government Expenditure (in 1000's of year 200 *reais*)

	(1)	(2)	(3)
VARIABLES	Overall Employment	Overall Employment	Overall Employment
Above	147.8	226.7**	217.8**
	(119.9)	(108.4)	(94.43)
Observations	2,818	2,818	2,818
R^2	0.008	0.179	0.286
State FE	NO	YES	YES
Year FE	YES	YES	YES
Poly. Degree	2	2	2
Flex Slopes	YES	YES	YES
Baseline Controls	NO	NO	YES
Clustered SE	Municipality	Municipality	Municipality

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Reduced Form Effects on Total Employment (Number of Jobs)

	(1)	(2)	(3)
VARIABLES	Ln Avg. Wage	Ln Avg. Wage	Ln Avg. Wage
Above	-0.0329	0.0202	0.0188
	(0.0460)	(0.0304)	(0.0313)
Observations	2,818	2,818	2,818
R^2	0.075	0.447	0.465
State FE	NO	YES	YES
Year FE	YES	YES	YES
Poly. Degree	2	2	2
Flex Slopes	YES	YES	YES
Baseline Controls	NO	NO	YES
Clustered SE	Municipality	Municipality	Municipality

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Reduced Form Effects on Ln(Average Monthly Earnings)

	(1)	(2)	(3)	(4)
VARIABLES	Less than HS	HS grad	Some college	College grad
Above	163.7**	49.21**	2.738	2.212
	(72.81)	(24.82)	(2.703)	(6.694)
Observations	2,818	2,818	2,818	2,818
R^2	0.285	0.219	0.332	0.275
State FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Poly. Degree	2	2	2	2
Flex Slopes	YES	YES	YES	YES
Baseline Controls	YES	YES	YES	YES
Clustered SE	Municipality	Municipality	Municipality	Municipality

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Reduced Form Employment Effects by Education Level

	(1)	(2)	(3)
VARIABLES	Private	Public - Municipal	Public - Other
Above	226.2**	-11.17	0.875*
	(89.99)	(16.12)	(0.521)
Observations	2,818	2,818	2,818
R^2	0.241	0.281	0.026
State FE	YES	YES	YES
Year FE	YES	YES	YES
Poly. Degree	2	2	2
Flex Slopes	YES	YES	YES
Baseline Controls	YES	YES	YES
Clustered SE	Municipality	Municipality	Municipality

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Reduced Form Employment Effects by Sector

	(1)	(2)
VARIABLES	Ln(Government Expenditure)	Ln Employment
Above	0.130***	
	(0.0289)	
Ln(Government Expenditure)		0.918*
		(0.507)
Constant	15.40***	-10.60
	(0.201)	(7.826)
Observations	2,818	2,818
R^2	0.548	0.696
State FE	YES	YES
Year FE	YES	YES
Poly. Degree	2	2
Flex Slopes	YES	YES
Baseline Controls	YES	YES
First Stage F Stat	16.29	16.29
Clustered SE	Municipality	Municipality

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 8: IV Estimates of Ln(Expenditure) on Ln(Employment)

	(1)	(2)
VARIABLES	Gen. Expenditure	Overall Employment
Above	1,318***	
	(326.5)	
Gen. Expenditure		0.165**
		(0.0745)
Constant	2,779	-2,002***
	(2,290)	(642.7)
Observations	2,818	2,818
R^2	0.496	0.309
State FE	YES	YES
Year FE	YES	YES
Poly. Degree	2	2
Flex Slopes	YES	YES
Baseline Controls	YES	YES
First Stage F Stat	16.29	16.29
Clustered SE	Municipality	Municipality

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 9: IV Estimates of Expenditure on Employment