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

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February 17, 2017

Abstract

This paper examines the causal effect of local government spending on labor markets in a developing country. 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 an additional 1% of spending at the local level (roughly $72,600) translates to an increase of about 20 formal jobs. This effect is much larger than other employment multipliers estimated in developed countries. We find that the size of the effect is driven by increases in employment from unskilled labor in the private sector, indicating that the transfer of federal funds can have a redistributive effect on the labor market, even if no such policy goal is explicitly stated.

JEL Classification: H72, O12, J68 ,

Keywords: Government Expenditure, Local Labor Markets, Government Multiplier

*We thank participants at the Northeast Universities Development Consortium, LACEA-LAMES Annual Meetings, the Annual Meetings of the Brazilian Econometric Society, the Pacific Conference for Development Economics, SOLE-EALE World Conference, and University of Michigan Informal Development Seminar, as well as Raj Arunachalam, John Bound, Charlie Brown, John DiNardo and Gaurav Khanna for their help and feedback. Please direct comments at bpthomp@umich.edu.

1 Introduction

Fiscal policy is a primary means by which the government can affect the state of the economy and researchers often debate the effectiveness of government spending at creating jobs. Government spending can also entail vastly different outcomes for economies in developing countries. On one hand, 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 a developing country. On the other hand, fiscal policy can potentially be more effective in creating formal employment due to the lower cost of a job and higher unemployment and informality in developing countries. As a consequence of more elastic labor supply curves in the formal sector, small changes in the labor demand generated by government spending have the potential to create large formal employment increase in developing countries.

In this paper we estimate the causal effect of local government spending on labor outcomes in Brazil. A central challenge faced by researchers estimating the economic effects of fiscal policy is the lack of existing identifying variation in government spending. Governments often change their spending as a response to contemporaneous economic shocks. In order to address this endogeneity issue, we use a regression discontinuity design to isolate exogenous variation in local government spending, which we then use to estimate the effect of spending on local labor market outcomes. We exploit a discontinuity in the allocation of intergovernmental transfers: amounts of transfers from the federal government to municipalities under the *Fundo de Participação dos Municípios* (FPM) program vary according to population thresholds. Municipalities in the same state within the same population bracket receive the same amount of transfers. However, municipalities just above and just below the population bracket thresholds, which are presumably similar in observable and unobservable dimensions, receive significantly different FPM transfers.

We first demonstrate that FPM transfers translate into government spending. In our sample, municipalities just above the population cutoff spend on average 7% more than those just below the cutoff. Considering the incentives that municipalities have to obtain larger FPM transfers, we also investigate whether municipalities can precisely manipulate which side of the cutoff they may be on. We show evidence that municipalities had little power to determine their position around the population threshold during the period of our analysis.[1]

[1] As evidence for the validity of the regression discontinuity design, we find no evidence of bunching around the

We focus mainly on formal employment using detailed data from an annual administrative survey covering the overwhelming majority of municipalities in Brazil. We estimate that an additional 1% of spending at the local level (roughly $72,604 on average) is associated with an increase of roughly 19.69 formal jobs in a municipality in a given year. This estimate can be translated to an estimated cost-per-formal-job-created of around $3,687 per year. We find that the majority of the job increase is composed of low-skilled and private-sector workers, and we do not find any detectable effects of government spending on average monthly wages.

In addition to estimating the impact of spending on employment and wages, we also investigate *how* municipal governments spend the "windfall" revenue associated with being above the population cutoff. Most of the spending is directed toward public investment rather than the public servant payroll: municipalities above the cutoff spend 14% more on public investments and only 6% more on the payroll of the municipal workers. We interpret this finding as consistent with the notion that mayors are forward-looking in the way they allocate government resources. Given that they are unable to lay off public servants or adjust wages downward in future periods, mayors might be discouraged to increase wages and employment in the public sector as a response to temporary positive revenue shocks. In contrast, public investments are more likely to generate better business environments and economic prosperity in the future. The increase in public investment also explains the substantial increase in employment in the private sector, given that municipalities often hire private contractors for local construction projects.

This paper adds to a growing body of recent literature that uses novel instruments to identify exogenous variation in local spending in the US and has yielded fairly varied estimates. Shoag (2010) uses variation in government pension windfalls to estimate the impact of state government spending on the economy, estimating a cost per job created of around $35,000. Wilson (2012) uses state-level spending from the American Recovery and Reinvestment Act (ARRA) of 2009, instrumenting government spending with allocation formulas and pre-determined factors such as the number of highway lane-miles in a state or the share of youth in total population and estimating a $125,000 cost per job created. Chodorow-Reich et al. (2012) uses formula-driven variation in federal transfers to states in 2009 associated with state-level Medicaid spending patterns before the Great Recession, finding a cost per job created around $25,000. Finally, Serrato and Wingender (2014) uses variation

population cutoff for the years 2002 to 2007. However, we find evidence of bunching for the year 2008, when population was estimated by a pre-announced population recount. See section 3 for the details.

in Census population counts to determine the allocation of government resources to estimate a cost per job of $30,000.[2]

While these findings are significant, this new literature on government spending has focused almost exclusively on developed countries.[3] Our results indicate that despite potentially important government failures, local government spending can be effective in creating formal jobs in developing countries. A possible explanation for the high job multiplier is the low productivity and associated low labor cost in Brazil, and indeed, the average monthly wage for the workers in our sample is only roughly 404 US dollars.[4] Nonetheless, higher unemployment rates and informality levels in Brazil might also be playing a role in government spending being even more effective in creating jobs, as it can generate a highly elastic labor supply of low-skilled workers to the formal sector.[5] As consequence, changes in the labor demand generated by an exogenous increase in government spending would lead to significant changes in formal employment and small changes in wages, which we observe in the data. While we cannot identify using available data whether these new jobs are associated with workers moving from unemployment or the informal sector, there is a substantial welfare gain for low-skilled workers to be employed in the formal sector.[6]

This paper also adds to the literature that studies the relationship between FPM discontinuities and municipal welfare measures. Litschig and Morrison (2013) use FPM cutoffs to identify the effects of government spending on long-term education and income outcomes over the period 1980-1991. The authors find that municipalities that received extra FPM funds over the period 1982-1985 benefited in terms of higher schooling and literacy rates and lower poverty rates in 1991. Brollo et al.

[2] Like our paper, most of these studies use windfall changes in revenue to examine how local government spending affects employment. However, most fiscal policy is implemented at the national level. The impact of fiscal policy at the national level might be mitigated by Ricardian equivalence as consumers might anticipate an increase in future taxes or interest rates.

[3] 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, 2014) use variation in World Bank spending projects to gain leverage on identifying the effects of fiscal spending on GDP. Kraay (2012) fails to find multipliers significantly different from doer on a sample of 29 developing (and almost entirely African) countries, and Kraay (2014) finds multipliers in the range of 0.3 to 0.5 on a larger sample of 102 developing countries. Caselli and Michaels (2013) uses variation in oil output among Brazilian municipalities to investigate the effect of government spending on many welfare indicators. They find modest-to-undetectable effects on household income. None of these papers estimate the effect of government spending on labor market outcomes.

[4] As a reference, according to the American Social Security Administration website, the average annual wage of an American worker in 2005 was $36,953.

[5] According to the World Bank data, in 2005 the unemployment rate in Brazil was 9.5% and the unemployment rate in the US was 5.2%.

[6] Using 2010 Census data, we find that workers in the formal sector without a high school degree earn on average 40% more than those in the informal sector.

(2013) uses the FPM discontinuities to examine the effect of additional government revenues on political corruption and on the quality of politicians. The authors estimate that the larger revenues generated by these transfers increase corruption and reduce the average education of candidates for mayor. Finally, Corbi et al. (2014) explore the FPM cutoffs in conjunction with luminosity data to estimate the effect of local government spending on GDP in Brazilian municipalities. Their point estimates for multipliers from various econometric specifications range from roughly 1.4 to 1.8.

This paper contributes to this literature in several capacities. While other studies have examined the effect of FPM transfers on education and poverty in the 1980's and political corruption and municipal GDP using more recent data, this paper looks at labor market outcomes. To our knowledge, this is the first study to present an estimate of how many formal jobs in Brazil can be created as a result of local government spending. This information is important because governments often use fiscal policy to affect weak labor markets. Additionally, this paper shows that municipalities spend most of the windfall revenue on public investment rather than payroll, which we interpret as mayors being forward looking on how they spend the temporary revenue. The increase in public investment implies that the majority of jobs are created in the private sector, demonstrating that rather than a crowd-out of private sector labor by local policy, we observe a crowd-in occurring as local governments often hire private companies to perform construction projects. Finally, this paper discusses the potential distributive effects of government spending. We find that virtually all the increase in employment is coming from workers with less than a high school degree, suggesting that local government spending has the potential to reduce inequalities in the labor market in Brazil.

Our paper is organized in the following way: Section 2 outlines the institutional background of the FPM program and the data we use for estimation. Section 3 discusses our estimation strategy and the validity of the regression discontinuity design. Section 4 presents our results, and Section 5 presents robustness checks. Section 6 concludes.

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 are the lowest level of governance.

Local political power, including the allocation of government resources, is concentrated within the executive government of each of these municipalities. Each has a directly elected mayor, or *prefeito*, that has major influence over the distribution of municipal funds, along with an elected council, or *Câmara dos Vereadores*.

Municipality governments provide an array of public services and are funded heavily by revenue sharing programs at the state and federal levels. As mandated by the Brazilian Constitution, municipalities are responsible for urban planning, land development, public transportation, and garbage collection. In addition, municipalities share responsibilities with the states and the federal government in the provision of energy, housing, health care and education. Most of the education spending by municipal governments is in primary education. In terms of health provision, municipalities often maintain local clinics and smaller hospitals. Urban development includes constructing and maintaining urban infrastructures. Municipalities might also develop social assistance programs such as cash transfers to households (Caselli and Michaels, 2013). In order to finance their operation, municipalities collect taxes on services and property. However, such revenue is relatively limited, and municipalities are heavily dependent on federal and state government transfers. In our analytical sample, municipal tax revenues constitute about 12% of total revenues in contrast to roughly 37% from the FPM transfer program.[7]

The FPM program is funded by federal taxes and is redistributed to states and municipalities according to sharing rules. From the total revenues raised by the Income Tax and Industrial Product Tax, the federal government allocates 23% to the FPM program, which is distributed to each of the 26 states according to fixed state shares. Within each state, FPM funds are then distributed to municipalities according to FPM coefficients determined by population brackets.[8] Table 1 presents the population brackets and their corresponding coefficients.

To be precise, the amount of FPM transfers to a municipality i and state s is

$$FT_{i,s} = \frac{FPM_s \lambda_i}{\sum_{j \in s} \lambda_j}$$

where FPM_s is the amount of resources allocated to state s, and λ_i is the FPM coefficient of

[7] From the reminder sources of revenues, 23% are transfers from the state government and 28% are other transfers from federal government different from FPM. None of these other transfers use population cutoffs.

[8] This scheme for linking transfers to population was decided during the dictatorship that came to power in 1964, in an effort to break with the clientele practice that transferred federal money to a narrow group of municipalities; Litschig (2012) has explored the history of the sharing rule in more detail.

municipality i based on its population size and $\sum_{j \in s} \lambda_j$ is the some of all FPM coefficient in state s. Note that as each state receives a different FPM_s, two municipalities in the same population bracket receive identical transfers only if they are located in the same state. As for the designated use of these funds, the program stipulates that 30% of the funds must be spent on education and health, but 70% is unrestricted (Brollo et al., 2013).

We show the population distribution of municipalities (measured in 2004) and FPM cutoffs in Figure 1. As can be seen in the figure, most Brazilian municipalities are very small, reflecting a large amount of local political division. We find that the population associated with the first FPM cutoff is around the 49th percentile of the population distribution of the municipalities, meaning that 49% of all municipalities had a population smaller than 10,189. The second and third cutoff represent the 59th and 67th percentiles of the population distribution respectively.

In terms of municipalities finances, general expenditures (*despesas não financeiras*) are subdivided into personnel spending, capital spending and other spending. Personnel spending include salaries, benefits, allowances, retirement income and pensions. In our analytical sample, it comprises around 45% of general spending of which a large majority (around 85%) goes to the active workforce in our analytical sample. Capital spending is associated with an increase in municipal government physical capital. Other spending includes any other expenses that the municipal government might face in a year, such as the purchase of non-durable goods.

Additionally, the relationship between revenues and expenditures is particularly tight in Brazilian municipalities due to the enactment of the Fiscal Responsibility Law (LRF) in 2000, which strengthened fiscal institutions and currently requires the presentation of fiscal administration reports at four-month intervals with a detailed analysis of the government budget execution. The LRF has strict penalties if compliance is breached, including prison terms for public officials and suspension of election rights. Among the constraints included in the LRF are borrowing and debt limits, as well as caps for labor expenditures, making it harder for a municipality to smooth out a windfall in revenues, even if it expects revenue volatility in the future.

2.1 Data Sources

Our data come from three main sources. First, the population used for FPM determination is estimated by the Brazilian Institute of Geography and Statistics, or *Instituto Brasileiro de Geografia* *e Estatística* (IBGE). IBGE calculates the population in each municipality based on previous Censuses and population counts, birth and death rates, as well as migration trends. These population counts are provided annually to the public on the website of the Federal Court of Audits, or *Tribunal de Contas da União* (TCU), a federal accountability agency responsible for oversight of federally distributed funds. The estimates for the next year's FPM coefficients are published in October, a few months before the declaration of the municipal government budget.

Second, public finance data for Brazilian municipality come from the *Finanças do Brasil* (Finances of Brazil) annual survey, or FINBRA, of the Ministry of Finance. These dataset include information on revenues, expenditures, and FPM transfers by municipality and year. This data is available from the website of the Brazilian National Treasury (*Tesouro Nacional*). We are interested in general "non-finance" spending, which refers to all spending of a municipality in a given year with the exception of paying off interest or debts from previous loans. For our intents, non-finance spending will be henceforth referred to as "general spending".[9]

Third, the number of jobs and wages, by sector and by education, at the municipality level come from the Annual Report of Social Information, or *Relaçao Anual de Informaçoes Sociais* (RAIS). This data (aggregated at the municipality level) is publicly available from the website of the of the Ministry of Labor and consists of the universe of formal employees of the country. One important feature of the data is that the RAIS only covers workers in the formal sector. For our main analysis, we restrict the sample to full-time workers (those who work 31 or more hours per week) employed as of December 31st in the reference year.[10] The data contain consistent information at the municipality level over the years 2002 to 2010.[11] Finally, we use the 2000 Census to obtain characteristics of the municipalities in the sample that will be used as controls in some of our estimations. Henceforth, all monetary variables will be presented in year 2010 reais.[12]

[9] "Finance" spending is a small fraction of overall spending (less than 1%) and does not jump at the cutoffs.

[10] As we do not observe individual identifiers and on the public version of RAIS (only job identifiers), by restricting the sample to those who work 31 or more hours, we ensure that we do not double count workers with two part-time jobs.

[11] Unfortunately, we cannot follow workers using the publicly-available RAIS data. As a result, we cannot test whether there is internal migration response to government spending using RAIS.

[12] In order to convert values from 2010 Reais to dollars, we use the average exchange rate of 2010 (0.57 dollars per 1 real).

2.2 Analytical Sample Construction

We make a few restrictions to the sample of municipalities that are used in the main analysis of this study. First, our study focuses on changes in government spending for municipalities around the first three population cutoffs: 10,189, 13,585, and 16,981 and we restrict our sample to observations within a +/-1,000 population bandwidth of these first cutoffs (we later show the sensitivity of our results to bandwidth choice, including using optimal bandwidths from Calonico et al., 2014). While there are multiple population brackets used in allocating FPM funds, at subsequent cutoffs the variation in FPM transfers is too small to affect municipal overall revenues, and therefore there is no "first stage" in terms of overall resources available for the municipality (Litschig and Morrison 2013). As a robustness check, we present results using all eight population FPM in the Appendix of the paper. While we find a weaker relationship between FPM cutoffs and government spending, we estimate an even higher job multipliers in this expanded sample, although the effect is less precisely measured. Again, these first three cutoffs represent, respectively, the 49th, 59th and 67th percentiles of the population distribution of municipalities of Brazil around a high concentration of Brazilian municipalities (Figure 1).

The second main restriction is that we examine only the years 2002-2007 - we omit the last three years of data. In 2007, the government implemented a recount of the population in each municipality (*Contagem 2007*) in order to update the FPM coefficients starting in 2008. Other studies using the FPM transfers have noticed that in 2007 and Census years (years in which there was a headcount rather than an estimate of the population), there seems to be bunching just above the FPM cutoffs, and it is suspected that mayors of municipalities close to their respective cutoffs had the opportunity to manipulate their population to receive more transfers in the following year (a specific result is shown in Monasterio, 2013). The precise manipulation of the running variable around the population cutoff would invalidate our identification strategy. Therefore, we omit data following the recount between 2008 and 2010.

We also omit from our analysis municipality observations with abnormally spending amounts, which we characterize as 50% higher or lower than the municipality revenue. For our analytical sample, this corresponds to only 3 observations. Finally, we also omit from our analysis municipalities that have formal labor markets that are less than 0.1% or greater than 40% of the estimated population size (129 data points). These observations are likely errors from the administrative dataset and do not change our analysis substantively. After these sample restrictions, we end up with 1,223 municipalities and 4,842 observations[13].

The main characteristics of the sample are presented in Table 2. The average population size is 12,932. The FPM transfers correspond to about 38% of all revenues of the municipality, demonstrating the high dependency of municipal budgets on this type of transfer. The average spending of a municipality in a given year is about 12.73 millions of 2010 Reais, which is roughly equivalent to 7.26 million 2010 US dollars. Average spending and revenues are very similar, illustrating how tight the budgets are within the sample, as predicted by the Fiscal Responsibility Law, discussed in the previous section.

In our final analytical sample, the average total number of jobs in the formal sector is around 1,020 from which 44% are in the private sector, 42% in the public sector and 13% in other sectors (e.g. non-profit organizations). Most of the labor force in these municipalities is unskilled, with 62% of the jobs being filled by workers without a high school degree. The average monthly wage is 710 year-2010 Reais (404 US dollars), evidence of substantially lower labor costs in Brazil relative to those in the US.

3 Identification and Estimation Strategy

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 assumption that observable and unobservable pre-treatment characteristics of municipalities are not discontinuous around the population 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. We argue that in our study, neither are cause for concern.

Exogeneity: Our design may be compromised if other programs involving municipality fi-

[13] We have an unbalanced panel as some municipalities leave the +/-1,000 population bandwidth overtime.

nances share the same population brackets as the FPM program; however, given the history of the determination of the cutoffs, this is not a cause for concern. As aforementioned, Litschig and Morrison (2013) note 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, they 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 validity of the RD can be compromised. Population estimates in non-census years are estimated independently by the IBGE and then verified by Brazil's Federal Court of Audits (the TCU). As stated in the data description, the population estimates are based on previous censuses and population counts, birth and death rates, as well as migration trends. Mayors are never directly involved in the creation of population estimates. While we cannot rule out that the threat of some manipulation of these estimates remains, we find no empirical evidence of manipulation for the period 2002-2007. Specifically, using a McCrary (2008) test, we find no evidence of discontinuous breaks in the population density, for those years, as shown in Figure 2.

However, as we noted in the description of our analytical sample, there was a population recount in 2007 with the aim of correcting potentially erroneous groupings of municipalities into FPM population brackets. The recount was announced nationwide and mayors were aware that a higher population could be translated to a higher FPM transfers in the following year. A McCrary (2008) test in Figure 3 shows clear evidence of large breaks in the density of observations around the discontinuity for the population used for FPM transfers in 2008.[14] It seems clear, based on our results and others, that municipalities were (and are) somehow manipulating their position around the cutoffs in years in which the population is counted instead of estimated. 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). To preserve exogeneity, we omit years 2008 and later from our analysis.

[14] Corbi et al. (2014) finds the same evidence of manipulation for the year 2008, and Monasterio (2013) has shown similar results for Census years.

3.2 Specifications

We estimate the effect of being just above the relevant threshold controlling for a polynomial in the running variable, including time and state fixed effects to “soak up” residual variation for municipalities within a given bandwidth of the population cutoff. Our specification follows the precedent of using a “polynomial” regression discontinuity estimator in the RD literature (Lee and Lemieux, 2010 and Imbens and Lemieux, 2008).

In order to pool the municipalities across the first three cutoffs, we follow the Litschig and Morrison (2013) estimation strategy: we first create a variable seg_{itj} that indicates whether the population of municipality i in year t is within a 1,000 bandwidth of the cutoff c_j:

$$seg_{itj} = 1[c_j - 1,000 \leq pop_{it} \leq c_j + 1,000] \text{ for } j = 1, 2{,}3$$

where c_j is the j's of the first three FPM cutoff presented in Table 1. As the distance between FPM cutoffs is always greater than 2,000, each municipality is unique to a segment seg_{itj} in a given year.

We then obtain the effect of being above a FPM cutoff on outcome Y_{it} for municipalities around the first three cutoffs by estimating the model:

$$Y_{it} = \beta \sum_{j=1}^{3} 1[pop_{it} \geq c_j] \times seg_{itj} + \sum_{j=1}^{3} g_j(pop_{it} - c_j) \times seg_{itj} + \sum_{j=1}^{3} \alpha_j seg_{itj} + \delta_t + \mu_s + \theta X_i + \varepsilon_{it}$$

where $g_j(.)$ is quadratic function with different slopes for $pop_{it} - c_j$ greater and lower than zero. This specification allows for different slopes for $g_j(.)$ at each cutoff level, controls for a population-bracket fixed-effect, and imposes a common effect β for all three pooled cutoffs. Year-fixed effects and state-fixed effects are captured by δ_t and μ_s respectively. Additionally, we also include time-invariant pre-treatment controls X_i (as measured by the 2000 Census) to soak up additional variation. In this estimation procedure, we only use observations of municipalities within a 1,000 bandwidth of the cutoffs c_j for $j = 1, 2, 3$. We test the sensitivity of our results to this bandwidth choice in section 5.2.

As in any RD design, our identification is based on the assumption that pre-treatment observable and unobservable municipality characteristics are continuously distributed around the threshold and therefore our identification strategy does not rely on covariates, year or state fixed effects. We include them only to eliminate small sample biases and improve the precision of estimates (Imbens and Lemieux, 2008). While for the main results of the paper we follow Brollo et al. (2013) and Litschig and Morrison (2013) and use state fixed effects and year fixed effects, we show in subsection 5.4 that our results are robust to the inclusion of state-year fixed effects and municipality fixed effects. We estimate Huber-White standard errors clustered at the municipality level.

4 Results

4.1 Government Revenues and Spending

Our identification relies on the discontinuity in FPM funds translating into higher government spending for municipalities above FPM cutoffs. For this purpose, we use detailed data from FINBRA on revenues and spending for each municipality in our sample during the period of analysis to determine how these funds influence spending.

We find that municipalities just above the first three cutoffs receive on average 14% more FPM transfers than those below the cutoff (Table 3). This result indicates that the mechanisms for FPM transfers are in accordance to what is established by law. We also find that the discontinuity in transfers translates to higher overall revenues for the municipality (Figure 4 and Table 3). We estimate that total revenues rise by 8%, on average, across FPM cutoffs. These findings suggest that municipalities are not able to fully compensate for revenue losses from being below the cutoff by increasing other sources of revenues, such as local taxes and other types of transfers. We indeed find no evidence discontinuities of other revenues around FPM cutoffs in section 5.

We also present evidence of the discontinuity in overall non-finance general expenditure around FPM cutoffs in Table 3 and in Figure 5. We estimate that government spending rises by 6.6% on average for municipalities across the cutoff, and the statistical significance of this estimate implies that the FPM population cutoff indicator is feasible as an instrument for government expenditure.[15]

4.2 Employment

We estimate that municipalities just above FPM cutoffs have on average 131 more jobs than those just below the cutoff, for an increase of around 8% (Table 4).[16] Note that while the graphical

[15]The F-statistic for this regression is 12.32.

[16]Figure 6 presents similar evidence for the discontinuity of employment around the cutoff.

evidence for the discontinuity in employment appears noisy, much of our finding is driven by observations located extremely close to the threshold. Accordingly, the size of our estimates increases as we move closer and closer to the cutoff.[17] These large estimate hides substantial heterogeneity: specifically, this increase in jobs is concentrated in certain skill segments of the labor market. Employment increases are significantly higher for those without college degrees. Jobs involving college-educated workers increase almost negligibly, while jobs involving those without a secondary (high-school) diploma and those who only have a secondary diploma increase by around 110 and 14 respectively, accounting for nearly all of the increase in overall jobs.

Given the characteristics of the municipalities of the sample, it is perhaps not surprising that there is some increase in the amount of unskilled formal labor. As presented in Table 2, most of the labor force in these small municipalities is low-skilled. However, estimating the same regressions with a log transformation shows a large significant increase in percentage as well as in levels (Table 5). Low-skilled labor increased by roughly 15%, substantially more than did labor among those with secondary degrees or higher. Therefore, even conditional on the larger amount of unskilled labor in our sample, unskilled labor increased proportionally more than relatively more skilled labor. [18]

The "source" of these low-skilled workers is not quite clear, and unfortunately our data are not informative in this dimension. These municipalities have a significant number of less-educated workers who are unemployed and in the informal sector, and as the economy grows with government spending, it is possible that these workers migrate to the formal jobs (Ulyssea, 2014). Moreover, as we will describe shortly, the increase in government spending is concentrated in public investments, which are often associated with highly intensive in low-skilled labor construction projects (David and Dorn, 2013).

There also exists heterogeneity in the estimates by sector. Most of the job growth is concentrated in the private sector (Table 6). This indicates that rather than a crowd-out of private sector labor by fiscal policy, we observe a crowd-*in* occurring. Though puzzling, the nature of our quasi-experiment

[17]In our discontinuity graphs, we plot the residuals of the outcome variables, conditional on year and state fixed effects and selected covariates. As employment generally varies substantially across these dimensions, we remove these as determinants of employment and plot the residuals. The reason we do this is to reduce the apparent variance in the graphical estimates. A discontinuity graph with the original outcomes is available under request.

[18]Note that the observation counts within skill groups are different than our total sample. We omit municipality-years from these analyses that contain no formal workers in these categories, owing these to administrative errors.

and the characteristics of the Brazilian labor market can offer an explanation these findings. Mayors may understand that being above the population cutoff might be an one-period, or temporary, revenue windfall, as they are not guaranteed the same level of transfers in the following year. Hiring public workers is necessarily associated with a higher payroll for an undetermined number of years, as public workers cannot be laid off in Brazil (Braga et al., 2009). As a consequence, mayors might choose to spend the extra revenue associated with being above the cutoff with private contractors, which seems to be consistent with our findings when looking at the type of government spending associated with extra FPM revenues in subsection 4.5. If true, this notion might offer some evidence as to how governments make hiring decisions in the face of uncertainty about the receipt of future funds.

4.3 Average Monthly Wages

We find small and insignificant effects of being above FPM cutoffs on wages by virtually all education levels and sectors (Tables 7 and 8) We interpret this result as evidence of the existence of a large supply of unskilled workers that are either unemployed or in the informal sector, which imply a very elastic labor supply curve in the formal sector in Brazil. As a result, labor demand shocks associated with the increase in government spending must generate small changes in wages and large increases in employment. Consequently, labor markets in Brazil become more responsive to government spending than the US. Nonetheless, we cannot rule out other potential explanations, such as that the rigid wage setting in Brazil implies that adjustments may happen more slowly or that internal migration can might mitigate any permanent wage premiums across municipalities (Arbache, 2001).[19]

4.4 Two-Stage Least Squares Estimates

In order to obtain a causal estimate, we examine the effect of local government spending on labor market outcomes using two-stage least squares estimation. In the first stage, we use the indicator for a municipality being above the cutoff as an instrument for the natural log of government expenditure. In the second stage we regress this instrumented log spending on the number of jobs

[19] As we cannot follow workers in the public available version of RAIS, we cannot rule out that workers are migrating from other regions.

and log average wages in each municipality. Using this method, we find that a 1% increase in government spending is associated with the creation of about 19.69 jobs (Table 9). Given that the average general expenditure in a municipality is roughly $7.26 million in our analytical sample,we calculate an average cost-per-job of around $3,687. As expected with the reduced form average monthly wage results, we do not find any evidence of an effect of spending on monthly wages.

Based on this estimation, we can test whether costs-per-job estimates are statistically different from those in the US literature. Wilson (2012) estimates a cost of around $125,000 per job, Chodorow-Reich et al. (2012) find a cost per job created around $25,000, and Serrato and Wingender (2014) estimate to be around $35,000. Based our the instrument variable regression, we estimate with 95% confidence an upper bound of $15,364 for the costs-per-job created in our sample. We conclude that the cost-per-job in Brazil is significantly lower than what the previous literature indicates for the US.

A possible explanation for the higher jobs multiplier is that labor is simply overall cheaper in Brazil than it is in the United States. Indeed, the average monthly wage in our sample is about roughly $407, much lower than in the United States (average monthly wage of worker in the US was $3,080 in 2005).[20] However, while we find that American wages are 7.5 times higher than the wages of workers in our sample, we estimate a jobs multiplier at least 10 times higher in Brazil than in the US. We conclude that the large number of unemployed and informal workers in Brazil is part of the explanation for the high job multipliers in the country.

The result of higher unskilled labor as a function of locally-allocated transfers and spending indicates that unrestricted government spending can have an implicit (or unstated) redistributive effect even if no such explicit policy goal is specified. If those from higher parts of the wealth distribution pay the majority of FPM funds, and these funds then are used to hire workers from the lower end of the wealth distribution, the official government spending in some sense represents an unofficial transfer from one end of the distribution to the other. It is important to note that because these results are in the context of local government, it may not be the case that these results would "scale up" to the federal level; federal spending programs may be different in the kinds of spending they incur and may employ from different parts of the wage distribution. However, our result provides evidence that largely unrestricted transfers and spending at the local level translate

[20] This estimation is based on the $36,953 average annual wage of an American worker in 2005, which is available on the American Social Security Administration website.

to the hiring of more unskilled labor, resulting in presumed welfare gains for those hired and a potentially inequality-reducing redistributive effect.

4.5 The Distribution of Spending

The FINBRA data allows for the breakdown of public expenditure by type of spending. We divide general expenditure into 3 broad categories: personnel spending, investments, and other spending. Personnel spending corresponds to total spending on public servants: either active, inactive or pensioners. It includes salaries, benefits, allowances, retirement income and pensions. Investments are expenses necessary for the execution of construction projects and for the purchase of the facilities, equipment and permanent material. These expenses must be associated with an increase in municipal government capital. Finally, other spending includes any other expenses that the municipal government might face in a year, such as the purchase of non-durable goods.

The most substantial percentage increase associated with the FPM transfers is investment (Table 10). Municipalities just above the population cutoff spend 14% more on investment. In contrast, we estimate very modest increases in personnel spending. We estimate a 6% increase in personnel spending for municipalities above the cutoff. Finally, we also find significant effects on other spending (5%).[21]

We interpret the substantial increase in investment and the modest increase in personnel spending as mayors being forward-looking in the way they allocate government resources. On one hand, an increase in payroll might be unsustainable in the future as as a municipality might fall below the cutoff in the following year, and as we have noted before, it is difficult for mayors to lay off public servants or adjust wages downward. On the other hand, public investments are more likely to represent a kind of long-term source of revenue in that they may generate a better business environment, which might itself increase future tax revenues.

[21] We also find stronger effects in levels of spending (absolute terms) of being above the cutoff on investment than personnel spending. Precisely, we estimate that that municipalities just above the population cutoff spend about 334 thousand Reais in investment and 317 thousand Reais more on personnel spending than municipalities just below the cutoff.

5 Robustness Checks

5.1 Other Sources of Revenues

A potential concern of papers that explore “windfall”-type revenue shocks is that governments may find ways to adjust for the unexpected loss of revenue by increasing other sources of revenues. In order to address this issue, we investigate whether municipalities that are below the population cutoff are able to increase their own tax revenues or whether they receive other types of transfers from federal and state governments.

We explore whether there is any discontinuity around the cutoff on proprietary revenues and taxes and other transfers. Proprietary revenues and taxes are composed primarily of revenues from the Territorial Urban Property Tax (IPTU), and Services Tax (ISS), which are the two main municipal taxes. Other transfers correspond to shares of other state and federal taxes to which municipalities are entitled. None of these other transfers use an allocation mechanism based on the FPM coefficients.

We do not find evidence of a decrease in the sum of these other sources of revenues around the population cutoff (Table 11). Indeed, we find null effects of being above the population cutoff on both for both own taxes and transfers revenues. This result is not surprising given the very limited ability that municipalities have to increase their own revenues in Brazil since the enactment of the Fiscal Responsibility Law.

5.2 Bandwidth Selection

Another potential concern in any regression discontinuity design is whether the results are sensitive to bandwidth choice. In most of the analysis of this paper, we restrict the sample to municipalities that are within a 1,000 population bandwidth of the first three FPM cutoff. We examine the sensitivity of our main results to bandwidth choice, varying it from 250 to 1,500 and present our results in Table 12 .

Overall, we find that the employment effects vary little with bandwidth choice, with the highest estimate of an increase of about 270 workers with the tightest bandwidth selection of 250 people on either side of the cutoff and the lowest estimate of 93 workers with a bandwidth of 1,250. In all cases we find significant effects of FPM transfers on employment at at least the 10% level of significance. We also find that the lack of detectable effect of government spending on average monthly wages is not sensitive to the choice of the bandwidth, with coefficients varying from -0.008 to 0.019.

Finally, we use the method of Calonico et al. (2014) to select the optimal bandwidths for local quadratic estimation (Table 13). We find greater discontinuity effects on employment using Calonico et al. (2014) bandwidths than those estimated in Table 4. Nonetheless, following the suggested method, we do not include any covariates, year or state fixed effects in the estimations in 13, generating greater standard errors and therefore lower significance levels.

5.3 Placebo Cutoffs

We also undertake a "placebo" exercise in which we estimate the model at false FPM population cutoffs (Table 14). We consider placebo thresholds of 11,887; 15,283 and 20,377. These cutoffs correspond to the mid points of the first three brackets of FPM transfers (Table 1). In these estimations, we use the same bandwidth (1,000 population count) and specification from the ones used in the main analysis of the paper. The estimated discontinuity at the placebo cutoffs of 11,887; 15,283 and 20,377 on employment are indistinguishable from zero.

5.4 Municipality and State-Year Fixed Effects

As in any RD design, our identification is based on the assumption that pre-treatment observable and unobservable municipality characteristics are continuously distributed around the threshold. Neither covariates, year nor state fixed effects are needed for identification and we include them only to eliminate small sample biases and improve the precision of estimates.[22] In this subsection, we show that our results are also robust to the inclusion of municipality-fixed effects and state-year fixed effects. In the former, identification comes through municipalities crossing population thresholds during the period of analysis. The later specification is justified by the fact that FPM transfers received by each municipality varies by state-year.

In Table 15 we estimate that a 1% increase in government spending is associated with an increase of about 17.47 jobs in the in the state-year fixed estimation and 10.03 in the municipality

[22] Litschig and Morrison (2013) and Brollo et al. (2013) use state fixed effects in their specifications and Corbi et al. (2014) uses municipality fixed effects.

fixed-effects specification. In both cases, we cannot reject at the 95% level that these job multipliers are different from 19.68 job multiplier we estimate in Table 9. As in the rest of the paper, we do not find any effect of government spending on average wages in those specifications. These results reassure the validity of our RD identification assumption.

6 Conclusion

We estimate sizable effects of government spending on employment in Brazil. Using our preferred estimation procedure, we find that a 1% increase in government spending is associated with an increase of about 19.68 jobs in the formal sector. This effect is much larger than other employment multipliers estimated in developed countries. We also fail to find any effect of government spending on average wages in virtually all estimations. This result is consistent with a theory that higher levels of unemployment and informal sector in a developing country generate a very elastic labor supply of workers to the formal sector. As a result, labor demand shocks generated by government spending cause a higher increases in employment and little change in wages.

We also find that most of the job increase is concentrated among the lower end of the skill distribution and in the private sector. Additionally, certain types of spending increase, proportionally, more than others: namely, investment spending is increasing proportionally more than payroll spending. These results potentially suggest that local governments in Brazil act in a forward-looking manner in the allocation of government resources. Instead of hiring public sector workers, local governments seem to be increasingly spending their resources on public investments that might generate future economic growth. The high investment also explains the substantial increase in private sector jobs, as municipalities tend to rely on private contractors for this type of job.

As stated above, we find large employment effects among the lower-skilled segments of the workforce. To the extent that skill level is correlated with household income, our results indicate that local government spending may have a potential redistributive effect where one is not necessarily intended. Because the spending is largely unrestricted, our finding has potential implications for the allocation of both federal and local funds in the developing world. Our future research agenda involves the examination of long term effects of government spending on employment and income inequality.

Additionally, the effects of fiscal spending may not be limited to the localities in which the funds are actually disbursed. In future work, we plan to examine geographic mobility of workers across labor markets in response to government spending by examining how these FPM transfers affect the labor markets of neighboring (but not directly affected) municipalities. We believe that providing some bounds on how flexible the labor market in a country like Brazil will be of great interest to those interested in the unintended consequences of fiscal policy. Finally, it is important to note that our paper uses windfall changes in revenue to examine how local government spending affects employment. However, most fiscal policy is implemented at the national level where these effects could be mitigated by Ricardian equivalence, as consumers might anticipate an increase in future taxes or interest rates as a response to more expenditure.

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Tables

Table 1: FPM Coefficients

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

Note: FPM coefficient is the coefficient used in the determination of FPM transfers. Total FPM transfers depend also on the state where municipality is located as well as the federal government tax revenues in a given year.

Table 2: Descriptive Statistics

Variable	**Mean**	**Standard Deviation**
Population	12,932	2,770
Municipality Finance		
FPM Transfers (1000's of Reais)	4,945	1,362
General Revenues (1000's of Reais)	12,949	7,275
General Expenditures (1000's of Reais)	12,737	7,224
Share of Governement Expenditure by Type		
Personnel Spending	44.5%	
Investment	10.9%	
Other Spending	44.6%	
Employment		
Overall Number of Jobs	1,020	935
Share of Jobs by Sector		
Private	44.2%	
Public	42.5%	
Other	13.3%	
Education		
Less than HS	62.3%	
HS grad	29.5%	
Some college	2.4%	
College grad	5.8%	
Average Monthly Wages by Sector (in Reais)		
Overall	710	235
Private	674	271
Public	907	541
Other	573	215
Observations	4,842	

Note: Monetary variables are measured in year 2010 Reais. The average exchange rate in 2010 is 0.57 dollars per real. Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007.

Table 3: Government Finance Variables Discontinuity Estimates

	Dependent Variable		
	Ln(FPM Transfers)	Ln(General Receipts)	Ln (General Expenditure)
Above the Cutoff	0.142	0.076	0.066
	(0.026)***	(0.019)***	(0.019)***
Observations	4,842	4,842	4,842
R-squared	0.413	0.663	0.634

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 4: Employment Discontinuity Estimates, Overall and by Education

Dependent Variable: Number of jobs					
	Overall	Worker's Education			
		Less than HS	HS grad	Some college	College grad
Above the Cutoff	130.8	110.31	14.13	3.081	3.778
	(50.5)***	(40.52)***	(12.66)	(2.644)	(4.025)
Observations	4,842	4,841	4,841	4,432	4,696
R-squared	0.611	0.547	0.569	0.354	0.377

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Difference in the number of observations across columns is due to missing employment information for particular education levels in some municipalities. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 5: Log Employment Discontinuity Estimates by Education

Dependent Variable: Log(Number of jobs)				
	Worker's Education			
	Less than HS	HS grad	Some college	College grad
Above the Cutoff	0.154	-0.004	0.022	0.061
	(0.072)**	(0.065)	(0.089)	(0.087)
Observations	4,841	4,841	4,432	4,696
R-squared	0.646	0.576	0.480	0.538

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Difference in the number of observations across columns is due to missing employment information for particular education levels in some municipalities. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 6: Employment Discontinuity Estimates by Sector

Dependent Variable: Number of jobs			
	Sector		
	Private	Public	Other
Above the Cutoff	111.1	11.27	8.45
	(43.4)**	(14.27)	(13.62)
Observations	4,842	4,842	4,842
R-squared	0.557	0.258	0.416

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 7: Wage Discontinuity Estimates, Overall and by Education

Dependent Variable: Log Average Wage					
	Overall	Worker's Education			
		Less than HS	HS grad	Some college	College grad
Above the Cutoff	0.006	-0.005	0.047	0.055	0.033
	(0.020)	(0.019)	(0.024)*	(0.046)	(0.042)
Observations	4,842	4,841	4,841	4,416	4,690
R-squared	0.456	0.541	0.357	0.193	0.247

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Difference in the number of observations across columns is due to missing wage information for particular education levels in some municipalities. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 8: Wage Discontinuity Estimates by Sector

Dependent Variable: Log Average Wage			
	Sector		
	Private	Public	Other
Above the Cutoff	-0.019	-0.011	0.001
	(0.025)	(0.033)	(0.022)
Observations	4,789	4,832	4,529
R-squared	0.396	0.324	0.410

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Difference in the number of observations across columns is due to missing wage information for particular sectors in some municipalities. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 9: Two Stage Least Square Estimations

	Dependent Variable	
	Number of Jobs	Log Average Wage
Ln Gen. Expenditure	1,968.7	0.098
	(763.3)***	(0.294)
Observations	4,842	4,842
R-squared	0.551	0.479
First Stage F Stat	12.32	

Note: First Stage: Ln Gen Expenditure is instrumented by indicator whether municipality has population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 10: Type of Government Spending Estimations

	Dependent Variable		
	Ln(Personnel Spending)	Ln(Investment)	Ln(Other Spending)
Above the Cutoff	0.063	0.137	0.050
	(0.023)***	(0.060)**	(0.025)**
Observations	4,839	4,820	4,841
R-squared	0.654	0.276	0.385

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Difference in the number of observations across columns is due to missing spending information in some municipalities. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 11: Discontinuity at Other Sources of Revenues

	Dependent Variable	
	Ln(Proprietary Revenues and Taxes)	Ln(Other Transfers)
Above the Cutoff	0.037	-0.003
	(0.056)	(0.031)
Observations	4,840	4,842
R-squared	0.651	0.542

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Difference in the number of observations across columns is due to missing tax revenue information in some municipalities. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 12: Sensitivity Test to Bandwidth Selection

Dependent Variable	Number of jobs		Log Average Wage	
Bandwidth	Coefficent Above	Std. Error	Coefficent Above	Std. Error
250	270.0***	85.1	0.014	0.035
500	206.0***	63.7	-0.008	0.028
750	126.0**	55.4	-0.000	0.023
1000	130.8***	50.5	0.006	0.020
1250	93.1*	48.3	0.019	0.018
1500	101.5**	46.1	0.015	0.017

Note: Each coefficient represent the impact of being above the first three cutoffs on each dependent variable. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities above and below the first three FPM cutoffs in the years 2002-2007. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 13: Employment Discontinuity Estimates, Optimal Bandwidth from Calonico et al. (2014)

Dependent Variable: Number of jobs					
	Overall	Worker's Education			
		Less than HS	HS grad	Some college	College grad
Above the Cutoff	240.93	166.82	44.975	9.341	13.087
	(137.03)*	(98.69)*	(35.255)	(6.046)	(8.907)
Bandwidth	332.2	334.3	375.0	380.8	322.2
Effective Observations	1,675	1,684	1,886	1,740	1,589

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff. Sample is restricted to municipalities in the years 2002-2007. Difference in the number of observations across columns is due to missing employment information for particular education levels in some municipalities. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 14: Placebo Cutoffs

Depedent Variable: Number of Jobs			
	Placebo Cutoff		
	11,887	15,283	20,377
Above the Placebo Cutoff	-106.9	-67.6	-187.2
	(72.7)	(86.2)	(127.8)
Observations	1,808	1,384	988
R-squared	0.586	0.628	0.671

Note: Above the Placebo Cutoff indicates municipalities with population above the placebo cutoff. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the relevant cutoff in the years 2002-2007. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

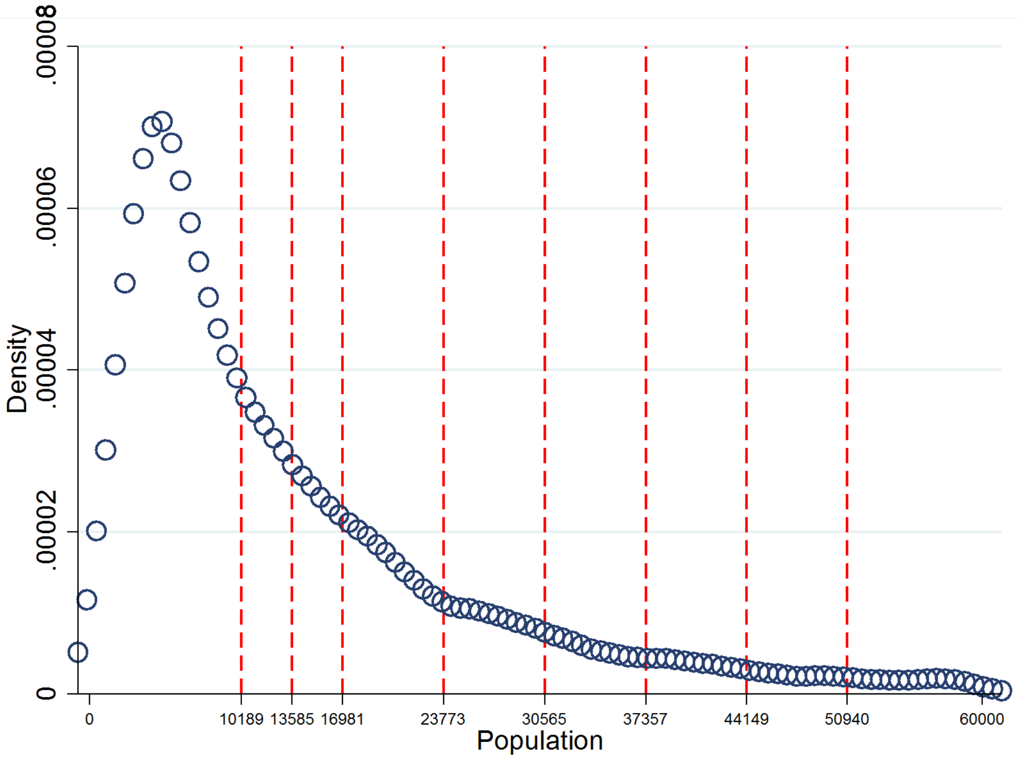
Table 15: Municipal and State-Year Fixed Effects

	Dependent Variable			
	Number of Jobs	Log Average Wage	Number of Jobs	Log Average Wage
Ln Gen. Expenditure	1,747.2	0.137	1,003.0	-0.135
	(737.7)**	(0.294)	(469.3)**	(0.298)
Observations	4,842	4,842	4,717	4,717
R-squared	0.594	0.501	0.136	0.251
First Stage F Stat	11.98		21.97	
Municipal Fixed Effects	No		Yes	
State & Year Fixed Effects	Yes		No	

Note: First Stage: Ln Gen Expenditure is instrumented by indicator whether municipality has population the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

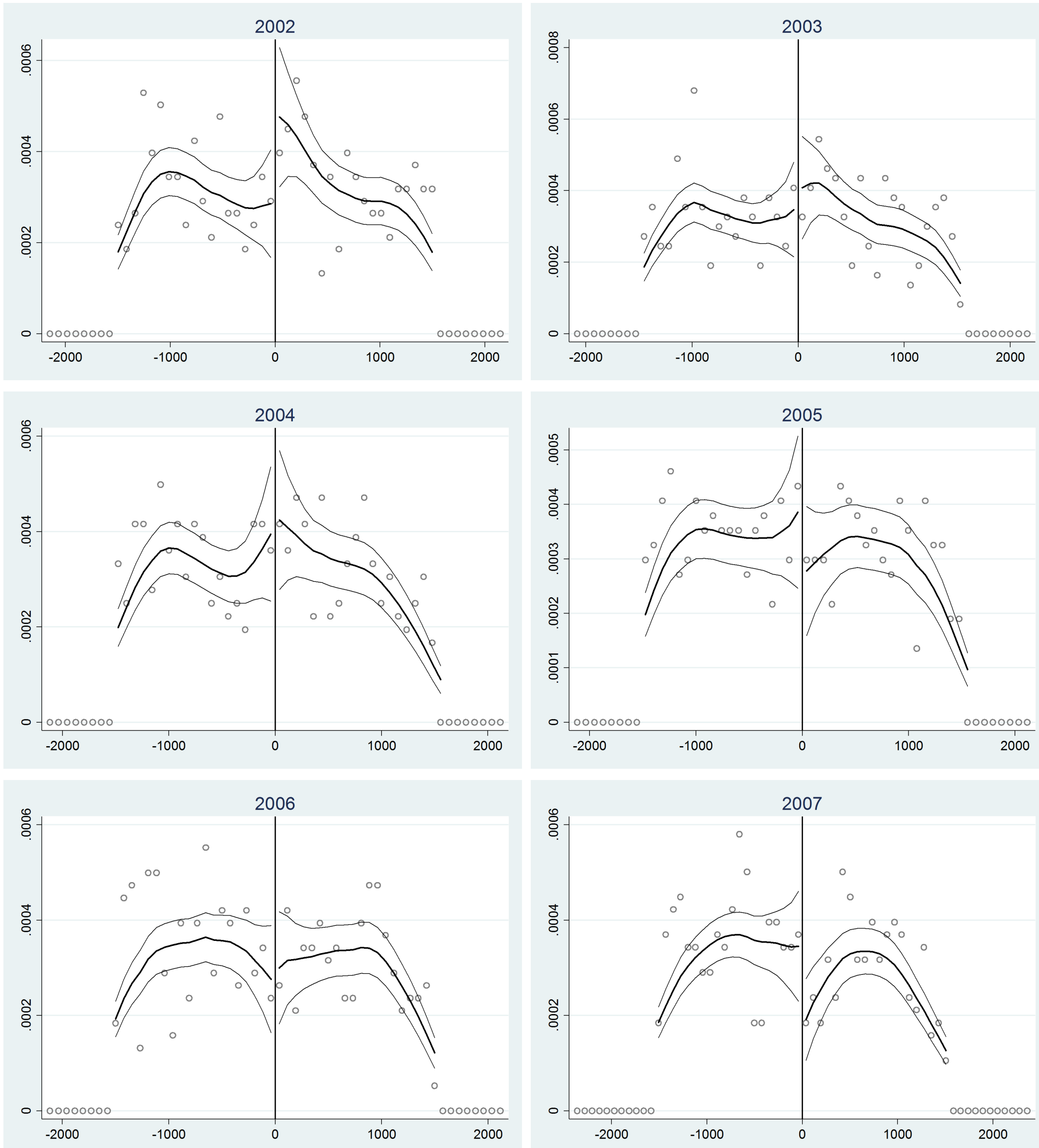
Figures

Figure 1: Population Distribution of Municipalities - 2004



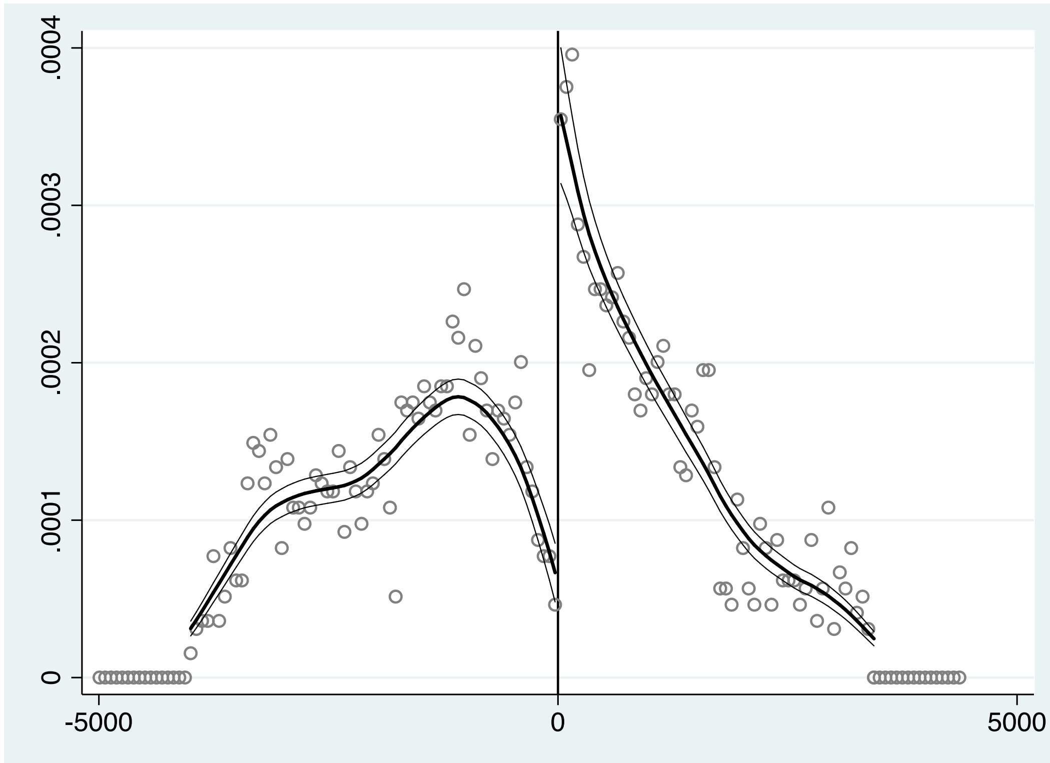
Note: Open circles represent the kernel density of the population distribution of municipalities in 2004, right truncated at 65,000 and using a 100 binwidth. The dotted red lines represent the eight FPM cutoffs.

Figure 2: McCrary Test, 2002-2007



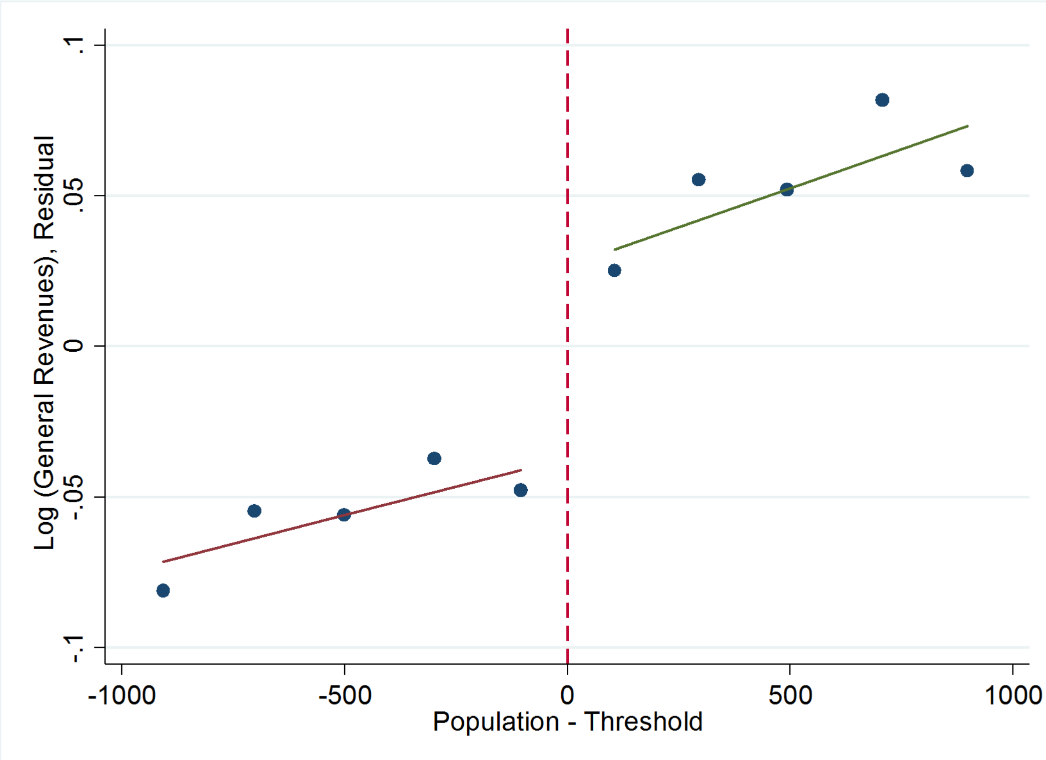
Note: Weighted kernel estimation of the log density of the distance to FPM cutoff performed separately on either side of the threshold. Optimal bandwidth and binsize as in McCrary (2008). Sample is restricted to municipalities with population with 1,500 inhabitants above and below the eight FPM cutoff. Population in 2002-2007 is estimated based on old Census, birth and death rates, projection of migration trends.

Figure 3: McCrary Test, 2008



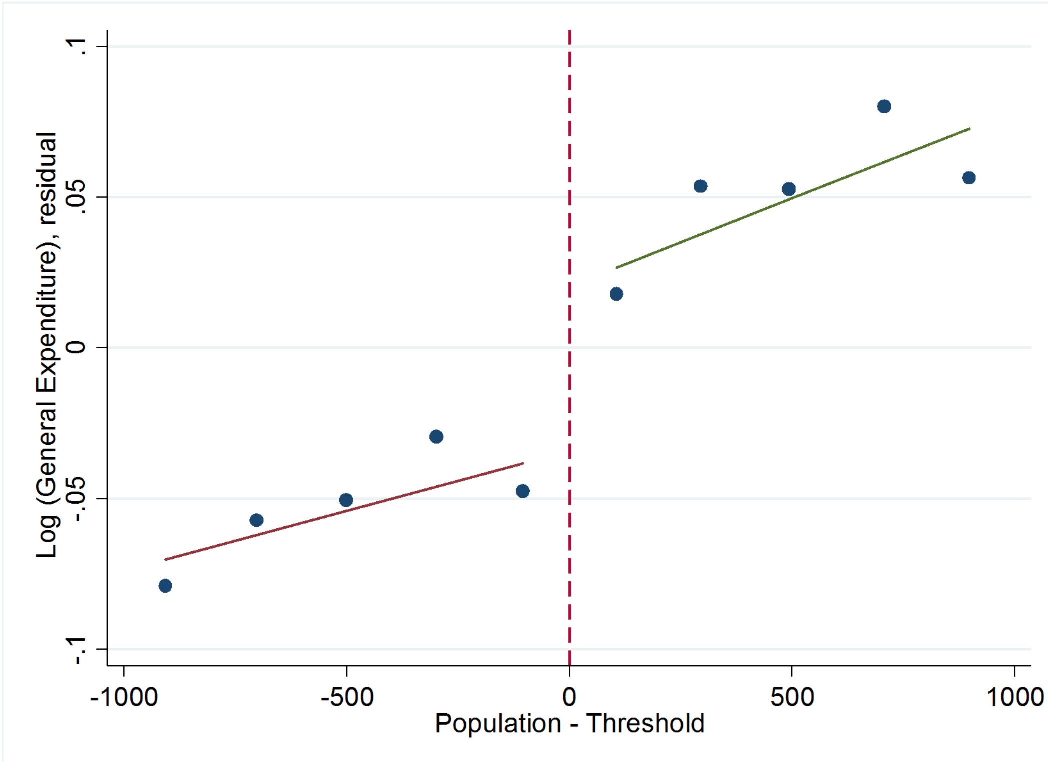
Note: Weighted kernel estimation of the log density of the distance to FPM cutoff performed separately on either side of the threshold. Optimal bandwidth and binsize as in McCrary (2008). Sample is restricted to municipalities with population with 1,500 inhabitants above and below the eight FPM cutoff. Population in 2008 was estimated based on the Recount of the population in 2007.

Figure 4: Ln(General Revenue) as a Function of Normalized Population



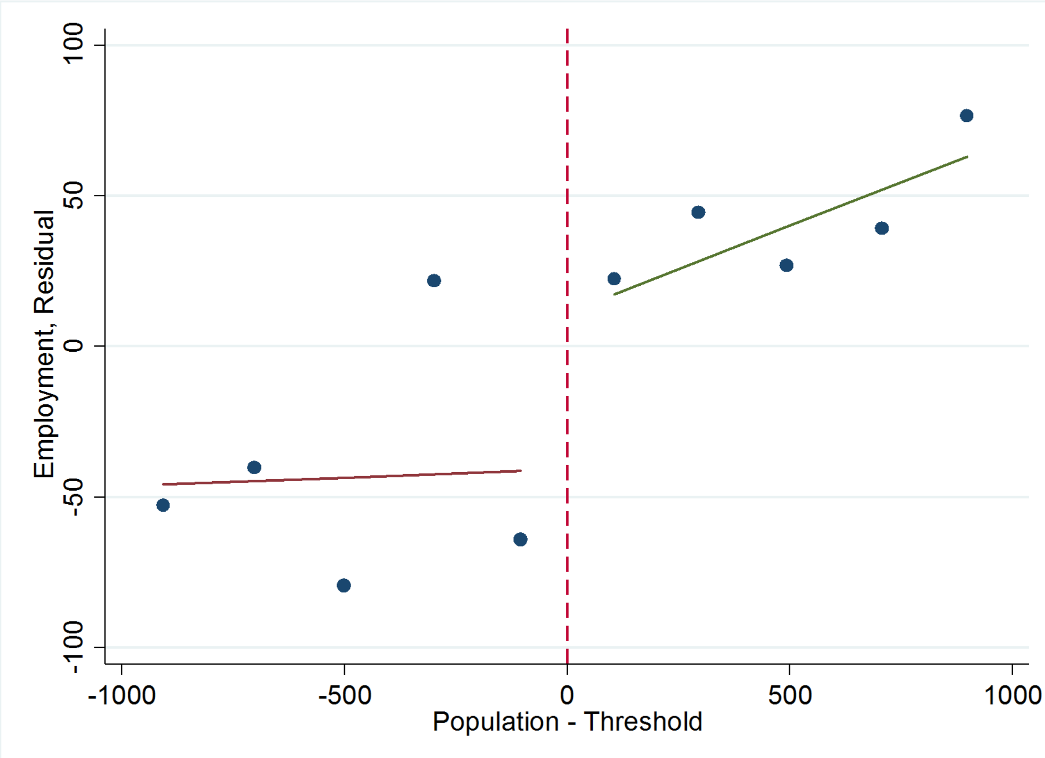
Note: Log (General Revenues), Residual is obtained from a regression of general revenues on year fixed effects, state fixed effects and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). We use residuals to make the local average graphs less nosier and consistent with our preferred specification. The discontinuity graph with the original outcomes is available under request. Open circles represent 200 population local averages and the lines are local linear fits below and above the FPM cutoff. Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007.

Figure 5: Ln(General Expenditure) as a Function of Normalized Population



Note: Log (General Expenditure), Residual is obtained from a regression of general expenditure on year fixed effects, state fixed effects and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). We use residuals to make the local average graphs less nosier and consistent with our preferred specification. The discontinuity graph with the original outcomes is available under request. Open circles represent 200 population local averages and the lines are local linear fits below and above the FPM cutoff. Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007.

Figure 6: Total Employment as a Function of Normalized Population



Note: Employment, Residual is obtained from a regression of total employment on year fixed effects, state fixed effects and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). We use residuals to make the local average graphs less nosier and consistent with our preferred specification. The discontinuity graph with the original outcomes is available under request. Open circles represent 200 population local averages and the lines are local linear fits below and above the FPM cutoff. Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007.

Appendix (for online publication)

All Eight Cutoffs

Throughout the paper we estimate the effect of government spending on labor market outcomes for municipalities around the first three FPM cutoffs. The first three cutoffs present the most significant discontinuity in terms of spending, as smaller municipalites are more dependent of FPM transfers (Litschig and Morrison 2013). Nonetheless, as a robustness check, in this section, we explore the discontinuity of on FPM transfers around all eight FPM cuttoffs. Using the same variable definitions from section 3, we first estimate the a reduced form model:

$$Y_{it} = \beta \sum_{j=1}^{8} 1[pop_{it} \geq c_j] \times seg_{itj} + \sum_{j=1}^{8} g_j(pop_{it} - c_j) \times seg_{itj} + \sum_{j=1}^{8} \alpha_j seg_{itj} + \delta_t + \mu_s + \theta X_i + \varepsilon_{it}$$

The results of these estimations as well as the estimation using the first three cutoffs, are reported on Table 1. Using the reduced form model, we estimate an slightly smaller effect of the eight FPM discontinuity on government spending than the first there cutoffs. This result likely reflects that bigger municipalities are less dependent on FPM transfers than bigger municipalities. Notably, Litschig and Morrison (2013) and Corbi et al. (2014) also find bigger discontinuity of government spending around the first FPM cutoffs, the first of which led Litschig and Morrison (2013) to restrict their analysis to municipalities around the first three cutoffs. In addition, there is a greater effect of the FPM discontinuity on the number of jobs when using the eight cutoffs rather than the three FPM cutoffs, although the effects are less precisely measured.

These reduced form estimations translate to the instrumental variable presented in Table 2. Using all the eight FPM cutoffs, we estimate that a 1% increase in government spending, roughly \$95,673 in the sample of municipalities around the first eight FPM cutoffs, is associated with the creation on 32.9 formal jobs. This estimate can be translated to a cost-per-formal-job-created of around \$2,908 per year, reflecting an even higher job multiplier effect of government spending in this expanded sample, although the effect is less precisely measured in the extended sample.

Table 1: All Cutoffs- Reduced Form

Dependent variable:	Ln (General Expenditure)	Number of jobs
Pooled cutoffs 1-3		
I[X > 0]	0.066	130.8
	(0.019)***	(50.5)***
Observations	4,842	4,842
R-squared	0.634	0.611
Pooled cutoffs 1-8		
I[X > 0]	0.058	191.4
	(0.018)***	(96.7)**
Observations	6,477	6,477
R-squared	0.795	0.666

Note: Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,500 inhabitants above and below each cutoffs. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2: All Cutoffs-Two Stage Least Square Estimations

Dependent Variable: Number of Jobs		
Sample	Pooled cutoffs 1-3	Pooled cutoffs 1-8
Ln Gen. Expenditure	1,968.70	3,297.4
	(763.3)***	(1,531.5)**
Observations	4,842	6,477
R-squared	0.551	0.644
First Stage F Stat	12.32	10.63

Note: First Stage: Ln Gen Expenditure is instrumented by indicator whether municipality has population above the first three FPM cutoffs in the first model and above the first eight FPM cutoffs in the second model. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the first model and above and below the first eight FPM cutoffs in the second model. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.