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# Regional Transfer Multipliers

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# 1 Introduction

The recent political crisis in the euro-area has brought the design of fiscal policy to the front stage of the public debate, generating renewed interest on the role of regional transfers in large currency unions (Farhi and Werning, 2016, Nakamura and Steinsson, 2014). This debate goes back to the early contributions of Robert Mundell (1961, 1973) and Peter Kennen (1971). Besides important theoretical issues, there is limited applied research quantifying the impact of regional transfers on local economies inside a currency union.

In this paper, we examine the impact of transfers-driven municipal expenditure on local labor markets in Brazil, where municipal receipts of federal transfers change abruptly at pre-determined population thresholds, allowing for a 'fuzzy' Regression Discontinuity Design (RDD). While municipalities belonging to the same population bracket receive the same amount of transfers in a given year and state from the federal government, municipalities with a few inhabitants above (below) a population threshold receive 20% more (less), on average. Hence, population fluctuations around the legislated cutoffs provide locally (close to the discontinuities) exogenous variation to identify the causal effects of externally-financed municipal government spending on economic activity. Our analysis exploits variation from more than $3,000$ municipalities over the period $1999-2014$ using high-quality microdata covering the bulk of private and municipal public sector employment contracts. The focus on numerous small geographical units over time allows controlling for time-invariant municipal factors, related to geography, history, local cultural and institutional features, and for country-wide and state trends, related to monetary policy, federal fiscal policy, and business cycles.

**Results Preview** Our analysis yields six main findings. First, changes in local government expenditure stemming from 'locally' exogenous shifts in federal transfers are associated with a significant boost in local employment. A USD $30,000$ increase in municipal spending is associated with one extra job in the municipal sector and three extra jobs in the private sector, implying a cost per job in the range of $8,000$ to $13,000$ USD per year. Second, the effect of federal transfers on wages is muted. Third, a simple production function mapping of the fuzzy-RD employment effects into income yields local income multipliers in the range of 1.3 to 2 (Chodorow-Reich, 2018). Based on a rate of job creation in the informal sector of around half of the one in the formal economy (arguably a conservative assumption), our estimated multiplier would be approximately 20% larger when accounting for the informal

economy. Fourth, most of the private sector employment response comes from services. Fifth, federal transfers are associated with increased firm entry (mainly in services) with similar employment effects across firm size. Sixth, a heterogeneity analysis reveals that the effects of public spending on employment tend to be larger for municipalities with lower income, small penetration of banks, smaller in size, and located in the (less-developed) North.

**Related Literature and Contribution.** Our work is related to the recent literature that examines the impact of government spending on local economic outcomes by exploiting cross-sectional variation (see Chodorow-Reich (2018) for a thorough overview of studies on geographic cross-sectional multipliers). Nakamura and Steinsson (2014) interact state-level military procurement and spending with US-level changes in military build-ups to identify the impact of fiscal shocks on state output. Shoag (2013) uses variation in the idiosyncratic component of U.S. states' portfolio of defined-benefit pension plan asset returns as an 'instrument' for local spending. Serrato and Wingender (2016) exploit federal spending reallocations across U.S. counties driven by unanticipated revisions to local population estimates to identify the effects of county-level government spending. These studies report local multipliers over the post-WWII period in the range of 1.4 to 2.6. Clemens and Miran (2012) find, however, subnational government spending multipliers below one, while Fishback and Kachanovskaya (2010) and Fishback and Cullen (2013) also find multipliers close to 1 for the post-Great Depression spending across U.S. states and for WWII military purchases across U.S. counties, respectively.[1] Becker, Egger and von Ehrlich (2010, 2013) examine the medium-run growth effects of EU structural fund grants, documenting positive but quite heterogeneous effects. We share with these recent studies the geographic cross-sectional approach and the effort to push on causation via exploiting some form of "quasi-random" variation.

Our first contribution is to provide evidence on the impact of local fiscal policy in a large emerging market against the backdrop of an empirical literature dominated by estimates for the United States and some other advanced economies, like Italy and Japan (notable exceptions are the cross-country analyses of Kraay, 2012, 2014).[2] Moreover, we examine the

[1] As for recent government interventions, Feyrer and Sacerdote (2012) use state variation in the seniority of the U.S. Congressmen as an 'instrument' for local government expenditure. Chodorow-Reich, Feiveson, Liscow and Woolston (2012) exploit pre-crisis variation on Medicare/Medicaid allocations to identify the effects of the 2009 American Recovery and Reinvestment Act (ARRA) on employment.

[2] In his thorough overview of the recent literature, Chodorow-Reich (2018) lists 13 (out of a total of 14) papers on industrial countries. He also comments on 7 empirical works focusing on various aspects of the 2009 American Recovery and Reinvestment Act.

impact of direct transfers from the federal government to localities rather than swings on local spending from an exogenous shock, like military buildup or higher stock returns. As such revenue-sharing mechanisms between national and local governments are present in both developed and developing countries, our analysis offers some insights on their stabilization effects.[3] A particularly important revenue-sharing mechanism between a central authority and regions that follows a discontinuous formula is the European Union Structural Funds (Becker *et al.* 2010), though the EU budget is small and its stabilization effects have only been recently explored (Coelho, 2018). The richness of the quasi-experimental variation and administrative data we exploit and the vast differences of Brazilian localities also allows us to move beyond average effects and explore heterogeneity.

Second, building on advances in labor economics (Angrist and Lavy, 2001; van der Klaauw, 2002; Hahn, Todd and Van der Klaauw, 2001), we bring into research in applied macroeconomics a 'fuzzy' regression discontinuity approach to identify the effects of local fiscal policy.[4] In this regard, our work connects with empirical works in political economy that examine the effect of federal transfers in Brazil on various political outcomes applying RD methods (Ferraz and Finan, 2010; Brollo, Nannicini, Perotti and Tabellini, 2013; Litschig and Morrison, 2013, Gadenne, 2017).

Third, we nest our RD estimates in a canonical currency union model to approximate the impact of municipal spending as if it were funded via local taxes rather than external transfers (federal in our application). This is important both because it allows quantifying the impact of regional transfers in currency unions against a reasonable counterfactual and because it connects the paper's results to the broader literature on the effects of government spending on aggregate economic activity (Farhi and Werning, 2016; Ramey, 2016).

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Acconcia, Corsetti and Simonelli (2014) exploit cuts in public spending triggered by the dismissal of Italian province governments suspected of mafia infiltration. Porcelli and Trezzi (2014) use variation on public reconstruction activity across Italian municipalities after an earthquake. Bruckner and Tuladhar (2014) exploit geographical variation within Japanese prefectures. In independently developed work, Braga, Guillen and Thompson (2017) look at the effects of local government spending in Brazil on employment across different levels of workers' educational attainment.

[3]Industrial country examples include Germany (Baskaran, 2017), Norway (Sørensen, 2017), Belgium (Boadway, 2006), the Czech Republic (Bergvall, 2006), and the United States (Serrato, et al. 2017). Examples of transfer schemes in developing countries are Turkey (Bergvall, 2006), Iraq (Aresti, 2016) and Nigeria (Qiao and Shah, 2008).

[4]See Fuchs-Schundeln and Hassan (2016) for an overview of works exploiting natural experiments in business cycle and growth research and Nakamura and Steinsson (2018) for an in-depth analysis of identification in business cycle macroeconomics.

**Structure** The paper is organized as follows. In the next section, we present the institutional framework of the allocation of federal transfers to Brazilian municipalities and describe the data. In Section 3, we present the fuzzy-RD framework and discuss identification. In Section 4, we examine the impact of federal transfers on employment and wages and provide calculations for the cost per job and the associated income local multipliers. In Sections 5, we report results regarding sectoral heterogeneity, firm size and firm entry. In Section 6, we explore heterogeneity across municipal characteristics. Section 7 summarizes. The Online Appendix provides details on the data, summary statistics, examples and various sensitivity checks. It also reports counterfactual simulations under alternative funding of local public spending based on the New-Keynesian currency-union model of Farhi and Werning (2016).

# 2 Institutional Framework and Data

## 2.1 The FPM Transfers Scheme

Brazil is organized at three levels of government: the federal union, 26 states and 1 federal district, and $5,565$ municipalities. The executive and legislative powers are organized independently at all three levels, while the judiciary is organized at the federal and state level. Municipal governments are managed by an elected mayor (Prefeito) and an elected council (Camara dos Vereadores), which are in charge of a significant portion of public goods provision, related to education, health, and small-scale infrastructure. Municipalities have limited ability to raise taxes, which on average correspond to 6% of total revenues in our sample of municipalities with less than $50,000$ inhabitants. Municipalities depend on transfers from states and the federal government. A major role is played by an automatic federal fiscal transfer scheme - the Fundo de Participação dos Municipios (FPM). FPM is the largest program of transfers to municipalities accounting for almost 80% of all types of federal transfers and 31% of total municipal revenues. FPM transferred $R\$29.5$ billion Brazilian Reais ($US\$14.8$ billion in current prices) from the national government to municipalities in 2006, the middle year of our sample.[5]

The FPM was introduced in 1965 as a constitutional amendment by the military government to distribute resources in an orderly and transparent fashion (and weaken local political elites). The allocation mechanism was shaped by subsequent legislation in 1981 (decree 1881)

[5]In comparison, Bolsa Familia, the largest conditional cash transfer program in the world targeting low-income households, distributed R$8.2 billion in 2006 prices ($US\$4.1$ billion in 2016 prices).

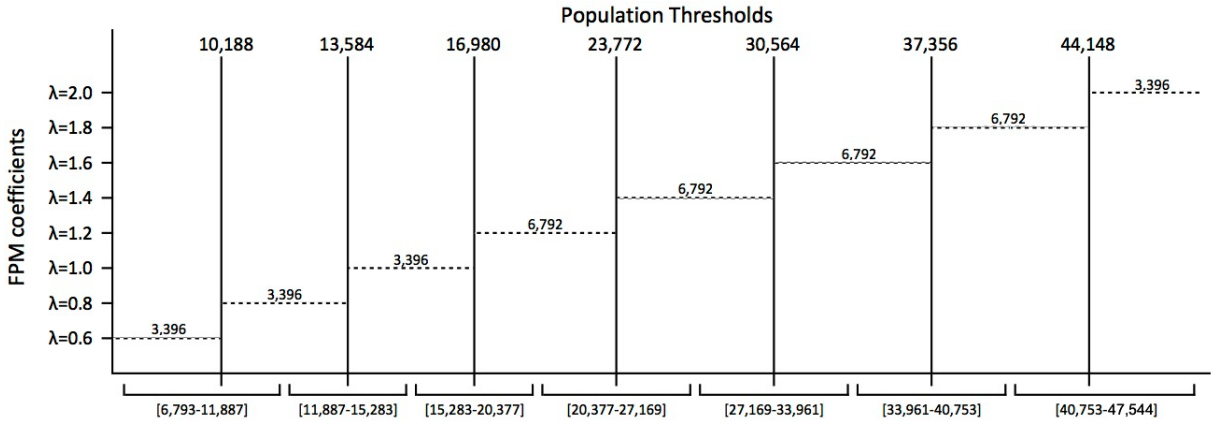



Figure 1: FPM Coefficients and Population Brackets

and was rectified by the 1988 Federal Constitution (Art. 159 Ib). There have been no changes in the allocation mechanism since then.

FPM allocates funds to municipalities yearly following a predetermined mechanism that relies on the total pool of FPM funds, municipality's state and a coefficient that depends on pre-specified population brackets. First, the total FPM proceeds are set every year. The FPM pool amounts to 22.5% of total revenues raised through the federal income tax and the federal industrial products tax. Second, there is a fixed share allocated to each state.[6] Third, a coefficient is assigned to each municipality depending on pre-specified population brackets. Let $FPM_i^k$ be the federal transfers received by municipality $i$ in state $k$ in a given year. The allocation mechanism formula is then:

$$FPM_i^k = FPM^k \frac{\lambda^i}{\sum_{i \epsilon k} \lambda^i},$$

where $FPM^k$ is the amount of (fixed) resources allocated to state $k$. $\lambda_i$ is the FPM coefficient of municipality $i$ based on its population. The fraction $\frac{\lambda^i}{\sum_{i \epsilon k} \lambda^i}$ is the share of state FPM transfers ($FPM^k$) allocated to municipality $i$ in state $k$ in a given year. Figure 1 plots FPM coefficients across the various population brackets. The brackets width is $3,396$ inhabitants for the three first cutoffs ($10,188$, $13,584$, and $16,980$); it doubles to $6,792$ people for cities larger than $16,981$ residents.

There are two interesting features of the FPM allocation mechanism. First, municipalities in the same bracket (in a given year and state) should get the exact same amount, indepen-

[6]The state shares of FPM transfers (reported in Appendix Table 1) are based on population/output per capita in 1991 and have not been altered ever since. The FPM formula applies to all municipalities with population less than approximately $150,000$ inhabitants that are not state capitals.

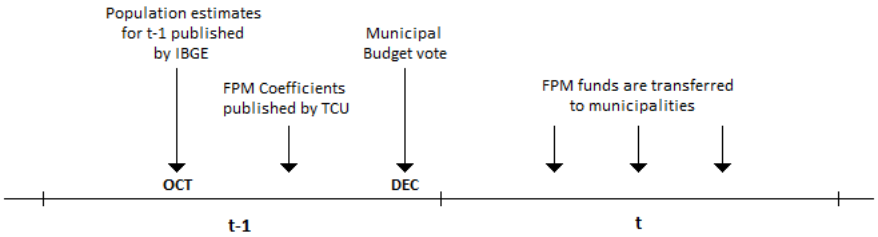



Figure 2: FPM Allocation Timelines

dently of their exact population. Second - and most importantly for our identification - federal transfers change discontinuously at the cutoffs. For example, the population of *Anita Garibaldi*, a municipality in the southern state of *Santa Catarina*, fluctuated between $9,991$ and $10,193$ during $2002-2007$. The population increased by only 13 inhabitants between 2002 and 2003 (from $10,180$ to $10,193$). As population crossed the first threshold ($10,188$), the FPM coefficient increased from 0.6 to 0.8 and so did transfers from $R\$1,204,762$ in 2002 to $R\$1,324,306$ in 2003. The population in 2004 fell by 38 inhabitants to $10,155$. Since *Anita Garibaldi* crossed back the first cutoff, FPM transfers dropped to $R\$1,098,906$. *Nova Trento*, another municipality in the same state also experienced an increase in population from $9,943$ to $10,006$. As it did not cross the threshold, FPM transfers fell from $R\$1,204,762$ to $R\$1,111,936$, as in 2003 there was a brief recession that lowered the total FPM funds.[7]

The FPM coefficients are based on yearly population estimates produced by the federal statistical agency, IBGE - Instituto Brasileiro de Geografia e Estatistica (Brazilian Institute of Geography and Statistics) - and supervised by a federal court. IBGE calculates municipal population for non-census years taking into consideration past censuses, regional birth and death rates, migration trends and other features. Figure 2 describes the time-line of the allocation. Population estimates are announced by October $31^{st}$. On this basis, the Federal Budget Court publishes the FPM coefficients for all municipalities. Then local authorities form the budget. Municipal councils approve the budget by the end of the year and FPM funds are then transferred during the following year.

[7]In the Appendix we exemplify the non-linear allocation mechanism of federal transfers discussing four additional examples. All monetary values throughout the paper are in Brazilian Reais (BRL) in constant 1998 prices. At the time of writing, this is equivalent to 3.2 BRL or $US\$1$ in current prices.

## 2.2 Grouping of Municipalities around the Discontinuities

As the number of municipalities falls with population and because reliance on federal transfers is smaller for larger cities, we follow Brollo, Nannicini, Perotti and Tabellini (2013) and focus on cities around the thresholds $1-7$, thereby examining the effect of federal transfers on the local economy for municipalities with a population between $6,793$ and $47,544$. This results in an unbalanced panel of $43,466$ observations, covering $3,279$ municipalities over $1999-2014$. Our sample covers 60% of Brazilian municipalities, accounting for 28% of Brazilian population, which was close to 175 (202) million in 2000 (2014). Each municipality-year observation is assigned to the nearest population cutoff. We construct seven population intervals centered on each discontinuity (Figure 1). The intervals are [6,793-11,887], [11,887-15,283], [15,283-20,377], [20,377-27,169], [27,169-33,961], [33,961-40,753] and [40,753-47,544].

Table 1 illustrates the richness of the experiment. Panel *A* shows that $1,410$ of the $3,279$ municipalities (43%) did not change population bracket in any given year. $1,087$ municipalities experience only positive jumps (33%), 93 cities experienced movements only to a lower population bracket (3%) and 689 municipalities (21%) experienced at least one positive and one negative jump. As our RD analysis focuses on the neighborhood around the seven cutoffs, Panel *B* gives tabulations restricting the sample to the 4% neighbourhood of the FPM cutoffs. Around one-third of the municipalities in the 'local' sample fluctuates around the cutoffs without crossing them (601 of $1,895$), while two-thirds move to a higher or lower FPM population interval or both.[8]

## 2.3 Data and Summary Statistics

We retrieve municipal public finances from the FINBRA database and FPM transfers from the National Treasury. Population estimates are provided by the IBGE. For local labor market outcomes (income, wages, and employment) we use the *Relação Anual de Informações Sociais* (RAIS; Ministry of Labor Administrative Dataset), over $1999-2014$. This is a high-quality administrative dataset assembled yearly by the Brazilian Ministry of Labor. Effectively, it is a census of the Brazilian formal labor market, containing detailed information from 2.2 million registered firms on 26.2 million contractual workers of a universe of 27.6 million according to the 2000 Brazilian census (De Negri *et al.*, 2001; Saboia and Tolipan, 1985; Amorim *et*

[8]Appendix Table 2 reports the number of observations (municipality-years) around each threshold. Appendix Table 3 gives the number of observations in the "local" sample by cutoff, while Appendix Tables $4-6$ give further descriptive patterns.

*al*, 2006).[9] We aggregate the micro-data at the municipal level and construct total earnings, mean wages, and employment at the private and local government sector. We also distinguish by sector, job tenure, and firm size. Providing accurate information in RAIS is required for workers to receive payments from government benefit programs and firms face fines for failing to report. Given its wide coverage and high-quality, a plethora of recent works use RAIS data to study a plethora of questions.[10]

In Table 2, we report summary statistics for population and municipal public finances. For income per capita only, we present average values (retrieved from the Census) for the year 2000: these increase with population and amount to an overall average around 1,900 Brazilian Reais (constant 1998 prices).[11] For the same year, income per capita at the national level was BRL 3,600.[12] FPM transfers is the most important source of funds for the municipalities in our sample, accounting for 31% of total revenues. Other important sources are state-level transfers and federal transfers (net of FPM), which account for 23% and 14%, respectively. Local taxes revenues account for 6%.[13] Turning to spending, the main categories are Local Administration (16%), Education (33%), Health (22%) and Housing & Urban Infrastructure (9%). There are institutional constraints preventing municipalities for borrowing and overspending, so local governments run balanced budgets; the median (average) surplus is only 0.1% (0.2%) of local income.

Appendix Table 6 - Panel *A* records total earnings in the municipal sector and the private sector, distinguishing between agriculture, manufacturing, and services. Earnings in agriculture account for 12% of total private sector earnings. Manufacturing and services account

[9]Formal sector accounts for 55-60% of salaried labor force in this time period (Meghir et al., 2015). We revisit this issue in Section 4.4 where we assess the extent to which our estimates might change if we were to account for the informal economy.

[10]RAIS covers nearly all formally employed workers with a signed work-card, providing access to benefits and offering legal labor protection rights. It omits interns, in-house workers and other minor employment categories. Self-employed and independent professionals recruiting employees are also included (Dix-Carneiro and Kovak, 2015). These data have been used by Dix-Carneiro (2014), Helpman, Itskhoki, Muendler and Redding (2017), Krishna *et al.* (2014), Lopes de Melo (2013), and Menezes-Filho and Muendler (2011).

[11]While municipal GDP is available at yearly frequency from IBGE (see Corbi, Papaioannou and Surico, 2014), these data are not directly measured but estimated using historical surveys and censi. This is unattractive for our purpose for two main reasons. First, the construction of the municipal GDP estimates assume that the share of each industry in the local economy remains constant across years. This is particularly worrisome in a set up that exploits within-municipality variation. Second, these historical surveys and censi are also used to estimate municipal population, thereby introducing a mechanical link between the running variable (local population) and the potential outcome (local GDP).

[12]This disparity reflects bigger cities' higher income. For example, income per capita Sao Paulo and Rio de Janeiro, which account for almost 20% of the country's population, was around BRL 18,900 in 2013.

[13]Other sources include mining and oil royalties, capital income, and many other smaller sources.

each for 44%. Appendix Table 6 - Panel $B$ and $C$ report the corresponding statistics for employment and average wages.

# 3 Identification

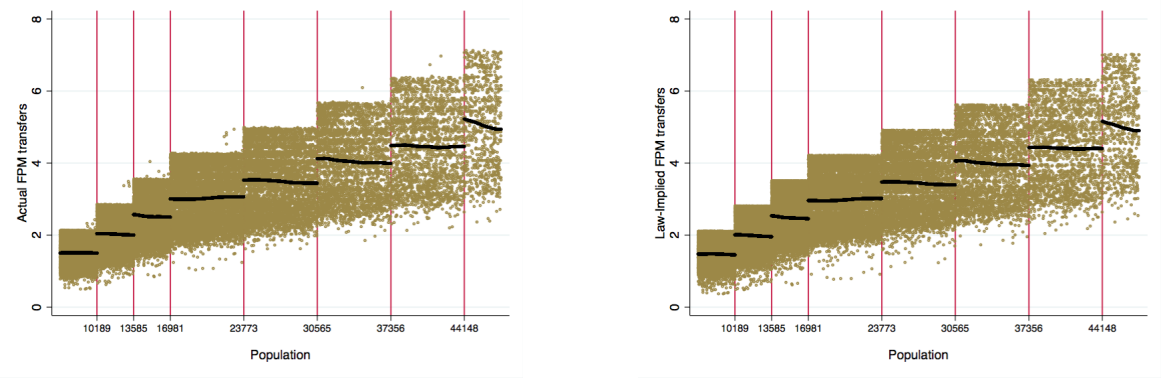
In this section, first describe the fuzzy regression discontinuity design that allows us to isolate the effect of spending driven by regional transfers on local labor markets. Then we discuss and present supportive evidence of the identifying assumptions.

## 3.1 Empirical Framework: Fuzzy RD Design

### 3.1.1 Source of Exogenous Variation

The allocation of FPM transfers to municipal governments is a non-linear function of population. While the level and changes in population are likely to depend on local economic conditions and other hard-to-observe factors, federal transfers change abruptly at several predetermined population thresholds. Hence, population movements around the cutoffs can be used as a source of exogenous variation to estimate the causal effects of regional transfers on labor market outcomes in the neighborhood of the thresholds (Angrist and Pischke, 2008). Figure 3a plots actual FPM transfers against population. The solid (red) vertical lines represent the FPM cutoffs (Figure 1). Small dots denote municipality-year observations. Thick (black) lines are running-means over population bins of 200 inhabitants. There is variability on transfers driven mostly by changes in the total FPM funds that fluctuate yearly at the federal level, as well as non-negligible differences across states. At the same time and most importantly for identification, there are visible jumps on transfers, when population crosses the FPM cutoffs.

Federal transfers are not shaped exclusively by the FPM allocation mechanism. This mis-assignment of funds has many causes, from simple misreporting to the fact that in the 1990s some municipalities split into two, but (temporarily) kept their former FPM coefficient. Figure 3b plots law-implied FPM transfers against population. Law-implied transfers are the exact amount each municipality would have received if the allocation mechanism was perfectly enforced in a given year. There is variability, as the total pot of the FPM program changes every year, but there are evident jumps of transfers at the FPM cutoffs.




(a) Actual Transfers

(b) Law-Implied Transfers

Figure 3: Actual and Law-Implied FPM Transfers around the cutoffs

### 3.1.2 Empirical Specifications

If FPM transfers are the only relevant factor that changes discontinuously at the cutoffs, we can estimate the impact of locally exogenous movements of municipalities across population thresholds on labor market outcomes running variants of the following specifications in the neighborhood ($h$) of the seven cutoffs ($c$):

$$FS \quad : \quad T_{i,t} = f(P^{c}_{i,t-1}) + \gamma_{FS}\tilde{T}_{i,t} + \delta_i + \delta_{ct} + \delta_{st} + \varepsilon_{i,t} \tag{1}$$

$$RF \quad : \quad Y_{i,t} = f(P^{c}_{i,t-1}) + \gamma_{RF}\tilde{T}_{i,t} + \delta_i + \delta_{ct} + \delta_{st} + \varepsilon_{i,t} \tag{2}$$

$$\forall P_{i,t-1} \quad \in \quad [c(1-h), c(1+h)]; \; h\{4\%, 3\%, 2\%)$$

The "first-stage" (FS) specification associates actual FPM transfers ($T_{i,t}$) to law-implied FPM transfers ($\tilde{T}_{i,t}$). Under perfect assignment, the coefficient on law-implied transfers ($\gamma_{FS}$) should be one and the in-sample fit perfect ($R^2 = 1$). The "reduced-form" (RF) specification links labor market outcomes ($Y_{i,t}$) - total earnings, employment, and average wages - to law-implied transfers ($\tilde{T}_{i,t}$).

$\delta_{st}$ are state-year dummies that capture aggregate developments (national and state level) such as federal tax proceeds, common monetary policy, and regional business cycles. They account for upward trends in wages and earnings and federal-level swings in the FPM pool of funds. The inclusion of the state-year constants is necessary, as FPM is also a function of time-invariant state shares and the time-varying ($\lambda$) coefficient of all municipalities in the state. Municipal fixed-effects, $\delta_i$, account for time-invariant factors shaping municipal fiscal

policy and economic conditions, related to geography, ecology, culture, local institutional quality, corruption, etc. $\delta_{ct}$ are cutoff-year constants, accounting for different trends across municipalities of different size.

$f(P^c_{i,t-1})$ is an RD-polynomial defined on normalized population (the 'running' variable in the RD jargon) that accounts for how far/close municipalities are from the closest FPM cutoff ($^c$) in the previous year ($t-1$). Following Angrist and Lavy (1999), Hahn, Todd and Van der Klaauw (2001), van der Klaauw (2002), and subsequent works in a similar context to ours (e.g., Brollo, Nannicini, Tabellini, and Perotti (2013)), we combine the estimation of the "first-stage" and the "reduced-form" specifications in an Instrumental Variable (IV) set-up, which isolates the effects on local labor market conditions of locally exogenous changes in federal transfers, stemming from the enforceability of the law, close to the FPM cutoffs. See also Angrist *et al.* (2014), Hinnerich and Pettersson-Lidbom (2012, 2014), and Pettersson-Lidbom (2012). The "fuzzy-RD" model reads:

$$IV: Y_{i,t} = f(P^c_{i,t-1}) + \gamma_{IV}\hat{T}_{i,t} + \delta_i + \delta_{ct} + \delta_{st} + \varepsilon_{i,t} \quad (3)$$

$$\forall P_{i,t-1} \in [c(1-h), c(1+h)];\ h\{4\%, 3\%, 2\%) \quad (4)$$

$\hat{T}_{i,t}$ denotes the component or federal transfers implied by FPM's non-linear allocation mechanism in each year.

We estimate two variants of this specification, which restrict estimation in the neighborhood of the seven cutoffs using two bandwidths ($h = 4\%$ and $2\%$).[14] As a starting point, we estimate simple OLS (reduced-form) and IV (fuzzy-RD) models without including any RD polynomials. This approach is transparent, simple and straightforward (Angrist and Lavy, 1999). However, it may yield imprecise estimates, as the bandwidth narrows, and not account well for differences in population when the bandwidth is wide. Thus, we also augment the local regressions with a rectangular kernel, i.e., cutoff-specific linear RD polynomials on normalized population, allowing for different slopes of the "running variable" for municipalities below and above the discontinuities.[15]

[14]The use of relative size neighborhood as opposed to absolute is due to the fact that the number of municipalities decrease in population size. In order not to lose many observations as we narrow the sample, we allow neighbourhoods to grow with population, as in Litschig and Morrison (2012). For example, consider the first and fourth cutoff (10,188 and 23,772). A 2%-neighbourhood include 1,141 and 801 observations, respectively. If we were to use an absolute neighbourhoods of 200 inhabitants, we would have 1,139 and 360 observations.

[15]Imbens and Lemieux (2008) write "*from a practical point of view, one may just focus on the simple*

The simplicity of the FPM mechanism and the fact that transfers within a state-year depend only on population render this setup ideal for our purpose (Eggers, Freier, Grembi and Nannicini, 2016). Another attractive feature of the FPM is the presence of many discontinuities. Thus, our results are not subject to the usual critique of RDD that the local estimates may not apply far from the discontinuity. Unlike earlier contributions exploiting the allocation of federal resources across municipalities in Brazil to study other outcomes (e.g., Brollo et al al., 2013), our RDD design is particularly strong, as by exploiting within-municipality variation, we account for unobserved features, something key as in a large and heterogeneous country, municipalities differ across many dimensions.

For completeness and robustness, we also estimate the specifications (1)-(3) in first differences. By doing so, the municipal fixed effects drop out and the specifications have a growth interpretation. We continue to account for state-year and cutoff-year fixed-effects. In these difference specifications, we also restrict estimation in the neighborhood of the seven cutoffs and we include linear RD polynomials on normalized populations on both periods, so that we correctly account for population growth. The specifications in differences also account for inertia in employment, government spending, and transfers.

Inference is based on heteroskedasticity-robust standard errors clustered at the micro-region level, which the IBGE defines as "*groups of economically integrated municipalities sharing borders and structure of production*".[16] This approach accounts for residual autocorrelation and spatial spillovers across nearby municipalities with economic links. This adjustment typically yields more conservative estimates as compared to simply clustering at the municipality or the state level.

## 3.2 Identifying Assumptions

Our RD design relies on four identifying assumptions.

*rectangular kernel, but verify the robustness of the results to different choices of bandwidth*". Lee and Lemieux (2010) argue that it is "*more transparent to just estimate standard linear regressions (rectangular kernel) with a variety of bandwidths, instead of trying out different kernels corresponding to particular weighted regressions that are more difficult to interpret*". For completeness, we also report specifications using all observations (both far and close to discontinuities) and conditioning on high-order RD polynomials (as Brollo, Nannicini, Tabellini and Perotti, 2013), obtaining similar results.

[16] See IBGE (1990, page 10). Our sample comprises 547 micro-regions with an average of 21 micro-regions per state and 5 municipalities per micro-region.

### 3.2.1 Federal Transfers at the Discontinuities

A *sine qua non* requirement is that FPM transfers change when municipalities cross FPM population thresholds. While "fuzzy"-RD does not require that the law is perfectly enforced, there has to be some enforcement. This is akin to the 'strong first-stage-fit' assumption in classical two-stage least squares.

In Table 3, we assess the link between actual and law-implied transfers. All specifications include state and year dummies to account for the fixed state shares and time variation on the size of the FPM program that changes as the Brazilian economy and federal proceeds grow. Odd (even) columns record local regression estimates in the 4% (2%) neighbourhood of FPM cutoffs. Column (1)-(4) report OLS specifications without municipality fixed-effects, so as to examine the enforceability of the FPM, as specified by the law.[17] The level specifications in row (1) yield a highly significant estimate of 1; the $R^2$ is around $0.85 - 0.9$, suggesting that enforcement is strong, but imperfect. Adding municipality constants (in columns (5)-(8)) does not change the results, as the coefficient continues to be one. Columns (9)-(12) report first-difference specifications. Due to the efficiency loss, the estimates slightly fall, though the coefficients are statistically indistinguishable from one.

Table 3 also reports log specifications (in row (2)). While the FPM law is not specified in logs, these specifications are useful as the fuzzy RD specifications linking earnings, wages, and employment to federal transfers is expressed in logs. The log-level specifications in columns (1)-(4) yield a coefficient of 1. When we add municipality constants (in (5)-(8)) that are however not required by the FPM law, the estimate slightly falls (0.97); the small attenuation is also present in the log-difference specifications (in (9)-(12)).[18]

Figure 4 presents an illustration of FPM transfers at the law-implied population cutoff. We first net state, year, and cutoff effects and then we plot the residuals, averaged over 75-unit intervals, pooling across all seven population cutoffs.[19] There is an evident jump of

[17] We thank Josh Angrist for pointing out that the "first-stage" should be specified in levels (rather than in logs) and without the municipality fixed-effects, as this specification should follow the FPM law.

[18] We also examined the link between actual FPM and law-implied FPM transfers across each of the seven cutoffs. There is a strong link across cutoffs (Appendix Table 8).

[19] The figure also reports a second-order polynomial in population size and 95% confidence intervals, fitted separately on each side of the pooled FPM threshold (population is normalized as the distance from the above or below threshold). All RD graphs in this paper follow a similar construction. This procedure follows the recommendation of Imbens and Lemieux (2008), Lee and Lemieux (2014), and Angrist and Pischke (2008), who propose "binning" (averaging) the data for the visualization figures, but using the "raw" non-averaged data in the regression analysis.

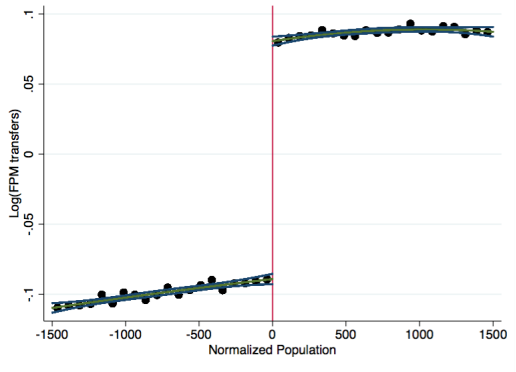



Figure 4: FPM Transfers around the cutoffs

federal transfers for municipalities on the "right" of the FPM thresholds. But while there is a one-to-one link between actual and law-implied transfers (Table 3), enforcement is imperfect.

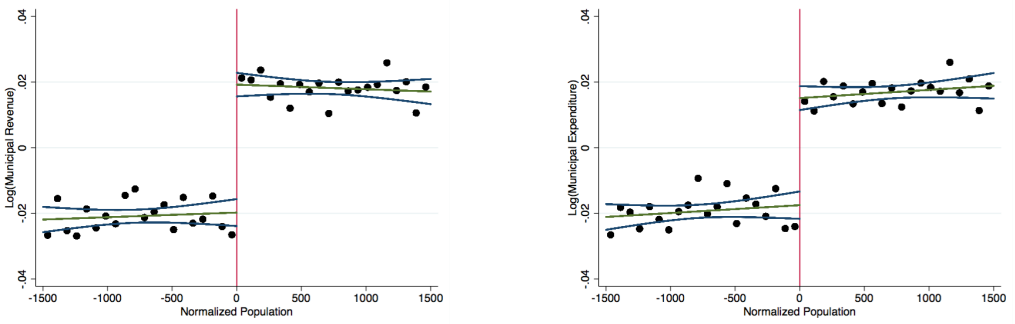
### 3.2.2 Municipal Government Revenues and Expenditure

A related necessary condition for identification is that municipal revenues and municipal expenditure change abruptly at the cutoffs.

Table 4 - Panel *A*, columns (1)-(4) reports level and log-level OLS estimates with municipality fixed-effects associating municipal revenues to law-implied FPM transfers in the neighborhood of the FPM cutoffs. The coefficient on law-implied FPM transfers in the level specifications is highly significant and, while somewhat unstable, ranges around 1 in the narrow 2%-bandwidth ($0.72 - 1.21$). Log-OLS estimates are more stable, ranging from 0.355 to 0.38, as these specifications account for outliers. The difference specifications reported in columns (5)-(8) also fluctuate around 1 in levels and around 0.32 in logs, close to the share of FPM in municipal revenues (0.31, Table 2).

Figure 5a provides a graphical illustration of these estimates, when we pool across all cutoffs. The figure plots averaged over 75-inhabitants bins of residuals of log revenues on municipality, state-year, and cutoff-year fixed-effects. There is an evident jump of municipal revenues for municipalities as they cross the FPM discontinuities.

Table 4 - Panel *B* reports corresponding estimates for municipal expenditure. OLS estimates without the first-order polynomials are close to 1. The estimates fall when we add cutoff-specific constants and linear polynomials (in (3)-(4)), though still we cannot reject a coefficient of unity. Log-OLS estimates are less sensitive to outliers and range from $0.30-0.34$, quite close to the share of FPM transfers to municipal spending (0.31, see Table 2). The dif-




(a) Municipal Revenues

(b) Municipal Expenditure

Figure 5: Municipal Revenues and Expenditure

ference specifications are also highly significant further showing that municipal expenditure responds to changes in law-implied FPM transfers. Figure 5b plots average municipal expenditure below and above the pooled discontinuities. While relatively more noisy than FPM transfers, municipal spending visibly changes discontinuously across the FPM cutoffs.[20]

### 3.2.3 Other Transfers of Municipal Revenues around the FPM Thresholds

Another condition for identification is that, besides FPM transfers, no other covariate relevant for labor markets move abruptly at the FPM thresholds. All other than FPM factors affecting employment, wages and total earnings should be continuous at the cutoffs (Imbens and Lemieux, 2008; Lee and Lemieux, 2010, 2014). This RD assumption is similar to the "exclusion restriction" in an IV setting requiring that the 'instrument' (law-implied FPM transfers around the cutoffs in our application) should affect the outcomes only via determining the endogenous variable (actual FPM transfers and associated municipal spending in our setting). This assumption cannot be directly tested, though there are many pieces of supportive evidence.

First, since we explore within-municipality variation in transfers and labor market outcomes, concerns that cities may differ systematically across geographic, institutional or other features (which apply to cross-sectional approaches) are not particularly severe. And even

[20]We also examined how municipal revenues and spending move in the neighborhood of each of the seven FPM cutoffs (Appendix Tables 9 and 10): the discontinuities in the the FPM allocation mechanism affect revenues and expenditure across each cutoff. Appendix Table 11 shows that as municipalities move to a higher FPM bracket current wage, current non-wage, and capital expenditure, all increase. All types of expenditure jump when municipalities cross to higher FPM population intervals. Appendix Table 12 shows that all main types of local government expenditure (education, health, housing and urbanization, public administration and other) increase as cities move to a higher FPM population bracket.

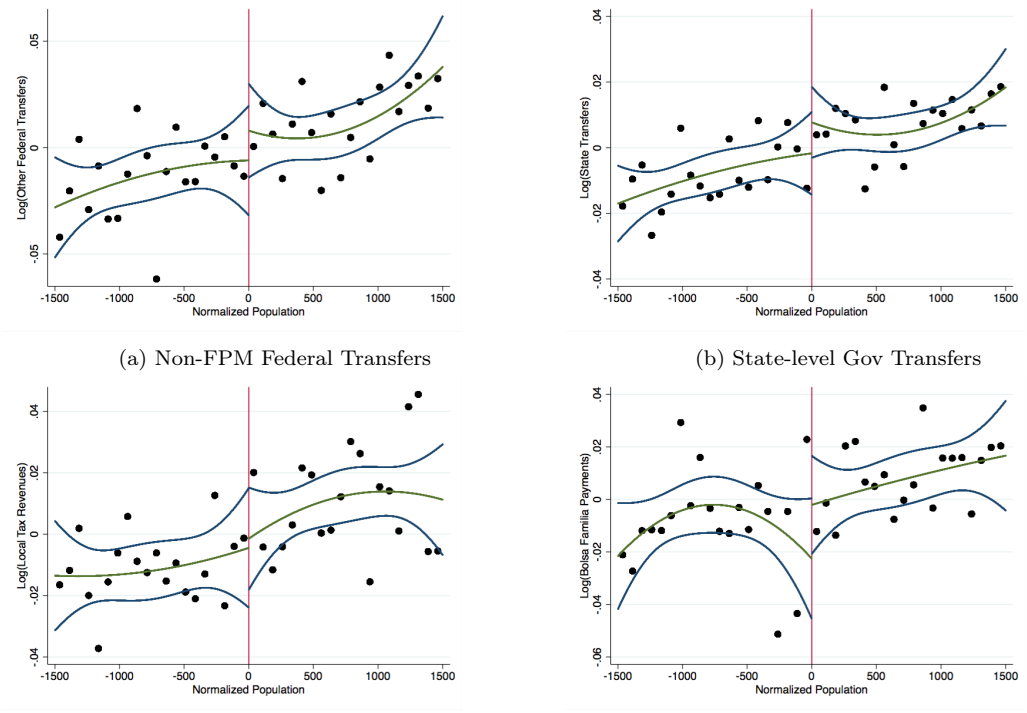
in the cross-section municipalities just above and just below the FPM thresholds do not differ much (Brollo, Nannicini, Tabellini and Perotti, 2013). Furthermore, Gadenne (2017) shows that municipalities moving to an adjacent FPM population bracket are similar to those that do not cross the cutoffs across many political economy features, such as the political alignment of the mayor to the federal government, political competition, and mayoral terms.

Second, to the best of our knowledge, there is no other federal or state grant scheme that follows a similar to FPM discontinuous allocation mechanism. One may worry that municipal governments gaining extra FPM funds may decide not to spend them. Likewise, municipalities that receive less FPM funds may obtain additional funding from the state or other federal transfer programs. These issues are, however, unlikely in our setting: municipalities run balanced budgets and their expenditure tightly adjusts to their revenues.

In Table 5, we test whether there are discontinuities on state transfers (Panel $A$), non-FPM federal transfers (Panel $B$), the disbursements associated with Bolsa Familia –a large cash transfer programme to low-income households– (Panel $C$), and local tax revenues (Panel $D$) in the neighborhood of the seven FPM population cutoffs. Starting with the evolution of state-level government transfers, the local regression estimates are small and statistically indistinguishable from zero. State transfers do not vary systematically at the population cutoffs where FPM transfers change sharply. The picture is similar when we study the evolution of non-FPM federal transfers. The local regression estimates are small, change sign, and are all statistically indistinguishable from zero. The coefficients on FPM in the specifications where transfers from the Bolsa Familia programme serve as the dependent variable are tiny. Local tax revenues seem to move at the FPM cutoffs, a result consistent with a local multiplier effect (as we show in the next Section), which in turn may induce a marginal increase on local tax revenues.[21]

Figures 6a-6d provide visualizations of these patterns. There is no abrupt change at the FPM discontinuities of state transfers, other-than-FPM federal transfers, disbursements associated with Bolsa Familia and local tax revenues.

[21] Local tax rates are not used as a stabilization tool. Increasing local tax rates is politically costly for mayors and local legislatures (Oliveira-Junior, 2014). The federal government has recently put forward legislation that aims to establish rules forcing municipalities to increase periodically their tax rates so to protect local mayors from public pressure to keep taxes low (Projeto de Lei do Senado (PLS) 46/2016). Smaller municipalities lack technical capacity to efficiently enforce such taxation. The Brazilian Development Bank has tried helping small municipalities modernize their tax system management in the past years (Gadenne, 2017).




(a) Non-FPM Federal Transfers

(b) State-level Gov Transfers

(c) Local Tax Revenues

(d) Bolsa Familia Payments

Figure 6: Types of Municipal Revenues around the cutoffs

### 3.2.4 Precise Systematic Manipulation

RDD strategies require that individuals (municipalities) have *imprecise* control over the running variable, population in our setting (Lee and Lemieux, 2010). If there is *precise* manipulation of population estimates *and* this correlates with the labor market outcome, then the estimates will not identify the causal effect of regional transfers.[22] Although it is impossible to directly test such assumption, we comment here for brevity and report in the appendix five sets of exercises. First, we perform a falsification test to check whether lagged 'treatment' variables (actual FPM, revenues and expenditure) vary abruptly at FPM cutoffs. We find that they do not, implying that municipalities narrowly above the FPM thresholds *are not* more likely to have been above or below the threshold in the previous year. Second, we conduct a placebo test in which all cutoffs are moved by 750 inhabitants, and examine whether the 'treatment' variables jump at the 'fake' discontinuities. In line with our identification strategy, there are no swings at the 'fake' cutoffs. Third, we examine whether the

[22]To invalidate RDD manipulation has to be perfect. Lee and Lemieux (2014) write: "*If individuals -even having some influence - are unable to precisely manipulate the assignment variable, a consequence of this is that the variation in the treatment near the threshold is randomized as though from a randomized experiment.*"

density of population and population changes are continuous at the cutoffs. The density plots uncover some manipulation in the population census years. So, we re-run all specifications excluding census years and find almost identical results. Forth, to minimize any manipulation concerns we estimate specifications focusing solely on municipality-years with either no movement across FPM intervals or falling into *lower* population intervals. Again the results are similar. Fifth, we also estimate restrictive specifications with municipality-term-specific mayoral constants so to account mayor-specific unobservables. The results are again similar.

# 4 Baseline Results

In this section, we first examine the responses of municipal sector employment and wages to locally exogenous swings in regional transfers. Second, we look at the impact of federal transfers on private sector earnings, employment and wages. Third, we report the cost per job and the local multiplier estimates. Forth, we discuss the potential impact of the informal economy on our estimates. Fifth, we summarize various robustness checks.

## 4.1 Employment and Wages in the Public Sector

Table 6 reports RD specifications that associate total municipal earnings, employment and wages to law-implied FPM transfers in the neighborhood of the FPM cutoffs (4% bandwidth in odd-numbered columns and 2% bandwidth in even-numbered columns). Panel *A* reports "reduced-form" log-level (columns (1)-(4)) and log-difference (columns (5)-(8)) OLS specifications, whereas Panel *B* gives the corresponding 'fuzzy' RD 2SLS estimates. Columns (1)-(2) and (5)-(6) give simple local regression estimates; columns (3)-(4) and (7)-(8) augment the specifications with linear polynomials on population distance from the discontinuity, allowing for different slopes above and below cutoffs and cutoff-specific constants (rectangular kernel).

Let us start with the specifications for total earnings of municipal public sector employees, reported in the first row of each panel. The coefficient implies that a one-percentage-point increase in FPM transfers generates an extra $0.17\% - 0.24\%$ increase in the total earnings of all municipal employees. The elasticity is stable across the various log-level specifications, thought there is some attenuation in the log-difference transformation.[23]

[23]These estimates are close to the ones in row (2) of Appendix Table 11, which reports the coefficient of law-implied FPM transfers on the total municipal wage bill. This is a non-trivial test as total earnings of municipal employees and the total wage bill of municipalities come from completely different datasets (RAIS - Ministry of Labor administrative dataset and FINBRA - Public Finance of Municipalities, respectively).

We decompose total municipal earnings into employment and average wage per worker to examine whether local authorities recruit more people or whether they raise wages in response to changes in federal transfers. Rows (2)-(3) report these estimates. Swings in regional transfers close to the FPM discontinuities affect both municipal employment and the mean wage rate, though the effects tend to be larger and more precise for employment. The log-difference specification estimates are typically smaller and less accurate than their log-level counterparts. As the first stage coefficients are close to one (Table 3), the "fuzzy" RD estimates (in Panel $B$) are close to the "reduced-form" estimates (in Panel $A$).[24]

## 4.2 Employment and Wages in the Private Sector

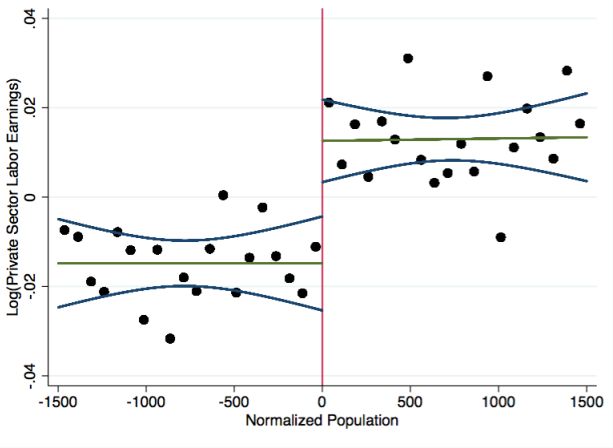
Table 7 reports "reduced-form" (Panel $A$) and "fuzzy" RD (Panel $B$) estimates, linking total labor earnings, employment and average wage per employee in the private sector with law-implied FPM transfers.

The elasticity between law-implied transfers and total private sector earnings is around 0.15 across the various log-level specifications (columns (1)-(4)). Since the first-stage fit is strong and the elasticity of actual and law-implied FPM transfers is close to one, the "fuzzy-RD" estimates are similar, ranging from $0.13 - 0.22$. The log-difference specifications (columns (5)-(8)) are also highly significant and only slightly lower, ranging between $0.12 - 0.22$. Conditional on time-invariant municipal factors, state-specific trends, and city-size (cutoff-specific) time trends, a twenty percent increase in federal transfers (roughly the step in the FPM allocation function, equation (1)) is associated with a $3\% - 4\%$ boost in private-sector labor income. This corresponds to roughly the mean of earnings growth (Table 2). Figure 7a gives a graphical illustration of this core result. We first net out municipality, cutoff-year and state-year fixed effects and then plot the residuals, averaged over 75-unit intervals, pooling across all seven population cutoffs. There is an evident jump (fall) in total labor earnings of private sector employees when municipalities move to a higher (lower) FPM population interval.

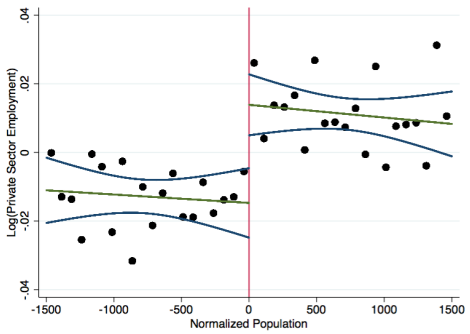
We then examine whether the impact of regional transfers on the local economy stems

[24]The sum of the estimate on log employment and log wage per worker are close, but not exactly equal to the coefficient on log earnings. The small discrepancy arises from (i) rounding of coefficients, and (ii) from the use of slightly different samples due to the fact that for a few municipalities we observe total earnings and/or number of employees, but not wages.

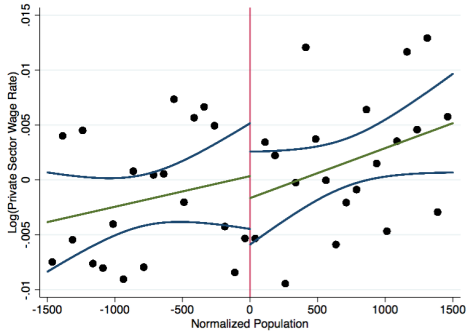
Appendix Table 13 shows that in response to increases in federal transfers local governments raise wages of "old" municipal employees and increase employment via hiring new public sector employees.




(a) Total Earnings




(b) Employment




(c) Wage per Worker

Figure 7: Total Earnings, Employment and Wage Rate in the Private Sector

from increased private sector hiring (employment) or via higher wages. The estimates in rows (2) and (3) reveal that regional transfers boost local economic activity, mostly via increasing private sector employment. The coefficients of log law-implied FPM on log private employment are always significant at standard confidence levels. This applies both in the log-level and log-difference specifications. The FPM private employment elasticity is around 0.14, ranging between $0.10 - 0.21$. In contrast to the significant FPM transfers-employment association, the transfers-mean wages elasticity in row (3) is small $(0.02 - 0.06)$ and in most specifications statistically indistinguishable from zero. Figure 7b illustrates the higher level of private employment on the right side of the pooled FPM discontinuities. In contrast, Figure 7c shows no evident change in average wages at the pooled FPM cutoffs.

## 4.3 Cost per Job and Local Income Multiplier

### 4.3.1 Employment Costs

In Table 8 - Panel $A$, using the elasticity formula we map the fuzzy-RD estimates to the cost of a job in the private sector (Table 7) and in the public sector (Table 6). Estimating the cost of a job in the private sector and local government allows us to compare our federal transfer estimates for Brazil with studies that have exploited variation in other countries and settings. Panel $A$ reports the number of jobs created for an increase of FPM transfers of $BRL$ $30,000$ (at constant 1998 prices) that corresponds to roughly 1% of average FPM transfers. An additional $BRL$ $30,000$ is associated with around one extra public-sector job in the log-level specifications (half a job in the log-difference model) and about three extra private-sector jobs in the log-level specification (two in the log-difference model). As the elasticities of regional transfers with public and private sector employment are both around 0.15, this difference reflects the fact that - in terms of employees - the size of the private sector is two-to-three times the size of the public sector (Table 2).

Alternatively, the estimates can be framed in terms of the average cost of an extra job per year. The first row of Panel $B$ in Table 8 reports the average cost per job. For an extra one job, regional (FPM) transfers need to increase -on average- by approximately $6,000$ to $13,000$ $USD$ at constant 2016 prices. The average cost of an extra job, implied by our RD estimates across relatively small Brazilian municipalities, is roughly one fourth of the corresponding calculation of about USD $30,000$ that Serrato and Wingender (2016) report across US counties (which are comparable in size to Brazilian municipalities).[25] This is consistent with the real wage and productivity gaps between Brazil and the United States.[26]

### 4.3.2 Local Multiplier

Several recent works on the local effects of fiscal policy present their results in terms of local multipliers (e.g., Nakamura and Steinsson, 2014; Acconcia *et al.*, 2014; Serrato and

[25]Focusing on the impacts of the American Recovery and Reinvestment Act (ARRA) of 2009, aimed to mitigate the economic effects of the Great Recession, Chodorow-Reich *et al* (2012) estimate a cost per job ranging between $16,000 - 50,000$ USD; Dube *et al.* (2012) estimate is close to $25,000$ USD, while Feyrer and Sacerdote (2012) estimate is around $50,000$ (though with a wide range). Adelino *et al.* (2012) estimate a cost per job of around $21,000$ USD and Shoag (2013) estimates a cost per job of around $35,000$ USD. See Chodorow-Reich (2018) for a detailed overview and discussion.

[26]The World Bank Indicators database approximates GDP per person employed in 2006 (midyear of our analysis) in Brazil at $28,081USD$ and in the United States at $102,981USD$.

Wingender, 2016 and Shoag, 2013). Chodorow-Reich (2018) proposes a simple and intuitive way to convert employment (cost per job) multipliers (estimates) to income/output multipliers. Assuming a neoclassical production function linking output ($Y_t$) to employment ($E_t$), hours worked ($H_t$) and productivity ($A$), without capital adjustment in the short-run [$Y_t = A(H_t E_t)^{1-\xi}$], output and employment multipliers are linked by the following expression:

$$\mu_Y = (1-\xi)(1+\chi)\frac{Y}{E}\mu_E. \tag{5}$$

$\mu_Y$ denotes the output/income multiplier and $\mu_E$ is the employment multiplier, the inverse of the estimated cost of a job reported in Table 8 - Panel $A$. $\chi$ represents the elasticity of hours per worker to total employment and $\xi$ refers to the share of capital in the production function.

Following Chodorow-Reich (2018), we parameterize this expression using Brazilian data to approximate the local income multiplier. We set the capital share to 1/3, as standard in the literature and also in line with Brazil-based evidence of Bugarin, Ellery-Jr and Gomes (2004). Following Santos (2016), we set the elasticity of hours to total employment to 0.12. Income/output per worker, $\frac{Y}{E}$, takes the value of $BRL\ 21,152$ - the average in the 2010 Brazilian Census.

Table 8 - Panel $B$ report the local income multiplier using Chodorow-Reich's mapping. This ranges between 1.1 and 2.6 across models, with a point estimate around 2 in our favourite specification in column (4).[27] These estimates are in line with the evidence from other recent studies focusing on developed countries, which report local output multipliers between 1.4 and 2.5 (see for instance Nakamura and Steinsson, 2014; Acconcia *et al.*, 2014; Serrato and Wingender, 2016; Shoag, 2013).

In the Online Appendix we calibrate a relatively standard Neo-Keynesian currency union model (Farhi and Werning, 2016) to the Brazilian economy and show that the calibrated model yield local multiplier estimates in the range of our regression estimates, typically around two. We also conduct simple model counterfactual simulations to map to the local multiplier estimates into their economy-wide counterparts; the stimulative effects of fiscal policy would have been substantially smaller (around $20\%-40\%$) if local government spending was financed by local tax revenues rather than regional transfers, as in our data.

[27] We varied the parameters in the mapping of employment to output multiplier formula, allowing the share of capital to range from 0.3 to 0.4 and the hours - employment elasticity from 0 (no adjustment of hours) to 0.5 (US based estimate). The local income multiplier is now centered around 2.1, ranging between $1.7 - 2.5$ for the specification in column (4).

## 4.4 Accounting for the Informal Economy

Our analysis is based on labour outcomes (earnings, employment, wages) from a high-quality administrative dataset assembled yearly by the Brazilian Ministry of Labor that, as such, only covers the formal economy. Given the importance of the unofficial economy in Brazil, in this subsection we discuss a plausible adjustment of the multiplier estimates to incorporate the potentially positive effects of transfer-driven municipal spending on the informal economy.[28]

A large empirical literature from various settings and countries reveals that productivity in the informal sector is considerably smaller than in the formal sector. La Porta and Shleifer (2008) review the evidence on the informal economy on several developing countries (including Brazil) and also report their own cross-country estimates. They show that unregistered firms are considerably less productive than registered firms, as they lack access to capital, are misgoverned, and are typically run by unskilled entrepreneurs. Meghir, Narita and Robin (2015) use rich Brazilian longitudinal individual-level data and a structural equilibrium wage-posting model with heterogeneous firms. Their evidence implies that the productivity of the median unregistered firm equals 0.55 of its formal counterpart.[29]

Our back-of-the-envelope calculation suggests that factoring in the informal economy yields an upward adjustment to the local income multiplier estimates of at most 20%. This is done by applying a simple adjustment formula that incorporates the potential productivity contribution of the informal jobs added to the economy due to FPM transfers, by definition not accounted for in our baseline estimates. First, we use the baseline estimate that a one percent increase in FPM transfers (roughly $R\$30,000$) is associated with 4 extra jobs, 3 in the formal private sector and 1 in the municipal public sector (Table 8 - Panel A). Second, we make the conservative assumption (especially in the context of a public spending boost) that the informal sector creates jobs at half of the rate of the formal sector. Under this scenario, a FPM transfer increase that generates 3 formal private sector jobs would also generate 1.5 informal jobs. Third, as the careful analysis of Meghir, Narita, and Robin (2015) suggests that the productivity contribution of the median informal firm is about 0.55

[28] Simple tabulations of the Brazilian Labor Force Survey 2002-2007 (PME) for individuals aged 23-65 (first-interview) reveals that the formality rate of salaried workers is 28%, 67% and 53% for agriculture, manufacturing, and services, respectively. As we will show in the next section, the effects of regional transfers on registered firms in agriculture is always small and never statistically different from zero.

[29] Prado (2011) employs a general equilibrium model to show that firms with lower productivity endogenously choose to operate in the informal sector. Schneider (2007) finds that the size of the Brazilian shadow economy is 42%, the median among other 21 Latin American countries.

of the contribution of a formal one, the overall increase in the private economy is 3 formal jobs plus 0.82 formal-sector-productivity-equivalent informal job (i.e. 1.5 informal jobs multiplied by 0.55 of the formal job productivity). Adding the extra job created in the public sector, it yields a coefficient of 4.82. So the adjustment factor of the local multiplier estimate is 4.82/4 or about 20%. We consider this to be an upper bound in our sample of small municipalities where the productivity of the informal sector is likely to be lower than the median estimate reported by Meghir, Narita, and Robin (2015) for the whole economy.

## 4.5 Sensitivity Analysis

We have performed a series of robustness checks, which for brevity we present and discuss in detail in the Online Appendix.[30] Our findings appear robust to: (i) dropping observations around the first cutoff (10, 188), which is close to the discontinuity in the pay of local politicians (10, 000) after 2004 (Ferraz and Finan, 2011), (ii) excluding the census years (2001, 2008, 2011 and 2012), for which there is evidence of manipulation in the population counts (although manipulation *per se* does not invalidate the RDD design), (iii) looking only at municipalities 'moving' to a lower FPM population bracket and municipalities without any FPM bracket movement, so as to minimize concerns of manipulation, (iv) replacing the municipal constants with mayor-specific fixed-effects to control for mayor's ability, (v) adding state-cutoff-year fixed effects, so as to further account for unobservables, (vi) including higher-order polynomials in population and exploring variation from observations both close and far from the cutoffs (as Brollo *et al.*, 2013), (vii) using a 2-year difference specification, as in Nakamura and Steinsson (2014), (viii) augmenting the specification with outcomes aggregated at the regional (micro-region[31]) level to look at geographical spillovers (which are negligible), (ix) allowing transfers to have a delayed effect on labor market outcomes to account for potential dynamics in the relationship between government spending and output, (x) using municipal government expenditure instead of actual transfers in the first stage, so as to account for potential savings or overspending, (xi) sectorial heterogeneity robustness checks and fine industry-level analysis (2-digit SIC), (xii) keeping only observations from municipalities that jump up and down during our sample period, so as to address sample composition issues,

[30] See Appendix Tables $14 - 25$.

[31] The IBGE defines as "*groups of economically integrated municipalities sharing borders and structure of production*" (see IBGE (1990, page 10)). Our sample comprises 547 micro-regions with an average of 21 micro-regions per state and 5 municipalities per micro-region.

and (xii) exploring whether there is a differential effect on firms behaviour depending on firm size.

# 5 Sectoral Heterogeneity

This Section examines the impact of regional transfers on private sector labor market outcomes across different sectors of the Brazilian economy. First, we look at broad and finer sectoral differences. Second, we scrutinize the effects of transfers on firms entry and firm size.

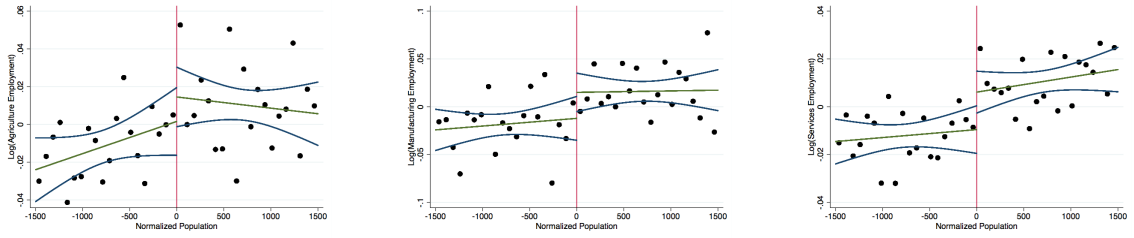
## 5.1 Baseline Estimates across Sectors

Table 9 report local RD estimates (in log-levels and log-differences) that examine the impact law-implied FPM transfers on total earnings, employment, and average wage per employee in agriculture, in manufacturing, and in services.

The results in Panel $A$ imply that regional transfers have no effect on agriculture. The coefficients of log FPM transfers on log total earnings in row (1) are unstable, small, and statistically indistinguishable from zero. Similarly, the FPM estimate in the employment and average wages (in rows (2) and (3)) changes sign and appears insignificant.

Panel $B$ records the estimates for manufacturing. The elasticities of total earnings in manufacturing with law-implied FPM transfers in row (1) are all positive, but statistically insignificant. When we decompose manufacturing earnings into employment and average wages, there is some evidence of a positive effect of FPM transfers on employment. The estimates in row (2) are positive, implying potentially sizeable effects (elasticity range $0.08 - 0.32$). However, the estimates are inaccurate and often statistically indistinguishable from zero. In contrast, the FPM transfers - average wages elasticity in row (3) is close to zero and never passes standard significance levels.

Panel $C$ gives the RD estimates for services. There appears to be a strong association between total earnings in services and federal transfers (row (1)). The coefficient is positive and highly significant across both the 2% and the 4% bandwidth, with and without the RD polynomial, in the level and the difference specifications. The elasticity is tightly estimated, ranging from $0.15 - 0.21$. A twenty percent increase in FPM transfers –roughly the average jump when a municipality moves across FPM cutoffs– is associated with a $3\% - 4\%$ increase in total earnings for services; this is to be compared with an average/median growth in earnings of around 10%. The specifications in rows (2)-(3) show that this effect is driven by




(a) Agriculture (b) Manufacturing (c) Services

Figure 8: Employment in Agriculture, Manufacturing and Services

employment. As municipalities move to a higher FPM population interval (and therefore local revenues and municipal spending rise), private employment in services increases considerably whereas the effects on average wages is small and statistically insignificant.

Figures 8 provide a visual illustration of the sector-specific patterns in private employment. the Appendix. Figure 8a shows that employment in agriculture does not change abruptly at the pooled FPM population cutoffs. Figure 8b illustrates a jump in manufacturing employment for municipalities moving to a higher FPM population interval. In line with the noisy estimates in Table 10 - Panel $B$, the jump is visible but not sharp. Figure 8c illustrates a clear jump in municipal employment in services for municipalities at the higher FPM population bracket.

Using the richness of the administrative dataset on wages and employment, we conducted also the analysis at a finer industry classification (2-digit ISIC). Figure 9 summarize the main findings using a local regression with a 4% bandwidth and 90%- confidence intervals (the results are reported for brevity in the Appendix). The decomposition reveals that employment in retail responds the strongest to swings in regional transfers close to the FPM cutoffs. Education, other services, and to a lesser extent construction appear among the main drivers of the sectoral findings in Table 9.

Overall, the results point out that the effect of FPM transfers on private sector earnings and employment (Table 9) is driven mostly by services, and especially retail. There is little evidence of an impact on wages and employment in agriculture and there seems to be only a weak effect on manufacturing employment. These results are consistent with the workhorse Neo-Keynesian macro models that suggest stimulative effects of fiscal policy on non-tradeables.

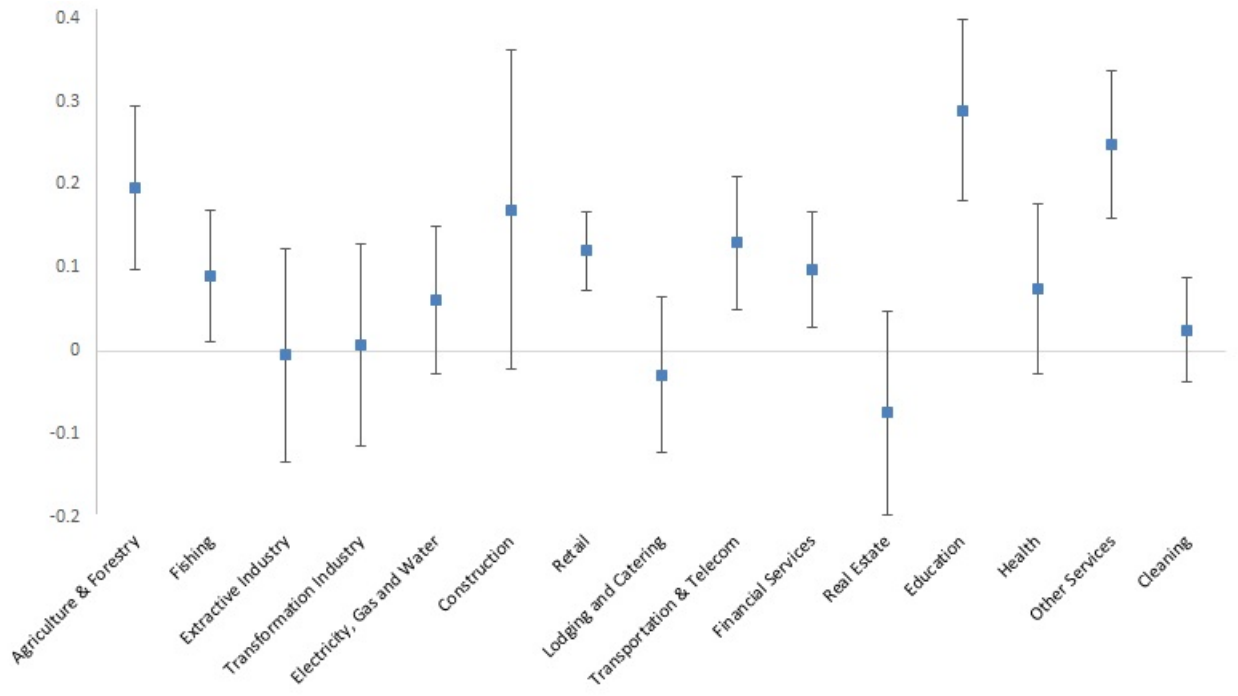



Figure 9: 2-Digit SIC Industry-Level Estimates

## 5.2 Firm Entry

In Table 10, we examine the impact of regional transfers on the number of firms (Panel $A$) and average firm size of existing firms, as measured by number of employees (Panel $B$). Looking at the number of firms, allows us to evaluate any possible effect of transfers-driven municipal spending on net entry (extensive margin), whereas studying the response of existing firms' size allow us to evaluate the effect of FPM transfers on the firms' size (intensive margin).[32]

A comparison of the estimates in row (1) of Panels $A$ and $B$ shows that in response of higher federal transfers, net entry increases substantially. In contrast, there is no significant impact of federal transfers on the size of the average firm. In rows (2), (3), and (4) we explore sectorial heterogeneity on fiscal policy's impact on net entry and firm size. Transfers-induced municipal spending stimulates new firms entry in services. The elasticities are highly significant and quite stable, around 0.08 (range 0.06 to 0.12). In contrast, the results for the other sectors are not robust. The coefficients in Panel $B$ are mixed and while services still appears the sector most likely to respond in terms of average size of existing firms, the estimates appears less accurate and stable.

[32]It is possible that increased entry may alternatively reflect formalization.

# 6 Heterogeneity by Municipal Characteristics

There are considerable differences in economic, institutional and financial development across Brazilian municipalities. Given the large number of localities, in this section, we investigate possible heterogeneity of the baseline estimates the link private sector employment to swings in regional transfers in the neighborhood of the seven FPM thresholds. In particular, heterogeneity across the following dimensions: municipal income, financial development, governance quality, size (as measured by population), and geography (North or South). Table 12 reports "reduced-form" RD estimates. For completeness, we continue reporting log-level (in columns (1)-(4)) and log-difference (in columns (5)-(8)) local regression estimates without and with RD polynomials using both a 4% and a 2% bandwidth.[33]

In Panel $A$, we compare employment effects of local fiscal policy across high income and low income cities using the median municipal income in 2000 to distinguish the two sets of localities. Less affluent municipalities tend to experience larger and more significant surges in private sector employment, in response to exogenous increases in regional transfers. The elasticities are highly significant across all permutations, ranging from 0.04 to 0.10. The private employment law-implied FPM transfers elasticities in higher income municipalities are smaller and in most specifications statistically indistinguishable from zero.

In Panel $B$, we explore heterogeneity with respect to financial development/access, as measured by the number of commercial banks local branches. The elasticities are positive and in most specifications statistically significant. However, the employment-transfers elasticity is larger and more precisely estimated (0.08) for cities with fewer bank branches municipality. The corresponding elasticity for the group of municipalities with a higher number of commercial bank branches is smaller, around 0.035. Hence, there is evidence of stronger effects in more-financially constrained municipalities, a result that is in line with the Neo-Keynesian framework (see Farhi and Werning, 2016; and the discussion in Section 7 of the Appendix).

In Panel $C$, we examine the role of local institutional quality in explaining the potentially diverse response of private employment to municipal spending. We split municipalities into relatively more/less institutionally developed, using the governance practices index calculated by the Brazilian Census Bureau (IBGE) and used by the Ministry of Planning as a tool to monitor the administrative performance of municipalities that reflects different aspects of

[33]The corresponding first-stage regression estimates (reported for brevity in the Appendix) imply a close to one-to-one relationship between actual and law-implied FPM transfers, with an $R^2$ though below one.

administrative capabilities at the municipality level in 2000 (see Naritomi, Soares and Assuncao, 2012). The estimates on law-implied FPM transfers are positive in all permutations for both the more and the less institutionally developed group of municipalities. The elasticity is more precisely estimated for relatively lower governance quality municipalities; the difference specifications suggest that the employment impact of increases in regional transfers is somewhat larger for low institutional quality municipalities.

In Panel $D$, we estimate the reduced-form specifications separately for smaller and larger municipalities. Following Brollo, Nannicini, Tabellini and Perotti (2013), we pool all municipalities around cutoffs $1-3$ (population range $6,793$ to $20,377$) and municipalities in cutoffs $4-7$ ($20,378-47,537$). Population size correlates with income, the penetration of banks, and institutional quality and can be though as a summary way to summarize development. The estimates yield a clear pattern. There are economically sizeable effects of federal transfers on private employment in smaller municipalities; the employment FPM transfers elasticity is stable, around 0.045 and always passes standard significance levels. In contrast, the elasticity on larger cities, while always positive, is weaker and in most specifications statistically indistinguishable from zero.

In Panel $E$, we examine the link between private employment and regional transfers separately for municipalities in the southern and the northern states. We do so, as there are non-negligible differences in geography and historical development between Brazil's North and South. The elasticity of law-implied FPM transfers and employment in the private sector is positive in both sets of municipalities. The level specifications yield somewhat larger and more precisely estimates elasticities for municipalities in The Northern states, though this pattern is not present in the difference specifications.

**Summary** The analysis suggests considerable heterogeneity on the employment effects of federal transfers induced swings on local public spending. While many of the municipal characteristics are correlated, the estimates suggest that the stimulative employment effects of regional transfers are stronger for relatively poorer municipalities with lower levels of financial access, smaller population, in the North, and -to a lesser extent- with lower quality governance.

# 7 Conclusions

We identify the effects of regional transfers on local labour markets in Brazil, a large currency union, applying a 'fuzzy-RD' design that exploits the highly non-linear allocation mechanism of funds from the federal government to municipalities. Federal transfers, municipal public revenues, and spending change abruptly at various pre-determined population cutoffs, according to yearly population estimates provided by the independent federal statistical agency and court of auditors. The fuzzy-RD estimates appear clear-cut. As municipalities cross the pre-assigned population cutoffs shaping federal transfers, there is a significant boost in private sector income and employment. For every $30,000$ USD increase in municipal government receipts from the federal government, the local economy witnesses an extra job in the public sector and three extra jobs in the private sector. The effect on wages is mild and insignificant. These employment estimates map into local income/output multipliers of around 2. The sizeable impact of federal transfers on private employment stems from services, especially retail, education and other services. Federal transfers are associated with increased entry in services. The stimulative effects of local government spending funded by regional transfers tend to be larger for municipalities with relatively lower income, less financial access, lower quality governance, relatively smaller and in the northern States. These results accord well a calibrated to Brazil workhorse open-economy Neo-Keynesian model. The model, however, implies significantly lower local multipliers if municipal spending was funded by local taxes, suggesting that regional transfers could be a useful stabilization tool in large currency unions.

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**Table 1 - Descriptive Evidence**
**Distribution of Municipalities; "Control" and "Treatment" Groups**

**Panel A: Full Sample**

| | No Movement | Bracket | Total |
|---|---|---|---|
| No Movement | 1410 | 1087 | 2497 |
| Moves to Lower Bracket | 93 | 689 | 782 |
| Total | 1503 | 1776 | 3279 |

**Panel B: Restricted Sample in the Neighborhood of the FPM Cutoffs (<4%)**

| | No Movement | Bracket | Total |
|---|---|---|---|
| No Movement | 601 | 976 | 1577 |
| Moves to Lower Bracket | 146 | 172 | 318 |
| Total | 747 | 1148 | 1895 |

Panel A reports the number of municipalities in the full sample that move to a higher/lower FPM population brackets at least once across the full sample period 1999-2014, and the number of municipalities that stay in the same FPM population bracket. Panel B repeats Panel A only for observation near a cut-off (relative bandwidths 4%).

**Table 2 - Summary Statistics: Population and Public Finance**

| | Population growth | | Sources of Revenue (% of Total) | | | | | Main Categories of Expenditures (% of Total) | | | | Income p.c. | total |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Population Bracket | mean | s.d. | FPM | Local taxes | State-level Transfers | Federal Transfers | Other Sources | Public Admin | Education | Health | Housing | BRL 2000 | obs |
| 6,793–10,188 | 0.011 | 0.054 | 0.33 | 0.05 | 0.25 | 0.13 | 0.25 | 0.17 | 0.31 | 0.22 | 0.09 | 1850 | 10,302 |
| 10,189–13,584 | 0.010 | 0.054 | 0.33 | 0.05 | 0.22 | 0.13 | 0.26 | 0.17 | 0.33 | 0.22 | 0.09 | 1831 | 8,329 |
| 13,585–16,980 | 0.011 | 0.052 | 0.33 | 0.05 | 0.22 | 0.13 | 0.27 | 0.16 | 0.34 | 0.23 | 0.10 | 1813 | 6,151 |
| 16,981–23,772 | 0.011 | 0.050 | 0.31 | 0.06 | 0.22 | 0.14 | 0.28 | 0.16 | 0.34 | 0.22 | 0.09 | 1878 | 8,419 |
| 23,773–30,564 | 0.013 | 0.043 | 0.28 | 0.07 | 0.22 | 0.15 | 0.29 | 0.16 | 0.35 | 0.22 | 0.10 | 2016 | 4,749 |
| 30,565–37,356 | 0.012 | 0.045 | 0.26 | 0.08 | 0.23 | 0.14 | 0.29 | 0.15 | 0.34 | 0.23 | 0.10 | 2113 | 2,983 |
| 37,357–44,148 | 0.012 | 0.047 | 0.24 | 0.09 | 0.23 | 0.15 | 0.30 | 0.15 | 0.34 | 0.23 | 0.10 | 2134 | 1,948 |
| 44,149–47,537 | 0.014 | 0.035 | 0.24 | 0.09 | 0.22 | 0.15 | 0.30 | 0.15 | 0.34 | 0.24 | 0.10 | 2272 | 585 |
| Total | 0.012 | 0.047 | 0.31 | 0.06 | 0.23 | 0.14 | 0.27 | 0.16 | 0.33 | 0.22 | 0.09 | 1901 | 43,466 |

The table reports the mean and standard deviation of three sets of variables per population bracket: municipal population growth, sources of municipal revenue as a share of total, and types of expenditure as share of total. Sources of municipal revenue include FPM transfers, local tax revenue which include ISS (service tax) and IPTU (property tax), state-level government transfers and federal-level government transfers (net of FPM) to municipalities. Other sources include mining and oil royalties, capital income, and many other smaller sources. The sample includes 43,466 yearly observations covering 3,279 Brazilian municipalities over the period 1999-2014.

**Table 3 - Actual and Law-Implied FPM Transfers**
**OLS and Fixed-Effect Estimates in Levels and Differences**

| | local estimates in levels without FE | | | | local estimates in levels with FE | | | | local estimates in differences without FE | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| bandwidth | <4% | <2% | <4% | <2% | <4% | <2% | <4% | <2% | <4% | <2% | <4% | <2% |
| dep.var. | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Actual FPM | 1.003*** | 0.995*** | 1.010*** | 1.008*** | 1.015*** | 1.002*** | 1.018*** | 1.012*** | 0.990*** | 0.993*** | 0.987*** | 0.982*** |
| OLS estimates | (0.005) | (0.005) | (0.007) | (0.009) | (0.006) | (0.007) | (0.007) | (0.010) | (0.007) | (0.010) | (0.007) | (0.011) |
| within (marginal) R2 | 0.92 | 0.91 | 0.86 | 0.83 | 0.992 | 0.993 | 0.992 | 0.993 | 0.89 | 0.91 | 0.88 | 0.89 |
| Log Actual FPM | 0.980*** | 0.966*** | 0.990*** | 0.976*** | 0.979*** | 0.965*** | 0.981*** | 0.966*** | 0.943*** | 0.946*** | 0.939*** | 0.934*** |
| OLS estimates | (0.007) | (0.007) | (0.010) | (0.011) | (0.008) | (0.008) | (0.009) | (0.011) | (0.008) | (0.011) | (0.009) | (0.013) |
| within (marginal) R2 | 0.89 | 0.88 | 0.84 | 0.83 | 0.989 | 0.99 | 0.989 | 0.99 | 0.88 | 0.90 | 0.84 | 0.89 |
| Observations | 11333 | 5643 | 11333 | 5643 | 11053 | 5113 | 11053 | 5113 | 7054 | 2594 | 7054 | 2594 |
| Municipality FE | No | No | No | No | Yes | Yes | Yes | Yes | No | No | No | No |
| State-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cutoff-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| First-order polynomial | No | No | Yes | Yes | No | No | Yes | Yes | No | No | Yes | Yes |

The table reports regression estimates associating actual municipal FPM transfers to law-implied FPM Transfers. Specifications (1)-(4) report local regression (RD) estimates in levels without municipal fixed-effects that restrict estimation in the neighborhood of the FPM cutoffs using two relative bandwidths (4% and 2%), without and with a rectangular kernel (first-order polynomial), respectively. Specifications (5)-(8) include municipal fixed-effects and specifications (9)-(12) report estimates in differences without fixed-effects. Row (1) reports OLS coefficient estimates when both the dependent and the independent variable are expressed in levels (no transformation). Row (2) reports OLS coefficient estimates when both actual FPM transfers (the dependent variable) and law-implied transfers (the independent variable) are expressed in logs. We construct municipal law-implied transfers applying the FPM allocation mechanism formula (see appendix). All specifications include state-year fixed effects and cutoff-year fixed-effects (constants not reported). The table also reports the within (marginal) R2. Heteroskedasticity-adjusted standard errors clustered at the micro-region are reported in parentheses below the coefficients. Significantly different from zero at 99% (***), 95% (**) and 90% (*) confidence level.

**Table 4 - Municipal Revenue and Expenditure around the FPM Cutoffs**
**Local Estimates in Levels and Differences**

| | local estimates in levels | | | | local estimates in differences | | | |
|---|---|---|---|---|---|---|---|---|
| bandwidth | <4% | <2% | <4% | <2% | <4% | <2% | <4% | <2% |
| dep.var. | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | **PANEL A - Municipal Revenue** | | | | | | | |
| Revenue | 1.522*** | 1.210*** | 0.706** | 0.717*** | 1.214*** | 1.231*** | 0.957*** | 0.941*** |
| | (0.161) | (0.190) | (0.315) | (0.257) | (0.087) | (0.125) | (0.103) | (0.153) |
| log (Revenue) | 0.381*** | 0.355*** | 0.370*** | 0.365*** | 0.323*** | 0.344*** | 0.317*** | 0.329*** |
| | (0.016) | (0.022) | (0.021) | (0.027) | (0.018) | (0.028) | (0.020) | (0.035) |
| | **PANEL B - Municipal Expenditure** | | | | | | | |
| Expenditure | 1.347*** | 0.934*** | 0.655*** | 0.601*** | 0.878*** | 0.872*** | 0.636*** | 0.604*** |
| | (0.133) | (0.164) | (0.215) | (0.213) | (0.100) | (0.133) | (0.118) | (0.167) |
| log (Expenditure) | 0.338*** | 0.299*** | 0.319*** | 0.308*** | 0.252*** | 0.287*** | 0.240*** | 0.264*** |
| | (0.017) | (0.023) | (0.022) | (0.029) | (0.018) | (0.024) | (0.021) | (0.031) |
| Observations | 11053 | 5113 | 11053 | 5113 | 7054 | 2594 | 7054 | 2594 |
| Municipality FE | Yes | Yes | Yes | Yes | No | No | No | No |
| State-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cutoff-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| First-order polynomial | No | No | Yes | Yes | No | No | Yes | Yes |

The table reports regression estimates associating municipal public finance variables to law-implied FPM Transfers. Panel A reports estimates for municipal revenues as the dependent variable and Panel B for municipal expenditure. Row (1) in each panel reports OLS coefficient estimates when both the dependent and the independent variable ( law-implied transfers) are expressed in levels (no transformation). Row (2) reports OLS coefficient estimates when both variables are expressed in logs. Specifications (1)-(4) report local regression (RD) fixed-effect estimates in levels that restrict estimation in the neighborhood of the FPM cutoffs using two relative bandwidths (4% and 2%), without and with a rectangular kernel (first-order polynomial), respectively. Specifications (5)-(8) report estimates in differences. We construct municipal law-implied transfers applying the FPM allocation mechanism formula (see appendix). All specifications include state-year and cutoff-year dummies (constants not reported). Heteroskedasticity-adjusted standard errors clustered at the micro-region are reported in parentheses below the coefficients. Significantly different from zero at 99% (***), 95% (**) and 90% (*) confidence level.

**Table 5 - Other Sources of Municipal Revenue around the FPM Cutoffs**
**Local Estimates in Levels and Differences**

| | local estimates in levels | | | | local estimates in differences | | | |
|---|---|---|---|---|---|---|---|---|
| bandwidth | <4% | <2% | <4% | <2% | <4% | <2% | <4% | <2% |
| dep.var. | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | **PANEL A: State-level government transfers** | | | | | | | |
| level-OLS | 0.059 | -0.004 | -0.077 | -0.005 | 0.021 | -0.005 | -0.032 | -0.085 |
| | (0.066) | (0.076) | (0.079) | (0.102) | (0.040) | (0.056) | (0.051) | (0.074) |
| log-OLS | 0.077** | 0.006 | 0.051 | 0.029 | 0.024 | 0.006 | 0.014 | 0.012 |
| | (0.033) | (0.047) | (0.039) | (0.055) | (0.039) | (0.054) | (0.046) | (0.062) |
| Observations | 11017 | 5100 | 11017 | 5100 | 7015 | 2583 | 7015 | 2583 |
| | **PANEL B: Federal-level government transfers (net of FPM)** | | | | | | | |
| level-OLS | -0.008 | -0.013 | -0.301 | -0.124 | 0.004 | 0.014 | -0.058 | -0.039 |
| | (0.086) | (0.090) | (0.184) | (0.161) | (0.062) | (0.078) | (0.071) | (0.113) |
| log-OLS | 0.063 | 0.020 | 0.031 | -0.006 | 0.132 | 0.213* | 0.132 | 0.265** |
| | (0.074) | (0.089) | (0.090) | (0.116) | (0.106) | (0.109) | (0.119) | (0.134) |
| Observations | 11017 | 5096 | 11017 | 5096 | 7013 | 2580 | 7013 | 2580 |
| | **PANEL C: Bolsa Familia total payments** | | | | | | | |
| level-OLS | 0.003 | 0.000 | 0.004 | -0.005 | -0.003 | -0.009 | -0.005 | 0.007 |
| | (0.003) | (0.006) | (0.004) | (0.012) | (0.002) | (0.008) | (0.004) | (0.012) |
| log-OLS | 0.131* | 0.150 | 0.183** | 0.197 | 0.020 | -0.035 | 0.001 | -0.049 |
| | (0.069) | (0.102) | (0.089) | (0.138) | (0.049) | (0.105) | (0.061) | (0.131) |
| Observations | 11038 | 5107 | 11038 | 5107 | 7043 | 2588 | 7043 | 2588 |
| | **PANEL D: Local tax revenues** | | | | | | | |
| level-OLS | 0.145*** | 0.064 | 0.008 | -0.043 | 0.0587** | 0.0748*** | 0.00459 | 0.0715** |
| | (0.047) | (0.064) | (0.053) | (0.077) | (0.026) | (0.027) | (0.031) | (0.032) |
| log-OLS | 0.104* | 0.042 | 0.134** | 0.093 | 0.199*** | 0.256*** | 0.187*** | 0.277*** |
| | (0.054) | (0.065) | (0.067) | (0.083) | (0.052) | (0.083) | (0.061) | (0.094) |
| Observations | 6952 | 3199 | 6952 | 3199 | 4347 | 1649 | 4347 | 1649 |
| Municipality FE | Yes | Yes | Yes | Yes | No | No | No | No |
| First-order polynomial | No | No | Yes | Yes | No | No | Yes | Yes |

The table reports regression estimates associating other sources of municipal revenue to law-implied FPM Transfers. Panel A-D report estimates for state-level government transfers, federal transfers (net of FPM), local tax revenue and Bolsa Familia payments as the dependent variable, respectively. Row (1) in each panel reports OLS coefficient estimates when both the dependent and the independent variable ( law-implied transfers) are expressed in levels (no transformation). Row (2) reports OLS coefficient estimates when both variables are expressed in logs. Specifications (1)-(4) report local regression (RD) fixed-effect estimates in levels that restrict estimation in the neighborhood of the FPM cutoffs using two relative bandwidths (4% and 2%), without and with a rectangular kernel (first-order polynomial), respectively. Specifications (5)-(8) report estimates in differences. We construct municipal law-implied transfers applying the FPM allocation mechanism formula (see appendix). All specifications include state-year and cutoff-year dummies (constants not reported). Heteroskedasticity-adjusted standard errors clustered at the micro-region are reported in parentheses below the coefficients. Significantly different from zero at 99% (***), 95% (**) and 90% (*) confidence level.

**Table 6 - FPM Transfers and Public Sector Labor Market Outcomes**
**Local Estimates in log-levels and log-differences**

| | local estimates in levels | | | | local estimates in differences | | | |
|---|---|---|---|---|---|---|---|---|
| bandwidth | <4% | <2% | <4% | <2% | <4% | <2% | <4% | <2% |
| dep.var. | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | **Panel A: Reduced-Form Estimates** | | | | | | | |
| log (Total Earnings) | 0.214*** | 0.171*** | 0.242*** | 0.230*** | 0.093*** | 0.079* | 0.0929** | 0.097 |
| | (0.033) | (0.040) | (0.040) | (0.054) | (0.031) | (0.043) | (0.037) | (0.059) |
| log (Employment) | 0.159*** | 0.135*** | 0.180*** | 0.160*** | 0.071** | 0.069* | 0.070** | 0.083 |
| | (0.030) | (0.040) | (0.038) | (0.052) | (0.029) | (0.039) | (0.034) | (0.051) |
| log (Wage per Worker) | 0.055*** | 0.036 | 0.062*** | 0.070** | 0.021 | 0.010 | 0.022 | 0.014 |
| | (0.017) | (0.026) | (0.022) | (0.027) | (0.016) | (0.022) | (0.018) | (0.032) |
| | **Panel B: Fuzzy RD (IV) Estimates** | | | | | | | |
| log (Total Earnings) | 0.218*** | 0.177*** | 0.246*** | 0.237*** | 0.098*** | 0.084** | 0.098*** | 0.104* |
| | (0.032) | (0.039) | (0.040) | (0.053) | (0.031) | (0.042) | (0.038) | (0.057) |
| log (Employment) | 0.162*** | 0.140*** | 0.183*** | 0.165*** | 0.075*** | 0.073* | 0.074** | 0.089* |
| | (0.030) | (0.039) | (0.037) | (0.051) | (0.029) | (0.038) | (0.035) | (0.049) |
| log (Wage per Worker) | 0.057*** | 0.037* | 0.063*** | 0.072*** | 0.023 | 0.010 | 0.024 | 0.015 |
| | (0.017) | (0.022) | (0.022) | (0.027) | (0.016) | (0.022) | (0.019) | (0.031) |
| Observations | 10759 | 4926 | 10759 | 4926 | 6823 | 2465 | 6823 | 2465 |
| Municipality FE | Yes | Yes | Yes | Yes | No | No | No | No |
| State-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cutoff-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| First-order polynomial | No | No | No | No | Yes | Yes | Yes | Yes |

The table reports regression estimates associating municipal public sector labor market outcomes to law-implied FPM Transfers. Panel A and B report Reduced-Form and Fuzzy RD (IV) estimates, respectively. Rows (1)-(3) reports fixed-effect regression coefficient estimates on Total Earnings, Employment and Wage per Worker. Specifications (1)-(4) report local regression (RD) fixed-effect estimates in levels that restrict estimation in the neighborhood of the FPM cutoffs using two relative bandwidths (4% and 2%), without and with a rectangular kernel (first-order polynomial), respectively. Specifications (5)-(8) report estimates in differences. We construct municipal law-implied transfers applying the FPM allocation mechanism formula (see appendix). All specifications include state-year and cutoff-year dummies (constants not reported). Heteroskedasticity-adjusted standard errors clustered at the micro-region are reported in parentheses below the coefficients. Significantly different from zero at 99% (***), 95% (**) and 90% (*) confidence level.

**Table 7 - FPM Transfers and Private Sector Labor Market Outcomes**
**Local Estimates in log-levels and log-differences**

| | local estimates in levels | | | | local estimates in differences | | | |
|---|---|---|---|---|---|---|---|---|
| bandwidth | <4% | <2% | <4% | <2% | <4% | <2% | <4% | <2% |
| dep.var. | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | **Panel A: Reduced-Form Estimates** | | | | | | | |
| log (Total Earnings) | 0.190*** | 0.124** | 0.230*** | 0.212*** | 0.126*** | 0.152** | 0.126*** | 0.207*** |
| | (0.044) | (0.058) | (0.052) | (0.072) | (0.035) | (0.061) | (0.042) | (0.077) |
| log (Employment) | 0.168*** | 0.096* | 0.211*** | 0.155*** | 0.103*** | 0.115** | 0.098** | 0.141* |
| | (0.045) | (0.054) | (0.055) | (0.069) | (0.034) | (0.058) | (0.040) | (0.075) |
| log (Average Wage) | 0.019 | 0.026 | 0.015 | 0.055* | 0.021 | 0.035 | 0.027 | 0.064** |
| | (0.023) | (0.028) | (0.028) | (0.033) | (0.018) | (0.024) | (0.020) | (0.029) |
| | **Panel B: Fuzzy RD (IV) Estimates** | | | | | | | |
| log (Total Earnings) | 0.194*** | 0.128** | 0.235*** | 0.219*** | 0.134*** | 0.160*** | 0.135*** | 0.222*** |
| | (0.044) | (0.058) | (0.052) | (0.072) | (0.036) | (0.059) | (0.043) | (0.076) |
| log (Employment) | 0.172*** | 0.099* | 0.215*** | 0.160*** | 0.110*** | 0.121** | 0.104** | 0.151** |
| | (0.045) | (0.054) | (0.055) | (0.068) | (0.035) | (0.056) | (0.041) | (0.073) |
| log (Average Wage) | 0.019 | 0.027 | 0.015 | 0.057* | 0.023 | 0.036 | 0.029 | 0.068** |
| | (0.022) | (0.028) | (0.028) | (0.033) | (0.018) | (0.023) | (0.021) | (0.028) |
| Observations | 10993 | 5074 | 10993 | 5074 | 7003 | 2572 | 7003 | 2572 |
| Municipality FE | Yes | Yes | Yes | Yes | No | No | No | No |
| State-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cutoff-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| First-order polynomial | No | No | No | No | Yes | Yes | Yes | Yes |

The table reports regression estimates associating municipal private sector labor market outcomes to law-implied FPM Transfers. Panel A and B report Reduced-Form and Fuzzy RD (IV) estimates, respectively. Rows (1)-(3) reports fixed-effect regression coefficient estimates on Total Earnings, Employment and Wage per Worker. Specifications (1)-(4) report local regression (RD) fixed-effect estimates in levels that restrict estimation in the neighborhood of the FPM cutoffs using two relative bandwidths (4% and 2%), without and with a rectangular kernel (first-order polynomial), respectively. Specifications (5)-(8) report estimates in differences. We construct municipal law-implied transfers applying the FPM allocation mechanism formula (see appendix). All specifications include state-year and cutoff-year dummies (constants not reported). Heteroskedasticity-adjusted standard errors clustered at the micro-region are reported in parentheses below the coefficients. Significantly different from zero at 99% (***), 95% (**) and 90% (*) confidence level.

**Table 8 - Federal Transfer Multipliers and Cost of a Job**
**Local Estimates in log-levels and log-differences**

| | local estimates in levels | | | | local estimates in differences | | | |
|---|---|---|---|---|---|---|---|---|
| bandwidth | <4% | <2% | <4% | <2% | <4% | <2% | <4% | <2% |
| dep.var. | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | **PANEL A: Number of Jobs Created per USD 30,000** | | | | | | | |
| Public Employment | 1.01 | 0.87 | 1.15 | 1.03 | 0.47 | 0.45 | 0.46 | 0.55 |
| | (0.19) | (0.24) | (0.23) | (0.31) | (0.18) | (0.23) | (0.22) | (0.30) |
| Private Employment | 2.98 | 1.69 | 3.72 | 2.73 | 1.88 | 1.91 | 1.77 | 2.38 |
| | (0.77) | (0.92) | (0.95) | (1.17) | (0.60) | (0.88) | (0.70) | (1.16) |
| | **PANEL B: Cost of a Job and Output Multiplier** | | | | | | | |
| Cost of a Job in US$ | 7,517 | 11,693 | 6,164 | 7,981 | 12,804 | 12,720 | 13,422 | 10,244 |
| | (1848) | (4919) | (1376) | (2583) | (4815) | (5694) | (5751) | (4230) |
| Output Multiplier | 2.12 | 1.36 | 2.58 | 1.99 | 1.24 | 1.25 | 1.19 | 1.55 |
| | (0.51) | (0.62) | (0.63) | (0.79) | (0.41) | (0.59) | (0.49) | (0.78) |
| Municipality FE | Yes | Yes | Yes | Yes | No | No | No | No |
| State-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cutoff-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| First-order polynomial | No | No | Yes | Yes | No | No | Yes | Yes |

The table reports estimates of job creation and the output multiplier associated with the impact of actual FPM transfers instrumented by law-implied transfers based on the Fuzzy RD (IV) coefficients of Tables 6 and 7. Panel A shows the equivalent number of jobs in the public and private sector labor markets created by a transfer of USD 30,000 in 2016 prices (equivalent to 30,000 Brazilian Reais in 1998 prices). These estimates are calculated by using the standard elasticity formula, that is, multiplying the IV coefficients in Tables 6 and 7 by the sample mean of (employment/fpm transfers) in each specification. Panel B reports the cost of a job and output multipliers. The cost of a job is calculated simply by dividing 30,000 by the total number of jobs (private and public) calculated in Panel A. The mapping from employment estimated to output multiplier is described in section 5.2. Specifications (1)-(4) report local regression (RD) fixed-effect estimates in levels that restrict estimation in the neighborhood of the FPM cutoffs using two relative bandwidths (4% and 2%), without and with a rectangular kernel (first-order polynomial), respectively. Specifications (5)-(8) report estimates in differences. We construct municipal law-implied transfers applying the FPM allocation mechanism formula (see appendix).

**Table 9 - Reduced-Form Estimates by Type of Activity**
**Earnings, Employment and Average Wage in Agriculture, Manufacturing and Services**

| | local estimates in levels | | | | local estimates in differences | | | |
|---|---|---|---|---|---|---|---|---|
| bandwidth | <4% | <2% | <4% | <2% | <4% | <2% | <4% | <2% |
| dep.var. | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | **PANEL A: Agriculture** | | | | | | | |
| log (Total Earnings) | 0.216 | 0.016 | 0.280 | -0.308 | 0.126 | -0.273 | 0.206 | -0.095 |
| | (0.231) | (0.291) | (0.305) | (0.384) | (0.221) | (0.251) | (0.260) | (0.296) |
| log (Employment) | 0.147* | 0.027 | 0.138 | -0.102 | 0.105 | -0.064 | 0.120 | 0.014 |
| | (0.085) | (0.101) | (0.106) | (0.131) | (0.081) | (0.107) | (0.090) | (0.129) |
| log (Average Wage) | -0.052* | -0.043 | -0.025 | -0.001 | 0.009 | -0.031 | 0.022 | 0.005 |
| | (0.027) | (0.029) | (0.034) | (0.033) | (0.029) | (0.034) | (0.036) | (0.041) |
| | **PANEL B: Manufacturing** | | | | | | | |
| log (Total Earnings) | 0.322 | 0.122 | 0.492 | 0.161 | 0.249 | 0.270 | 0.302 | 0.370 |
| | (0.281) | (0.396) | (0.340) | (0.465) | (0.319) | (0.513) | (0.370) | (0.613) |
| log (Employment) | 0.148 | 0.078 | 0.269** | 0.174 | 0.143* | 0.253* | 0.207** | 0.322* |
| | (0.105) | (0.149) | (0.126) | (0.192) | (0.087) | (0.143) | (0.105) | (0.186) |
| log (Average Wage) | -0.002 | -0.025 | -0.050 | -0.017 | 0.037 | 0.039 | 0.011 | 0.034 |
| | (0.040) | (0.052) | (0.052) | (0.061) | (0.041) | (0.056) | (0.046) | (0.069) |
| | **PANEL C: Services** | | | | | | | |
| log (Total Earnings) | 0.186*** | 0.151** | 0.176*** | 0.210*** | 0.155*** | 0.189*** | 0.137*** | 0.202** |
| | (0.047) | (0.058) | (0.057) | (0.072) | (0.039) | (0.063) | (0.047) | (0.079) |
| log (Employment) | 0.156*** | 0.146** | 0.148** | 0.195** | 0.108*** | 0.169*** | 0.0837* | 0.170** |
| | (0.043) | (0.054) | (0.053) | (0.068) | (0.036) | (0.060) | (0.044) | (0.078) |
| log (Average Wage) | 0.030 | -0.002 | 0.025 | 0.012 | 0.0472*** | 0.0184 | 0.0544** | 0.030 |
| | (0.023) | (0.027) | (0.026) | (0.034) | (0.018) | (0.026) | (0.021) | (0.031) |
| Municipality FE | Yes | Yes | Yes | Yes | No | No | No | No |
| State-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cutoff-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| First-order polynomial | No | No | Yes | Yes | No | No | Yes | Yes |

The table reports regression estimates associating municipal private sector labor market outcomes to law-implied FPM Transfers according to the type of activity. Panel A-C report estimates on Agriculture, Manufacturing and Services, respectively. Rows (1)-(3) in each panel reports fixed-effect regression coefficient estimates on Total Earnings, Employment and Wage per Worker. Specifications (1)-(4) report local regression (RD) fixed-effect estimates in levels that restrict estimation in the neighborhood of the FPM cutoffs using two relative bandwidths (4% and 2%), without and with a rectangular kernel (first-order polynomial), respectively. Specifications (5)-(8) report estimates in differences. We construct municipal law-implied transfers applying the FPM allocation mechanism formula (see appendix). All specifications include state-year and cutoff-year dummies (constants not reported). Heteroskedasticity-adjusted standard errors clustered at the micro-region are reported in parentheses below the coefficients. Significantly different from zero at 99% (***), 95% (**) and 90% (*) confidence level.

**Table 10 - Firm Entry and Firm Size in Agriculture, Manufacturing and Services**

| | local estimates in levels | | | | local estimates in differences | | | |
|---|---|---|---|---|---|---|---|---|
| bandwidth | <4% | <2% | <4% | <2% | <4% | <2% | <4% | <2% |
| dep.var. | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | **PANEL A: Number of Firms** | | | | | | | |
| All Firms | 0.064** | 0.047 | 0.079*** | 0.075** | 0.035** | 0.054** | 0.037** | 0.042 |
| | (0.025) | (0.031) | (0.031) | (0.037) | (0.016) | (0.024) | (0.016) | (0.027) |
| Agriculture | 0.036 | -0.025 | 0.024 | -0.057 | 0.039 | -0.032 | 0.030 | -0.030 |
| | (0.045) | (0.057) | (0.059) | (0.074) | (0.034) | (0.051) | (0.034) | (0.054) |
| Manufacturing | 0.091** | 0.026 | 0.084 | 0.030 | 0.034 | 0.014 | 0.032 | -0.015 |
| | (0.044) | (0.064) | (0.053) | (0.074) | (0.037) | (0.063) | (0.038) | (0.066) |
| Services | 0.074*** | 0.087*** | 0.094*** | 0.120*** | 0.054*** | 0.100*** | 0.054*** | 0.089*** |
| | (0.026) | (0.033) | (0.032) | (0.039) | (0.020) | (0.031) | (0.020) | (0.034) |
| | **PANEL B: Firm Size** | | | | | | | |
| All Firms | 0.045 | 0.020 | 0.055 | 0.035 | 0.031 | 0.055 | 0.030 | 0.056 |
| | (0.034) | (0.044) | (0.043) | (0.056) | (0.030) | (0.048) | (0.030) | (0.051) |
| Agriculture | 0.030 | -0.032 | 0.024 | -0.060 | 0.029 | -0.043 | 0.045 | -0.066 |
| | (0.058) | (0.077) | (0.074) | (0.100) | (0.067) | (0.097) | (0.068) | (0.104) |
| Manufacturing | -0.036 | 0.031 | 0.020 | 0.112 | -0.054 | 0.090 | -0.062 | 0.083 |
| | (0.073) | (0.094) | (0.092) | (0.124) | (0.073) | (0.100) | (0.075) | (0.105) |
| Services | 0.051 | 0.043 | 0.024 | 0.055 | 0.058* | 0.088* | 0.053 | 0.093* |
| | (0.032) | (0.041) | (0.040) | (0.052) | (0.032) | (0.051) | (0.033) | (0.054) |
| Municipality FE | Yes | Yes | Yes | Yes | No | No | No | No |
| State-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cutoff-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| First-order polynomia | No | No | Yes | Yes | No | No | Yes | Yes |

The table reports regression estimates associating private sector firm entry (Panel A) and firm size (Panel B) to law-implied FPM Transfers according to the type of activity. Rows (1)-(4) in each panel reports fixed-effect OLS coefficient estimates on (1) firms in all sectors, (2) agriculture, (3) manufacturing and (4) services. Column (1)-(4) report local regression (RD) fixed-effect estimates in levels that restrict estimation in the neighborhood of the FPM cutoffs using two relative bandwidths (4% and 2%), without and with a rectangular kernel (first-order polynomial), respectively. Column (5)-(8) report estimates in differences. We construct municipal law-implied transfers applying the FPM allocation mechanism formula (see appendix). All specifications include state-year and cutoff-year dummies (constants not reported). Heteroskedasticity-adjusted standard errors clustered at the micro-region are reported in parentheses below the coefficients. Significantly different from zero at 99% (***), 95% (**) and 90% (*) confidence level.

**Table 11 - Employment Estimates Heterogeneity Analysis (standardized coefficients)**

| | local estimates in levels | | | | local estimates in differences | | | |
|---|---|---|---|---|---|---|---|---|
| bandwidth | <4% | <2% | <4% | <2% | <4% | <2% | <4% | <2% |
| dep.var. | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | **Panel A: Municipal Income** | | | | | | | |
| High Income | 0.037*** | 0.019 | 0.049*** | 0.035* | 0.013 | 0.006 | 0.011 | 0.015 |
| | (0.014) | (0.016) | (0.016) | (0.020) | (0.009) | (0.017) | (0.011) | (0.022) |
| Low Income | 0.058*** | 0.036* | 0.070*** | 0.052** | 0.076*** | 0.093** | 0.074*** | 0.104** |
| | (0.016) | (0.020) | (0.018) | (0.023) | (0.024) | (0.040) | (0.026) | (0.046) |
| | **Panel B: Financial Development** | | | | | | | |
| More Developed | 0.034*** | 0.020 | 0.043*** | 0.036* | 0.028** | 0.036* | 0.025* | 0.046* |
| | (0.013) | (0.015) | (0.015) | (0.019) | (0.012) | (0.021) | (0.015) | (0.028) |
| Less Developed | 0.084*** | 0.048** | 0.092*** | 0.063** | 0.085** | 0.087 | 0.082** | 0.093 |
| | (0.017) | (0.023) | (0.019) | (0.026) | (0.033) | (0.064) | (0.034) | (0.066) |
| | **Panel C: Governance Quality** | | | | | | | |
| Better Governance | 0.051*** | 0.035* | 0.064*** | 0.052** | 0.025** | 0.023 | 0.022 | 0.033 |
| | (0.014) | (0.019) | (0.016) | (0.023) | (0.012) | (0.022) | (0.014) | (0.028) |
| Worse Governance | 0.043*** | 0.018 | 0.055*** | 0.035* | 0.063*** | 0.076** | 0.060*** | 0.085** |
| | (0.014) | (0.016) | (0.017) | (0.019) | (0.021) | (0.035) | (0.023) | (0.041) |
| | **Panel D: Municipality Size** | | | | | | | |
| Cutoff 1-3 | 0.057*** | 0.033* | 0.061*** | 0.043** | 0.044*** | 0.049* | 0.042** | 0.057* |
| | (0.015) | (0.018) | (0.016) | (0.020) | (0.017) | (0.027) | (0.017) | (0.031) |
| Cutoff 4-7 | 0.010 | 0.004 | 0.023 | 0.040 | 0.0339* | 0.038 | 0.025 | 0.063 |
| | (0.020) | (0.025) | (0.026) | (0.036) | (0.018) | (0.028) | (0.023) | (0.042) |
| | **Panel E: Geography** | | | | | | | |
| North | 0.044* | 0.012 | 0.057** | 0.029 | 0.079*** | 0.078* | 0.078*** | 0.090* |
| | (0.024) | (0.029) | (0.026) | (0.033) | (0.028) | (0.047) | (0.030) | (0.054) |
| South | 0.049*** | 0.037** | 0.061*** | 0.052*** | 0.014 | 0.024 | 0.012 | 0.034 |
| | (0.012) | (0.015) | (0.015) | (0.018) | (0.010) | (0.017) | (0.011) | (0.022) |
| Municipality FE | Yes | Yes | Yes | Yes | No | No | No | No |
| State-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Cutoff-year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| First-order polynomial | No | No | Yes | Yes | No | No | Yes | Yes |

The table reports heterogeneity analysis estimates associating municipal private sector employment to law-implied FPM Transfers. Panel A-E exhibit heterogeneity estimates according to High/Low Municipal Income in 2000, More/Less Financial Development measured by the presence of a commercial bank branch in the municipality, High/Low Governance Quality as reported by Naritomi, Soares, and Assuncao (2012), Municipality Size and Geography (see Appendix Table 1). High/Low are defined as above/below the sample median. A Bust (Boom) is defined when growth in total earnings in the previous year was negative (positive). Specifications (1)-(4) report local regression (RD) fixed-effect estimates in levels that restrict estimation in the neighborhood of the FPM cutoffs using two relative bandwidths (4% and 2%), without and with a rectangular kernel (first-order polynomial), respectively. Specifications (5)-(8) report estimates in differences. We construct municipal law-implied transfers applying the FPM allocation mechanism formula (see appendix). All specifications include state-year and cutoff-year dummies (constants not reported). Heteroskedasticity-adjusted standard errors clustered at the micro-region are reported in parentheses below the coefficients. Significantly different from zero at 99% (***), 95% (**) and 90% (*) confidence level.