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Regional Transfer Multipliers

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A series of discontinuities in the allocation mechanism of federal transfers to municipal governments in Brazil allow us to identify the causal effect of public spending on local labour markets, using a "fuzzy" Regression Discontinuity Design (RDD). Our estimates imply a cost per job of about 8,000 US dollars per year and a local income multiplier around two. The effect comes mostly from employment in services and is more pronounced among less financially developed municipalities.

Key words: Natural experiment, "fuzzy" RD, Government spending, Employment, Wages.

JEL Codes: E62, H72, C26

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 (Nakamura and Steinsson, 2014; Farhi and Werning, 2016). This debate goes back to the contributions of Robert (Mundell, 1961, 1973) and Peter Kenen (1969). Besides important theoretical issues, there is limited applied research quantifying the impact of regional transfers on local economies inside a currency union.

In this article, we examine the impact of transfers-driven municipal expenditure on local labour 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 cutoffs provide locally (*i.e.* close to the discontinuities) exogenous variation to identify the causal effects of externally-financed municipal spending on economic activity. Our analysis exploits variation from more than 3,000 municipalities over the period 1999–2014 using high-quality micro-data covering the bulk of private formal economy and public sector

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employment contracts. The focus on numerous small geographical units over time allows us to control for time-invariant municipal factors, related to geography, history, cultural and institutional features, and for country-wide and state trends, related to monetary policy, federal fiscal policy, and the business cycle.

Our analysis yields five main findings. First, changes in local government expenditure stemming from "locally" exogenous shifts in federal transfers are associated with a significant boost in employment in the formal sector. A USD 30,000 increase in municipal spending is associated with one extra job in the public 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 multipliers in the range of 1.3 to 2. As our data and estimates refer to the formal economy, we discuss the potential impact of the informal sector and present suggestive calculations of a slightly larger multiplier when accounting for the informal economy. Fourth, transfers are associated with increased firm entry. Fifth, most of the private sector employment response comes from services and is concentrated among less financially developed municipalities, consistent with the predictions of a prototypical open-economy New-Keynesian (NK) model.

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 Nakamura and Steinsson, 2014; Ulyssea, 2018; Serrato and Wingender, 2016; Clemens and Miran, 2012; Fishback and Kachanovskaya, 2010; Fishback and Cullen, 2013. For a thorough review, see Chodorow-Reich, 2018). These studies typically report local multipliers in the range of 1–2.6. 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. But we also seek to advance this empirical literature along three more novel dimensions.

First, we 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 U.S. and a few other advanced economies.[1] Notable exceptions are the cross-country analyses of Kraay (2012, 2014) and two recent independently developed works that analyse the impact of aggregate demand shocks in Brazil. Braga *et al*. (2017) find that transfers-driven local shocks increase low-skill employment. Bernstein *et al*. (2018) explore the impact of global commodity prices on entrepreneurship. Similar versions of the Brazilian revenue-sharing mechanism between national and local governments is also present in several developed and developing countries, thereby making our empirical analysis of a wider relevance for understanding the stabilization effects of redistributive transfer mechanisms in a currency union.[2]

Second, building on advances in labour economics (Angrist and Lavy, 2001; Hahn *et al*., 2001; Van der Klaauw, 2008), we bring into applied macroeconomic research a "fuzzy" regression discontinuity approach to identify the effects of fiscal policy.[3] In this regard, our work connects with works in political economy that examine the effect of federal transfers in Brazil on political

1. For instance, Chodorow-Reich (2018) surveys thirteen (out of a total of fourteen) papers on industrial countries; he further discusses seven empirical works focusing on various aspects of the 2009 American Recovery and Reinvestment Act (*e.g.* Feyrer and Sacerdote, 2012, Chodorow-Reich *et al*., 2012). For example, Acconcia *et al*. (2014) and Porcelli and Trezzi (2014) study the effects of local government spending across Italian provinces and municipalities, whereas Bruckner and Tuladhar (2014) focus on Japanes prefectures.

2. Industrial country examples include Germany (Baskaran *et al*., 2016), Norway (Sørensen, 2017), Belgium, the Czech Republic (Bergvall *et al*., 2006), the U.S. (Serrato and Wingender, 2016) and the European Union (Becker *et al*., 2013, 2010; Coelho, 2018). Examples of transfer schemes in developing countries are Turkey (Bergvall *et al*., 2006), Iraq (Aresti, 2016), and Nigeria (Qiao and Anwar, 2008).

3. See Fuchs-Schuendeln 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.

outcomes applying RD methods (Brollo *et al.*, 2013; Litschig and Morrison, 2013; Gadenne, 2017).

Third, the richness of the quasi-experimental variation and the vast differences of Brazilian localities also allows us to move beyond average effects and explore heterogeneity. In particular, we connect with a recent stream of theoretical works which emphasize the role of trade openness and liquidity constraints in altering the transmission of government spending, using an otherwise standard open-economy currency union New-Keynesian framework (Corsetti *et al.*, 2013; Nakamura and Steinsson, 2014; Farhi and Werning, 2016). Our empirical results are consistent with the predictions of this class of currency union models.

The article 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 multipliers. In Section 5, we analyse the heterogeneous response of employment and labour income across municipalities along two dimensions suggested by theory, openness, and liquidity constraints. Section 7 summarizes. The Online Appendix provides details on the data, summary statistics, examples of the allocation mechanism, and various sensitivity checks. It also reports counter-factual 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: (1) the federal union, (2) 26 states and 1 federal district, and (3) 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 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.[4]

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

It allocates funds to municipalities yearly according to a predetermined procedure as follows. 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, a fixed share

4. In comparison, Bolsa Familia, the largest conditional cash transfer programme in the world targeting low-income households, distributed R$8.2 billion in 2006 prices (*US*$4.1 billion in 2016 prices).

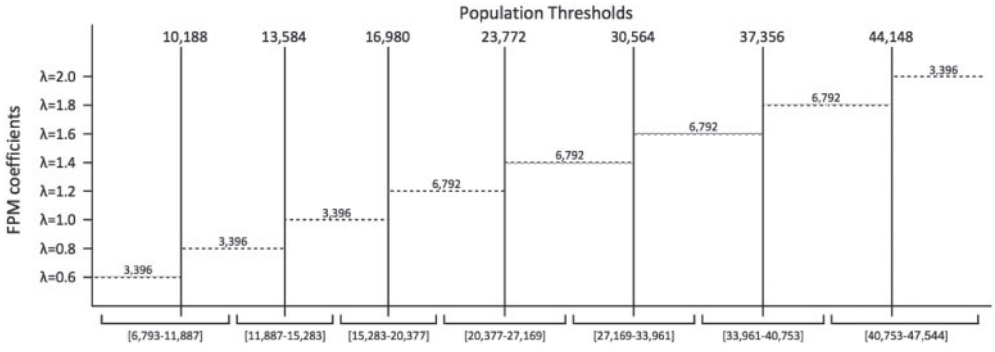

FIGURE 1
FPM coefficients and population brackets

is allocated to each state.[5] 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:

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

where FPM^k is the amount of resources allocated to state k. λ_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. In Figure 1, we plot FPM coefficients across the 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. First, municipalities in the same bracket (in a given year and state) should get the exact same amount, independently 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 thirteen 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.[6]

5. The state shares of FPM transfers (reported in Online 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.

6. In the Online 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.

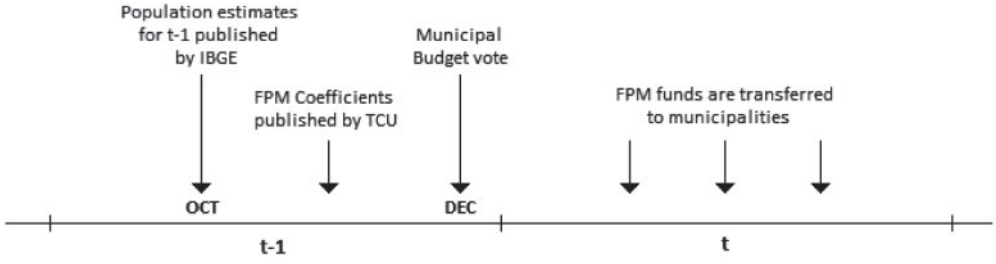

FIGURE 2
FPM allocation timelines

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 31st of each year. 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 year-end and FPM funds are then transferred during the following year.

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 *et al.* (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. In all, 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 neighbourhood 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.[7]

7. In Online Appendix Table 2, we report the number of observations (municipality-years) around each threshold. In Online Appendix Table 3, we report the number of observations in the "local" sample by cutoff, while Online Appendix Tables 4–6 give further descriptive patterns.

TABLE 1
Descriptive evidence. Distribution of municipalities; "Control" and "Treatment" groups

Panel A: full sample			
	No movement	Moves to higher bracket	Total Total
No movement	1,410	1,087	2,497
Moves to lower bracket	93	689	782
Total	1,503	1,776	3,279
Panel B: restricted sample in the neighbourhood of the FPM cutoffs (<4%)			
	No movement	Moves to higher bracket	Total Total
No movement	601	976	1,577
Moves to lower bracket	146	172	318
Total	747	1,148	1,895

Notes: 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%).

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 labour market outcomes (income, wages, and employment), we use the *Relação Anual de Informações Sociais* (RAIS; Ministry of Labour Administrative Dataset), over 1999–2014. This is a high-quality administrative dataset assembled yearly by the Brazilian Ministry of Labour. Effectively, it is a census of the Brazilian formal labour 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 (Saboia and Tolipan, 1985; De Negri *et al*., 2001; Amorim *et al*., 2006).[8] We aggregate the micro-data at the municipal level and construct total earnings, mean wages, and employment at the private and local government sector. 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 large number of recent works use RAIS data to study a plethora of questions.[9]

In Table 2, we report summary statistics for population and municipal public finances. For income per capita, 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).[10] For the same year, income per capita at the national level was BRL 3,600. This disparity reflects bigger cities' higher income. For example, income per capita São

8. Formal sector accounts for 55–60% of salaried labour 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.

9. RAIS covers nearly all formally employed workers with a signed work-card, providing access to benefits and offering legal labour 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 *et al*. (2017), Krishna *et al*. (2014), Lopes de Melo (2018), and Menezes-Filho and Muendler (2011).

10. While municipal GDP is available at yearly frequency from IBGE (see Corbi *et al*., 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

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	SD	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	1929	10,034
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	1882	8,309
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	1901	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	1948	8,451
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	2041	4,785
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	2221	3,027
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	2343	1,989
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	2327	720
Total	0.012	0.047	0.31	0.06	0.23	0.14	0.27	0.16	0.33	0.22	0.09	1978	43,466

Notes: 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.

Paulo and Rio de Janeiro, which account for almost 20% of the country's population, was around BRL 18,900 in 2013. 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%.[11] Turning to municipal government 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.[12]

3. IDENTIFICATION

In this section, we first describe the fuzzy regression discontinuity design that allows us to isolate the effect of spending driven by regional transfers on local labour 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 pre-determined 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 labour market outcomes in the neighbourhood of the thresholds (Angrist and Pischke, 2008). In Figure 3a, we display 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. Within each population bracket, there is variability in transfers, driven mostly by changes in the total FPM funds which 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. In Figure 3b, we plot 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 programme changes every year, but there are evident jumps of transfers at the FPM 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

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).

11. Other sources include mining and oil royalties, capital income, and many other smaller sources.

12. Municipal local government spending is persistent, with an autoregressive coefficient of around 0.36 unconditionally and 0.54 conditional to moving up one of the population thresholds that trigger an increase in FPM transfers (see Online Appendix Table 8).

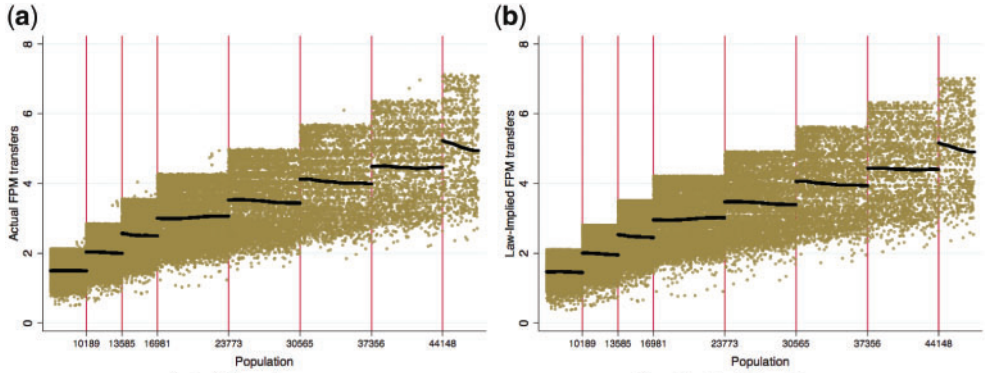

FIGURE 3

Actual and law-implied FPM transfers around the cutoffs. (a) actual transfers; (b) law-implied transfers

movements of municipalities across population thresholds on labour market outcomes running variants of the following specifications in the neighbourhood (h) of the seven cutoffs (c):

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

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

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

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 (γ_{FS}) should be one and the in-sample fit perfect ($R^2 = 1$). The "reduced-form" (RF) specification links labour market outcomes ($Y_{i,t}$)—total earnings, employment, and average wages—to law-implied transfers ($\tilde{T}_{i,t}$).

δ_{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 (λ) coefficient of all municipalities in the state. Municipal fixed-effects, δ_i, account for time-invariant factors shaping municipal fiscal policy and economic conditions, related to geography, ecology, culture, local institutional quality, corruption, etc. δ_{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) 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 *et al.* (2001), Van der Klaauw (2002), and subsequent works in a similar context to ours (*e.g.* Brollo *et al.* (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 labour market conditions of locally exogenous changes in federal transfers, stemming from the enforceability of the law, close to the FPM cutoffs.[13]

13. See also Hinnerich and Pettersson-Lidbom (2014), and Pettersson-Lidbom (2012).

The fuzzy-RD model reads:

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

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

where $\hat{T}_{i,t}$ denotes the component of 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 neighbourhood 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-RDD) models without including any RDD 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]

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 *et al.*, 2018). 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.*, 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 hard-to-account-for dimensions.

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, and also include linear RDD polynomials on normalized populations on both periods, so that we correctly account for population growth. We also require that municipalities remain within the same bandwidth around any given population threshold in both years t and $t-1$. 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 auto-correlation 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.

14. The use of relative as opposed to absolute sized neighbourhoods is due to the fact that the number of municipalities decrease in population. In order to avoid losing 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 includes 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 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".

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. *Identifying assumptions*

Our RD design relies on four identifying assumptions.

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 a fuzzy-RD framework 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 programme 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. Columns (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]

In Figure 4, we present an illustration of FPM transfers at the law-implied population cutoff. We first net state, year, and cutoff fixed 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 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 condition for identification is that municipal revenues and municipal expenditure change abruptly at the cutoffs.

In Table 4—Panel *A*, columns (1)–(4), we report level and log-level OLS estimates with municipality fixed-effects associating municipal revenues to law-implied FPM transfers in the neighbourhood of the FPM cutoffs. The coefficient on law-implied FPM transfers in the level specifications is highly significant and, while somewhat unstable, fluctuates 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 they account for outliers. The estimates of the specifications in differences reported in

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 (Online Appendix Table 9).

19. The figure also reports a split third-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 discontinuity 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.

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	11,333	5,643	11,333	5,643	11,053	5,113	110,53	5,113	7,054	2,594	7,054	2,594
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

Notes: 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 neighbourhood 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 Online 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.

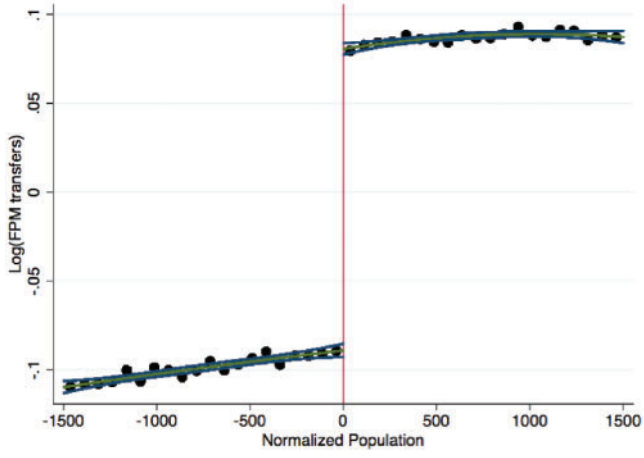

FIGURE 4
FPM transfers around the cutoffs

columns (5)–(8) also move around 1 in levels and around 0.32 in logs, close to the share of FPM in municipal revenues (0.31, Table 2).

In Figure 5a, we provide 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.

In Table 4—Panel *B*, we report 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.[20] Log-OLS estimates are less sensitive to outliers and lie in the interval 0.30–0.34, quite close to the share of FPM transfers to municipal spending (0.31, see Table 2). The difference specifications are also highly significant, further showing that municipal expenditure responds to changes in law-implied FPM transfers. In Figure 5b, we plot average municipal expenditure below and above the pooled discontinuities. While more noisy than FPM transfers, municipal spending visibly changes discontinuously across the FPM cutoffs.[21]

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 labour 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;

20. The slight instability of estimates is a common feature when we add polynomials in standard RD applications. Figure 5 yields clear-cut graphical results as it averages out noise.

21. We also examined how municipal revenues and spending move in the neighbourhood of each of the seven FPM cutoffs separately (Online Appendix Tables 10 and 11). Online Appendix Table 12 shows that both current and capital expenditure increase at the discontinuities. Online Appendix Table 13 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 Online Appendix, 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 these "fake" cutoffs.

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%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A—Municipal revenue							
Level-OLS	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-OLS	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							
Level-OLS	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-OLS	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	11,053	5,113	11,053	5,113	7,054	2,594	7,054	2,594
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

Notes: 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 neighbourhood 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 Online 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.

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 labour market outcomes, concerns that cities may differ systematically across geographic, institutional or other features (which apply to cross-sectional approaches) are not particularly severe. Municipalities just above and just below the FPM thresholds do not differ considerably (see Brollo *et al*., 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 programmes. These issues are, however, unlikely in our setting: municipalities run balanced budgets and their expenditure tightly adjusts to their revenues.

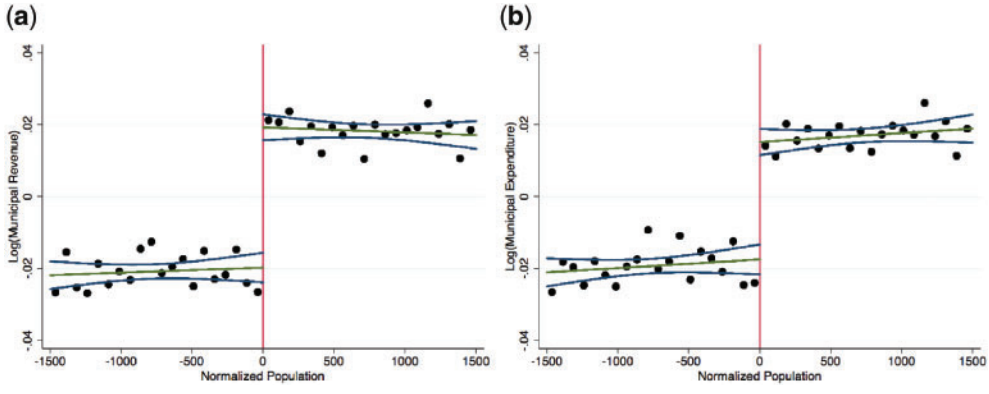

FIGURE 5

Municipal revenues and expenditure. (a) municipal revenues; (b) municipal expenditure

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 neighbourhood 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 an increase on local tax revenues.[22]

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

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 labuor market outcome, then the estimates will not identify the causal effect of regional transfers.[23] Although it is hard to directly test such assumption, we only comment

22. 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).

23. 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."

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%
	(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	11,017	5,100	11,017	5,100	7,015	2,583	7,015	2,583
	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	11,017	5,096	11,017	5,096	7,013	2,580	7,013	2,580
	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	6,952	3,199	6,952	3,199	4,347	1,649	4,347	1,649
	Panel D: Local tax revenues							
Level-OLS	0.145***	0.064	0.008	−0.043	0.0587**	0.0748***	0.0046	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	11,038	5,107	11,038	5,107	7,043	2,588	7,043	2,588
Municipality FE	Yes	Yes	Yes	Yes	No	No	No	No
First-order polynomial	No	No	Yes	Yes	No	No	Yes	Yes

Notes: 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), Bolsa Familia payments and local tax revenue 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 neighbourhood 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 Online 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.

here (for brevity) and report in the Online Appendix two sets of exercises. First, we examine whether the 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. Second, to minimize any manipulation concern, we estimate specifications focusing solely on municipality-years with either no movement across FPM intervals or falling into *lower* population intervals. Our results are not overturned by this modification.

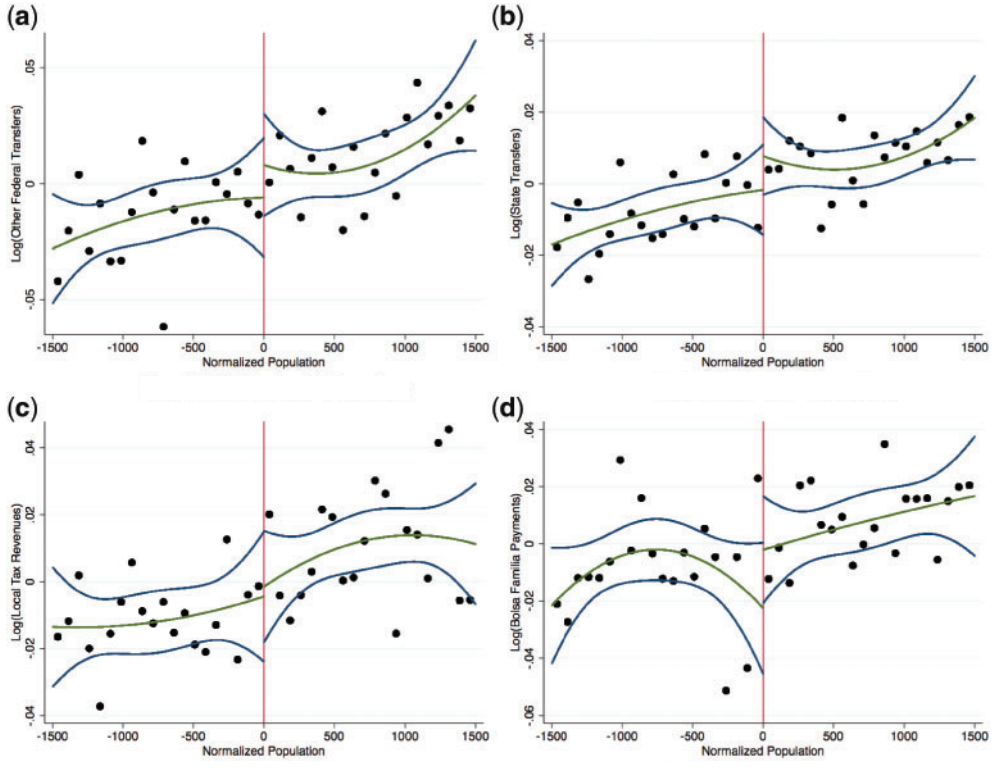

FIGURE 6

Types of municipal revenues around the cutoffs. (a) Non-FPM federal transfers; (b) state-level Gov transfers; (c) local tax revenues; (d) Bolsa familia payments

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. Sixth, we provide some evidence looking at firm entry.[24]

4.1. *Employment and wages in the public sector*

In Table 6, we report RD specifications that associate total municipal earnings, employment and wages to law-implied FPM transfers in the neighbourhood of the FPM cutoffs (4% bandwidth in odd-numbered columns and 2% bandwidth in even-numbered columns). In Panel *A*, we record reduced-form log-level (columns (1)–(4)) and log-difference (columns (5)–(8)) OLS

24. In Online Appendix Table 6—Panel *A*, we record 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 each for 44%. Online Appendix Table 6—Panel *B* and *C* report the corresponding statistics for employment and average wages.

TABLE 6

FPM transfers and public sector labour 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*	0.093**	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	10,759	4,926	10,759	4,926	6,823	2,465	6,823	2,465
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

Notes: The table reports regression estimates associating municipal public sector labour 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 neighbourhood 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 Online 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.

specifications, whereas Panel *B* gives the corresponding "fuzzy" RD 2SLS estimates. Columns (1)–(2) and (5)–(6) give local regression estimates; columns (3)–(4) and (7)–(8) add 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.[25]

We then 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

25. These estimates are close to the ones in row (2) of Online 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 different datasets (RAIS—Ministry of Labour administrative dataset and FINBRA—Public Finance of Municipalities, respectively).

TABLE 7
FPM transfers and private sector labour 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	10,993	5,074	10,993	5,074	7,003	2,572	7,003	2,572
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

Notes: The table reports regression estimates associating municipal private sector labour 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 neighbourhood 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 Online 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.

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*). Online Appendix Table 14 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.[26]

4.2. *Employment and wages in the private sector*

Table 7 reports reduced-form (Panel *A*) and fuzzy-RD (Panel *B*) estimates, linking total labour 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

26. 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 (1) rounding of coefficients, and (2) from the different samples due to the fact that for a few municipalities we observe total earnings and/or number of employees, but not wages.

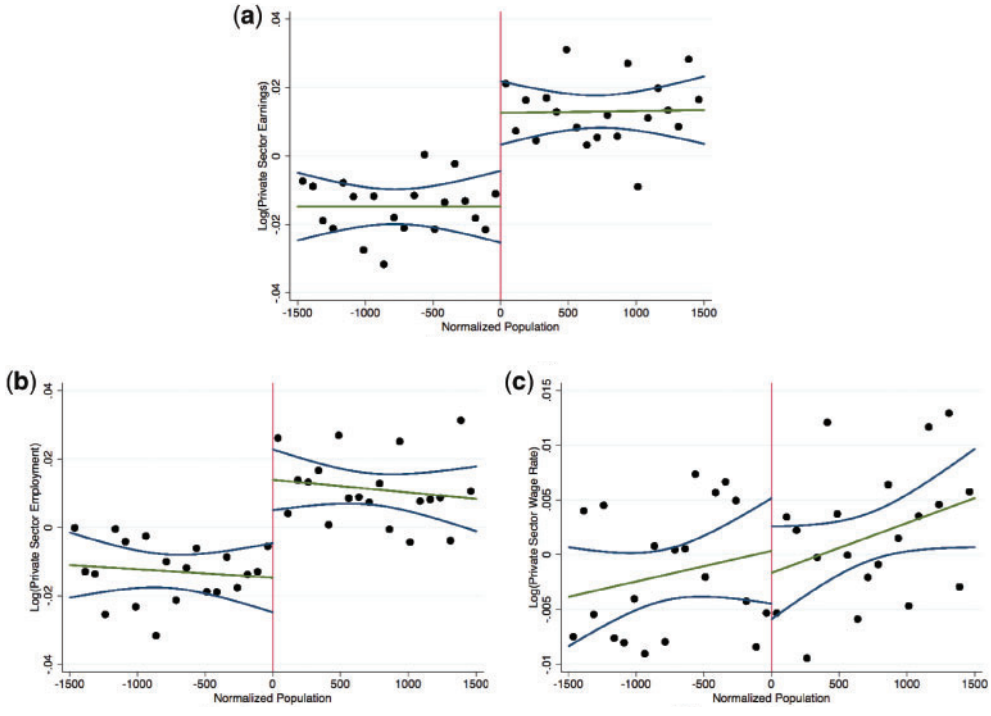

FIGURE 7

Total earnings, employment and wage rate in the private sector. (a) total earnings; (b) employment; (c) wage per worker

the elasticity of actual and law-implied FPM transfers is close to one, the fuzzy-RD estimates are similar, ranging from 0.13 to 0.22. The log-difference specifications (columns (5)–(8)) are also highly significant, ranging between 0.12 and 0.22. Conditional on time-invariant municipal factors, state-specific trends, and city-size (cutoff-specific) time trends, a 20% increase in federal transfers (roughly the step in the FPM allocation function) is associated with a 3%–4% boost in private-sector labour income. This corresponds to roughly the mean of earnings growth (Table 2). In Figure 8a, we provide 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. The inner line is a split third-order polynomial in population size, fitted separately on each side of the pooled FPM thresholds at zero, and outer lines are the 95% confidence interval of the polynomial. There is an evident jump (fall) in total labour earnings of private sector employees when municipalities move to a higher (lower) FPM population interval.

We then examine whether the impact of regional transfers stems 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 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 and 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. The higher level of private employment on the right side of the pooled FPM discontinuities is illustrated in

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
	(1,848)	(4,919)	(1,376)	(2,583)	(4,815)	(5,694)	(5,751)	(4,230)
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

Notes: 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 labour 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. Standard errors are calculated analogously. 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. Standard errors of the cost of a job are calculated using a first-order Taylor approximation. 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 neighbourhood 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 Online Appendix).

Figure 8b. In contrast, Figure 8c 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. In Panel *A*, we report 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 RDD 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 U.S. counties (which are comparable in size to Brazilian municipalities).[27] This is in line with the real wage and productivity gaps between Brazil and the U.S.[28]

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 Wingender, 2016; 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:

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

μ_Y denotes the output/income multiplier and μ_E is the employment multiplier, the inverse of the cost of a job reported in Table 8 (Panel *B*). The parameter χ represents the elasticity of hours per worker to total employment and ξ refers to the share of capital in the production function.

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.

In Table 8 (Panel *B*), we 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).[29] 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). Also in line with these studies, we do not find much evidence of regional spillovers (see Online Appendix Table 27).

In the Online Appendix, we calibrate a standard New-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. 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

27. 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 and 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.* (2017) 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.

28. The World Bank Indicators database approximates GDP per person employed in 2006 (midyear of our analysis) in Brazil at 28,081*USD* and in the U.S. at 102,981*USD*.

29. 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 (U.S. based estimate). The local income multiplier is centered around 2.1, ranging between 1.7 and 2.5 for the specification in column (4).

20%–40%) if local government spending was financed by local tax revenues rather than regional transfers, as in our data.

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 Labour that, as such, only covers the formal economy. Given the importance of the unofficial economy in Brazil, we now present a plausible adjustment of the multiplier estimates to incorporate the effects of transfer-driven municipal spending on the informal economy. Our back-of-the-envelope adjustment proceeds in three steps.

First, we start with the baseline multiplier estimates that regard the official economy (Table 8, Panel *A*). An increase in FPM transfers of *R*$30,000, roughly 1% of total municipal receipts from the federal transfer programme, is associated with four new jobs, three in the private sector and one in the municipal public sector.

Second, we need an estimate of the productivity gap between formal and informal firms. For this, we rely on Meghir *et al*. (2015) careful analysis of formal and informal labour markets in Brazil in the 2000s. Combining longitudinal surveys with a structural equilibrium wage-posting model with heterogeneous firms, they approximate that the productivity contribution of the median informal firm is about 55% of the contribution of registered firms (see also Ulyssea, 2018). This estimate is in accord with a large literature across various settings and countries revealing sizable productivity differences between informal and formal sectors (see La Porta and Shleifer, 2008).

Third, simple tabulations of the Brazilian Labour Force Survey 2002–2007 (PME) for individuals aged 23–65 years reveals that the formality rate of salaried workers is 28%, 67%, and 53% for agriculture, manufacturing, and services, respectively. Schneider (2007) finds that the size of the Brazilian shadow economy is 42%, the median among other 21 Latin American countries. Accordingly, we assume that informal jobs are created at roughly half of the rate of their formal sector counterparts.

Putting together our local multiplier estimates (step 1), the productivity gap of the informal economy (step 2), and our assumption on informal sector job creation (step 3), we get an adjustment factor $4.82/4 \simeq 1.2$. For the numerator, we first multiply the 1.5 informal sector jobs (step 3) with their relative productivity of 55% (step 2) to obtain 0.82 formal-sector-productivity-equivalent informal jobs; to this value of 0.82, we add the three formal private sector jobs and the one municipal sector post associated with the increase in local public spending (step 1). The denominator is simply the sum of jobs created in the formal sector. This suggests that the multiplier estimates (Table 8, Panel *B*) need to be adjusted by roughly 20%. Furthermore, to the extent that the increase in municipal spending may cause the informal economy to shrink (*e.g.* more informal firms choose to register and labour reallocates to the formal sector), then the informal economy adjustment factor will be even lower.

4.5. *Sensitivity analysis*

We have performed a series of robustness checks, which for brevity we discuss in detail in the Online Appendix (See Online Appendix Tables 15–33). In Table 9, we summarize the main sensitivity checks when we focus on the 4% bandwidth. Our findings appear robust to: (1) 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); (2) excluding the census years (2001, 2008, 2011, and 2012), for which there is some evidence of manipulation in the population counts (although manipulation per se does not invalidate the RDD design); (3)

TABLE 9
Main robustness checks (4%-sample around cutoffs). Public and private employment

Robustness analysis		Public employment		Private employment	
		Coeff.	Std. error	Coeff.	Std. error
Excluding observations around threshold 1		0.185***	(0.039)	0.111**	(0.050)
Excluding observations in census years		0.164***	(0.038)	0.144***	(0.056)
Excluding obs with positive changes in population brackets		0.197***	(0.039)	0.181***	(0.054)
Mayor-specific fixed-effects		0.122**	(0.031)	0.092**	(0.039)
Cutoff-state-year fixed effects		0.181***	(0.032)	0.157***	(0.046)
Global RD full sample (3-rd order polynomial)		0.341***	(0.050)	0.228***	(0.063)
Cross-sectional specification without regional FE		0.240***	(0.044)	0.135	(0.131)
Cross-sectional specification with meso-region FE		0.260***	(0.043)	0.204*	(0.105)
Cross-sectional specification with micro-region FE		0.267***	(0.043)	0.288***	(0.098)
2-year differences		0.128***	(0.027)	0.122***	(0.042)
IV estimates with government spending as first-stage		0.463***	(0.085)	0.498***	(0.133)
Dynamic effects	fpm	0.121***	(0.023)	0.045*	(0.028)
	Lag. depvar	0.333***	(0.550)	0.550***	(0.020)
State-year dummies		Yes		Yes	
Cutoff dummies		Yes		Yes	
First-order polynomial		No		Yes	

Notes: The table reports estimates of the main robustness checks associating Public and Private Employment to law-implied FPM Transfers. The complete set of estimates are available in the online appendix. Columns (1) and (3) report local regression (RD) estimates for public and private employment, respectively, that restrict estimation in the 4%-neighbourhood of the FPM cutoffs and columns (2)–(4) report the accompaning heteroskedasticity-adjusted standard errors clustered at the micro-region. The first three rows reports specifications that exclude observations around cutoff 1, in census years (2001, 2008, 2011, and 2012) and with positive changes in population brackets. Rows 4 and 5 include mayor-term-specific fixed-effects and cutoff-state-year fixed effects to the baseline. The next specification uses all the observations from the sample, both close and far from the cutoffs, and condition on 3rd-order population on normalized population. (module of the distance from the nearest cutoff). Rows 7–9 report local RD estimates from simple cross-sectional specifications without any regional dummies, with meso-region and with micro-region dummies, respectively. Row 10 explores a 2-year difference specification and row 11 reports Fuzzy-RD (IV) estimates akin to the ones in Tables 6 and 7, but using government spending in the first-stage as opposed to actual FPM transfers. Row 12 includes the lag of the depedent variable as an additional control to the baseline level specification. We construct municipal law-implied transfers applying the FPM allocation mechanism formula (see Online Appendix). All specifications include state-year and cutoff fixed-effects (constants not reported). Significantly different from zero at 99% (***), 95% (**) and 90% (*) confidence level.

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, by excluding cities that move to a higher population bracket; (4) interacting the municipality constants with mayor-specific indicators to control for mayor's and council's ability; (5) adding state-cutoff-year fixed effects, so as to further account for unobservable time-varying state features; (6) including higher-order polynomials in population and exploring variation from observations both close and far from the cutoffs (as Brollo *et al*., 2013); (7) using a 2-year difference specification, as in Nakamura and Steinsson (2014); (8) augmenting the specification with outcomes aggregated at the regional (micro-region) level to look at geographical spillovers (which appear negligible, a result that is in line with parallel works); (9) allowing transfers to have a delayed effect on labour markets; (10) using municipal government expenditure instead of actual transfers in the first stage, to allow for potential savings or overspending; (11) controlling for inertia adding

TABLE 10
Firm entry and firm size. 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 firms							
FPM transfers	0.068**	0.050	0.084***	0.075**	0.034**	0.054**	0.026	0.056*
	(0.025)	(0.031)	(0.030)	(0.036)	(0.016)	(0.025)	(0.018)	(0.030)
	Panel B: firm size							
FPM transfers	0.075*	0.045	0.091*	0.065	0.033	0.060	0.041	0.086
	(0.034)	(0.042)	(0.042)	(0.054)	(0.030)	(0.048)	(0.034)	(0.058)
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

Notes: 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. Columns (1)–(4) report local regression (RD) fixed-effect estimates in levels that restrict estimation in the neighbourhood 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.

a lagged dependent variable; (12) keeping only observations from municipalities that jump up and down during our sample period, so as to address sample composition issues; (13) when we run cross-sectional specifications, replacing the municipality fixed-effects with coarse (meso) or fine (micro) region constants. In addition, (14) when we do not account at all for local features (with municipality, micro or meso region fixed-effects), the fit of the model deteriorates considerably, making some estimates insignificant for the full sample. When we focus on services, the coefficients retain significance even when we remove geographical fixed effects.

We also show that our results do not reflect "reverse causation" by performing two placebo tests. First, we artificially move all cutoffs to the right (and left) of the population distribution by 500, 750, and 1000 inhabitants and find that neither actual FPM transfers nor municipal expenditure exhibit jumps at the "fake" discontinuities (see Online Appendix Figure D.13). Second, we follow the suggestions of Eggers *et al.* (2018) and undertake a falsification test in which lagged 'treatment' variables are viewed as outcomes in the RD analysis. Lagged municipal revenues and expenditure do not vary abruptly at the FPM cutoff (see Online Appendix Section D.2.4).

4.6. *Further evidence. Firm entry and average firm size*

In Table 10, we exploit the micro-structure of the administrative dataset to examine the impact of regional transfers on the number of firms (Panel *A*) and average firm size of existing firms, as measured by the number of employees (Panel *B*). The estimates show that in response of higher federal transfers, net entry increases substantially. While the impact on the size of the average firm is less statistically significant, the estimated elasticities are of similar magnitude. While this is an interesting novel finding, it is likely that at least partly the increased net entry reflects firm formalization.

5. HETEROGENEITY

In this section, we use the richness of our set-up that includes thousands of municipalities to explore heterogeneity on the impact of federal transfers on private sector labour market outcomes along two crucial dimensions from a theoretical standpoint—trade openness and financial development.[30]

5.1. *Motivation*

Notwithstanding its policy relevance, our focus on heterogeneity is motivated by the predictions of the workhorse open-economy New-Keynesian model when applied to regions inside a currency union (Gali and Monacelli, 2008; Corsetti *et al*., 2013, Nakamura and Steinsson, 2014, Farhi and Werning, 2016, Shoag, 2013, Chodorow-Reich, 2018). While modelling details differ, this framework typically predicts that an increase in local government spending generates a stronger local demand effect (and thus a higher "relative" multiplier, in the jargon of Nakamura and Steinsson, 2014) whenever: (1) a larger share of the public-spending-driven increase in demand is spent on locally produced output and (2) a larger share of the income boost associated with the fiscal expansion is used to finance private consumption due to a hand-to-mouth type of behaviour.

While it is challenging to map theoretical concepts to empirical measures, we consider two ways of proxying openness and financial constraints. Regarding openness, we note that services comprise a significant share of non-tradable and locally produced output, while manufacturing and agriculture goods are more exposed to trade. Thus we re-run our baseline specifications by sectors and explore whether the link between FPM transfers and private employment is stronger and more precisely estimated for services. Regarding liquidity constraints, we build on earlier empirical research that has routinely used private credit, the presence of banks, and income as proxies for consumers' access to credit markets.

5.2. *Trade openness*

In Table 11, we report local RD estimates (in log-levels and log-differences) that examine the impact of law-implied FPM transfers on total earnings, employment and average wage rate in services, manufacturing, and agriculture.

In Panel *A*, we report the 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 the both bandwidths, with and without the RD polynomial, in the level and the difference specifications. The elasticity is tightly estimated, ranging from 0.15 to 0.21. A 20% 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 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.

30. Braga *et al*. (2017) explore another aspect of federal transfers' heterogeneity. They show that small Brazilian municipalities that receive higher FPM transfers experience a boost in private employment among the unskilled, as compared to college and high school graduates. They also document larger employment effects in small localities.

TABLE 11

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: Services				
Log (Total Earnings)	0.186***	0.151**	0.176***	0.210***	0.155***	0.189***	0.137***	0.202**
	(0.047)	(0.059)	(0.056)	(0.072)	(0.039)	(0.062)	(0.047)	(0.079)
Log (Employment)	0.156***	0.146**	0.148**	0.195**	0.108***	0.169***	0.084*	0.170**
	(0.043)	(0.054)	(0.053)	(0.069)	(0.036)	(0.060)	(0.043)	(0.077)
Log (Average Wage)	0.030	0.002	0.025	0.012	0.047***	0.018	0.0544**	0.030
	(0.023)	(0.028)	(0.026)	(0.034)	(0.018)	(0.026)	(0.021)	(0.031)
				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)	(0.086)	(0.141)	(0.105)	(0.184)
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: 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)
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

Notes: The table reports regression estimates associating municipal private sector labour market outcomes to law-implied FPM Transfers according to the type of activity. Panels A–C report estimates on Services, Manufacturing and Agriculture, 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 neighbourhood 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.

In Panel *B*, we focus on manufacturing. The elasticities of total earnings in manufacturing with respect to 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, the range is wide (0.08–0.32) and the estimates are often statistically indistinguishable from zero. The FPM transfers – average wages elasticity in row (3) is close to zero and never passes standard significance levels.

The results in Panel *C* 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 negligible. Similarly, the FPM effects on employment and average wages (rows (2) and (3)) change sign and appear insignificant.

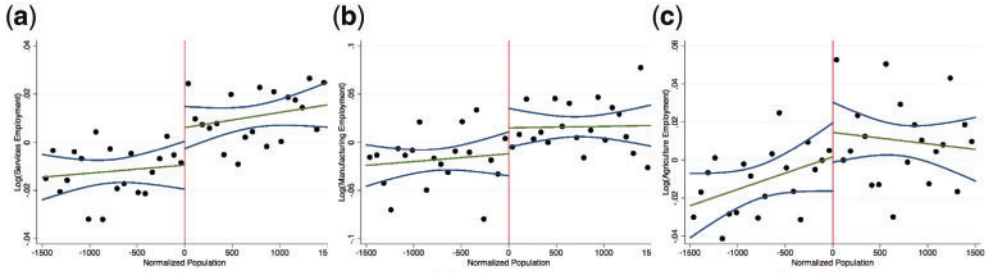

FIGURE 8

Employment in agriculture, manufacturing and services. (a) services; (b) manufacturing; (c) agriculture

In Figure 8, we provide a visual illustration of the sector-specific patterns in private employment. 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. Figure 8a visualizes a clear jump in municipal employment in services for municipalities at the higher FPM population bracket. 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 displays that employment in agriculture does not change abruptly at the pooled FPM population cutoffs.

We also conducted the analysis at a finer industry classification (two-digit ISIC). In Figure 9, we summarize the main findings using a local log-level specification with a 4% bandwidth, no RD polynomial and 90%-confidence intervals clustered at the micro-region level (the full set of results are reported in Online Appendix Table 34). Employment in retail responds the most to swings in regional transfers close to the FPM cutoffs. Private education, other services and to a lesser extent construction appear among the main drivers of the broad sectoral findings in Table 11.[31]

5.3. *Financial constraints*

We explore the potential heterogeneity of the baseline multiplier with respect to the financial friction dimension, allowing the effect of transfers on employment to differ across municipalities in which consumers are more or less likely to face liquidity constraints (and exhibit hand-to-mouth behaviour). We use three proxies for financial constraints: private credit (as a share of municipal income), the presence of commercial banks, and municipal income.[32] In Table 12, we report estimates that allow for heterogeneity in the link between FPM transfers and employment across relatively more and less financially developed municipalities. Unlike most estimates in the article, the coefficients reported in Table 12 are not simple elasticities. They are calculated by applying the elasticity formula, multiplying the log–log specification estimates associated with each group (*i.e.* more or less financially constrained) by their respective sample mean of the ratio

31. We further show in Online Appendix Table 38 that the FPM transfers—employment elasticity is especially strong in the sample of Brazilian municipalities with above-median share of services.

32. Data on private credit and the presence of commercial banks are retrieved online from the ESTBAN database from the Central Bank of Brazil website for 2000. Municipal income indicates total household income from the 2000 Brazilian Population Census aggregated at the municipal level.

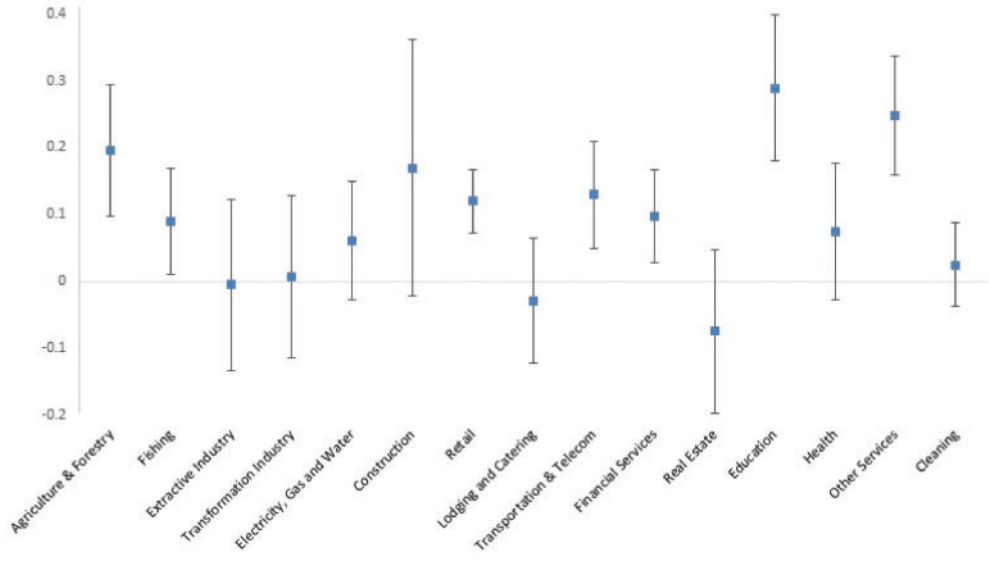

FIGURE 9
Two-digit SIC industry-level estimates

private employment/law-implied transfers.[33] For completeness, we continue reporting level (in columns (1)–(4)) and difference (in columns (5)–(8)) local regression estimates without and with RDD polynomials using both a 4% and a 2% bandwidth.

In Panel *A*, we explore whether the impact of federal transfers on private employment varies across municipalities with lower/higher level of private credit. Using the median value of private credit as a share of local income at the beginning of the sample (in 2000), we group municipalities into a relatively high/low financial development. The specifications in row (1) focus on municipalities with below-median private credit. The estimates are large and precisely estimated (0.04–0.05), passing standard significance thresholds in all permutations both in the level and difference specifications, suggesting that federal transfers boost private employment in localities with low levels of private credit (where financial frictions for consumers will most likely be considerable). Row (2) reports the estimates in the more financially developed municipalities. The level estimates appear unstable within a wide range. The difference specifications estimates are quite small (around 0.02) and never pass statistical significance thresholds.

Figure 10 presents an illustration of the heterogeneous effect of FPM transfers on private employment according to the pervasiveness of private credit. We first net municipality, state-year, and cutoff-year effects and then we plot the residuals, averaged over 75-unit intervals, pooling across all population cutoffs. There is an evident jump of employment in municipalities with less credit but no apparent effect for municipalities with credit as a share of local income above the sample median.

In Panel *B*, we distinguish between cities with (1) at least one bank branch and (2) no bank branches, respectively 69.3% and 30.7% of the sample. The estimates are positive and in most specifications statistically significant. However, the employment-transfers link is larger and more

33. Since the averages of private employment and transfers of more (less) financially developed municipalities may be different, simple log–log elasticities in this case are not informative of the relative magnitude of the marginal effect of an extra dollar in transfers on employment.

TABLE 12

Private employment estimates heterogeneity according to credit, banking and income. Marginal effects calculated applying the Elasticity Formula

	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: private credit							
Less credit	0.056***	0.043***	0.064***	0.055***	0.037***	0.039**	0.036***	0.042**
	(0.012)	(0.017)	(0.014)	(0.020)	(0.010)	(0.016)	(0.012)	(0.019)
More credit	0.083**	0.003	0.113**	0.044	0.024	0.023	0.016	0.040
	(0.042)	(0.049)	(0.051)	(0.060)	(0.023)	(0.035)	(0.027)	(0.046)
p-value (equality test)	0.45	0.33	0.24	0.81	0.45	0.58	0.36	0.93
	Panel B: financial development (local bank branch)							
No bank branch	0.056***	0.039***	0.060***	0.046***	0.033***	0.026*	0.032***	0.028**
	(0.010)	(0.015)	(0.011)	(0.016)	(0.009)	(0.014)	(0.010)	(0.015)
With bank branch	0.058	0.022	0.080*	0.057	0.029	0.046	0.022	0.062
	(0.035)	(0.040)	(0.042)	(0.050)	(0.021)	(0.033)	(0.025)	(0.043)
p-value (equality test)	0.95	0.62	0.59	0.84	0.81	0.53	0.67	0.37
	Panel C: income							
Lower income	0.040***	0.025*	0.048***	0.035**	0.037***	0.043**	0.036***	0.048**
	(0.011)	(0.013)	(0.012)	(0.016)	(0.011)	(0.018)	(0.012)	(0.020)
Higher income	0.132***	0.066	0.174***	0.123*	0.027	0.013	0.019	0.035
	(0.049)	(0.057)	(0.058)	(0.070)	(0.022)	(0.037)	(0.027)	(0.049)
p-value (equality test)	0.05	0.43	0.02	0.15	0.59	0.36	0.47	0.72
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

Notes: The table reports heterogeneity analysis estimates associating municipal private sector employment to law-implied FPM Transfers. Panels A–C exhibit heterogeneity estimates according to High/Low Private Credit as a share of Income in 2000, More/Less Financial Development measured by the presence of a commercial bank branch in the municipality in 2000 and Municipal Income in 2000 (below/above the median). Specifications (1)–(4) report local regression (RD) fixed-effect estimates in levels that restrict estimation in the neighbourhood 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.

precisely estimated (0.08) for municipalities without a bank branch, where liquidity constraints and "hand-to-mouth" behaviour are more likely. The corresponding estimates for the group of municipalities with at least one commercial bank branches is smaller, around 0.035.

In Panel *C*, we compare the employment effects of federal transfers driven swings of local spending across municipalities below and above the median income per capita in the beginning of the sample. Less affluent municipalities tend to experience significant surges of private sector employment, in response to exogenous increases in federal transfers. The estimates in the low-income municipalities are highly significant in both level and difference specifications, and range between 0.03 and 0.05. In contrast, the law-implied FPM transfers–private employment link in the above-median income municipalities are quite unstable.[34]

34. In the Online Appendix we also allow the link between private employment and transfers to differ for big and small municipalities, as well as cities in the North and South. The simulative effects of transfers is larger in smaller localities (centered around cutoffs 1–3) as compared to larger cities (centered around cutoffs 4–7). This result is in line

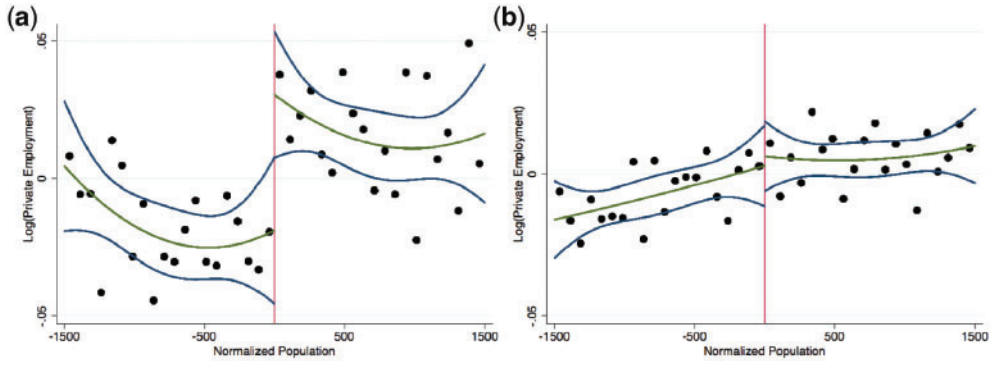

FIGURE 10

Transfers and private sector employment according to Municipal Private Credit. (a) less credit; (b) more credit

5.4. *Summary*

The analysis uncovers considerable heterogeneity in the employment effects of federal transfers induced swings in municipal spending. First, the stimulative impact of transfers are concentrated on employment in services, mostly the non-tradable sectors of retail, education, and construction. Second, the relative local multiplier is larger in municipalities where financial constraints are likely to be more stringent: localities without bank branches, with low levels of private credit, and low municipal income. Both results are consistent with the predictions of standard open-economy New-Keynesian models. In the Online Appendix, we calibrate to the Brazilian economy the theoretical model of Farhi and Werning (2016), which provides closed-form solutions to local multipliers inside currency unions under different parameterization of openness and financial constraints (share of "hand-to-mouth" consumers). The calibration shows that our estimates fall well within the model prediction range.

6. CONCLUSION

We identify the effects of regional transfers on local formal 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 revenues, and spending change abruptly at various pre-determined population cutoffs, according to yearly population estimates provided by the independent federal statistical agency. As municipalities cross the 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, on average, an extra job in the public sector and three extra jobs in the formal private sector. The effect on wages is small and insignificant. These employment estimates map into local multipliers of slightly below 2. The impact of federal transfers on private employment stems from services and is more pronounced in municipalities where consumers are more likely to face liquidity constraints. These heterogeneity results accord well with the workhorse open-economy New-Keynesian framework which predicts highly stimulative effects of transfers when consumption is tilted towards non-tradable (such as services) and consumers exhibit hand-to-mouth behaviour.

with the parallel work of Braga *et al.* (2017), who further show that the FPM programme led to increases in low-skill employment, while it had no impact on high-school and college graduates.

While our estimates are based on a high-quality administrative dataset that covers almost all formal employment contracts of registered firms, they do not cover the informal economy. Given the prominence of the informal economy in several emerging markets, however, and the possibility that public spending may affect the size and growth of the unregistered economy, future research efforts would be purposefully devoted to collect and scrutinize detailed data on the informal sector. This will accomplish a fascinating, yet missing, further step in the research agenda of measuring the effects of fiscal policy on the local economy.

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Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

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