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WORKING PAPER SERIES Nº 2015-03

DEPARTMENT OF ECONOMICS, FEA-USP
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JEL Codes: H72; H77.

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We are grateful to Luis Araujo, Marcelo Arbex, Klenio Barbosa, Felipe Garcia, Cristine Pinto, Michihito Ando, Emilson Silva, Fernando Postali, Vladimir Ponczeck, and Breno Sampaio for helpful comments and discussions. We also thank seminar participants at the 34th Meeting of the Brazilian Econometric Society, 2013, 15th Public Economic Theory Conference (PET14, Seattle, USA), 70th Annual Congress of the International Institute of Public Finance. All remaining errors are our own. We would like to thank Bruno Barsanetti, Fernando Parmagnani, Isabela Furtado, Priscila Bacalhau, Marco Brancher, and João Leite by work from research assistant.

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1. Introduction

The recent literature on the role of intergovernmental grants in local public finance emphasizes the importance of robust empirical strategies to determine the causal relationship between one specific type of transfer and local public expenditures and taxes (Dahlberg et al., 2008, Litschig and Morrison, 2013, Lundqvist, 2013, Lundqvist et al, 2013). Unfortunately, the results concerning the crowding-out effect and the crowding-in effect (i.e., the flypaper effect) are mixed, depending on the nature and the context of those transfers. The results suggest that larger grants can be used as intended to stimulate the public sector or as unintended to finance private consumption through tax cuts.

Our main contribution is to estimate causal effects of both conditional and unconditional exogenous grants on total local public spending, local taxes and on specific types of expenditures and explores whether those targeted grants stick to the specific sector as initially expected in Brazilian municipalities.[1] The simultaneous presence of unconditional and conditional nonmatching grants in Brazilian municipalities allows us to answer three important questions regarding public finance.

First, we test whether unconditional grants lead to more public spending than a reduction in tax rates (full crowding-in hypothesis) or an increase in local income. Second, we investigate whether unconditional grants (full fiscal decentralization) stimulate more fiscal responses than conditional grants (partial fiscal decentralization). As we evaluate specific public spending on health and education, we also examine whether the conditionalities imposed on transfers play a role in the observed effect on public spending (fungibility hypothesis). Third, because some municipalities also receive oil windfalls that are unconditional grants, we additionally explore whether and to what extent the effects of natural resource transfers and traditional unconditional transfers differ. Therefore, as our main contribution, we estimate the causal effects of different types of transfers from the federal government to local government.

Three aspects call attention to the relationship between different levels of government in Brazil and render it distinct from other federations. The first aspect concerns the composition of municipalities' revenues. Municipalities' own tax revenue represents only 21% of their total revenue, whereas more than 60% of transfers come from the federal government.[2] The second

[1] Dahlberg et al. (2008) argues that research on specific grants programs (i.e., studies by Knight, 2002 and Gordon, 2004) can be sensitive to the institution design.

[2] The opposite figure is observed in other federations, where municipalities' own revenues are the major source of local government financing. For instance, this context differs from that in Australia, where transfers from the Commonwealth to the states are more important than are transfers to municipalities, and Canada or Argentina, where federal transfers to municipalities have historically been small and have decreased over the years.

aspect concerns the composition of municipalities' grants. General-purpose (unconditional) and specific-purpose (conditional) grants are equally important for Brazilian local governments, as the former represents nearly 60% of the transfers received by the municipalities while the latter, the other 40%. Third, according to the current Brazilian Constitution, municipal governments have great autonomy to define and allocate their budgets. As both the executive and the legislative branches are elected every four years by a compulsory vote, Brazil provides an appropriated environment to test the effects of fiscal decentralization.[3]

Grants of a different nature might have different effects on recipients' fiscal behavior. According to Boadway and Shah (2007), general-purpose grants may have a small expansionary effect on local spending because they do not affect the relative prices of local public goods (i.e., no substitution effect occurs). Rather, corresponding to general budget support, such grants increase local budgets only. Local expenditures would increase to a lesser extent than the grant funds because they are partly used to provide tax relief to residents. Substantial evidence suggests that the allocation of funds to local spending from general-purpose grants is larger than that from an equal increase in private income, implying that money may not be fungible. This phenomenon is known as the flypaper effect.

A vast literature focuses on the effects of intergovernmental grants on local spending behavior. According to Gamkhar and Shah (2007), evidence of the effect of general-purpose grants is mixed. There is evidence of full displacement, as indicated by a non-significant coefficient for the grant variable in the expenditure equation. However, there is also evidence of the crowding-in effect (i.e., flypaper effect), and several explanations for its existence are provided (Oates, 1979; Courant, Gramlich, and Rubinfeld, 1979; Filimon, Romer, and Rosenthal, 1982; Craig and Inman, 1982, 1986; Hamilton, 1986; Cai and Treisman, 2004; Kornai; 1979; Dahlby, 2011).[4] Recently, the focus has turned to identification strategies for open-ended intergovernmental transfers (Dahlberg et al, 2008; Lundqvist, 2013; Lutz, 2010; Litschig and Morrison, 2013), and both full and partial crowding-in effects have been reported, corroborating the context-dependent nature of the results.

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[3] Local governments from India and UK also receive transfers, but governments in these countries have almost no autonomy to establish their budgets (expenditures and revenue). In Australia, transfers to municipalities are mostly for a specific purpose, primarily for social housing and transportation. South Africa used to have a mix of unconditional and conditional transfers, but in 1998, a policy shift led to a formula-based system of primarily unconditional grants (Bahl, 2001).

[4] For Brazilian data, see Mattos, Rocha, and Arvate (2011). Another strand of the literature casts doubt on the existence of the flypaper effect and suggests that empirical studies may be overestimating this effect. Most explanations focus on identifying flaws in the empirical strategy, such as functional misspecification (Becker, 1996), omitted variables (Hamilton, 1983), mistakes in classifying conditional transfers as lump-sum grants (Moffitt, 1984), and potential differences between the short- and long-run effects of grants on local expenditures (Gramlich, 1977; Gramlich and Galper, 1973).

Close-ended nonmatching grants can provide resources to local governments as long as they are spent on a particular purpose, and they do not require the municipality to finance a certain percentage of expenditures by using its own resources. [5] The addition of this conditionality may not affect local spending behavior, however. The condition imposed on the grant does not matter if the municipality is already spending more than the grant amount and if the predicted expansionary effect of such a grant is smaller than the resources received. In fact, conditional grants differ from unconditional grants only if the municipality receiving the money is spending less than the grant amount without the imposition of the condition. Otherwise, the grant is, in fact, unconditional, and it would have the same effect as if the municipality had received the money to spend freely.

Many papers have estimated the response of local public expenditures to close-ended grants (Megdal, 1987; Mofitt, 1994; Wickoff, 1991) or have focused on the fungibility aspects of close-ended intergovernmental transfers (McGuire, 1975; 1978, and Zampelli, 1986; and Van de Walle and Mu, 2007). Recently, in line with the recent literature that uses convincing identification strategies, Knight (2002) finds evidence of a full crowding-out effect, whereas Gordon (2004) finds a strong crowding-in effect for the first year of transfers.[6] Ando (2015) exploits two different exogenous variations in the formula of grants, and use a regression kink design and an instrumental variable approach, to evaluate the effects of fiscal equalization grants on local expenditures. His main concern, however, is to show that those effects depend on the institutional or formula settings of intergovernmental grants. He concludes that in fact there are heterogeneous grant impacts on rich and poor municipalities in Japan.

To the best of our knowledge, this paper represents the first attempt to estimate the differential effects of unconditional transfers, conditional transfers, and natural resource transfers on local public expenditures while addressing the endogeneity of those grants. Moreover, we compare the effects of conditional and unconditional grants with the effects of local income changes while still confronting identification problems.

We use four identification strategies. First, to identify the effects of unconditional transfers on local public expenditures and tax relief, we exploit the fact that the rule of distribution of federal resources to local governments is discontinuous with respect to population thresholds and use a fuzzy regression discontinuity design (RDD) procedure, as in Brollo, Nannicini, Perotti, and

[5] Conditional transfers may or may not require a matching provision. Conditional matching grants require that grant recipients use their own resources to finance part of expenditures. Because such grants reduce the relative price of public goods, the municipality consumes more of public goods from a given budget (substitution effect). An income effect also arises, given that the municipality ends up with a surplus of resources that can be devoted to increased consumption of public goods. Both effects stimulate higher spending on public goods. Matching grants, however, are not important in Brazil.

[6] Bergstrom, Dahlberg, and Mork (2004) estimate that general grants have smaller effects than targeted grants on employment. Becker (1996), Gamkhar and Oates (1996), Turnbull (1998), and Berg and Rattsø (2006) also use instruments for federal transfers.

Tabellini (2013).[7] Second, we follow the approach in Paxson (1992) to build historical deviations from the historical mean of rainfall data as instruments for variations in local GDP (also restrict to agricultural-based municipalities). Third, we create an instrument for conditional transfers to education (*Fundo de Desenvolvimento do Ensino Basico* – FUNDEB). For this purpose, we follow Kosec (2014) and use as an instrument the predicted transfers for education, which are calculated by using the 1997 rule of redistribution, according to which local governments could not influence the criteria for redistribution. Fourth, following Caselli and Michaels (2009), we build an instrument for the royalties of oil production, for which we estimate virtual royalties based on each field's oil production and the specific rules of distribution.

Our results suggest that the effect on local public expenditures does not statistically differ between unconditional and conditional grants. More important, our estimations suggest that unconditional and conditional transfers have a full crowding-in effect on aggregate public spending (each $1 of unconditional and conditional grant receipts increases expenditures on local governments by $1). Additionally, we find evidence that the effect of unconditional transfers on local public spending is greater than the corresponding effect of local income variation, providing further evidence for the flypaper effect. The effects of unconditional transfers on education spending are smaller than the effects of FUNDEB (conditional education transfer) but still greater than the corresponding effect of local income changes. Similar results are found for the relationship between conditional health transfers (PAB-SUS) and local public health expenditures.

The paper is organized into six sections beyond this introduction. The second section presents the fiscal transfer system in Brazil, showing the main grants from federal to local governments. The third section briefly describes the data, and the fourth section presents our empirical strategy, including the development of instrumental variables (IVs) for each transfer. The fifth section discusses our main results, as well as additional evidence when GDP is treated as endogenous and when an alternative identification strategy—a RDD, where treated and untreated municipalities are distinguished by a break in the rule of distribution of unconditional grants—is used. The sixth section evaluates the effects of transfers on specific spending, education, and health. The seventh section summarizes the main conclusions.

2. Fiscal Transfers in Brazil

Brazilian federalism relies heavily on transfers, which represent an important source of revenues for municipalities. Figure 1 below shows the ratio between local taxes and the total grants received for 2002-2008. Note that taxes represented 20% of the grants received in 2002, and although the share increased, taxes represented only 30% of transfers in 2008.

[7] In contrast to Litschig (2012), we find no evidence in our period that the rules can be overruled.

Insert Figure 1 here

Municipalities' dependency on transfers is heterogeneous and is related to population size. Smaller municipalities have a larger dependency, i.e., a tax–grant ratio close to 0.2, which is larger to the ratio for larger municipalities.[8] We also note that education spending corresponds to 25% of all public expenditures, whereas health spending represents only 16%.

The most important transfer to Brazilian Municipalities is called Municipalities' Participation Fund (Fundo de Participação dos Municípios - FPM). It can be classified as a mandatory, unconditional, nonmatching, revenue-sharing grant, and it corresponds to 23.5% of the income taxes and industrialized products taxes that are collected by the federal government. It is a redistributive transfer because it aims to reduce regional inequalities.

Municipalities that are state capitals receive 10% of FPM transfers (FPM-Capitais), and the other municipalities receive 86.4% of the resources (FPM-Interior). The remaining 3.6% of the resources (FPM-Reserva) are distributed to municipalities with at least 142,633 inhabitants.

The 10% of funds transferred to capitals is distributed according to coefficients based on the ratio between the population of the capital, the sum of the population of all capitals, and the inverse of the per capita income of the state where the municipality is located.

The 86.4% of funds transferred to the other municipalities is distributed according to coefficients based on the size of the population of each municipality (Table 1). The complementary 3.6% is distributed by using the criteria of the FPM-Capitals.

Insert Table 1 here

Because the amount that must be allocated to each municipality is clearly established, FPM transfers are transparent and free of political pressure. Furthermore, local governments have autonomy on how to spend those transfers.[9] On one hand, such autonomy is desirable because the municipality, by better understanding the preferences of the population, can allocate the transferred resources according to local needs and preferences. On the other hand, this autonomy allows for a loss of returns to scale in spending and for less effort in tax effort. The official number of inhabitants in any particular municipality is measured every ten years by the Brazilian Census, and an independent federal agency called the Instituto Brasileiro de Geografia e Estatistica (IBGE)

[8] Transfers from states to the municipalities are called transfers of ICMS (Imposto sobre Circulação de Mercadorias e Serviços), which is the value added tax of state competence. From all the ICMS revenue collected by each state, 25% must be transferred to the municipalities, of which three-fourths is allocated to the municipality where the revenue originated and one-fourth is allocated among all municipalities in accordance with the legislation of each state. As part of the transferred resources consist of shared revenues, largely reflecting the capacity of municipalities to generate their own resources, disparities among municipalities are amplified instead of corrected. ICMS transfers are also mandatory, unconditional and nonmatching; however, they are not be evaluated because they are not as equalizing as FPM.

[9] Although Litschig (2013) shows that the population measure could be manipulated by municipalities in the 1990s, such manipulation has not been observed since 2000, when the federal governments started to use census data and IBGE estimates for the population. See also Brollo et al. (2012) and Finan and Ferraz (2012) for further evidence and discussion.

provides annual estimates of local populations between census years. The central government then uses these estimates to distribute FPM funds to municipalities. We explore the yearly fluctuation in local populations across thresholds to quantify the causal effects of government spending on economic activity.

Although natural resources belong to the Union, some revenue is transferred to the states and municipalities as financial compensation for the exploitation of those resources in their territories (or neighboring territories). In fact, municipalities are the primary beneficiaries of royalty payments, having received 34% of oil windfall revenues in 2008. Further, the number of producing municipalities increased from 103 in 1997 to 123 in 2008. Similar to FPM transfers, these royalty transfers are unconditional and nonmatching.

In 1997, a new law implemented important changes to the system of royalty payments. Specifically, the payments increased from 5% to 10% of the production value, the oil value was indexed to the international prices, and extra payments were assigned to highly productive oil fields (these extra payments are called "participações especiais").

Oil production has increased dramatically since the enactment of Law no. 9478/1997, reaching 600 million barrels in 2008, more than double that produced in 1997. This increase was largely driven by offshore production, which increased from less than 200 million barrels annually in 1994 to 663 million barrels annually in 2008. By contrast, onshore production remained practically stagnant during this period at 65 million barrels annually (Monteiro and Ferraz, 2010).

Table 2 summarizes the main beneficiaries of royalty revenues, according to the location of the oil reserve (land or sea) and the royalty rate.

Insert Table 2 here

The greatest amount of resources is transferred to producing or neighboring municipalities when production is offshore. Given the distribution shown in Table 2, the royalty revenue of each state or municipality is proportional to the oil production in the wells on their territory. When production occurs on the continental shore, IBGE calculates the number of wells under the neighbor municipality jurisdiction by using the projection of the municipality's geographic coordinates on the sea. [10] According to Monteiro and Ferraz (2009), 90% of the oil produced in Brazil comes from offshore wells.

Caselli and Michaels (2009) argue that oil production can be treated as exogenous to local characteristics. Because municipalities are very small, they are not politically able to lobby and/or

[10] The unequal distribution of resources and the new perspectives of resources brought by the pre-salt discovery led to intense political pressure for new distribution criteria. At the end of 2012, the Brazilian Congress approved new legislation that reduced the participation of producers and neighbors in the sharing of revenue and that increased the distribution of revenues to nonproducing municipalities and states.

bribe PETROBRAS (or the multinational extracting companies) to drill near them or to increase the amount of oil extracted. To support this argument, the authors run a regression predicting oil output where the variable distance from the federal capital appeared to be statistically non-significant. Thus, oil output does not seem to be affected by political influence. [11] We start our analysis by taking royalties as exogenous. However, because geographic location is not the only (but is the main) determinant of oil windfall distribution and because the size of the population and the location of production plants, pipelines, and transportation facilities also affect the amount paid to each locality, we present the results by using an instrument for royalties similar to the one used by Caselli and Michaels (2009), and we consider a subsample of fifty-five (55) municipalities.

Conditional transfers are primarily used to subsidize education and health. Spending in these areas is considered a priority by the central government, which aims to ensure that the resources are not diverted to other activities.

In 1998, a conditional transfer to education was established. The federal government introduced a new mechanism for financing public education called FUNDEF, which ran from 1999 to 2006 and aimed to equalize the available resources in education across municipalities. This mechanism comprised 15% of the States' Participation Fund (FPE), the FPM, sales tax on goods and services (ICMS, including appeals related to the exemption of exports mentioned in Supplementary Law 87/1996), and excise tax on industrialized products related to exports (IPIexp). Its purpose was to finance only primary education. In 2007, FUNDEB replaced FUNDEF with the aim to finance all levels of education, from pre-school to high school. Table 3 summarizes the rules of resource distribution to education.

Insert Table 3 here

From the total amount collected, each municipality receives resources according to the number of students enrolled in its public schools. From 1998 to 1999, the total number of children enrolled in municipal primary schools in each municipality in the state, the total number of children enrolled in state primary schools in the state, and the annually set federal minimum expenditure per primary school student determined the exact amount that each municipality received from the fund.

[11] Petrobrás is an enterprise controlled by the federal government. Caselli and Michaels (2009) also note that "unlike many Brazilian institutions, Petrobrás actually has a strong record and reputation for integrity – at least in recent years. This record has been explicitly recognized by international NGOs operating in the natural resources area, e.g., Transparency International (2008)". They also provide evidence that oil production affects the economy only through the revenue that it generates for the local governments. Support for this hypothesis is provided by a regression that shows that oil windfalls do not have an effect on local nonindustry GDP per capita. Monteiro and Ferraz (2009) offer additional evidence in this direction, showing that oil windfalls do not affect the number of firms, the amount of private payroll, or the number of private employees.

From 2000 to 2004, the algorithm defined the federal top-off amount based on the students' enrollment level so that children in grades 1-4 of primary school were weighted differently from children in grades 5-8. From 2005 to 2006, the algorithm began to define the federal top-off based on not only primary school level but also urbanization status. Thus, information on the number of children were enrolled in grades 1-4 in urban primary schools, grades 1-4 in rural primary schools, grades 5-8 in urban primary schools, and grades 5-8 in rural primary schools was required. After 2007, FUNDEB required that the number of students enrolled in urban vs. rural preschools, secondary schools, and adult education be taken into account.

FUNDEB obviously limits municipalities' autonomy because the resources are necessarily linked to education, as they are conditional transfers. Moreover, the Brazilian Constitution (article 212) establishes that each municipality should spend at least 25% of their total revenue (including taxes and transfers). Some municipalities impose a larger limit, however. For instance, the councilor of the city of São Paulo (the capital of the richest state in the country) defines a 30% limit.

Our benchmark health transfer—Piso de Atenção Básica (PAB)—is a financing mechanism for the Brazilian Health System (Sistema Único de Saúde – SUS) that focuses on comprehensive health care. It was created in 1996, implemented in the first half of 1998, and expanded in 2001.[12] The aim of this mechanism is to equalize local resources to guarantee a national minimum standard in health services. The amount is transferred monthly to each municipality according to a formula that linearly links the amount of resources (fixed PAB) to the number of inhabitants of the municipality in that year according to IBGE. Resources must be devoted only to current and capital expenses that are related to basic health and that are in accordance with the guidelines of the Municipal Health Plan. Regarding health, the Brazilian Constitution also establishes that a minimum of 15% of the total revenue is spent on health (articles 156, 158, and 159). Table 4 presents data on ordinances and the per capita amount transferred to each municipality during the period under analysis.[13]

Insert Table 4 here

[12] PAB consists of a fixed part, which intends to fund primary care, understood as a set of actions that must be provided at the first level of attendance of the health system and that aim at prevention, treatment, and cure. Primary case therefore involves medical consultation in, e.g., basic specialties, vaccinations, emergency care, and minor surgeries. The PAB also consists of a variable part that is intended to develop specific actions for primary care.

[13] The variable part of PAB, on the other hand, is transferred as incentives. Municipalities receive the resources if they adopt programs that are considered a priority by the Ministry of Health, such as PACS (Programa Agente Comunitários de Saúde), PSF (Programa Saúde da Família), and the Program against Malnutrition (Programa de Combate às Carências Nutricionais), and if they engage in strategic actions such as the implementation of Basic Pharmacy of SUS (Farmácia Básica do SUS) and Actions of Sanitation Surveillance (Ações de Vigilância Sanitária).

3. Data

Municipal public accounting started to produce centralized information regarding fiscal variables in 1989.[14] However, there were different levels of data disaggregation, and only after 2001 were the variables that we use in our investigation published consistently. This is primarily because 1073 municipalities were created from 1993 to 2001.[15] Our sample ends in 2008 because this period allows us to capture two terms of municipal government and to consider the largest number of control variables. Our observation units are the 5564 Brazilian municipalities, and we use different databases to build our variables of interest for the period between 2002 and 2008.

Fiscal data are from the National Treasury's site (*Finanças do Brasil - FINBRA – Dados Contabeis dos Municipios*). They include variables related to local public expenditures (general expenditures, education expenditures, and health expenditures), revenue from municipal taxes (property and service taxes), transfers received by the municipalities from the central government (FPM, PAB, FUNDEF/FUNDEB, and royalties), all other transfers received from state and federal (only residual) governments (other transfers received), visibility (property tax as a share of service tax), and fiscal complexity (property tax as a share of revenue taxes). All the variables are deflated by the general index price (IGP-DI, base = 2000) and considered in per capita terms.

Table 5 shows the descriptive statistics for the fiscal variables of all our variables for the entire sample, for municipalities with a population under 169,800 inhabitants, and for municipalities whose percentage of agricultural GDP is larger than the Brazilian median.

Insert Table 5

The population data come from IBGE. We consider the population divided by 100,000 (population in 100,000 inhabitants), births minus mortality/population, the percentage of the population under 1 year old and between 70 and 79 years old, the percentage of women in the population, the percentage of individuals who completed elementary school, and the percentage of individuals who completed higher education.

Table 6 summarizes the demographic, political, and education variables. The public service data are from Ipeadata's site (Ministry of Planning).[16] They correspond to the percentage of families included in the Family Health Program, the percentage of housing with potable water, the

[14]See https://www.tesouro.fazenda.gov.br/pt/politica-fiscal/planejamento-fiscal/resultado-tesouro-nacional/809.
[15] See also Caselli and Michaels (2013).
[16] See http://www.ipeadata.gov.br/

percentage of housing with water service, the percentage of housing with garbage service, the percentage of housing with sewage service, and the percentage of housing with lighting service.

The political data are extracted from Ipeadata's site (Ministry of Planning). We consider the percentage of councilors elected in the same party as the mayor, the percentage of individuals with an elementary school education, and the percentage of individuals with higher education. Although the general results are presented on an annual basis, the dummies and percentage of councilors are the same for four years (from 2002 to 2004 and from 2005 to 2008) for municipalities.

Insert Table 6 here

Finally, Table 7 presents the descriptive statistics for our two measures of local income and their instrument—rainfall. First, as a measure of local income, we consider the sum of all formal wages within each municipality to determine the total for formal wages. The data come from *Relação Anual de Informações Sociais* (RAIS). As an additional measure of local income, we also consider the municipalities' GDP obtained from Ipea (Ipeadata - Ministry of Planning), which calculates an estimate of the local GDP between census years (2000 and 2010).

Insert Table 7 here

Our instrument for local income corresponds to the variation in rainfall with respect to the historical mean (1900-2000). We follow Paxson (1992) and construct the difference between the actual rainfall for each season (summer, autumn, winter, and spring) every year and its historical mean (last century, 1900-2000) and standard deviation. We also compute the corresponding figure for each term of municipal government (2001-2004 and 2005-2008).

We obtain historical data on precipitation from the Terrestrial Air Temperature and Precipitation: 1900-2008 Gridded Monthly Time Series, Version 1.02 (Matsuura and Willmot, 2009). This dataset provides worldwide monthly mean precipitation data at 0:5x0:5 degree resolution. The number of nearby weather stations that influences a grid-node estimate is 20 on average.

To build the regional rainfall variable, we follow two steps. First, each municipality is located within a square defined by the four closest nodes to its centroid's latitude and longitude. We denote this square a grid. Second, the monthly mean precipitation for each municipality is calculated as the weighted average of the respective weather information recorded at each of the four nodes of the respective grid.[17]

4. Empirical Strategy

To estimate the effect of unconditional and conditional grants on total, education, and health public expenditures for municipalities, we use the following equation:

[17] See the details of this approach in our supplemental material.

$$G_{i,t} = \alpha_i + \beta_0 Y_{i,t} + \beta_u UT_{i,t} + \beta_c CT_{i,t} + \beta_3 Royalties_{i,t} + \beta_4 Controls_{i,t} + \gamma_t + \varepsilon_{i,t} \qquad (1)$$

where $G_{i,t}$ denotes the per capita total public expenditures, the per capita health public expenditures, or the per capita education public expenditures for municipality *i* in period *t*; *Y* represents local income; *UT* denotes the vector for unconditional transfers for municipality *i* at time *t* (FPM and royalties); and *CT* denotes the vector for conditional transfers for municipality *i* at time t (FUNDEB for education and PAB-SUS for health). *Controls* represent a vector aiming to capture all other variables that may affect public expenditures. Finally, variable α_i represents the fixed effect for each municipality, γ_t denotes time dummy variables that capture common effects for all municipalities over time, and $\xi_{i,t}$ is the error term.[18] Under the assumption of strict exogeneity below, one can identify all **β** effects:

$$E\left[\varepsilon_{i,t} \middle| \begin{matrix} Y_{i1}, \dots, Y_{i,T}, UT_{i,1}, \dots, UT_{i,T}, CT_{i,1}, \dots, CT_{i,T}, Royalties_{i,1}, \dots, Royalties_{i,T}, Controls_{i,1}, \\ \dots, Controls_{i,T}, \alpha_i \end{matrix}\right] = 0,$$

$$t = 1, \dots, T \qquad (2)$$

(2)

However, one might expect that the amount of expected transfers to be received in the future could be determined by previous decisions related to local public expenditures, in which case the strict exogeneity assumption would be violated (Becker, 1996). In other words, although grants do often vary considerably, most of the variation is endogenous in the sense that the variation is due to structures that are themselves directly related to expenditures: Municipalities with characteristics associated with high expenditures (e.g., those with a large share of children) typically receive more grants precisely because they need to spend more, reflecting needs only. An easy solution for this endogeneity problem involves controlling for all characteristics that determine expenditures in a regression analysis. In addition, one can closely investigate how grants are determined and can search for experimental-type features where the amount of grants varies but the underlying needs do not. We use such a strategy in this paper. A similar argument can be applied to the endogeneity of local income. In this case, we rely on Paxson's approach (1992) and use unexpected rainfall as an instrument for exogenous variations in income.

More specifically, we want to test the magnitude of the coefficient β_u. To rigorously identify the role of unconditional transfers, one must compare β_u with the coefficient for conditional transfers (β_c), income (β_0), and royalties (β_3) by using convincing identification strategies. If general-purpose nonmatching grants have the smallest stimulatory effect on local spending, one

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[18] We restrict the analysis by using panel regressions that are not weighted by population and not panel-quantile regressions. The results for regressions weighted by population and panel-quantiles are qualitatively similar and are available upon request.

might expect $\beta_u < \beta_c$. The literature on the flypaper effect also compares the coefficient for income (β_0) with the corresponding coefficient for the block grant. The context of our study differs, however, because Brazilian municipalities receive both unconditional (block) transfers (FPM and royalties) and conditional (block) transfers. Therefore, in our case, to verify the existence of the flypaper effect, one has to compare the magnitudes of β_u and β_c with the magnitude of β_0. We present the effect of each type of transfer separately to compare their effects on public spending.

When the dependent variable is per capita spending on health or education, an additional aspect is the potential effect of the fungibility of grants; i.e., local governments might decide to spend the transfers in a category different from the federal government's initial intentions. In other words, conditional transfers might increase by less than the amount of the grant because local governments can apply the funds to different types of local public expenditures. For instance, the remainder of the grant in fact might increase the consumption of other goods and services. A specific test for this hypothesis is whether $\beta_c = 1$.

We describe below our strategies to identify the effects of unconditional transfers, conditional transfers, and local income to isolate these effects from those due to possible (omitted) nonobserved variables (Hamilton, 1983 and Wyckoff, 1991), reverse causality, or simultaneity (Knight, 2002).

4.1 Endogenous Income

One of the main assumptions in the traditional empirical estimates of the flypaper effect is that GDP is exogenous (see, for example, Inman (1971); Weicher (1972); Gramlich and Galper (1973); Feldstein (1975); Hamilton (1986); Wyckoff (1991) and Case, Hines and Rosen (1993)). However, local income might be related to or even caused by local public expenditures. For instance, if a municipality increases the number of local public employees, "local GDP" should increase.[19]

However, we allow for endogenous local GDP, and similar to Paxson (1992), we use weather variability as an instrument for transitory income to estimate the response of public expenditures to this income variation. Because of the difficulties associated with measuring permanent income, we focus on the explicit measurement of unexpected income produced by rainfall shocks.

Unexpected income is measured by using the following equation:

$$Y^U_{i,t} = \beta_i + \beta_1 X^U_{i,t} + \beta_2 Z_{i,t} + \beta_t + \varepsilon_{i,t} \qquad (3)$$

where β_i is the fixed effect for municipality i; X_{it} is a set of municipality-specific variables that affect unexpected income; Z_{it} is a set of control (observable) variables at the municipality level; X^U

[19] We consider another measure of local income captured by mass of formal wages. The results are qualitatively similar and are available upon request.

consists of deviations of municipality i's rainfall in each of the four seasons from the average over one hundred years (1900-2000) and deviations from the squared averages; Z_{it} is a set of municipality-specific variables that could affect the level of unexpected income (we consider the same controls for all exercises, including health status and other socio-demographic variables; see Table 8); β_t is a year effect common to all municipalities; and ε_{it} is a random error term.

We understand that unexpected rainfall results in an exogenous variation of local income and, consequently, is a good instrument for GDP. In addition, the instrument does not vary across households within the same municipality, but it can vary across years in the same municipality and across municipalities. [20]

Table 8 below summarizes our results. Column (1) presents the first stage in which all (17) thresholds are considered together; column (2) considers log variables; column (3) considers the separate threshold discontinuities into three brackets (1-3, 4-7, and 8-17); and column (4) includes all possible control variables in the first stage. The last two columns restrict the sample to agricultural (5) and coastal (6) municipalities.

Insert Table 8 here

The instrument seems robust, with a large F-test for all but one model. Our strategy allows for an increase of more than five times the FE estimated income effect for the linear model (see table A.1 column 1)). The results show that even when we control for all possible variables, the instrument remains significant, with an F-statistic of 13.05. We also restrict the sample to municipalities with agriculture production as their main economic activity (column 5), and we find that our instrument continues to perform adequately. Only when we restrict the sample to coastal municipalities (i.e., municipalities that receive royalties) do we observe that the instrument is no longer significant; hence, we cannot trust its second-stage estimation.[21]

4.2. Endogenous Unconditional Transfers: FPM

As discussed previously, differences in FPM transfers for a given municipality over two consecutive years are primarily due to either a change in the population (to a higher or lower population threshold) or an increase or decline in the central government's fiscal revenues used to compute FPM transfers. Moreover, even municipalities with a similar population and per capita income might have different coefficients because they belong to different states. However, these

[20] The graphics for the average historical rainfall (1990-2000), the average of each term (2001-2004 and 2005-2008), and the first stage of each estimation when we proxy income by the municipal per capita GDP are presented in the supplementary material.

[21] Table A1 also shows an important robustness check which is related to the possible problem of validation exclusion. By using education expenditures as dependent variables it becomes plain that variation in the rainfall should not affect education expenditures directly. Comparing columns (1) and (3) one can note that the sign and magnitude of the estimated income effects on general expenditures follow qualitatively that one estimated for education expenditures.

transfers are larger in per capita terms for less populated municipalities, potentially motivating them to form new municipalities (Ponczek and Mattos, 2013).

We find that the rule of distribution of FPM transfers provides a treatment assignment mechanism that is typical of a (fuzzy) RDD for obtaining robust estimates. Treatment assignment (receiving high versus low federal transfers) depends on the running variable, population size. However, the probability of being treated conditionally on the running variable is known to have relevant discontinuities at multiple thresholds. The fuzzy design arises from the possibility of misassignment around the cutoffs, with municipalities near each threshold appearing in both the treatment group and the control group. Thus, not all municipalities receive the amount of transfers that they should receive based on their IBGE population estimate and residing state.

Therefore, we investigate the robustness of the flypaper effect by exploring the discontinuity in the rule of distribution of unconditional grants as an exogenous variation in the level of transfers received by similar municipalities. This strategy provides us with the opportunity to propose an additional test of how unconditional (lump-sum) grants are allocated between local public and private goods (Bradford and Oates 1971). Given that federal grants are passed down to the local population as either a decrease in taxes and fees (crowding-out effect) or an increase in public expenditures (crowding-in effect), we can compare our results with the literature aiming to identify the causal effects of grants on broad economic outcomes, such as total spending (local public goods) and taxes (private goods) (Dahlberg et al. 2008; Lutz 2008; Lundqvist 2013).[22]

Although theoretical transfers are a step function of the population (Brollo et al, 2013), actual transfers do not necessarily follow through. We consider theoretical transfers as the treatment assignment and actual transfers as the observed treatment. The treatment assignment is exogenous around the policy thresholds, although the observed treatment may also be influenced by additional factors, such as politicians' ability to sidestep the exogenous assignment rule or other random elements. As long as actual transfers depend on theoretical transfers, we can use the latter as an instrument in a (fuzzy) regression discontinuity setup. We can run the following reduced form, similar to that in Imbens and Lemieux (2008), Garibaldi et al. (2009), and Brollo et al. (2013):

$$FPM_i = f(pop_i) + \beta_1 FPM_i^t + \gamma_t + \phi_s + v_i$$

$$G_i = f(pop_i) + \beta_1 FPM_i + \gamma_t + \phi_s + v_i \qquad (4)$$

where $f(pop_i)$ is a high-order polynomial in the population of municipality i; FPM corresponds to actual FPM transfers, instrumented by its counterpart defined in the Brazilian Constitution (FPM^t

[22] See also Lundqvist, H., M. Dahlberg, and E. Mörk (2013) for the effect of general grants on public employment.

are theoretical transfers); γ_t are time fixed effects; φ_s are state fixed effects, and v_i is an error term clustered at the municipality level.

We therefore re-build the amount of transfers for each municipality by considering only the rule of transfers before any political interference or endogenous decision on the part of municipalities. To do so, we use the following steps. First, we collect data on federal government revenues, income tax (IR), and a tax on industrialized products (IPI), which is a consumer tax that is exogenous to municipalities' decisions.

Next, we multiply this amount by 23.5% to determine the total amount of resources that should be available for redistribution to municipalities for each year in our sample. As shown before, according to Law No. 5.172/1966, of the total amount of FPM transfers, 10% belongs to the capitals, 86.4% belongs to the municipalities of the interior, and the remaining 3.6% is the Reserve Fund, which is distributed among countryside municipalities with more than 142,633 inhabitants (Decree Law no. 1.881/1981 and Complementary Law no. 91/1997, art. 3) Finally, to calculate the local sharing coefficient, we use information on the population provided by the IBGE (interior municipalities) and on the per capita income of the state (capitals), as published by the government each year. This coefficient may be different from the final coefficient if there was a reduction in the coefficient for the year 1997 (Complementary Law no. 91/1997 (art. 2) and Supplementary Law no. 106/2001 (art. 1)). The municipalities that had a reduction in their coefficients in 1997 are called municipalities supported by Supplementary Law no. 91/1997 and those that did not have such a reduction are called nonsupported municipalities. This same law also applies to other types of municipalities, namely, countryside municipalities and those entitled to the Reserve Fund. We consider municipalities with up to 169,800 inhabitants and exclude state capitals. For each municipality and year, we simulate the corresponding FPM transfers to be received by using our first observation for the population in 2002. In so doing, we can compute our instrument for the actual amount of FPM transfers. We aim to identify the increase or decrease in municipalities' population that shifts the municipality to a different threshold.

The causal effect that we are identifying is local for two reasons. First, because of the RDD, the effect refers only to observations around the thresholds. Second, because of the IV setup, the effect refers only to compliers, that is, municipalities that received larger transfers because of the (exogenous) FPM revenue-sharing mechanism. However, the identification of compliers neglects a subpopulation that might also be of interest: the always-takers, municipalities that receive larger transfers independently of their position above or below each population threshold.

The parameter of interest is β_1. The function f(.) is a flexible function of the population. In fact, we use polynomials and allow them to differ between the positive and the negative parts of the support of population bracket.

Insert Figure 2A and 2B

Examining the confidence intervals produced, we can note a clear discontinuity in the first three cut-offs and a less pronounced discontinuity at cut-offs 4-7. These are the same thresholds used in Brollo et al. (2013) and Litschig and Morrison (2013). Nevertheless, we attempt to consider possible discontinuities at all population thresholds up to the 17th cut-off.

Table 9 presents our results. Column (1) presents the first stage in which all thresholds are considered together, column (2) considers the variables in logarithms, column (3) separates threshold discontinuities into three brackets (1-3, 4-7, and 8-17), and column (4) includes all possible control variables in the first stage. The last two columns restrict the sample to agricultural (5) and coastal (6) municipalities.

Insert Table 9 here

We also conduct a first-stage regression to evaluate the validity of our instruments, and we find that our instruments are valid (based on an F test) in all but one model. Only for coastal municipalities does our instrument not hold. Our strategy allows for not only an increase of more than twice the effect of traditionally estimated unconditional transfers on total expenditures but also a lower coefficient for health expenditures (see table A.2. on appendix). We also note that even when we control for all possible variable, this instrument is significant, with an F-statistic of 12.10. We also restrict the sample size to municipalities that are agriculture dependent (with a share of agriculture production above the median of the country; column 5) and observe that our instrument also performs well with this sample. Therefore, we proceed to the analysis of FPM effects in the second stage.

4.3. Endogenous Educational Transfers: FUNDEB

Because education transfers (FUNDEB) depend on variables that are endogenous to municipalities (number of students enrolled), local governments may increase enrollment to receive larger transfers. In other words, local revenues per capita can be a response to education policy. Additionally, municipalities with larger taxes demand better educational policies.

Another potential source of bias may be related to political aspects. As suggested by de Janvry et al. (2009), mayors with high discount rates (or corrupt behavior) may not only generate higher revenue to maximize private gains but also decrease investments in education because of their long-term pay-offs. The authors find that second-term mayors in Brazilian municipalities, who are not eligible for reelection, have less transparent policies and are less likely to reduce school drop-out rates by using federal funds designated for this purpose. By contrast, Ferraz and Finan (2009) show that first-term mayors misappropriate 27% fewer resources than second-term mayors, a finding that they also link to electoral incentives.

The best strategy to address these identification problems is to develop a valid instrument for conditional transfers to education. This instrument should affect FUNDEB transfers but should not affect other factors influencing the demand for FUNDEB transfers. We follow Kosec (2013) and simulate a municipality's tax revenue by using the algorithms of the laws in each year; however, we use pre-reform (1997) data on tax revenue and enrollment. We also allow some municipalities to have the right to access a larger share of the funds through the take-over of state public schools, exploiting the nation-wide rate at which municipal governments assumed the state's role in the provision of primary education from 1998 to 2008. Federal rates of takeover are exogenous to pre-primary education policy in any given municipality. Therefore, to better predict how enrollment expands over the sample period without introducing endogenous information, we simulate that each municipality took over a state primary school that is similar to the national average. Consequently, FUNDEB transfers can increase more rapidly in municipalities after the reform state than before, when federal involvement in primary education was more important.

Table 10 presents the first-stage results of our IV strategy. Column (1) presents the first stage in which all thresholds are considered together, column (2) considers the variables in logarithms, column (3) separates threshold discontinuities in three brackets (1-3, 4-7, and 8-17), and column (4) includes all possible control variables in the first stage. The last two columns restrict the sample to agricultural (5) and coastal (6) municipalities. Our instrument is robust (the F-statistic is above 6 for all models), and the coefficient for all estimated FUNDEB's IV lies between 1.3 and 2.3.

Insert Table 10 here

4.4 Endogenous Oil Transfers: Royalties

Our last step for identifying the effect of unconditional transfers on general and specific-purpose public spending involves building an IV for royalties. We follow Caselli and Michaels (2013) and recover oil and gas production for each field for our period of analysis. We then apply the rule of royalty distribution to municipalities according to (exclusively) the oil and gas extraction of these recovered data to construct our IV. In this way, we can separate the amount of royalties received owing to refineries and installations built in some municipalities that possibly make royalty transfers endogenous to those municipalities.

We use two strategies. As our first strategy, we consider royalties to be exogenous. Because only 127 municipalities received royalties in 2002 and because we focus on all Brazilian municipalities, any attempt to instrument these transfers would deeply reduce our sample. As our second strategy, in line with Caselli and Michaels (2009), we build virtual royalty transfers as instruments for royalties. By considering public data on oil and gas extraction for each oil field in Brazil and each municipality's coefficient participation in those royalties, we can calculate virtual royalties' transfers to recipients' municipalities. Those virtual transfers are exogenous to municipalities' influence because the extraction of oils is not politically determined. Further, the variation across municipalities in our measure of oil output must be overwhelmingly driven by the size of the oilfield, the technical difficulty of extracting the oil in that particular location, and the share of the oilfield that is "captured" by the continental extension of the municipal boundaries (Caseli and Michaels, 2009). As shown in Table 11, our instrument seems to be valid (the F-statistic larger than 2), except in the case of agricultural municipalities. The IV coefficient is estimated to be between 0.6 and 1.1.

Insert Table 11 here

Transfers to municipalities conditional on health expenditures depend on the population in a linear fashion, and this rule applies to all municipalities. One potential source of endogenous variation is a change on the aggregated fiscal revenues that compose PAB annually. This change in fiscal revenue could be caused by aggregated demand or supply shocks to the Brazilian economy. We therefore decide to consider that municipalities cannot manipulate the amount of health transfers received, and we use those transfers directly in the regression.

5. Results

5.1. Expenditures and Tax Revenues

5.1.1. Main results

Table 12 presents the results for the linear specification model and refers only to the municipalities that contain up to 169,800 inhabitants, excluding those with state capitals. Column (1) shows the result for general expenditures, column (2) for tax revenues, column (3) for education expenditures, and column (4) for health expenditures. We focus the analysis on the effects of income, unconditional transfers (FPM and royalties), and conditional transfers (FUNDEB and PAB-SUS).

Our results consistently show that income has a smaller effect on public expenditures than any instrumented transfers. We estimate that a $1 increase in local GDP leads to an increase of 0.021 in general expenditures and of 0.008 on both tax revenues and education expenditures.[23] We

[23] Our procedure seems to correct for the magnitude of income and transfers. We find that after we instrument for local GDP, the coefficient is more than five times (two times and eight times) higher when we use total expenditures (tax

also find a statistically significant positive effect of FPM transfers on expenditures (0.882). This figure is close to that obtained by Inman (1971), Case, Hines, and Rosen (1993), Feldstein (1975), and Olmsted, Denzau and Roberts (1993), which are equal to 0.65, 0.58, 0.6, and 0.68, respectively. Finally, we find a nonsignificant effect of FPM transfers on tax collection (-0.025).

The effects of FPM transfers on education expenditures (0.245) and health expenditures (0.155) are both positive and significant and are close to the levels established by law for total revenue (i.e., municipalities must spend 25% on education and 15% on health.) The effect of education transfers (FUNDEB) on general expenditures is also positive and significant (0.562) but lower than the corresponding effect of FPM transfers. FUNDEB transfers also act by reducing both tax revenues (-0.12) and health expenditures (-0.04).

We estimate that an R$1 increase in royalties implies only an R$0.50 increase in total expenditures and an R$0.20 increase in health expenditures. The larger effect on total expenditures (approximately 1.3) is due to health transfers. An increase in health transfers also has a strong effect on health expenditures (0.848).

Our instruments for FPM and FUNDEB transfers aim to capture the ex-ante conditions for each recipient municipality, excluding local governments' policies or political movements to receive larger benefits, which were possibly not captured by our controls. Therefore, a plausible explanation for our results in columns (3) and (4) is that the amount of FPM transfers (royalties and FUNDEB) received by the municipalities may be positively related to these local policies/political movements to obtain more transfers, which, in turn, induce an upward bias (downward) on the FPM (royalties and FUNDEB) coefficients. This finding essentially implies that municipalities with larger FPM transfers per capita (less populated municipalities), on average, are those with larger public expenditures. By contrast, municipalities that receive lower FUNDEB grants are those that are willing to make a greater effort to increase their share, which would bias our estimates downward. Finally, the main beneficiaries of royalties are municipalities that are less interested in additional transfers and, therefore, are municipalities that have a downward bias for that coefficient. Health transfers (PAB-SUS) present a consistent coefficient that close to that for the IV estimations.

Insert Table 12 here

We could not find any statistical difference between the effects of unconditional and conditional transfers on local public spending. This result is consistent with the theoretical prediction that municipalities already devote a larger amount of resources than the amount of transfers to those specific purposes (education or health). More important, our estimations suggest

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revenues and education expenditures) as the dependent variable. This result indicates that the potential downward bias for that coefficient is attenuated. We find no effect of GDP on health expenditures.

that unconditional and conditional transfers have a full crowding-in effect on local spending; i.e., for each $1 of those transfers, we estimate an increase of $1 on local expenditures. However, the coefficient for FPM is statistically similar to the coefficient for PAB-SUS but larger than that for FUNDEB, which, in turn, is similar to the coefficient for royalties.

Regarding the comparison of the effects between local income and unconditional transfers, the estimated effect of income is lower than the corresponding effect of FPM transfers. We estimate that a $1 increase in local residents' income has a much smaller effect on local public spending than a $1 increase in general-purpose transfers (FPM or royalties). The response of general expenditures to FPM transfers is, in fact, quite large: 0.882. While the response of general expenditures to per capita royalties is much lower, 0.497, it is nonetheless larger than the effect of income. Grant money, therefore, seems to stick in the public sector, providing additional support to the phenomenon known as the flypaper effect.

McGuire (1978) finds evidence that approximately 64% to 69% of education grants and 76% of noneducation grants (federal grants to local grants) in the United States are fungible. He also finds an increase in the fungibility of grants from 1964 to 1971, which he explains by the increase in the capacity of bureaucrats to manipulate budgets and thus to avoid complying with the restrictions on grant use. By contrast, Zampelli (1986) finds no fungibility effect when he evaluates the effect of aid at different levels of government. Shah (1989) reaches the same conclusion in analyzing the effects of provincial transportation assistance on municipalities in Alberta, Canada[24].

To analyze more carefully the effect of conditional and unconditional transfers on local public spending, we focus on the most important expenditures of Brazilian municipalities. Almost 60% of local government spending goes to education and health; thus, we analyze the effects of income, unconditional transfers, and conditional transfers on these specific areas.

First, we analyze the effects of unconditional transfers (FPM and royalties) on education spending. We find evidence that both types of general-purpose transfers have greater stimulatory effects than income. The results indicate that $1 received in FPM transfers tends to increase expenditures on education by $0.25, whereas a $1 increase in income only increases expenditures on education by $0.008. Royalties, on the other hand, does not seem to have a statistically significant impact on education expenditures, but they do increase health expenditures by $0.21. This result implies that the magnitudes of the effects of FPM transfers and royalties on education spending statistically differ. This amount is very close to the estimates of Dahlberg et al. (2008) and Lundqvist (2013), which vary between 0.6 and 0.90, and much larger than the estimates of Ehrenberg (1973).

[24] See McGillivray and Morrissey (2000) for a review of the fungibility literature, particularly its role and insufficiency as an approach in Assessing Aid (World Bank 1998),

However, the effect of transfers on the average tax rate is nonsignificant. This result is similar to that found by Dalhberg et al. (2008), who also do not find a significant effect of grants on private goods. This result is, however, contrary to the evidence found by Lundqvist (2013), who identifies a significant and negative effect that is larger than our estimate for FUNDEB (-0.14 versus -0.03, respectively).

Next, we compare the effects of unconditional transfers and conditional transfers on education spending. We find that a \$1 increase in FUNDEB transfers has a greater effect on education spending than a \$1 increase in unconditional transfers (\$0.935 and \$0.245, respectively). The effect of unconditional transfers on education spending is much smaller, as expected. Additionally, the value of 0.951 is not statistically different from one, providing evidence on favor of the fungibility effect.[25]

Our estimates of the effect of FUNDEB transfers on education spending can be compared with those of Knight (2002) for two reasons. First, both estimates relate to the effects of specific-purpose expenditures. Second, both empirical strategies aim to eliminate the possible upward bias in the conditional transfer estimates. However, our strategy, which consists of building a virtual FUNDEB that is not exposed to municipalities' decisions, is different from the political channel used by Knight (2002), although we also control for the same political variables. However, while Knight finds evidence of a full crowding-out effect, our observed effect of conditional transfers is smaller, although we nonetheless find a statistical difference between the effects of conditional transfers and income.

Our results are comparable to those of Gordon (2004), who considers the effects of the Title I program on local revenue and expenditures. She finds a large effect for the first year (coefficient 1.4), but the effect becomes nonsignificant after three years. However, the mean effect over these three years (0.8) is very close to our estimated coefficient (0.7).

Regarding health expenditures, the results are generally similar to those obtained for education expenditures. The effect of unconditional transfers is much larger than the effect of income. Further, we find that conditional health transfers have a greater effect on health spending than unconditional transfers, as expected. However, the conditionality imposed by the central government does not ensure that the funds are being spent on health, as the coefficient for health transfers is only 0.848. Local governments, therefore, seem to shift their health transfers to other areas. Additionally, education transfers seems to negatively influence (-0.04) health expenditures.

[25] Therefore, our IV must have captured the effort of some municipalities to obtain a larger number of pupils in their municipal school system to receive a higher volume of transfers, which should be positively associated with preferences for local public goods. Once we control for these preferences for public goods by instrumenting FUNDEB, we find that spending on education increases by the same amount of grants received (FUNDEB), suggesting that Brazilian municipalities are not diverting resources transferred to education to other purposes.

The coefficient for FPM is 0.15, and the coefficient for GDP is close to zero. By contrast, the coefficient for royalties is 0.2. [26]

5.1.3 Functional forms

One of the main criticisms of the flypaper effect concerns the lack of robustness of the empirical findings. Becker (1996), for example, argues that the estimates of the flypaper effect are sensitive to the functional form used. To address this question, we also provide regressions wherein all variables are presented in logarithms (Table 13).

Insert Table 13 here

The results remain qualitatively the same. Regarding the effect of transfers on total public spending, our results show that local public spending is more elastic to unconditional transfers (0.336 for FPM, 0.11 for royalties) than conditional transfers (0.05 for FUNDEB, 0.034 for PAB) and that the elasticity of conditional transfers is closer to the elasticity of income (0.079). Note that only unconditional transfers seem to have a consistently large effect (elasticity-wise) on total public spending.

FPM and FUNDEB transfers act by reducing tax revenues, as opposed to income, corroborating evidence of the flypaper effect, as shown by Dalberg et al. (2008). The elasticity effect of royalties on tax collection is positive but much lower than the income elasticity effect.

Additionally, our results confirm that FUNDEB transfers (elasticity equal to 0.32) and PAB (elasticity equal to 0.17) consistently have a greater effect on the respective type of public spending (education and health) than unconditional transfers and income. The income elasticity effect is estimated to have the smallest effect on education and health expenditures, with even a negative effect on health expenditures. Royalties seem to positively affect health expenditures (elasticity equal to 0.14), whereas FUNDEB transfers seem to negatively affect spending on health (elasticity equal to - 0.11).

5.1.4. Robustness checks

Brollo et al. (2011) use many different population brackets as a robustness check in identifying the exogenous variation of FPM transfers. Because we consider municipalities with up to 167,800

[26] When we restrict the sample to the municipalities that present an agricultural GDP larger than the Brazilian median, our second-stage results are qualitatively similar to our main results, with one important distinction: we find a much larger effect of health transfers on total expenditures (2.17 versus 1.39). These results confirm that the largest effect on total expenditures is produced by health transfers, followed by FPM transfers, royalties, and FUNDEB transfers. FUNDEB transfers are the only type of transfer that influences (negatively) tax revenues. Education transfers and health transfers do have a much larger effect on their respective expenditures than other types of transfers. When we restrict the sample to coastal municipalities, we estimate a much larger effect of royalties on total expenditures (1.264), on education expenditures (0.18), and on health expenditures (0.31). All the remaining transfers do not seem to affect total expenditures. FUNDEB transfers, however, still have a negative effect on tax revenues.

inhabitants across 17 different population brackets, we also present the results for three sets of population brackets (1 to 3, 4 to 7, and 8 to 17) to verify the sensitivity of the results to the choice of brackets.

According to the results presented in Table 14, the only set of where the (second-stage) coefficient for FPM is significant with respect to local public expenditures is the first one. Brollo et al. (2013) and Litschig and Morrison (2013) find the same evidence. We estimate a much larger effect of FPM transfers on total expenditures (2.18), education expenditures (0.57), and health expenditures (0.94) for the first bracket than we had previously estimated (0.84, 0.4, and 0.15, respectively). More important, the results for FUNDEB and PAB are qualitatively similar to the previously obtained results for all dependent variables considered.

Insert Table 14 here

We also include all the control variables in the regressions, in addition to the running variable and time and fixed effects, to determine whether the responses of the variables of interest change (Table 15).

Insert Table 15 here

The results confirm that income has a negligible effect on total expenditures and a very small, although statistically significant, effect on education expenditures (0.008). Surprisingly, the effect of income on health expenditures is still negative (-0.015) and significant. The estimated effect of all transfers is lower than one. Moreover, we cannot reject the null hypothesis that unconditional and conditional transfers have different effects on total expenditures.

5.1.5. Subsamples

We also consider two different subsamples that are restricted (i) to municipalities that present an agricultural GDP larger than the Brazilian median and (ii) to coastal municipalities. Table 16 presents the results. We find a large effect in our first stage for these samples (columns (5) and (6) in Tables 8-10). For agricultural municipalities, our second-stage results are qualitatively similar to our main results (Table 12) with one important distinction: we find a much larger effect of health transfers on total expenditures (2.17 versus 1.39). These results confirm that the largest effect on total expenditures is produced by health transfers, followed by FPM transfers, royalties, and FUNDEB transfers. However, FUNDEB transfers are the only type of transfer that influences (negatively) tax revenues for this subsample. Education transfers and health transfers do have a much larger effect on their respective types of expenditures than other transfers.

For coastal municipalities, we estimate a much larger (and the only significant) effect of royalties on total expenditures (1.264 versus 0.81 in Caselli and Michaels (2011)), education expenditures (0.18 versus 0.13 in Caselli and Michaels (2011)), and health expenditures (0.31

versus 0.10 in Caselli and Michaels (2011).) Notably, no other transfers seem to affect public expenditures. FUNDEB transfers still have a negative effect on tax revenues.

Insert Table 16 here

6. Conclusion

This paper estimates the effect of unconditional and conditional grants on Brazilian municipalities' fiscal behavior for the period between 2002 and 2008. We explore a variety of transfers received by local governments (unconditional grants, including windfall resources, and conditional grants) and build instruments for each possible endogenous transfer based on its rule of distribution. Consequently, we can carefully identify the causal effect of grants on public expenditures. More important, because variation lies within countries, we can ignore differences in political and fiscal institutions and the degree of fiscal competition (Dalhberg et al., 2008).

The effect of general-purpose grants on public spending is diverse. There is evidence of full displacement, in which case the coefficient for the grant variable in the expenditure equation is not statistically significant. There is also ample evidence that the effect of general-purpose grants is larger than the effect of an equal increase in private income, providing evidence for the flypaper effect (Hines and Thaler,1995; Gamkhar and Shah, 2007).

We use an RDD approach that exploits the discontinuity of the rule of distribution of FPM transfers according to population ranges in order to strengthen our empirical strategy. In a recent study, Dahlberg et al. (2008) aim to enhance their identification procedure by using a discontinuity in the grant distribution formula in Sweden, according to which only municipalities with a net out-migration above 2% receive grants, in order to evaluate the effect of unconditional grants on local spending. They find evidence of a strong flypaper effect—a full crowding-in effect of transfers. Our estimated effect on public expenditures is half (0.5) that found by Dahlberg et al. (2008), suggesting the possibility of a crowding-out effect of transfers instead of a crowding-in effect. Similar to Dahlberg et al. (2008), we find no statistically significant effect of unconditional transfers on tax relief.

We compare the effects of unconditional transfers and income on public spending by using two proxies for income (local GDP). We use as instruments for FPM transfers the discontinuity in the population thresholds. For income measures, following Paxson (1992), we use rainfall variation as an instrument. The obtained evidence reinforces the results previously found for the flypaper effect (an average coefficient for FPM equal to 0.8 and for income equal to 0.01).

The literature regarding the effect of specific-purpose nonmatching grants is somewhat limited. Gordon (2004) evaluates Title I, a program that makes nonmatching transfer resources to school districts according to their number of poor children. The grants must guarantee compensatory education to disadvantaged children, such as outside classroom instruction for small groups. The results provide evidence of the flypaper effect in the first year, but the effects on spending disappear after three years. We find evidence that conditional grants (FUNDEB for education and PAB for health) have a strong effect on specific public spending. Our coefficients for conditional transfers are statistically different from those in most of the cases (evidence of fungibility) and are much larger than the effects of unconditional transfers (average coefficient equal to 0.2). Thus, conditional grants seem to have the expected effect: they increase public expenditures in their respective category to an amount close to the amount of the grant, but they have a different effect from unconditional transfers.

In summary, our estimations do not suggest that unconditional and conditional transfers have a full crowding-in effect on local public spending. Further, the effect of these transfers does not differ based on the size of local government. However, the effects of unconditional transfers on education and health spending are smaller than the effects of conditional transfers but larger than the effects of income, corroborating evidence of the flypaper effect for Brazilian municipalities.

These findings indicate that the lack of local government autonomy caused by conditional grants seems to induce consistent spending on categories of public expenditures determined by the central government.

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Tables

Table 1: FPM coefficients – All municipalities excluding state capitals

Brackets of population	Coefficient	Brackets of population	Coefficient	Brackets of population	Coefficient	Brackets of population	Coefficient
Up to 10,188	0.6	30,565-37,356	1.6	71,317-81,504	2.6	120,049-142,632	3.6
10,189-13,584	0.8	37,357-44,148	1.8	81,505-91,692	2.8	142,633-156,216	3.8
13,585-16,980	1.0	44,149-50,940	2.0	91,693-101,880	3.0	Above 156,216	4.0
16,981-23,772	1.2	50,941-61,128	2.2	101.881-115,464	3.2		
23,773-30,564	1.4	61,129-71,316	2.4	115,465-129,048	3.4		

Source: Decree no. 1881 of 1981.

Table 2: Royalty revenue destination (%) until 2012

	Up to 5%		Above 5%	
Beneficiary	Land	Sea	Land	Sea
Producing state	70	30	52.5	22.5
Producing municipality	20	30	15	22.5
Neighboring (or affected) municipality	10	10	7.5	7.5
Navy Ministry		20		15
Special Fund		10		7.5
Science and Technology Ministry			25	25

Source: Laws 7.990/89 and 9478/97

Table 3: FUNDEB composition: 2007-2020

Revenue	Year 2007	2008	2009/2020
FPE	16.66%	18.33%	20%
FPM	16.66%	18.33%	20%
ICMS	16.66%	18.33%	20%
IPIexp	16.66%	18.33%	20%
Exports	16.66%	18.33%	20%
Heritage and donations tax (ITCMD)	6.66%	13.33%	20%
State car sale tax (IPVA)	6.66%	13.33%	20%
Property tax (ITR)	6.66%	13.33%	20%

Table 4: Data on ordinances, per capita amount transferred by individual, and number of ordinances

Data	Per capita monthly	Number of ordinances
1998/January	R$10	84
2002/January	R$10.50	2034
2003/April	R$12	398
2006/April	R$13	650
2006/August	R$15	2133
2008/September	R$ 16	2490
2008/December	R$ 17	3067

Note: R$ is current Brazilian real.

Table 5: Fiscal variables – Descriptive statistics

	All municipalities			Munic. under 169,800 inhabitants excluding state capitals			Munic. under 169,800 inhabitants excluding state capitals and whose % of agricultural GDP is larger than the median in Brazil		
Variables (per capita)	***Obs.***	***Mean***	***Std. Dev.***	***Obs.***	***Mean***	***Std. Dev.***	***Obs.***	***Mean***	***Std. Dev.***
General expenditures	42,588	510.69	811.02	41,426	512.33	195.60	16,192	468.39	235.61
Education expenditures	42,432	165.82	313.14	41,274	167.08	317.25	16,129	151.23	71.85
Health expenditures	42,432	117.89	130.58	41,274	117.68	131.98	16,129	108.20	58.82
Revenue taxes	42,588	52.77	84.67	41,426	49.57	79.27	16,192	55.84	64.60
FPM	44,480	212.48	156.38	43,302	216.82	156.09	16,705	155.20	87.73
PAB	38,346	7.89	2.41	37,312	7.90	2.32	14,438	7.87	2.05
FUNDEF/FUNDEB	44,487	73.80	40.21	43,309	74.47	40.33	16,708	68.26	35.20
Royalties	44,485	6.20	76.79	43,307	6.11	76.58	16,708	4.44	81.32
Other transfers	36,791	253.46	197.04	35,772	254.17	198.69	14,015	263.58	186.60

Visibility	42,525	32.91	26.49	41,363	32.70	26.66	16,178	37.13	24.50
Fiscal complexity	42,474	200.81	12,673.77	41,312	203.84	12,850.75	16,161	111.67	324.89

Source: Finbra.Visibility and Fiscal Complexity are not per capita variables.

Table 6: Demographic and education variables – Descriptive statistics

	All municipalities			Munic. under 169,800 inhabitants, excluding state capitals			Munic. under 169,800 inhabitants, excluding state capitals and whose % of agricultural GDP is larger than the median in Brazil		
Variables	***Obs.***	***Mean***	***Std. Dev.***	***Obs.***	***Mean***	***Std. Dev.***	***Obs.***	***Mean***	***Std. Dev.***
Population per 100,000 inhabitants	44,493	0.32	1.92	43,315	0.18	0.22	16,708	0.26	0.27
Birth-Mort	44,465	1.04	0.55	43,287	1.04	0.55	16,698	1.04	0.54
% under 1 year old	44,488	1.88	0.48	43,310	1.88	0.48	16,705	1.85	0.46
% 1 and 4 years	44,488	7.87	1.78	43,310	7.88	1.79	16,705	7.77	1.73
% 5 and 9 years	44,488	10.16	1.89	43,310	10.19	1.90	16,705	10.05	1.83
% 10 and 14 years	44,488	10.56	1.84	43,310	10.60	1.84	16,705	10.29	1.64
% 15 and 19 years	44,488	10.41	1.40	43,310	10.42	1.40	16,705	10.18	1.25
% 20 and 29 years	44,488	16.61	1.80	43,310	16.55	1.78	16,705	16.81	1.66
% 30 and 39 years	44,488	13.77	1.98	43,310	13.71	1.97	16,705	14.35	1.79
% 40 and 49 years	44,488	10.94	2.15	43,310	10.90	2.15	16,705	11.36	2.01
% 50 and 59 years	44,488	7.86	1.78	43,310	7.86	1.79	16,705	7.89	1.79
% 60 and 69 years	44,488	5.47	1.44	43,310	5.49	1.44	16,705	5.32	1.42
% 70 and 79 years	44,488	3.09	1.04	43,310	3.11	1.04	16,705	2.90	1.02
% women as a share of municipal population	44,488	49.20	1.49	43,310	49.14	1.46	16,705	49.05	1.50
% garbage service	36,265	57.88	27.98	35,275	57.00	27.77	13,689	63.65	27.30
% sewage service	36,265	25.22	32.53	35,275	24.43	32.24	13,689	26.41	33.93
% lighting service	36,263	86.52	16.91	35,273	86.28	17.00	13,687	88.36	15.76
% councilor same party as mayor	41,600	28.38	16.20	40,445	28.63	16.29	15,621	25.71	14.36
Mayor-gov. same party	44,504	0.23	0.42	43,314	0.23	0.42	16,708	0.23	0.42
Gov-pres. same party	44,504	0.23	0.35	43,314	0.14	0.35	16,708	0.12	0.33
Mayor–gov.-pres. same party	44,504	0.03	0.18	43,314	0.03	0.17	16,708	0.03	0.18
% Element. school	44,493	26.50	10.93	43,309	25.65	10.96	16,708	28.98	10.24
% Higher ed.	44,493	5.94	4.85	43,309	5.85	4.82	16,708	5.47	3.90

Source: IBGE, School Census, IPEADATA.

Table 7: Local income and rainfall data descriptive statistics

	All municipalities			Munic. under 169,800 inhabitants excluding state capitals			Munic. under 169,800 inhabitants excluding state capitals and whose % of agricultural GDP is larger than the median in Brazil		
Variables	***Obs.***	***Mean***	***Std. Dev.***	***Obs.***	***Mean***	***Std. Dev.***	***Obs.***	***Mean***	***Std. Dev.***
Mass of wages – units per capita	44,477	60.97	74.45	43,299	57.85	69.82	16,708	72.01	64.64
Local GDP units	44,488	4,077.65	4,455.29	43,310	4,008.83	4,433.35	16,705	5,236.01	4,936.29
$(R_H - R_{2000\text{-}2008})_{Summer}$	44,480	11.43	42.96	43,283	11.41	42.96	16,708	10.34	41.19
$(R_H - R_{2000\text{-}2008})_{autumn}$	44,480	-0.58	37.17	43,283	-0.68	37.00	16,708	-0.02	36.09
$(R_H - R_{2000\text{-}2008})_{winter}$	44,480	0.46	25.21	43,283	0.49	25.09	16,708	-4.47	23.99
$(R_H - R_{2000\text{-}2008})_{spring}$	44,480	-1.39	26.83	43,283	-1.43	26.78	16,708	-0.50	28.39

Source: Rais, IPEADATA, Matsuura and Wilmott 92009). Elaborated by authors. The value of local GDP units is multiplied by 1,000.

Table 8: First-stage local GDP

Independent variables	Full sample (1)	Ln variables (2)	Different - FPM brackets (3)	All control variables (4)	Agricultural municipalities (5)	Coast municipalities (6)
	Dependent variable: Income - Local per capita GDP					
$(R_H - R_{2000\text{-}2008})_{summer}$	2.667***	0.0001	2.608***	1.500***	3.673***	14.186
	(0.590)	(0.00007)	(0.595)	(0.496)	(1.002)	(24.096)
$(R_H - R_{2000\text{-}2008})_{summer}^2$	-0.048***	-8.29e-06**	-0.048***	-0.016***	-0.060***	0.425
	(0.006)	(8.31e-07)	(0.006)	(0.005)	(0.011)	(0.343)
$(R_H - R_{2000\text{-}2008})_{autumn}$	1.115**	0.001***	1.198***	0.456	0.996	-19.734
	(0.447)	(0.000)	(0.446)	(0.417)	(0.770)	(18.361)
$(R_H - R_{2000\text{-}2008})_{autumn}^2$	-0.014	-0.00001***	-0.012	0.009	-0.030**	-0.070
	(0.012)	(1.35e-06)	(0.013)	(0.011)	(0.014)	(0.322)
$(R_H - R_{2000\text{-}2008})_{winter}$	-13.188***	-0.005***	-13.227***	-4.049***	-8.290***	-54.890
	(1.681)	(0.0001)	(1.691)	(1.071)	(1.904)	(34.443)
$(R_H - R_{2000\text{-}2008})_{winter}^2$	0.053***	0.00003***	0.055***	0.039**	0.026	-1.581
	(0.017)	(2.32e-06)	(0.017)	(0.015)	(0.029)	(1.039)
$(R_H - R_{2000\text{-}2008})_{spring}$	4.501***	0.001***	4.611***	3.086***	3.130**	-2.119
	(0.867)	(0.0001)	(0.864)	(0.689)	(1.223)	(26.297)
$(R_H - R_{2000\text{-}2008})_{spring}^2$	0.004	0.00001***	0.006	-0.036*	-0.001	-0.022
	(0.016)	(2.36e-06)	(0.016)	(0.019)	(0.021)	(0.285)
Observations	35,750	31,392	35,750	31,309	14,014	306
R^2	0.488	0.671	0.483	0.537	0.539	0.433
F-statistic (income)	40.13***	177.62***	29.05***	13.05***	11.26***	1.57

Note: Robust standard errors are presented in parentheses. Standard errors are clustered at the municipal level. *** Statistically different from zero at the 0.01 level of significance. ** Statistically different from zero at the 0.05 level of significance. * Statistically different from zero at the 0.1 level of significance. **Control variables:** (1) Other transfers (per capita) received from state and federal (residual) government, population, population squared, population cubed, and time dummies. (2) Other transfers received from state and federal (residual) government per capita, population, population squared, population cubed, and time dummies. (3) Other transfers received from state and federal government per capita, Brackets 1 - 3*population, Brackets 1 - 3*population squared, Brackets 1 - 3*population cubed, Brackets 4 - 7*population, Brackets 4 - 7*population squared, Brackets 4 - 7*population cubed, and Brackets 8 - 17*population, Brackets 8 - 17*population squared, Brackets 8 - 17*population cubed, and time dummies. (4) Other transfers received from state and federal (residual) government per capita, population per 100,000 inhabitants, births minus mortality/population, percentage of population under 1 year old (between 1 and 4 years old, 5 and 9 years old, 10 and 14 years old, 15 and 19 years old, 20 and 29 years old, 30 and 39 years old, 40 and 49 years old, 50 and 59 years old, 60 and 69 years old, and 70 and 79 years old), percentage of female population, percentage of housing with garbage service, percentage of housing with sewage service, percentage of housing with lighting service (families variables are lagged), visibility, fiscal complexity, percentage of councilors elected in the same party as the mayor, percentage of individuals who completed elementary school, percentage of individuals who completed higher education, and time dummies. (5) Other transfers received from state and federal (residual) government per capita, population, population squared, population cubed, and time dummies. (6) Other transfers received from state and federal (residual) government per capita, population, population squared, population cubed, and time dummies.

Table 9: First-stage per capita FPM

	Full sample	Ln variables	Different FPM brackets	All control variables	Agricultural municipalities	Coastal municipalities
	Dependent variable: Per capita actual FPM					
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)
Brackets of population between 1 and 17	0.007***	0.101***	-	0.005***	0.005***	-0.001
	(0.001)	(0.004)	-	(0.001)	(0.001)	(0.001)
Brackets of population between 1 and 3	-	-	-0.001***	-	-	-
	-	-	(0.000)	-	-	-
Brackets of population between 4 and 7	-	-	-0.001***	-	-	-
	-	-	(0.000)	-	-	-
Brackets of population between 8 and 17	-	-	-0.001***	-	-	-
	-	-	(0.000)	-	-	-
Observations	35,750	31,392	35,750	31,309	14,014	306
R^2	0.584	0.931	0.956	0.616	0.642	0.599
F-statistic (Brackets btw. 1 and 17)	16.86***	92.47***		12.10***	7.92***	1.19
F-statistic (Brackets btw. 1 and 3)	-	-	91.53***	-	-	-
F-statistic (Brackets btw. 4 and 7)	-	-	96.71***	-	-	-
F-statistic (Brackets btw. 8 and 17)	-	-	36.51***	-	-	-

Note: Robust standard errors are presented in parentheses. Standard errors are clustered at the municipal level. *** Statistically different from zero at the 0.01 level of significance. ** Statistically different from zero at the 0.05 level of significance. *

Statistically different from zero at the 0.1 level of significance. **Control variables:** (1) Other transfers (per capita) received from state and federal (residual) government, population, population squared, population cubed, and time dummies. (2) Other transfers received from state and federal (residual) government per capita, population, population squared, population cubed, and time dummies. (3) Other transfers received from state and federal government per capita, Brackets 1 - 3*population, Brackets 1 - 3*population squared, Brackets 1 - 3*population cubed, Brackets 4 - 7*population, Brackets 4 - 7*population squared, Brackets 4 - 7*population cubed, and Brackets 8 - 17*population, Brackets 8 - 17*population squared, Brackets 8 - 17*population cubed, and time dummies. (4) Other transfers received from state and federal (residual) government per capita, population per 100,000 inhabitants, births minus mortality/population, percentage of population under 1 year old (between 1 and 4 years old, 5 and 9 years old, 10 and 14 years old, 15 and 19 years old, 20 and 29 years old, 30 and 39 years old, 40 and 49 years old, 50 and 59 years old, 60 and 69 years old, and 70 and 79 years old), percentage of female population, percentage of housing with garbage service, percentage of housing with sewage service, percentage of housing with lighting service (families variables are lagged), visibility, fiscal complexity, percentage of councilors elected in the same party as the mayor, percentage of individuals who completed elementary school, percentage of individuals who completed higher education, and time dummies. (5) Other transfers received from state and federal (residual) government per capita, population, population squared, population cubed, and time dummies. (6) Other transfers received from state and federal (residual) government per capita, population, population squared, population cubed, and time dummies.

Table 10: First-stage per capita education transfers (FUNDEB)

	Full sample	Ln variables	Different FPM brackets	All control variables	Agricultural municipalities	Coastal municipalities
	Dependent variable: Per capita actual FUNDEB					
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)
Per capita calculated FUNDEB	1.946***	0.101***	1.947***	1.323***	2.316***	1.648***
	(0.075)	(0.004)	(0.075)	(0.084)	(0.093)	(0.489)
Observations	35,750	31,392	35,750	31,309	14,014	306
R^2	0.318	0.312	0.963	0.440	0.355	0.534
F-statistic	115.36***	92.47***	97.04***	43.81***	98.43***	6.34***

Note: Robust standard errors are presented in parentheses. Standard errors are clustered at the municipal level. *** Statistically different from zero at the 0.01 level of significance. ** Statistically different from zero at the 0.05 level of significance. * Statistically different from zero at the 0.1 level of significance. **Control variables:** (1) Other transfers (per capita) received from state and federal (residual) government, population, population squared, population cubed, and time dummies. (2) Other transfers received from state and federal (residual) government per capita, population, population squared, population cubed, and time dummies. (3) Other transfers received from state and federal government per capita, Brackets 1 - 3*population, Brackets 1 - 3*population squared, Brackets 1 - 3*population cubed, Brackets 4 - 7*population, Brackets 4 - 7*population squared, Brackets 4 - 7*population cubed, and Brackets 8 - 17*population, Brackets 8 - 17*population squared, Brackets 8 - 17*population cubed, and time dummies. (4) Other transfers received from state and federal (residual) government per capita, population per 100,000 inhabitants, births minus mortality/population, percentage of population under 1 year old (between 1 and 4 years old, 5 and 9 years old, 10 and 14 years old, 15 and 19 years old, 20 and 29 years old, 30 and 39 years old, 40 and 49 years old, 50 and 59 years old, 60 and 69 years old, and 70 and 79 years old), percentage of female population, percentage of housing with garbage service, percentage of housing with sewage service, percentage of housing with lighting service (families variables are lagged), visibility, fiscal complexity, percentage of councilors elected in the same party as the mayor, percentage of individuals who completed elementary school, percentage of individuals who completed higher education, and time dummies. (5) Other transfers received from state and federal (residual) government per capita, population, population squared, population cubed, and time dummies. (6) Other transfers received from state and federal (residual) government per capita, population, population squared, population cubed, and time dummies.

Table 11: First-stage per capita royalty transfers

	Full sample	Ln variables	Different FPM brackets	All control variables	Agricultural municipalities	Coastal municipalities
	Dependent variable: Per capita royalty transfers					
Independent variables	(1)	(2)	(3)	(4)	(5)	(6)
Per capita calculated oil transfers	0.885**	1.192***	0.884**	0.766**	0.696**	0.679*
	(0.428)	(0.043)	(0.428)	(0.345)	(0.307)	(0.345)
Observations	35,750	31,392	35,750	31,309	14,014	306
R^2	0.346	0.198	0.346	0.356	0.445	0.410
F-statistic	3.54***	87.33***	3.07***	3.55***	1.18	2.34**

Note: Robust standard errors are presented in parentheses. Standard errors are clustered at the municipal level. *** Statistically different from zero at the 0.01 level of significance. ** Statistically different from zero at the 0.05 level of significance. * Statistically different from zero at the 0.1 level of significance. **Control variables:** (1) Other transfers (per capita) received from state and federal (residual) government, population, population squared, population cubed, and time dummies. (2) Other transfers received from state and federal (residual) government per capita, population, population squared, population cubed, and time dummies. (3) Other transfers received from state and federal government per capita, Brackets 1 - 3*population, Brackets 1 - 3*population squared, Brackets 1 - 3*population cubed, Brackets 4 - 7*population, Brackets 4 - 7*population squared, Brackets 4 - 7*population cubed, and Brackets 8 - 17*population, Brackets 8 - 17*population squared, Brackets 8 - 17*population cubed, and time dummies. (4) Other transfers received from state and federal (residual) government per capita, population per 100,000 inhabitants, births minus mortality/population, percentage of population under 1 year old (between 1 and 4 years old, 5 and 9 years old, 10 and 14 years old, 15 and 19 years old, 20 and 29 years old, 30 and 39 years old, 40 and 49 years old, 50 and 59 years old, 60 and 69 years old, and 70 and 79 years old), percentage of female population, percentage of housing with garbage service, percentage of housing with sewage service, percentage of housing with lighting service (families variables are lagged), visibility, fiscal complexity, percentage of councilors elected in the same party as the mayor, percentage of individuals who completed elementary school, percentage of individuals who completed higher education, and time dummies. (5) Other transfers received from state and federal (residual) government per capita, population, population squared, population cubed, and time dummies. (6) Other transfers received from state and federal (residual) government per capita, population, population squared, population cubed, and time dummies.

Table 12: General results

	Per capita general expenditures	Per capita tax revenues	Per capita education expenditures	Per capita health expenditures
Independent variables	(1)	(2)	(3)	(4)
Per capita income	0.021***	0.008***	0.008***	-0.001
	(0.003)	(0.002)	(0.001)	(0.001)
Per capita FPM	0.882***	-0.025	0.245***	0.155***
	(0.063)	(0.046)	(0.031)	(0.024)
Per capita education transfers (FUNDEB)	0.562***	-0.125***	0.935***	-0.041*
	(0.073)	(0.047)	(0.035)	(0.025)

Independent variables	(1)	(2)	(3)	(4)
Per capita royalty transfers	0.497***	-0.012	0.021	0.200***
	(0.122)	(0.045)	(0.023)	(0.034)
Per capita health transfers (PAB)	1.396***	-0.339*	-0.219	0.848***
	(0.316)	(0.194)	(0.136)	(0.107)
Observations	35,750	35,750	35,598	35,598
R^2	0.869	0.249	0.714	0.761

Note: Robust standard errors are presented in parentheses. Standard errors are clustered at the municipal level. *** Statistically different from zero at the 0.01 level of significance. ** Statistically different from zero at the 0.05 level of significance. * Statistically different from zero at the 0.1 level of significance. Control variables: other transfers (per capita) received from state and federal government, population, population squared, population cubed, and time dummies. Sample includes municipalities with under 169,800 inhabitants and excludes state capitals.

Table 13: Ln General results

	(1)	(2)	(3)	(4)
	Ln Per capita general expenditures	Ln Per capita tax revenues	Ln per capita education expenditures	Ln per capita health expenditures
Independent variables	(1)	(2)	(3)	(4)
Ln per capita income	0.079***	0.452***	0.033	-0.178***
	(0.018)	(0.067)	(0.022)	(0.031)
Ln per capita FPM	0.336***	-0.348***	0.191***	0.062
	(0.025)	(0.099)	(0.034)	(0.055)
Ln per capita education transfers (FUNDEB)	0.055***	-0.302***	0.326***	-0.113***
	(0.012)	(0.048)	(0.018)	(0.021)
Ln per capita royalty transfers	0.116***	0.101***	0.070***	0.146***
	(0.012)	(0.031)	(0.010)	(0.018)
Ln per capita health transfers (PAB)	0.034***	0.046	-0.029***	0.171***
	(0.008)	(0.030)	(0.011)	(0.014)
Observations	31,392	31,377	31,142	31,074
R-squared	0.861	0.563	0.671	0.552

Note: Robust standard errors are presented in parentheses. Standard errors are clustered at the municipal level. *** Statistically different from zero at the 0.01 level of significance. ** Statistically different from zero at the 0.05 level of significance. * Statistically different from zero at the 0.1 level of significance. Control variables: other transfers (per capita) received from state and federal government, population, population squared, population cubed, and time dummies. Sample includes municipalities with under 169,800 inhabitants and excludes state capitals.

Table 14: General results using different brackets of population

	Per capita general expenditures	Per capita tax revenues	Per capita education expenditures	Per capita health expenditures
Independent variables	(1)	(2)	(3)	(4)
Per capita income	0.012***	0.008***	0.005***	-0.003***
	(0.003)	(0.002)	(0.001)	(0.001)
Brackets of population between 1 and 3	2.186***	-0.120	0.575***	0.949***
	(0.508)	(0.291)	(0.164)	(0.173)
Brackets of population between 4 and 7	3.704***	0.277	0.289	0.845***
	(0.780)	(0.417)	(0.221)	(0.217)
Brackets of population between 8 and 17	0.135	-0.532	1.454***	-0.856
	(1.820)	(1.003)	(0.564)	(0.722)
Per capita education transfers (FUNDEB)	0.279**	-0.110**	0.848***	-0.077**
	(0.124)	(0.044)	(0.033)	(0.032)

Per capita royalty transfers	0.647***	-0.022	0.070***	0.221***
	(0.113)	(0.039)	(0.017)	(0.035)
Per capita health transfers (PAB)	1.937***	-0.372*	-0.003	0.813***
	(0.509)	(0.200)	(0.150)	(0.140)
Observations	35,750	35,750	35,598	35,598
R2	0.764	0.242	0.686	0.630

Note: Robust standard errors are presented in parentheses. Standard errors are clustered at the municipal level. *** Statistically different from zero at the 0.01 level of significance. ** Statistically different from zero at the 0.05 level of significance. * Statistically different from zero at the 0.1 level of significance. Control Variables: Ln other transfers (per capita) received from state and federal government, Brackets 1 - 3*population, Brackets 1 - 3*population squared, Brackets 1 - 3*population cubed, Brackets 4 - 7*population, Brackets 4 - 7*population squared, Brackets 4 - 7*population cubed, and Brackets 8 - 17*population, Brackets 8 - 17*population squared, Brackets 8 - 17*population cubed, and time dummies. Sample includes municipalities with under 169,800 inhabitants and excludes state capitals.

Table 15: General results using different controls

	Per capita general expenditures	Per capita tax revenues	Per capita education expenditures	Per capita health expenditures
Independent variables	(1)	(2)	(3)	(4)
Per capita income	0.002	-0.009	0.008***	-0.015***
	(0.005)	(0.006)	(0.002)	(0.004)
Per capita FPM	0.817***	-0.072	0.259***	-0.055
	(0.091)	(0.106)	(0.048)	(0.065)
Per capita education transfers (FUNDEB)	0.926***	0.097	0.818***	0.217***
	(0.094)	(0.098)	(0.057)	(0.073)
Per capita royalty transfers	0.788***	0.238***	0.020	0.404***
	(0.119)	(0.089)	(0.037)	(0.058)
Per capita health transfers (PAB)	0.684***	-0.665***	-0.053	0.618***
	(0.254)	(0.246)	(0.134)	(0.168)
Observations	31,309	31,309	31,183	31,183
R^2	0.915	0.120	0.704	0.366

Note: Robust standard errors are presented in parentheses. Standard errors are clustered at the municipal level. *** Statistically different from zero at the 0.01 level of significance. ** Statistically different from zero at the 0.05 level of significance. * Statistically different from zero at the 0.1 level of significance. Control variables: other transfers (per capita) from state and federal government, population per 100,000 inhabitants, births minus mortality/population, percentage of population under 1 year old (between 1 and 4 years old, 5 and 9 years old, 10 and 14 years old, 15 and 19 years old, 20 and 29 years old, 30 and 39 years old, 40 and 49 years old, 50 and 59 years old, 60 and 69 years old, and 70 and 79 years old), percentage of female population, percentage of housing with garbage service, percentage of housing with sewage service, percentage of housing with lighting service (families variables are lagged), visibility, fiscal complexity, percentage of councilors elected in the same party as the mayor, percentage of individuals who completed elementary school, percentage of individuals who completed higher education, and time dummies. Sample includes municipalities with under 169,800 inhabitants and excludes state capital municipalities.

Table 16: General results for agricultural and coastal municipalities

	Per capita general expenditures	Per capita tax revenues	Per capita education expenditures	Per capita health expenditures
		Agricultural municipalities		
Independent variables	(1)	(2)	(3)	(4)
Per capita income	0.014***	0.010***	0.006***	-0.002
	(0.004)	(0.002)	(0.001)	(0.001)
Per capita FPM	0.663***	0.067	0.273***	0.118**
	(0.168)	(0.095)	(0.066)	(0.054)
Per capita education transfers (FUNDEB)	0.245**	-0.275***	0.833***	-0.025
	(0.107)	(0.068)	(0.041)	(0.042)
Per capita royalty transfers	0.555***	-0.056	0.041*	0.195***
	(0.143)	(0.047)	(0.024)	(0.044)
Per capita health transfers (PAB)	2.176***	0.127	-0.281	1.024***

	(1)	(2)	(3)	(4)
	(0.497)	(0.335)	(0.192)	(0.202)
Observations	14,014	14,014	13,951	13,951
R^2	0.863	0.248	0.738	0.709

		Coastal municipalities		
	(5)	(6)	(7)	(8)
Per capita income	-0.035	0.000	-0.003	-0.009
	(0.030)	(0.012)	(0.004)	(0.007)
Per capita FPM	-3.959	-1.009	-0.313	-0.570
	(3.107)	(1.419)	(0.561)	(0.664)
Per capita education transfers (FUNDEB)	2.944	-4.838**	0.682	0.307
	(3.761)	(2.165)	(0.619)	(0.901)
Per capita royalty transfers	1.264***	0.080	0.182***	0.313***
	(0.449)	(0.202)	(0.067)	(0.092)
Per capita health transfers (PAB)	24.347	-2.190	7.712	4.197
	(27.483)	(17.503)	(6.314)	(8.954)
Observations	306	306	306	306
R^2	0.549	-0.090	0.648	0.602

Note: Robust standard errors are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1. **Control Variables**: Per capita other transfers received from state and federal government, population, population squared, population cubed, and time dummies. The results include municipalities without state capitals and with under 169,800 inhabitants

Figures

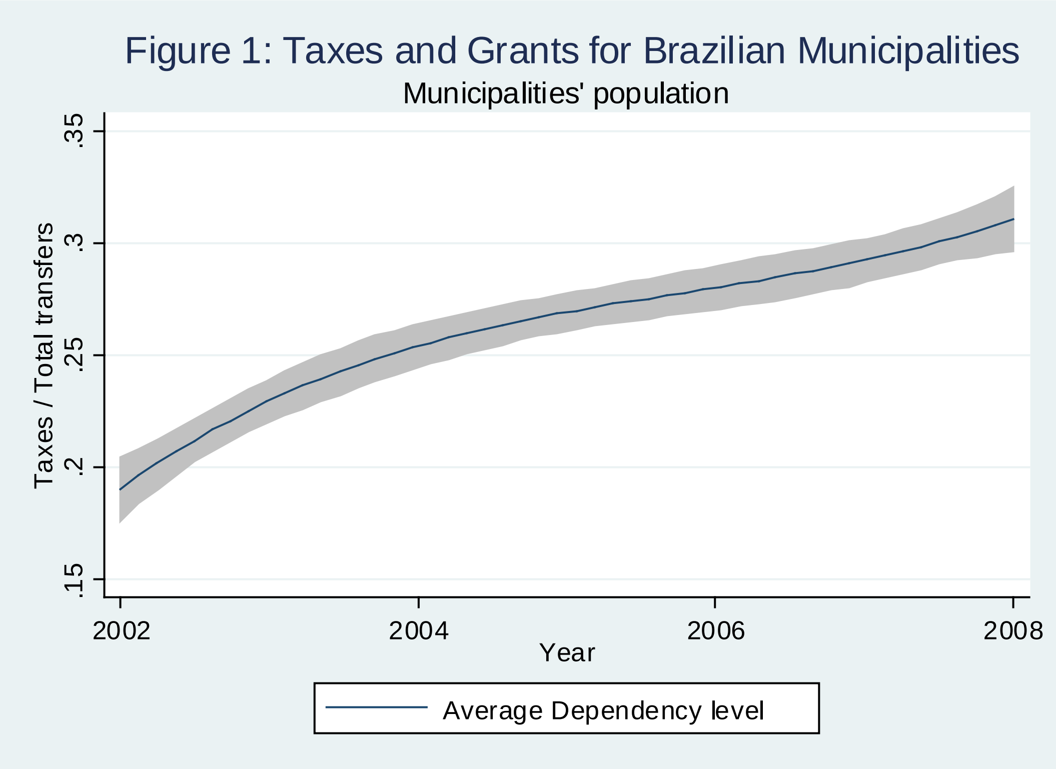
Figure 1: Taxes and Grants for Brazilian Municipalities
Municipalities' population
Taxes / Total transfers
.15
.2
.25
.3
.35
2002
2004
2006
2008
Year
Average Dependency level

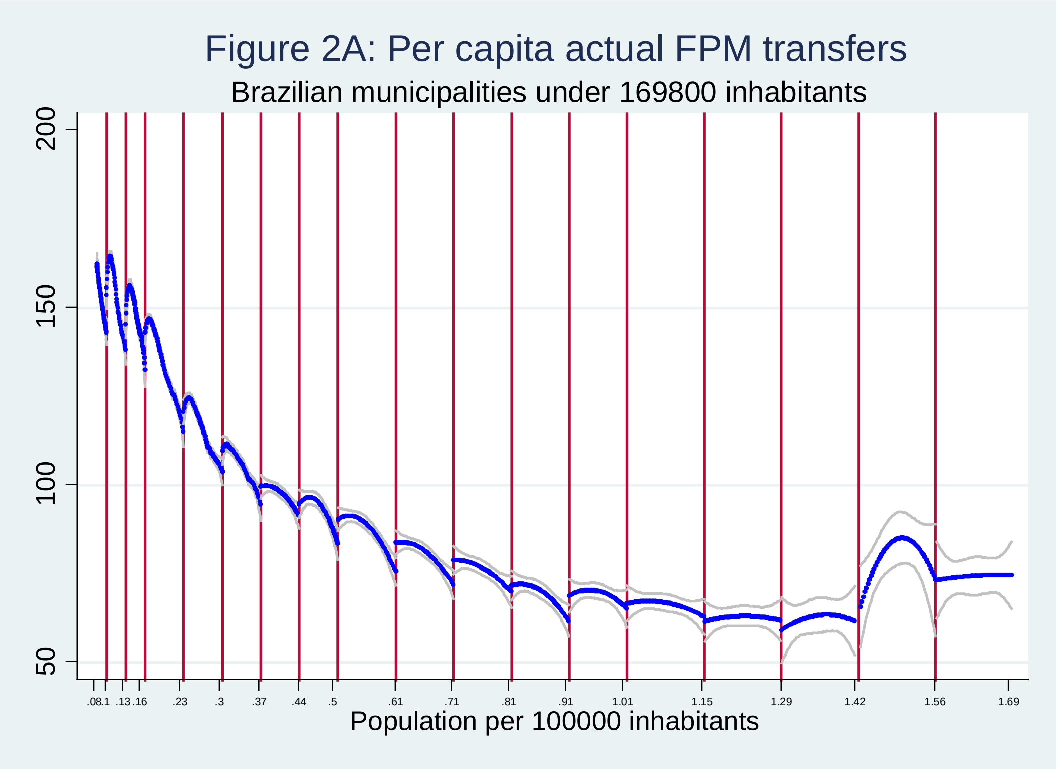
Figure 2A: Per capita actual FPM transfers
Brazilian municipalities under 169800 inhabitants
200
150
100
50
.08.1 .13.16 .23 .3 .37 .44 .5 .61 .71 .81 .91 1.01 1.15 1.29 1.42 1.56 1.69
Population per 100000 inhabitants

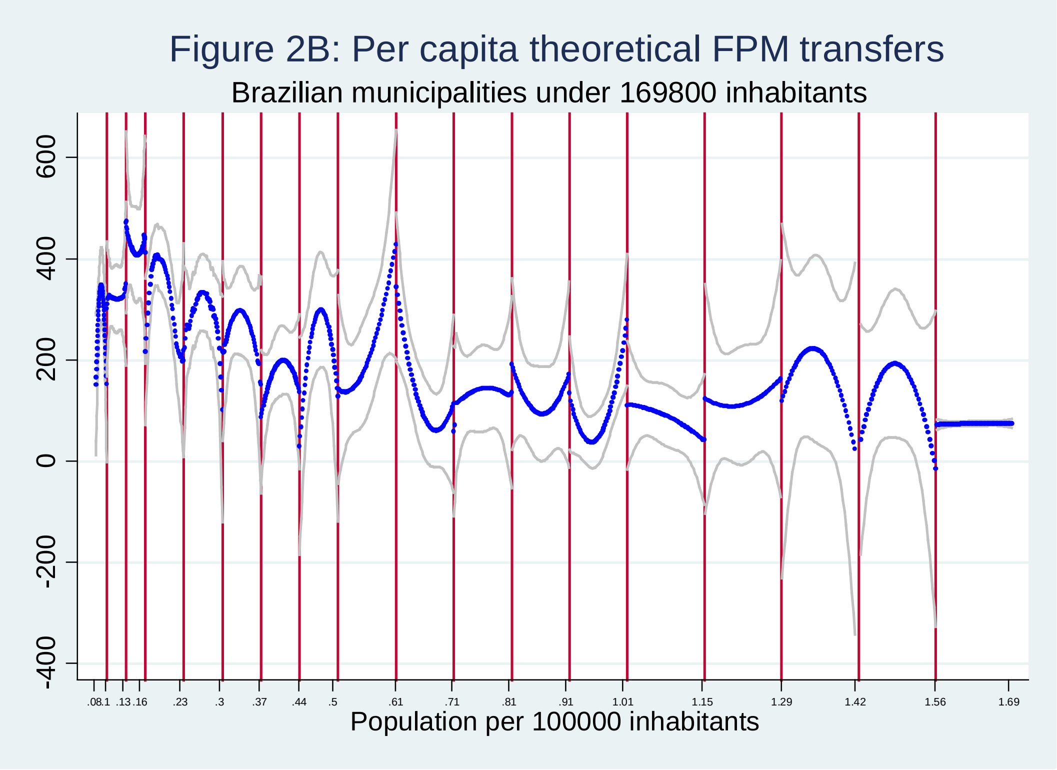
Figure 2B: Per capita theoretical FPM transfers
Brazilian municipalities under 169800 inhabitants
600
400
200
0
-200
-400
.08.1 .13.16 .23 .3 .37 .44 .5 .61 .71 .81 .91 1.01 1.15 1.29 1.42 1.56 1.69
Population per 100000 inhabitants

Appendix

Table A.1.: Comparing Per capita income coefficients without and with IV

	Per capita general expenditures	Per capita tax revenues	Per capita education expenditures	Per capita health expenditures
	General results			
Per capita income with FE	0.004***	0.003***	0.001**	0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Per capita income with FE-IV	0.021***	0.008***	0.008***	-0.001
	(0.003)	(0.002)	(0.001)	(0.001)
	Ln General results			
Ln Per capita income with FE	0.096***	0.263***	0.081***	0.078***
	(0.008)	(0.023)	(0.011)	(0.012)
Ln Per capita income with FE-IV	0.079***	0.452***	0.033	-0.178***
	(0.018)	(0.067)	(0.022)	(0.031)
	With different brackets of population			
Per capita income with FE	0.004***	0.003***	0.001**	0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Per capita income with FE-IV	0.012***	0.008***	0.005***	-0.003***
	(0.003)	(0.002)	(0.001)	(0.001)
	Different controls			
Per capita income with FE	0.005***	0.003***	0.001	-0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Per capita income with FE-IV	0.002	-0.009	-0.008***	-0.015***
	(0.005)	(0.006)	(0.002)	(0.004)
	Agricultural municipalities			
Per capita income with FE	0.005***	0.002**	0.001**	0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Per capita income with FE-IV	0.014***	0.010***	0.006***	-0.002
	(0.004)	(0.002)	(0.001)	(0.001)
	Coastal municipalities			
Per capita income with FE	0.005	-0.003***	-0.003	-0.009
	(0.005)	(0.001)	(0.002)	(0.007)
Per capita income with FE-IV	-0.035	0.000	-0.003	-0.009
	(0.030)	(0.012)	(0.004)	(0.007)

Note: Robust standard errors are presented in parentheses. Standard errors are clustered at the municipal level. *** Statistically different from zero at the 0.01 level of significance. ** Statistically different from zero at the 0.05 level of significance. * Statistically different from zero at the 0.1 level of significance. Sample includes municipalities with under 169,800 inhabitants and excludes state capitals. The controls variables used here are the same mentioned on each table

Table A.2.: Comparing unconditional transfers coefficients considering the existence or not different brackets of population as IV

	Per capita general expenditures	Per capita tax revenues	Per capita education expenditures	Per capita health expenditures
	Unconditional from General results			
Per capita FPM	0.882***	-0.025	0.245***	0.155***
	(0.063)	(0.046)	(0.031)	(0.024)
Per capita royalties transfers	0.497***	-0.012	0.021	0.200***
	(0.122)	(0.045)	(0.023)	(0.034)
	Unconditional with different brackets of population			
Brackets of population between 1 and 3	2.186***	-0.120	0.575***	0.949***
	(0.508)	(0.291)	(0.164)	(0.173)
Brackets of population between 4 and 7	3.704***	0.277	0.289	0.845***
	(0.780)	(0.417)	(0.221)	(0.217)
Brackets of population between 8 and 17	0.135	-0.532	1.454***	-0.856
	(1.820)	(1.003)	(0.564)	(0.722)
Per capita royalty transfers	0.647***	-0.022	0.070***	0.221***
	(0.113)	(0.039)	(0.017)	(0.035)

Note: Robust standard errors are presented in parentheses. Standard errors are clustered at the municipal level. *** Statistically different from zero at the 0.01 level of significance. ** Statistically different from zero at the 0.05 level of significance. * Statistically different from zero at the 0.1 level of significance. Sample includes municipalities with under 169,800 inhabitants and excludes state capitals. The controls variables used here are the same mentioned on each table