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# **Tax Me, But Spend Wisely? Sources of Public Finance and Government Accountability**

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# Tax Me, But Spend Wisely? Sources of Public Finance and Government Accountability

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## Abstract

Existing evidence suggests that extra grant revenues lead to little improvements in public services in developing countries - but would governments spend tax revenues differently? This paper considers a program that invests in the tax capacity of Brazilian municipalities. Using variations in the timing of program uptake I find that it raises local tax revenues and that the increase in taxes is used to improve both the quantity and quality of municipal education infrastructure. In contrast increases in grants over which municipalities have the same discretion as over taxes have no impact on any measure of local public infrastructure. These results suggest that the way governments are financed matters: governments spend increases in tax revenues more towards expenditures that benefit citizens than increases in grant revenues.

## 1 Introduction

The idea that increases in tax revenues go hand in hand with more accountable and efficient public spending is at the heart of interpretations of the emergence of representative governments in the West (North and Weingast, 1989, Lindert, 2003). Whether increasing the capacity to tax of governments in today's developing countries would

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have a similar effect is an open question. It is also an important one as more public investments in infrastructure are necessary to further the economic development of these countries (Duflo, 2011).

The large literature looking at the consequences of increases in government revenues in developing countries is disappointing in this respect: it typically finds that they have little impact on public health, education or social infrastructure and are often wasted or diverted.<sup>1</sup> A common trait of studies in this literature however is that they consider variations in non-tax revenues, possibly because variations in tax revenues that are unrelated to other determinants of public spending are particularly hard to come by.

This paper first asks whether increases in governments' capacity to tax have a positive impact on the provision of public infrastructure in the context of Brazilian municipalities. To do this I study a program that helps municipalities increase their tax revenues by subsidizing investments in local tax administrations. I consider whether the program increased local tax revenues and whether the extra revenue generated were spent on improving local public services. Participation to the tax-capacity program is voluntary but the particular timing of its implementation enables me to estimate its causal impact on outcomes: local governments decide when to apply to the program but the date at which they start it is determined by constraints faced by the supplier of the program. This makes it possible to separately identify the impact of the program from a potential selection effect.

I then consider whether local governments spend these tax revenues differently from non-tax (transfer) revenues. Variations in non-tax revenues come from a rule determining how much federal transfers municipalities receive, as also used by Litschig and Morrison (2013) and Brollo *et al.* (2013).<sup>2</sup> The rule specifies that transfer revenues increase discontinuously with local population size at 14 population thresholds, so identification of the impact of transfer revenues comes from municipalities that cross these thresholds over time. I compare how governments spend increases in tax and transfer revenues using a 14 years panel dataset on municipal revenues and expenditure outcomes, primarily the quality and quantity of locally funded public education infrastructure.

Brazilian local governments are a good context in which to ask whether governments spend tax revenues differently from non-tax revenues for several reasons. First,

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<sup>1</sup>For example, Reinikka and Svensson (2005) show that schools in Uganda receive only a small share of funds allocated to them by the central government, Olken (2007) estimates that more than 20% of grants that local governments in Indonesia receive to finance road projects are diverted, and Svensson (2000) finds some evidence that aid increases corruption in politically divided countries.

<sup>2</sup>See also Corbi *et al.* (2014).

municipalities control a significant share of public revenues (roughly one-fifth) and are responsible for key public expenditures. Their main spending responsibility, and the main expenditure outcome I consider, is education, an area in which Brazil’s performance is generally considered disappointing compared to countries at similar levels of development (Ferraz *et al.*, 2012). Municipalities are in charge of primary education and shoulder much of the blame for this; there is both scope and need for more local investments in education. Second, local governments have the same discretion over the allocation of the transfer revenues considered here as over their own tax revenues, so there are no legal or administrative reasons for the two being spent differently. Third, there is evidence that Brazilian local governments do not use increases in non-tax revenues to improve local infrastructure but instead waste or divert it (Caselli and Michaels, 2013, Ferraz and Monteiro, 2010, Brollo *et al.*, 2013).<sup>3</sup> Asking whether tax revenues are similarly wasted or diverted is particularly relevant in this context.

I find that the program is successful in raising local tax revenues: a 1 Real investment in tax capacity leads to an annual increase in tax revenues of roughly 1 Real per year after 5 years. Moreover, the increase in tax revenues generated by the program leads to a 4 to 5% increase in the quantity of municipal education infrastructure and an improvement in an index of the quality of the infrastructure of one-tenth of a standard deviation. I find some evidence that literacy rates increase slightly in municipalities that take part in the program, in line with results in Harbison and Hanushek (1992) and Gomes Neto and Harbison (1996) which suggest increases in education infrastructure improve students’ outcomes in this context, though results lack robustness. An increase in transfer revenues of the same size has no impact on education infrastructure. I then consider alternative uses of public revenue. Neither tax nor transfer revenues have a significantly positive impact on municipal health infrastructure. Some evidence on what transfer revenues are spent on is found in Brollo *et al.* (2013) who show that they lead to an increase in corruption. In contrast I find evidence suggesting that tax revenues have no impact on corruption.

To interpret these results as evidence that governments spend revenues from different sources differently one must rely on stronger assumptions than those required to interpret the estimates of the impacts of both tax and transfer revenues as causal. My main estimates are obtained on different groups of municipalities so we must assume that municipalities taking part in the tax capacity program and those affected by the transfer allocation rule do not have different marginal propensities to spend on educa-

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<sup>3</sup>An important exception is Litschig and Morrison (2013) who find that these same grants also lead to better education outcomes in the 1980s. I discuss their results in detail below.

tion, health and corruption out of *all* types of public revenues. Whilst this assumption cannot be tested, I show two pieces of reassuring evidence. First, results are the same when I restrict the sample to the (small) group of municipalities that take part in the program and are affected by the transfer allocation rule. Second, the particular design of this rule - it creates 14 different points in the distribution of municipalities at which the local impact of transfer revenues can be estimated - allows me to consider whether the marginal propensity to spend transfer revenues varies with observable municipal characteristics. I find no evidence of such variation. In particular I show that municipalities that look extremely similar to those that enrolled in the tax capacity program do not spend their transfer revenues differently.

Several models of public resources allocation could explain this result. I find some evidence that the difference between how tax and transfer revenues are spent is smaller in municipalities where there is a local radio station potentially informing citizens about public budgets. This is in line with principal-agent models of public finance in which asymmetries of information allow politicians to capture more rents (Besley and Smart, 2007) if we assume that citizens are better informed about increases in taxes than increases in transfers. I discuss and look for evidence of other mechanisms that could lead to tax revenues being spent differently from transfer revenues and show that the results are unlikely to be due to different characteristics of the tax and transfer variations studied here that are not necessarily related to their source (predictability, size and sign of the variations in revenues).

This paper contributes to the literature on public finance in developing countries in two ways. First, by evaluating the impact of a tax capacity program I present estimates of the returns to investment in tax capacity. Second, this paper is to the best of my knowledge one of the first to consider the impact of *tax* revenues on publicly provided infrastructure. One recent exception is a paper by Martinez (2016) who compares how local governments in Colombia spend local tax revenues and revenues from oil royalties and finds that increases in tax revenues have a positive effect on the provision of public services, suggesting the results found for Brazil in this paper may hold in other contexts. The idea that the growth of states' capacity to tax is an important covariate of economic development (Besley and Persson, 2009) motivates a growing literature that studies the determinants of tax compliance in developing countries.<sup>4</sup> My results suggest that part of the correlation between tax capacity and economic development

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<sup>4</sup>See for example de Paula and Scheinkman (2010), Olken and Singhal (2011), Carrillo *et al.* (2011), Kumler *et al.* (2015), Pomeranz (2015), Best *et al.* (2015), Naritomi (2015), Khan *et al.* (2015), Cagé and Gadenne (2015).

can be interpreted as causal as I show that governments that tax more also invest more in human capital infrastructure.

The results are closely related to the literature which considers whether the way governments are financed affects their behavior. Fisman and Gatti (2002) establish a positive relationship between the proportion of US states' revenues derived from federal transfers and the number of convictions of public employees for abuse of public office. Similarly Zhuravskaya (2000) provides evidence that outcomes affected by public policy improve when Russian cities keep more of their tax revenues.<sup>5</sup> I build on these previous findings by using variations in tax and non-tax revenue that stem from clearly identified sources and considering variations in publicly provided infrastructure that are directly controlled by governments.<sup>6</sup>

This paper speaks more generally to the larger literature on the political economy of public good provision (see Banerjee *et al.* (2013) and Olken and Pande (2012) for recent reviews). I focus on the impact on public good provision of one institutional characteristic - government's capacity to tax - which has so far not been studied.<sup>7</sup> Relatedly, these findings also contribute to debates on how to finance development. The idea that aid revenues may not be spent as well as tax revenues has long been discussed by policy practitioners and researchers alike (OECD, 2010a, Besley and Persson, 2011, Deaton, 2013) but technical aid on revenue-raising management has always been the poor cousin of official development aid (OECD, 2010b). This paper shows that a resource mobilization program in place in Brazil for nearly two decades has been successful in providing long term sources of funds to local governments. It suggests that technical help in tax capacity building may lead to an increase in government resources which is more conducive to public investments in human capital than traditional budget-support development aid. Finally this paper speaks to debates regarding the optimal form of decentralization in developing countries (see Gadenne and Singhal (2014) for a review) by considering whether revenue decentralization - increasing local government's capacity to tax - affects public delivery outcomes for a given level of administrative and expenditure decentralization.

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<sup>5</sup>See also Jin *et al.* (2005) and Fan *et al.* (2009).

<sup>6</sup>There is also a large literature devoted to explaining the fly-paper effect, the fact that a dollar received by a community in the form of a grant to its government results in greater public spending than a dollar increase in community (private) income - see for example Knight (2002), Singhal (2008), Dahlby (2011). The results presented here suggest that increases in private income could nevertheless improve public infrastructure more than increases in grant income if they lead to higher tax revenues.

<sup>7</sup>The results are also consistent with the literature on the natural resource curse which finds that governments' revenues from the exploitation of natural resources are typically are wasted or diverted (Van der Ploeg, 2011). One explanation for this empirical regularity is that resource-rich countries have little need to levy taxes and therefore respond to their citizens' demands.

The paper is organized as follows. Section 2 presents the context of study, the tax and transfer policies of interest and the data used. Section 3 provides a conceptual framework to formalize the hypotheses of interest and section 4 describes the empirical strategy I use to test them. Section 5 presents the main results regarding the impact of the tax and transfer policies, and finally section 6 attempts to compare how local governments spend revenues from different sources and discusses potential mechanisms for the results.

## 2 Context and data

### 2.1 Local expenditure responsibilities

The Brazilian constitution devolves substantial expenditure and revenue raising responsibilities to the country's more than 5,000 local governments. Mayors and local councils, elected every 4 years, are in charge of allocating one-fifth of all public spending. This paper focuses on different types of local public expenditure variables which are all inputs in the production of human capital. Municipalities report how they allocate their spending amongst budget items but reported spending is known to be weakly, if at all, correlated with actual spending on local infrastructure (see for example Caselli and Michaels (2013)). I therefore consider measures of inputs directly financed by local governments that are not reported by municipal authorities. I mostly study inputs not human capital outcomes because the hypothesis of interest relate to how governments choose to allocate their revenues and because of data limitations: there is no measure of outcomes at the local level measured in a consistent way over the years 1998 to 2011.<sup>8</sup> Inputs also react faster to local policy choices than outcomes; using these measures maximizes the probability that we will see an impact of local revenues on outcomes.

The main measure of public expenditure outcomes I consider is municipal education infrastructure. Education is the largest local budget item (it represents a third of municipal expenditures on average) and local governments are in charge of pre-primary and primary schools: they provide infrastructure, school lunches and transportation and hire and pay teachers. Physical school infrastructure is the type of local education input that is the most likely to be under-funded: municipalities receive federal grants earmarked for expenditures on school staff, school lunches or school transport

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<sup>8</sup>Data on school dropouts is available annually only until 2006, some data on students' learning outcomes is available from 2007. Data on literacy rates is only available in population census years, ie once per decade. I provide some limited evidence using data from the 2000 and 2010 census below.



but not for physical teaching infrastructure.<sup>9</sup>I therefore focus on physical school infrastructure as the type of input that is the most likely to be affected by changes in non-earmarked revenues but also discuss results for school employees. There is ample anecdotal evidence that the supply of municipal education infrastructure has not kept up with the increase in demand over the past two decades in Brazil (OECD, 2011)<sup>10</sup>. Furthermore, there is causal evidence that increases in both the quantity and quality of education infrastructure have a positive impact on student achievements in developing countries<sup>11</sup> and some evidence in Harbison and Hanushek (1992) and Gomes Neto and Harbison (1996) that physical school infrastructure has a positive impact on student performance in Brazil. Similarly Ferraz *et al.* (2012) show that test scores are lower in municipalities with worse-equipped schools due to corruption. I complement the study of education inputs by looking at the one measure of education levels available at the municipal level in both the 2000 and 2010 Brazilian population census: literacy rates for inhabitants aged 5-9, 10-14 and 15-19.

I use panel data on the quantity and quality of municipal education infrastructure from the annual school census conducted by the Ministry of Education. I consider the number of classrooms in use in municipal schools per thousand school-age inhabitants to measure the quantity of municipal education infrastructure available at the lowest level of disaggregation possible. I combine the eight variables related to the quality of the infrastructure that are measured consistently over the period (number of municipal schools with computers, with internet, with a sports facility, a library, television/video equipment and connected to the sewage and electricity systems) using principal component analysis to construct a quality index.

I turn to two other expenditure outcomes to complement the results on education: health infrastructure and corruption. Health is the second largest municipal budget item comprising just under a quarter of local expenditures on average. Local governments share responsibility for most of the infrastructure of primary and preventive health units through the Family Health Program with state governments, so health infrastructure could also be affected by changes in local revenues. Data on the number of municipal health units come from a census of health facilities conducted in 1999, 2002, 2005 and 2009.

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<sup>9</sup>60% of the largest education grant, *FUNDEB*, must fund teacher's salaries. The *PNAE* grant funds school lunches, the *PNATE* grant school transportation.

<sup>10</sup>A recent PISA study argues that lack of infrastructure is the main reason for one of the major challenges of primary education in Brazil (OECD, 2011). It further shows that municipalities that successfully improved local education outcomes often did so by investing in new school infrastructure.

<sup>11</sup>See for example Glewwe and Jacoby (1994) for evidence on the role of classrooms, Banerjee *et al.* (2007) for evidence on the role of computers in classrooms, and a review in Glewwe and Kremer (2006).

Information on proxies for municipal corruption levels is available since the start of a federal anti-corruption program in 2003. Since then every six months local governments are randomly chosen through a public lottery to be audited by staff of the independent audit agency *Controladoria-Geral da União* (CGU). They audit the use of earmarked grants received by municipalities by collecting administrative documents, interviewing citizens and conducting random checks in municipal agencies. Ferraz and Finan (2011a) estimate using the audit reports that 8% of audited revenues were diverted in the period 2001-2003. Several teams have coded the reports for different time periods and samples of municipalities; I consider both the indexes compiled by Brollo *et al.* (2013) for the 925 municipalities with less than 50,000 inhabitants audited over the period 2003-2008 and those constructed by Litschig and Zamboni (2012) for the 862 municipalities audited between 2003 and 2006. The corruption dataset is a repeated cross-section of municipalities.<sup>12</sup>Data on alternative uses of funds (such as debt reduction, local police, municipality sewage light and transport systems) is not available.

## 2.2 Local public revenues

### 1. The tax policy

#### 2.2.1 The tax capacity (PMAT) program

Brazilian local governments are in charge of collecting and setting the rates of two main local taxes, a service tax and an urban property tax. Local tax revenues represent 13% of total tax revenues on average, roughly 2% of GDP. Anecdotal evidence suggests local administrations have little capacity to enforce tax payments. Municipal staff have outdated tax registers, little institutional memory and weak methods to accurately assess tax liabilities; high costs of understanding and paying taxes combined with low penalties of getting caught lead many citizens to non-compliance. Some local officials have publicly admitted to tolerating a situation of permanent tax amnesty where tax arrears are never recovered (Afonso and Araujo, 2006, BNDES, 2002).<sup>13</sup>

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<sup>12</sup>Allocating a date to the audit data is complicated. Auditors are typically supposed to audit the use of federal grants over the last two-three years but sometimes report irregularities that occurred five years ago. Moreover the date at which the irregularity occurred is often not specified in the reports - possibly because it can be hard to pin down. In my main specification I say that an irregularity measure corresponds to the year of the lottery if the audit took place in June of that year or later, I also consider what happens when an irregularity is allocated to a date one or two years prior to the lottery as a robustness check.

<sup>13</sup>A study of property tax collection in Brazil's largest metropolitan areas estimates for example that over 40% of urban property is not registered with the tax authorities (de Carvalho, 2006).

The *Programa de Modernização da Administração Tributária* (PMAT program) was launched in 1998 by the Brazilian Development Bank (BNDES) to remedy this situation and increase municipalities' tax revenues. It provides local governments with subsidized loans to invest in modernizing their tax administration in order to improve their tax capacity.<sup>14</sup> Expenditures financed by the loans can be divided in three categories. First, municipalities improved their capacity to gather information on potential taxpayers by updating tax registers and investing in skills and software to analyze and cross-check administrative data. Second, they increased their capacity to enforce tax payments through streamlining audit processes and recovering tax arrears. Third, they lowered taxpayers' costs of complying by multiplying the means and frequency of tax payments and simplifying their interactions with the authorities. The paper's web appendix discusses evidence on the actions financed by the program.

The timing of the program is of particular interest. Selection in the program is voluntary; municipalities choose when to apply and I observe applications that occur between 1998 and 2009. They then wait between a couple of months and four years to receive their first loan (all eventually obtain their first loan). We see from Figure 1 that the distribution of application dates over time is smoother than that of start dates which bunches around a few years. This is due to changes in the conditions in which the program was supplied as the resources allocated to review applications varied over time. The BNDES processed all applications itself for the first 3 years and took over 2 years on average to authorize a project. In 2002 most of the application process was contracted out to the public bank *Banco do Brasil* whose involvement initially greatly accelerated the process until it decided to cut down resources allocated to the program in 2005. This explains the large spike in the number of municipalities starting the program in 2002, 2003 and 2004. In 2007 another public bank, *Caixa General* was contracted to help with the administrative backlog.

Municipalities apply and start the program in the same order, suggesting there was little they could do to shorten their waiting time. The timing of the program's implementation implies that municipalities choose when to apply but the date at which they start the program (start receiving the loan) is largely out of their hands. Controlling for the timing of municipalities' selection into the program will help identify its impact, as I explain below.

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<sup>14</sup>The municipality's future FPM transfers (see below) are used as collateral for the loan: should a municipality fail to pay back its loan the BNDES has the power to block payment of FPM transfers. All loans have been repaid fully.

### 2.2.2 Understanding selection in the program

339 municipalities (hereafter PMAT municipalities) start a program between 1999 and 2009.<sup>15</sup> To understand which factors determine selection into the program I estimate a hazard model of the probability of applying. The main reason invoked by public officials to explain their decision to join the program is that they thought their tax collection was below potential. I therefore include a set of variables measured prior to the start of the program (1998) that proxy for potential tax collection: GDP per capita, population size, the share of services in GDP (a proxy for the tax base of the service tax) and the share of urban population (a proxy for the tax base of the urban property tax).<sup>16</sup> I also include distance to the 10 closest municipalities already in the program to proxy for potential information about the program: the BNDES did little advertising and participants often told me they found out about the program from observing neighboring municipalities implementing it. Results are presented in Table 1. Richer, bigger and more urban municipalities, and those with more neighbors already in the program, are as expected more likely to apply to the program. When controlling for these variables PMAT municipalities did not levy more taxes in 1998.

Local governments could also have applied because they had a higher than average need for public revenues. I find no evidence that municipalities apply when they have less municipally-provided health and education infrastructure. Political considerations seem to play a role. Mayors in their second (last) term are less likely to apply perhaps because they anticipate that the program’s full impact on revenues may take more than one term (four years) to materialize. In column 2 I restrict the sample to the post 2001 period for which information on the mayor’s education is available. More educated mayor apply more often and may be better at both collecting tax revenues and providing local infrastructure so I consider specifications using only variations within a mayor’s time in office below.

Applying to the program could finally be a response to economic or political shocks, for example a local recession that depresses tax revenues. Whether program participation is driven by time-varying shocks is important to the subsequent analysis as I exploit variations in program participation across time and space for identification. One of the main threats to the validity of this approach is the existence of time-varying unobserved covariates that are correlated with program participation, tax collection or

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<sup>15</sup>These cover roughly 40% of the Brazilian population.

<sup>16</sup>See the web appendix for a description of the variables and their source. 1998 was a recession year in Brazil so municipal GDP and tax revenues may have been particularly low in that year. Using 1996 tax and GDP data instead does not affect the results.

expenditure outcomes. The assumption that there are no such covariates cannot be tested but the existence of a correlation with observed time-varying covariates would cast doubt on its plausibility. Column 3 therefore tests whether shocks influenced program participation by including lagged changes in tax revenue per capita, GDP per capita and population per capita; column 4 considers whether municipalities' decision to join was driven by different trends prior to 1998. None of the lagged variables have an impact and there is no evidence of different pre-1998 trends.

## 2. The transfer policy

The most important source of municipal public revenue (30%) is the *Fundo de Participação dos Municípios* (FPM), a transfer from the federal government established by the Constitution. I focus on this transfer for two reasons. First, local governments have exactly the same discretion over how to spend it as they have over local tax revenues. This extends to federal monitoring and auditing policies which target the use of earmarked transfers (these constitute the bulk of non-FPM and tax municipal revenues) but not that of FPM transfers or local tax revenue.<sup>17</sup>

Second, FPM revenues are allocated to municipalities on the basis of their population following a rule which creates quasi-exogenous variations in the amounts of revenues municipalities receive. Municipalities are divided into population brackets that determine the coefficients used to allocate total FPM resources among them, with higher population brackets corresponding to higher coefficients. Formally, the rule specifies that the amount of FPM transfers received by municipality  $i$  in state  $s$  and year  $t$  is:

$$FPM_{i,s,t} = \frac{FPM_{s,t} \lambda(P_{i,t-1})}{\sum_{i \in s} \lambda(P_{i,t-1})} \quad (1)$$

where  $\lambda(\cdot)$  is a step-wise function of estimated local population in the previous year  $P_{i,t-1}$ ,  $FPM_{s,t} = \gamma_s FPM_t$  is equal to the share of total resources  $FPM_t$  allocated to state  $s$  and  $\lambda(\cdot)$  and  $\gamma_s$  are time-invariant. This rule applies to all municipalities that are not state capitals and have less than 142,633 inhabitants.<sup>18</sup>

<sup>17</sup>In particular the randomized audits used to construct the corruption variables do not directly consider the use of tax revenue and FPM transfers. However some earmarked grants require that municipalities contribute some of their 'own' revenues (defined legally as FPM transfer revenues or taxes) to the program they fund; we can think of the audits as reflecting the overall quality of government spending. Importantly, there is no reason to think that the use of tax revenue is more closely (indirectly) audited than that of FPM transfers, or vice versa.

<sup>18</sup>Total FPM resources consist of 23.5% of revenues from the federal income tax and federal tax on manufactured products. The  $\gamma_s$  are a function of state population and income per capita, with bigger

The Federal Audit Court sets each municipality's coefficient based on the population estimates calculated annually by the Brazilian statistical institute (IBGE). Data on FPM transfers for the years 1998 to 2011 are obtained from the Brazilian Treasury and I apply the FPM allocation rule (1) to the population estimates to compute the amounts municipalities should receive if the rule was perfectly implemented (predicted transfers). Figure 2 plots real (left panel) and predicted (right panel) FPM revenue per capita averaged over 100 inhabitant cells against municipal population estimates. Per capita transfers decrease with population size except at the population cutoffs where they increase discontinuously. Jumps in both real and predicted transfers are less visible from cutoff 7 onwards as differences in shares received by each state and increases in total FPM revenues over time introduce some noise. This will be partialled out in the regression analysis.<sup>19</sup>

### 3 Conceptual framework

This section formalizes the hypotheses tested in the remainder of the paper. Consider a politician who controls public revenues  $R$  coming from two sources: taxes  $T$  and non-tax revenues  $F$ . In this context taxes are local municipal taxes and non-tax revenues are transfers but this framework could also represent a federal government receiving non-tax revenues from the exploitation of natural resources or development aid. The politician can choose to spend revenues on local public expenditure outcomes  $E$  which are either the provision of public infrastructure that potentially increase the welfare of citizens ( $G$ ) or rents that only increase his own welfare ( $C$ ). These are determined by the budget constraint  $F + T = G + C$ , observable ( $Z$ ) and unobservable ( $W$ ) characteristics of the local government and potential participation to a tax capacity program ( $P = 1$  is the government participates). I write expenditure outcomes in government  $i$  as:

$$E_i = E(T_i(Z_i, W_i, P_i), F_i(Z_i, W_i), Z_i, W_i), \text{ for all } E_i = G_i, C_i \quad (2)$$

The paper tests three hypothesis of interest.

**Hypothesis 1: The tax capacity program increases tax revenues.**

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and poorer states receiving a larger share. The paper's web appendix presents the population brackets, the values of the FPM coefficients ( $\lambda(P_{i,t-1})$ ) and average real and predicted transfers in each bracket.

<sup>19</sup>The law creates 15 thresholds, but there are too few observations around the last cutoff to observe a clear jump in FPM revenues. I restrict estimation to the first 14 thresholds in what follows.

The first parameter of interest is the impact of the program on local tax revenues,  $T_{P_i}$ , averaged over all municipalities that participate in the program. As explained above the program improves governments' capacity to enforce taxes and reduces the cost of tax compliance. Hypothesis 1 states that  $T_{P_i} > 0$ , ie that politicians will use these changes to increase tax revenues. The next two hypotheses of interest relate to the relationship between revenues and expenditure outcomes.

**Hypothesis 2: An increase in tax revenues leads to more provision of public infrastructure.**

**Hypothesis 3: Governments spend revenues from taxes and transfers differently.**

The tests of hypotheses 2 and 3 consist in estimating the partial derivatives  $E_{T_i}$  and  $E_{F_i}$  averaged over sub-samples of the population. Hypothesis 2 states that  $G_{T_i} > 0$ , whilst hypothesis 3 assumes that marginal propensities to spend out of taxes and transfers are different:  $E_{T_i} \neq E_{F_i}$  for  $E = G, C$ .

Several mechanisms could lead to a difference between how tax and non-tax revenues are spent. First, increases in tax revenues affect the information citizens have on public budgets differently from increases in transfer revenues. Political agency models of public finance argue that politicians capture more rents and provide less public goods when there are asymmetries of information over elements of the public budget (see Besley and Smart (2007)). Increases in tax revenues are by definition observed by the tax-paying part of the population, increases in transfers from higher level of governments may not be observed as well. As shown in the theoretical (web) appendix this assumption would lead to increases in taxes having a larger effect on the provision of public services and a smaller effect on corruption than an increase in non-tax revenues of the same size.<sup>20</sup>

Second, increases in tax revenues could change citizens' behavior. Citizens may demand more from politicians when they pay more taxes - political scientists have coined this the 'no representation without taxation' hypothesis (Ross, 2004, Moore, 2007). Assuming that citizens' utility is submodular in the public and private goods or that individuals suffer from some version of the sunk cost fallacy would formalize this argument: citizens paying more taxes to their local government will also be more willing to exert effort to monitor the politician. Increases in transfers do not impose a (direct) cost on them and hence does not affect their interactions with the politician.<sup>21</sup>

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<sup>20</sup>A related mechanism simply assumes that politicians are budget-maximizing and points out that when the share of taxes in their total budget increases they have more incentives to invest in public infrastructure if they expect these investments to increase their future tax base (Weingast, 2009).

<sup>21</sup>In the case considered here non-tax revenues are transfers from the federal government, funded by federal taxes paid by citizens. The arguments developed above also imply that this type of 'non-

Third, sorting of citizens with different tastes for public spending across local governments, in the spirit of Tiebout (1956), could lead to the source of public revenues affecting expenditure outcomes. In the Tiebout framework local spending must be funded by local taxes for population sorting to occur and gains from decentralization to arise. An increase in taxes in a local government could attract citizens that are both more willing to pay taxes and more likely to demand a certain type of public services or less tolerant of corruption. I assess the plausibility of these mechanisms in the Brazilian context below. Note that these mechanisms suggest that tax revenues will not only be spent differently, they will also be spent ‘better’ than transfer revenues - more on the provision of public infrastructure and less on corruption:  $G_T > G_F$  and  $C_T < C_F$ .

## 4 Empirical strategy

### 4.1 The tax experiment

I exploit the timing of the implementation of the tax capacity program to identify its impact on tax revenues - parameter  $T_P$  above- and the causal effect of taxes on public expenditure outcomes - parameter  $E_T$  above. We have seen that take-up is not driven by observable shocks and that trends were not different in PMAT and non-PMAT municipalities prior to 1998. This motivates the use of a difference-in-differences specification as it seems a priori reasonable to assume that unobservable characteristics that could confound identification are also fixed over time. Formally, I test Hypotheses 1 and 2 above by estimating the following model:

$$T_{i,t} = \pi_T PMAT_{i,t} + \delta Z_{i,t} + \gamma_t + \mu_i + \epsilon_{i,t} \quad (3)$$

$$E_{i,t} = \beta_T T_{i,t} + \eta Z_{i,t} + \gamma_t + \mu_i + \nu_{i,t} \quad (4)$$

where  $PMAT_{i,t}$  is an indicator equal to 1 if municipality  $i$  started a program in a year  $s \leq t$ ,  $T_{i,t}$  is tax revenues per capita and is instrumented by  $PMAT_{i,t}$  in equation (4),  $E_{i,t}$  are expenditure outcomes of interest,  $Z_{i,t}$  are time-varying covariates,  $\gamma_t$  are year fixed effects and  $\mu_i$  municipality fixed effects. Time-varying controls  $Z_{i,t}$  are proxies for the size of municipal tax bases (municipal GDP per capita, shares of services and agriculture in GDP and population) and for preferences of the administration that could affect tax policy (mayor’s political party, political competition during the last

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tax’ revenues will be spent differently from local tax revenues if we assume that increases in federal taxes paid are only weakly correlated with increased in local transfers received, or that citizens do not understand this link well. Both these assumptions are likely to hold in practice.



election, term limits). All specifications in the paper allow for arbitrary covariance structure within municipalities.<sup>22</sup>

The key identifying assumption required for the interpretation of  $\pi_T$  as the average effect of the program on taxes ( $T_P$ ) and  $\beta_T$  as the impact of tax revenues on expenditure outcomes ( $E_T$ ) is that the (conditional) evolution of tax collection and expenditure outcomes in PMAT and non-PMAT municipalities would have been the same in the absence of the program. The fact, established in the previous section, that time-varying covariates do not follow different trends in PMAT municipalities prior to the start of the program is reassuring in that respect. To further assess the plausibility of this assumption I look for differences in the evolution of outcomes between PMAT and non-PMAT municipalities prior to the start of the program by estimating a flexible reduced form specification of the program’s impact on all outcomes of interest:

$$Y_{i,t} = \sum_{j=-11}^{11} \pi_{Tj} PMAT_{j,i,t} + \theta Z_{i,t} + \gamma_t + \mu_i + \nu_{i,t}. \quad (5)$$

Here  $Y_{i,t}$  is tax revenues or expenditure outcomes,  $PMAT_{j,i,t}$  is equal to 1 if municipality  $i$  in year  $t$  started a PMAT program  $j$  years ago ( $j \geq 0$ ), or will start a program in  $j$  years ( $j < 0$ ). Testing for pre-treatment trends is equivalent to a test that the  $\pi_{Tj}$  are equal to zero for  $j < 0$ . This specification also allows me to consider the time profile of the program’s impact.

Whilst reassuring, the absence of different trends prior to the start of the program would not be sufficient to rule out self-selection into the program because of time-varying shocks to unobservable characteristics  $W_{i,t}$  that also affect outcomes. However, such shocks would affect outcomes as soon as municipalities self-select in the program even if the program itself hasn’t started yet. I include an indicator equal to one if the municipality has applied to the program in specifications (3) and (4) and all following specifications that include the program participation variable to test for any potential ‘selection on unobservables’ effect. This test is only valid if municipalities cannot manipulate the time they spend waiting between their application date and their start date, I provide some evidence in line with this assumption below.

To interpret  $\beta_T$  as the impact of higher taxes on municipal expenditure outcomes ( $E_T$ ) we must in addition assume that any impact of the program on outcomes comes

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<sup>22</sup>Error correlation in the cross-section dimension of the panel could also be a concern if local governments’s tax policies respond to their neighbors’ policies. Clustering at the state-year level to allow for such correlation however hardly affects the standard errors in all the specifications used below (results available from the author upon request).

only from its effect on taxes (exclusion restriction). Municipalities were explicitly not allowed to use program loans on education or health services, any deviation from this rule was expected to be punished so it is a priori reasonable to assume the loan does not have a direct impact on public infrastructure, I discuss below how the magnitude and time profile of the effects rules out the possibility that the loan itself enabled municipalities to fund the observed increases. We must also assume that the tax administration changes did not directly affect how governments spend their revenues, I provide a test of this assumption when discussing the interpretation of the findings.

A final concern arises if pre-treatment characteristics potentially correlated with the dynamics of the outcome variable are unbalanced between PMAT and non-PMAT municipalities (Heckman *et al.*, 1998). Table 2 presents descriptive statistics of municipalities prior to the start of the program by program status. PMAT municipalities are richer, bigger and levy more taxes in 1998 than the average municipality, as expected from the analysis of determinants of selection. They are also more likely to be the seat of a local branch of the judiciary and a radio station, two characteristics known to affect municipal corruption levels (Ferraz and Finan, 2008, Litschig and Zamboni, 2012). Covariate unbalance is a concern here so I complement my empirical analysis by estimating a propensity score-weighted version of equations (3) and (4) following Hirano and Imbens (2001) (see also Hirano *et al.* (2003)). This eliminates potential bias due to covariate unbalance by 1) restricting the sample to observations in the common support of the covariate distribution and 2) obtaining balance by re-weighting the control group observations by a function of their estimated propensity to join the program.<sup>23</sup> More details on the construction of the weights and the common support sample are in the web appendix.

The common support sample consists of 3,724 municipalities (276 PMAT municipalities and 3,448 non-PMAT). The last column of Table 2 shows that restricting and weighting the sample of control municipalities leads to a reasonable balance in pre-treatment characteristics. The balance across groups improves, including for covariates that are not used to estimate the propensity score: total public revenues, municipal health and education infrastructure, life expectancy, education level, local judiciary or radio presence and measures of corruption.

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<sup>23</sup>This is done by estimating a model of the probability that a municipality joins the program as a function of the set of pre-treatment covariates  $Z$  used in Table 1, obtaining the predicted probability  $\hat{P}(W)$  and then estimating the specifications of interest with weights equal to unity for the treated and  $\hat{P}(W)/(1 - \hat{P}(W))$  for the controls. Hirano *et al.* (2003) show that this estimator is efficient and Wooldridge (2007) that ignoring the first-stage estimation of the selection probabilities when performing inference yields conservative standard errors. All results below present standard-errors non-adjusted for first stage estimation, as bootstrapping procedures suggest there is little efficiency lost in doing so.

## 4.2 The transfer experiment

I use the FPM allocation rule to estimate the causal effect of non-tax revenues on public expenditure outcomes (parameter  $E_F$  above). The variations created by the rule are typical of a fuzzy regression discontinuity design: the probability of treatment (higher FPM revenues) increases discontinuously when the running variable (municipal population) reaches the cutoffs but there are cases of mis-assignment around the cutoffs. I consequently use an indicator equal to one if population is above a cutoff as an instrument for non-tax revenues per capita whilst flexibly controlling for municipal population size on both sides of the cutoffs.

Several articles have used this research design before: Litschig and Morrison (2013) compares municipalities that were just below or just above the cutoffs in 1982, Brollo *et al.* (2013) and Corbi *et al.* (2014) exploit cross-municipalities differences in FPM transfers in the 2000-2010 period. My estimation strategy differs from that used in those papers as I mostly consider within-municipality variations: I identify the impact of FPM transfers from municipalities that cross a cutoff over time.<sup>24</sup> Getting rid of variations across municipalities facilitates the comparison with the impact of tax revenues generated by the program for two reasons. First, comparing municipalities on both sides of the thresholds would use variations in transfer revenues which potentially exist since 1981 (the year in which the cutoffs were last updated). This cumulated difference in transfer revenues created by the cutoffs *between* municipalities could be considerably larger than the increase in tax revenues created by the program, but the increase in revenues *within* municipalities when they reach a cutoff is on average similar to that generated by the program (see below). Second, over the period municipalities experience increases in transfer revenues for roughly the same amount of time as PMAT municipalities are observed in the program (7 years): municipalities that change population bracket in the sample remain in that bracket but close to the cutoff 5 years on average.<sup>25</sup> I report results obtained using variations both across and within municipalities as a robustness check.

This research design identifies  $E_F$  if municipal characteristics determining outcomes vary smoothly as a function of population. As shown in Lee and Lemieux (2010) the assumption that the density of the treatment-determining variable is continuous is sufficient for continuity of observable and unobservable characteristics. This assumption

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<sup>24</sup>The web appendix lists the number of times a municipality is observed crossing each threshold: there are at least 2500 observations each for the first 5 thresholds, 850 for the next 5 and 350 for the last 4.

<sup>25</sup>Here I define ‘close to the cutoff’ as above the cutoff but below the midpoint between that cutoff and the cutoff above it - see specifications 6 and 7 below.

allows municipalities to have some control over their population size as long as this control is imprecise so that we can think of treatment status as randomized close to the cutoff. Municipal population estimates are constructed annually by the IBGE in a top down fashion unaffected by the political process: in each year experts impute a rate of population growth to each state and then each municipality based on their relative growth rates between the last two available census. These estimates are then used by Brazil’s high court to determine FPM transfers and the high court publishes its own estimates of municipal population in most years. Several papers find evidence that the high court’s estimates do not always match the IBGE’s and that the density of the high courts’ estimates is abnormally high just above some cutoffs (Monasterio, 2013, Litschig, 2012). None of these papers find that the IBGE population estimates themselves are manipulated, I only use these estimates in what follows.

I provide more details on the construction of the population estimates and several checks on the continuity of municipal population around the cutoffs in the web appendix. I implement the formal check for continuous density at the cutoffs suggested by McCrary (2008) both on the pooled sample and for each cutoff separately. I also run two additional validity checks motivated by the use of within-municipality variations for identification which is new to this paper. I first consider whether the probability of crossing a FPM cutoff is different from the probability of crossing any other population cutoff by plotting population growth rates between years  $t$  and  $t - 1$  as a function of distance to the cutoff at time  $t - 1$ . I also check for the balance of pre-treatment characteristics by considering whether municipalities that will cross a threshold at time  $t + 1$  differ systematically at time  $t$  from those that won’t along any observable characteristic. None of these test suggest a violation of the identifying assumption.<sup>26</sup>

Following Imbens and Lemieux (2008) my main estimation approach is to use local linear regressions in samples around each cutoff using a rectangular kernel. I complement this by using all municipalities and controlling flexibly for population using spline polynomials.<sup>27</sup> All specifications exclude observations with a (lagged) population

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<sup>26</sup>The existence of other government policies discontinuous in municipal population size could also bias the estimates. There is one such policy: the wage of local councillors is capped and increases discontinuously when population reaches 10,000, 50,000 and 100,000 inhabitants. Transfers do not increase at these thresholds but FPM cutoffs 1,8, and 13 are nearby (at 10,188, 50,940 and 101,880). This is a potential cause for concern as Ferraz and Finan (2011b) show that these higher wages attract more educated and productive politicians who could choose to allocate budgets differently. I consider results excluding cutoffs 1,8 and 13 as a robustness check in the web appendix.

<sup>27</sup>This allows for a non-linear effect of population on outcomes which differs on both sides of the cutoffs and is particularly important because FPM revenues per capita are declining in population size on both sides of the cutoffs (Figure 2). I choose the order of the polynomial such that it best matches the local linear estimates.

of more than 142,633 inhabitants as these are not affected by the allocation rule, or below 6,792, as these are below the ‘mid-point’ between 0 and the first cutoff, following Litschig and Morrison (2013) and Brollo *et al.* (2013). I first allow the impact of the discontinuities on FPM revenues  $\pi_F$  and that of FPM revenues on expenditure outcomes  $\beta_F$  to differ at each cutoff  $c$  by estimating the following equations:

$$F_{i,t} = \sum_{c=1}^{14} [\pi_{Fc} D_{i,t} + f_c(P_{i,t-1}) + g_c(P_{i,t-1})(P_{i,t-1} - cutoff_c)] 1_c + \pi_z Z_{i,t} + \gamma_t + \mu_i + \epsilon_{i,t} \quad (6)$$

$$E_{i,t} = \sum_{c=1}^{14} [\beta_{Fc} F_{i,t} + f_c(P_{i,t-1}) + g_c(P_{i,t-1})(P_{i,t-1} - cutoff_c)] 1_c + \beta_z Z_{i,t} + \gamma_t + \mu_i + \epsilon_{i,t} \quad (7)$$

where  $D_{i,t} = 1[P_{i,t-1} > cutoff_c]$  and  $c = 1, 2, \dots, 14$

and (when the specification is a local linear regression):

$$1_c = 1[cutoff_c(1-p) < P_{i,t-1} < cutoff_c(1+p)], \quad p = 2, 5\%, \quad f_c(P_{i,t}) = a_c P_{i,t-1} \quad \text{and} \quad g_c(P_{i,t}) = b_c P_{i,t-1}$$

or (when the specification is a polynomial):

$$1_c = 1[midpoint_{c,c-1} < P_{i,t-1} < midpoint_{c,c+1}], \quad f_c(P_{i,t}) = a_c P_{i,t-1} + a_c P_{i,t-1}^2 + \dots + a_c P_{i,t}^n, \\ g_c(P_{i,t}) = b_c P_{i,t-1} + b_c P_{i,t-1}^2 + \dots + b_c P_{i,t}^n$$

where  $P_{i,t}$  is estimated population size in municipality  $i$  and year  $t$ ,  $F_{i,t}$  is FPM revenues per capita and is instrumented by  $D_{i,t}$  in equation (7),  $E_{i,t}$  are expenditure outcomes of interest, the indicators  $1_c$  divide the sample in segments around the cutoffs, and  $midpoint_{c,c+1}$  is equal to the midpoint in between two population cutoffs. I include year fixed effects and covariates  $Z_{i,t}$  to control for variations in total FPM resources over time and chance correlation with treatment status.

To facilitate the comparison with the impact of tax revenues I also consider a summary measure of the impact of FPM revenues on expenditure outcomes by pooling observations from all cutoffs. I create a new population variable  $\tilde{P}$  equal to estimated

lagged population scaled by the value of the nearest cutoff:

$$\tilde{P}_{i,t} = \sum_{c=1}^{14} [P_{i,t-1} - cutoff_c] 1_c$$

and estimate

$$F_{i,t} = \pi_F D_{i,t} + \sum_{c=1}^{14} [f_c(\tilde{P}_{i,t}) + g_c(\tilde{P}_{i,t})(\tilde{P}_{i,t} > 0)] 1_c + \sum_{c=1}^{14} 1_c + \pi_z Z_{i,t} + \gamma_t + \mu_i + \nu_{i,t} \quad (8)$$

$$E_{i,t} = \beta_{F_c} F_{i,t} + \sum_{c=1}^{14} [f_c(\tilde{P}_{i,t}) + g_c(\tilde{P}_{i,t})(\tilde{P}_{i,t} > 0)] 1_c + \sum_{c=1}^{14} 1_c + \beta_z Z_{i,t} + \gamma_t + \mu_i + \epsilon_{i,t} \quad (9)$$

where all variables are as above.

Estimation on the pooled sample allows for 28 different slopes (or 28 different polynomials in  $P$  when the full sample is used), one each on either side of the 14 cutoffs, but imposes common effects  $\beta_F$  and  $\pi_F$ .

### 4.3 Comparing tax and transfer revenues

I jointly estimate the impact of tax and non-tax revenues by considering the following equation:

$$E_{i,t} = \beta_T T_{i,t} + \beta_F F_{i,t} + \sum_{c=1}^{14} [f_c(\tilde{P}_{i,t}) + g_c(\tilde{P}_{i,t})(\tilde{P}_{i,t} > 0)] 1_c + \sum_{c=1}^{14} 1_c + \delta Z_{i,t} + \gamma_t + \mu_i + \epsilon_{i,t} \quad (10)$$

where outcomes  $E_{i,t}$  are expenditure outcomes of interest, program participation  $PMAT_{i,t}$  and the indicator  $D_{i,t}$  are used as instruments for  $T_{i,t}$  and  $F_{i,t}$ , all other variables are as above and  $Z_{i,t}$  includes an indicator of whether the municipality has applied to the tax capacity program. I flexibly control for population size using spline polynomials or local linear regressions as above.

I have discussed above the assumptions needed for the interpretation of  $\beta_T$  and  $\beta_F$  as estimates of the causal impacts of tax or non-tax revenues on outcomes ( $E_T$  and  $E_F$ ). To interpret the comparison of the two estimates as a test of Hypothesis 3 ( $E_{Ti} \neq E_{Fi}, \forall i$ ) an additional assumption on the heterogeneity of these parameters is

needed. The specification in equation (10) identifies two local average treatment effects (Imbens and Angrist, 1994):  $\beta_F$  identifies the average  $E_F$  among municipalities close to a cutoff and  $\beta_T$  the average  $E_T$  among those whose tax revenues increase thanks to the program. Heterogeneity of the parameters across the populations affected by each instrument could therefore lead to estimates that are different even if any given municipality spends tax and transfer revenues in the same way.<sup>28</sup> The average PMAT municipality is larger and richer than the average municipality affected by the FPM transfer allocation rule and may for example have stronger public capacity to improve local infrastructure, or conversely less of a need for more public infrastructure, so the bias cannot be signed.

The particular design used to identify  $E_F$  allows us to partially test this assumption because it provides us with 14 local estimates of  $E_F$ . This helps in two ways. First, considering the distribution of these 14 different estimates of  $E_F$  is one way to assess the extent to which the parameter varies in the population and provides bounds that can be compared to the estimate of  $E_T$ . Second, some of the sub-populations around the cutoffs are a priori more comparable to PMAT municipalities because they have similar characteristics.

I therefore estimate equation (10) on different samples to estimate the impact of tax and transfer revenues on outcomes among municipalities that are as comparable as possible. I consider a sample of municipalities that are ‘close enough’ to the cutoffs (5% bandwidth); this specification implies that the impact of tax revenues is estimated using only PMAT municipalities that are also affected by the instrument for transfers. I then restrict estimation to the weighted common support sample: in this specification non-PMAT municipalities are weighted by a function of their estimated propensity score so the impact of transfers is estimated for an average municipality which is by construction very similar to that on which the impact of tax revenues is estimated.

Finally, I estimate equation (10) on a sample consisting only of municipalities that join the program and are affected by the transfer allocation rule, using the number of years a municipality has been participating in the program as an instrument for tax revenues. This allows me to estimate  $E_F$  and  $E_T$  on the same sub-sample of municipalities and test whether heterogeneity in the way all revenues are spent is driving a potential difference between the estimates, at the cost of substantially reducing the sample size.

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<sup>28</sup>In other words, the difference between  $\beta_T$  and  $\beta_F$  in (10) is a test of the equality of  $E_T$  and  $E_F$  if these parameters are homogenous in the population.

## 5 Main results

In this section I first present evidence regarding the impact of the tax capacity program on tax revenues (Hypothesis 1) and on the effect of tax revenues on municipal education infrastructure (Hypothesis 2). I then turn to the impact of the transfer allocation rule on both transfer revenues and education infrastructure.

### 5.1 The tax experiment

#### 5.1.1 Hypothesis 1: Impact of the program on tax revenues

Figure 3 present graphical evidence on the impact of the program on tax revenues. The graphs plot the estimated  $\pi_{Tj}$  from equation (5): each point on the solid lines summarizes the effect of having been in the program for  $j$  years (for  $j > 0$ ) or of starting the program in  $j$  years (for  $j < 0$ ) compared to the year just before the program started.<sup>29</sup> The top graph considers the impact of the program on all PMAT municipalities, the bottom graph only on the 57 PMAT municipalities that waited two years after applying before they received their first loan (other PMAT municipalities are dropped). There is no evidence that municipalities that eventually join the program experience different trends prior to its start: the estimated  $\pi_j$  are very close to 0 for  $j < 0$ . This is also true for municipalities that wait 2 years between their application date and their start date; we see no change in outcomes at the date at which they self-select ( $j = -2$ ).

Table 3 reports estimates of the impact of the tax capacity program on tax revenues (equation (3)) in panel A. In columns 1 and 2 the model is estimated on the whole sample and with and without covariates, in column 3 the sample is restricted to the common support sample and non-PMAT observations are weighted by a function of their propensity score. The impact of the program is slightly smaller when we include controls and re-weight observations, suggesting that unbalanced treatment characteristics introduce a small bias. Tax revenues increase by 10-11 Rs per capita thanks to the tax capacity program on average, ie after seven years in the program. This is a 10% increase with respect to the baseline level. As municipalities receive 9.6 Rs per capita in loans through the program on average this implies that each Real invested in tax administration roughly yields an extra 1 Real in tax collection each year.

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<sup>29</sup>I only plot estimates from 5 years before to 5 years after the start of the program, because the majority of PMAT municipalities are observed for that period, but the regressions include the full set of dummies, as specified in equation (5). Results are unaffected when I exclude municipalities that are not observed 5 years before and after the start of the program.



The last two columns present robustness checks for the main estimates by considering alternative specifications. In column 4 I replace municipality with municipal administration fixed effects. More educated mayors are more likely to join the program, this could bias results if they are also better at collecting taxes but the estimate is unaffected. The estimates in column 5 are obtained on a sample consisting only of the 339 municipalities that join the program and replacing the indicator for program participation with a variable equal to the number of years since the municipality started a program. This allows me to use only municipalities which will join the program in later years as a control group at the cost of imposing a linear impact of the program over time. If the findings above are due to different trends in PMAT and non-PMAT municipalities we should see no impact of the program in this sample. The estimated impact of having been an extra year in the program, at 3.2 extra tax revenues per capita, is in line with the average impact of the program in the previous columns.

The estimated impact of having applied to the program but not received the loan yet is always close to zero and imprecisely estimated, in line with the graphical evidence above. This can only be interpreted as evidence that there is no effect on outcomes of simply selecting into the program if we think that municipalities cannot control the amount of time they wait between applying and starting the program. In particular, if municipalities that are less motivated to increase taxes wait longer the lack of effect of having applied to the program could simply reflect differences between municipalities that wait and those that do not. Evidence presented in the web appendix suggests this is not the case, as the impact of the program on outcomes does not vary in a systematic way with the amount of time a municipality waited after applying to the program. Overall, this evidence confirms that unobserved time-varying shocks are not driving the results, and in particular that an increase in motivation of the local administration is not a sufficient condition for the observed change in outcomes. It may however be a necessary condition: imposing the program on municipalities in which local officials are not interested in increasing tax collection is unlikely to yield similar outcomes. We should therefore interpret the 10% increase in tax collection as the program's average treatment effect on the treated: the impact of the program on tax revenues amongst municipalities that join.<sup>30</sup>

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<sup>30</sup>I only weight non PMAT municipalities by a function of their propensity score in the weighted difference-in-differences specification, in line with the method developed by Hirano and Imbens (2001) to estimate average treatment effect on the treated. I could obtain an estimate of the average treatment effect on the whole population by also weighting PMAT municipalities, but this is not appropriate in this context.

### 5.1.2 Hypothesis 2: Impact of tax revenues on education infrastructure

Figures 4 and 5 present graphical evidence on the evolution of local education infrastructure before and after the start of the program. We see an increase in education infrastructure after the program starts, in line with the increase in tax revenues observed in Figure 3. Panel B of Table 3 shows the impact of a 10 Rs increase in tax revenues thanks to the program on the quantity and quality of municipal education infrastructure (equation (4)). Tax revenues increase the quantity of classrooms in use in municipal schools by 0.32-0.45 per thousand school-age inhabitants, a 4-5% increase relative to the baseline level. The index of quality of municipal school infrastructure increases by 0.115-0.14, roughly one-tenth of a standard deviation.<sup>31</sup> The estimates are stable across specifications and samples and we see no impact of having applied to the program on education infrastructure in the regression estimates or the graphical evidence. I also consider the impact of tax revenues on the number of school employees in municipal schools. Results show that the extra revenues generated by the program were not used to hire more school employees, as expected given that this particular expenditure is funded through specific grants (see web appendix). Finally, the web appendix provides some results regarding the reduced-form impact of the program on literacy rates of cohorts that could have attended municipal schools over the period of study, using information from the 2000 and 2010 census. We see a small impact (1 percentage point) on the literacy rates of children aged 5 to 9 but it is not robust to changes in the specification used.

A back of the envelope calculation suggests the magnitudes in Table 3 are plausible. PMAT municipalities spend on average 28% of their 450 Rs per capita in revenues on education and have 9.2 classrooms per 1000 school-age inhabitants on average. Assuming the average propensity to spend on all types of education infrastructure is the same this means they open a new classroom for each extra 13.7 Rs per capita in revenues ( $13.7 = 0.28 \times 450 / 9.2$ ). The tax capacity program increases annual tax revenue by roughly 10 Rs, this would lead to an extra 0.73 classrooms if all the increase was spent on education, and an extra 0.22 classrooms if 28% of it was spent on education. The estimated increase in classrooms is within this range. In contrast the amount lent by the program - a one-time transfer of 9.6 Rs on average - could only have funded this extra education infrastructure for one year. Figures 4 and 5 indicate an increase in education infrastructure which lasts at least 5 years and hence could not have been

<sup>31</sup>The web appendix presents the impact of the program on each indicator of school quality separately. The aggregate impact is driven by changes in the number of schools with computers, TV, a science lab and an internet connection.

financed by the loan.

## 5.2 The transfer experiment

Figure 6 plots the residuals from a regression of transfer revenues per capita on municipality and year fixed effects and a set of 14 segment dummies against the scaled population variable  $\tilde{P}_{i,t}$ . There is a clear jump at the population cutoffs of roughly 13 Rs that dwarfs any variation away from the cutoffs. Figures 7 and 8 plot the residuals from a regression of the number of classrooms in municipal schools (Figure 7) or the index of municipal school quality (Figure 8) on municipality, year and segment fixed effects against scaled population. There is no evidence of a jump when population size reaches a threshold for either of these outcomes. Table 4 presents regression estimates. Columns 1 through 4 control for population size using local linear regressions around each cutoff, restricting the sample to a 2% bandwidth around the cutoffs in columns 1 and 2, and to a 5% bandwidth in columns 3 and 4, column 5 uses the entire sample of municipalities affected by the transfer allocation rule and controls for a spline third-order polynomial in population size. Columns 1 and 3 present results obtained from a specification without covariates and municipality fixed effects, thereby using variations both within and across municipalities for identification.

In the first panel, I show the impact of the population discontinuities on FPM transfer revenues per capita (equation (8)). We see that transfer revenues increase sharply by 12-14 Rs per capita at the population cutoffs on average. Estimates of equation (9) in the second panel show that the impacts of transfer revenues on the quantity and quality of education infrastructure are on average zero - they are very small, not statistically significant, and change sign across specifications. Including covariates, using both variations both within and accross municipalities, or using different methods to control for population size does not affect the results.<sup>32</sup>

I investigate whether the null effects in Table 4 are in fact averages over positive and negative (or noisy) effects in the web appendix by considering graphical and regression evidence for each cutoff separately. The first two brackets experience an increase of more than 20 Rs per capita, around the last four the increase is less than 5 Rs. The estimated impact of transfers on education infrastructure quantity is statistically significant at the 10% level in 5 cases out of 70 estimates (2 positive, 3 negative), and that on education infrastructure quality in 1 case (positive) out of 70 estimates. This

<sup>32</sup>Results in the web appendix further show that the lack of impact of transfer revenues on outcomes is robust to excluding the three cutoffs which are near population thresholds at which the wages of local councillors increase.

is roughly what we would expect if all parameters were zero. At no threshold are these positive effects robust across specifications. These findings indicate that increases in FPM transfers are not used to fund improvements in these two measures of municipal education infrastructure. There is no impact, as expected, of FPM transfers on the number of employees in municipal schools - see the web appendix.

### 5.2.1 Comparison with the literature

We have found above that municipal education infrastructure quantity and quality do not increase with transfer revenues. This contrasts with results in Litschig and Morrison (2013): they find that transfers increase schooling per capita and literacy rates amongst local governments in Brazil in the 1980s. In the web appendix I replicate their analysis of the impact of transfers in the early 1980s on 1990 Census outcomes by looking at the impact of transfers in the early 2000s on 2010 Census outcomes. I find no impact of transfers on education outcomes. The difference between the results presented here and their study cannot therefore be due to the use of different outcomes, empirical strategy, or time lag between the increase in transfers and the measure of outcomes; it is more likely explained by three key differences between their setting (the 1980s and early 1990s) and the 1998-2011 period studied here.

First, their object of study is small municipalities (those around the first 3 population thresholds) in the 1980s, a period during which Brazilian local governments had a lot less revenues than in the 2000s, and hardly any tax revenues.<sup>33</sup> Increases in FPM revenues played a larger role in relaxing government's budget constraints back in the 1980: they represented nearly half of total revenues in 1980 compared to a third in my period of study. Second, their main outcome of interest - literacy in adults aged 19 to 28- is much higher in the 2010 census (close to 90%) than in the 1990 census they consider (78%), leaving less room for improvement. Third, and most importantly, they study an extremely large increase in transfer revenues of a magnitude never observed since 1985. They consider cumulated transfers in the 1982-1985 period which were determined by municipal population measured in the 1980 census. From 1985 onwards population estimates were revised annually, leading to a much smaller effect on cumulated future transfer revenues of being above a population cutoff in any given year (see web appendix). The increase in FPM revenues they study thus represents 2.5% of local GDP in rural areas (1.4% in urban areas) compared to less than 0.3% of GDP in the 1998-2011 period.

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<sup>33</sup>In particular, the large grants earmarked for education that municipalities currently receive were all created after the mid-1990s.

## 6 Comparing tax and non-tax revenues

This section first presents some evidence that attempts to test whether municipalities spend increases in tax and transfer revenues differently (Hypothesis 3). The results discussed above indicate that increases in tax revenues lead to higher municipal education infrastructure ( $G_T > 0$ ) whereas increases in transfer revenues do not ( $G_F = 0$ ). To test whether revenues from different sources are spent differently we must consider whether the difference between our estimates of  $G_T$  and  $G_F$  could be due to the fact that they are obtained on groups of municipalities with different characteristics that lead them to spend increases in all types of revenues differently. I present several results suggesting that heterogeneity in  $G_T$  and  $G_F$  is unlikely to be driving the observed difference, though I cannot completely rule out this explanation for the entire sample of municipalities. I then discuss possible mechanisms for the observed difference and show evidence that allows me to rule out some of them.

### 6.1 Results

Table 5 presents results from the estimation of equation (10) on different samples to attempt to estimate the impact of tax and transfer revenues on outcomes among comparable municipalities, as discussed above. Results in the first column are obtained on the whole sample with municipality fixed effects, results in column 2 include municipal administration fixed effects. Results in columns 3 and 4 consider smaller samples: in column 3 the estimation uses only municipalities in a 5% bandwidth around the FPM cutoffs; in column 4 the sample is the weighted common support sample.<sup>34</sup>

The impact of non-tax revenues on municipal education infrastructure in Panels A and B is never statistically significant regardless of the specification used and it is always much smaller than the impact of tax revenues. The estimates of  $\beta_T$  are very similar when estimated on smaller samples in columns 3 and 4 though not always statistically significant. A formal test of  $\beta_F = \beta_T$  is given in the last line of both panels. The null hypothesis is rejected when the whole sample is used; it cannot always be rejected among smaller samples but this is because standard errors increase – point estimates are similar.

The stability of results across samples suggests there is little underlying variation in the impact of transfer revenues on outcomes. The web appendix presents further evidence that this impact is unlikely to differ significantly in the population by pre-

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<sup>34</sup>The first stages for tax and transfer revenues on these different samples are presented in the web appendix.

senting estimates of  $G_F$  around each cutoff separately and, for each segment of the population around a FPM cutoff, descriptive characteristics of variables which could affect the impact of public revenues on outcomes and the average weight of non-PMAT municipalities used in the common support regression. Municipalities around cutoffs 9 to 12 are very similar to PMAT municipalities, including along characteristics that are likely correlated with demand for education (similar baseline GDP, public revenues and health and education infrastructure in particular) and are given more weight in the propensity-score weighted specification. It is reassuring to see that the impact of transfer revenues on municipal education infrastructure is not different around cutoffs 9 to 12.

Finally, Table 6, Panel A presents estimates of the impact of tax and non-tax revenues on outcomes amongst only municipalities that join the program at some point and are affected by the transfer allocation rule.<sup>35</sup> This allows me to test whether tax and non-tax revenues are spent differently by the same municipalities, albeit on a smaller sample. The population cutoffs increase non-tax revenues by a precisely estimated 10 Rs even in this smaller sample. We see no impact of this increase in non-tax revenues on education infrastructure outcomes. Overall, whilst I cannot test the assumption that  $G_F$  is homogenous in the population, the evidence suggests that i) variations in municipalities' marginal propensity to spend out of all types of revenues are likely too small to explain the observed difference between the estimates of  $E_T$  and  $E_F$ , and ii) a small sub-sample of municipalities amongst which both impacts can be estimated do spend tax and non-tax revenues differently.

The results above show that one type of local public infrastructure increases with tax revenues and not with non-tax revenues. This is in line with the hypothesis that tax and non-tax revenues are spent differently but does not necessarily indicate that tax revenues are spent 'better'. Municipalities could be spending non-tax revenues on other types of expenditures that also potentially increase welfare and local human capital. An interesting candidate is health, which is the second largest municipal budget item and for which some data is available but only for four years. I consider whether transfer revenues are spent more towards local health infrastructure than tax revenues by estimating equation (10) using the number of municipal health establishments per 100,000 inhabitants as outcome variable. Results are presented in paper's online Appendix. We cannot reject the hypothesis that both tax and non-tax revenues have no impact on the number of municipal health units but can reject that non-tax revenues

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<sup>35</sup>This excludes state capitals and municipalities whose population exceeds 142,633 or is lower than 6,792 at some point over the period, ie 25% of municipalities that join the program.

have a bigger impact than taxes, though results are fairly sensitive to the specification used.

Some evidence on what FPM transfers are used for is found in Brollo *et al.* (2013) who, in the same period and using a similar identification strategy, find that they lead to more corruption amongst municipalities close to the first four population cutoffs.<sup>36</sup> I cannot directly compare the impact of tax and non-tax revenues on corruption as corruption data is available for extremely few municipalities close to cutoffs 9 to 12 and these are the municipalities which are the most relevant for the comparison. Instead I look for evidence suggestive of an impact of tax revenues on corruption by comparing non PMAT municipalities with PMAT municipalities that were audited either before they start the program or after they start the program. I estimate a modified version of equation (4) where municipality fixed effects are replaced by a dummy  $J_i$  equal to 1 if municipality  $i$  joins the tax capacity program at some point in the period and a set  $X$  of time-invariant municipal characteristics measured in the 2000 Census<sup>37</sup>:

$$C_{i,t} = \beta_T T_{i,t} + \delta_1 Z_{i,t} + \delta_2 X_i + \eta J_i + \gamma_t + \epsilon_{i,t} \quad (11)$$

Results are presented in the paper’s web appendix. I consider as proxies for corruption  $C_{i,t}$  the three measures compiled by Litschig and Zamboni (2012) and the two measures compiled by Brollo *et al.* (2013) from the randomized audits of municipalities. I find that PMAT municipalities audited prior to the start of the program were roughly as corrupt as non-PMAT municipalities and that the increase in tax revenues thanks to the program does not lead to any increase in the number of irregularities reported in the audits. This suggests that an increase in tax revenue does not increase corruption in a context where increases in transfer revenues do, at least among the smaller municipalities considered by Brollo *et al.* (2013). Interestingly one of the mechanisms that leads to transfer revenues increasing corruption in Brollo *et al.* (2013) cannot explain the observed difference between tax and non-tax revenues in this paper. They argue that higher revenues give politicians more room to grab rents without disappointing voters and that this in turn leads to worse quality (because rent-seeking) politicians being elected when transfer revenues are high. Evidence in column 3 in Table 5 shows

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<sup>36</sup>See Brollo *et al.* (2013), Figure 2 and Table 3: the broad measure of corruption increases by 0.11 on average at the cutoffs from a baseline of 0.79 and the narrow measure by 0.15 from a baseline of 0.46. This translates into 14% and 33% increase on average at the four first cutoffs where transfer revenues per capita increase by 18.5 Rs.

<sup>37</sup>These are urban population, inequality, life expectancy, median education level, whether the municipality has a local radio station and whether it is the seat of a local branch of the judiciary. All other variables are as above, program participation is used as an instrument for tax revenues.

that results hold using only variations within a municipal administration - holding the quality of politicians constant - so the idea that different types of revenues attract different types of politicians cannot explain the results.

There is no data available on other potential uses of municipal revenues, but fiscal data allows me to consider non-spending consequences of increases in revenues. Municipalities could choose to use extra revenues to decrease their indebtedness, decrease taxes (when transfers increase) or as leverage to obtain more discretionary federal transfers which sometimes require that municipalities commit some of their own revenues (defined as local taxes or FPM transfers) to a particular type of expenditure. The last panel of Table 5 considers the effect of tax or transfer revenues on municipal public spending per capita. We see that an extra 10 Rs in tax or transfer revenues leads to an increase in public spending that is very close to 10 Rs. This suggest there is no, or very little, crowding out (or in) of other types of revenues and that increases in revenues are not used to decrease municipal debt.<sup>38</sup> Overall the findings are consistent with the idea that tax revenue are spent ‘better’ than transfers: tax revenues have a bigger impact on municipal infrastructure that potentially benefit citizens and possibly a smaller impact on corruption than non-tax revenues.

## 6.2 Mechanisms

Different mechanisms could explain the difference between how tax and non-tax revenues are spent, as explained above. I first show that it is unlikely that different characteristics of the revenues not necessarily related to their source lead to them being differently spent. I then discuss mechanisms that may explain the results. All results discussed in this sub-section can be found in the paper’s web appendix unless indicated otherwise.

### 6.2.1 Characteristics of the revenues unrelated to their source

The average increase in non-tax revenues generated by the population cutoffs is of roughly the same size as the average increase in taxes thanks to the tax capacity program (10 Rs) so scale effects cannot explain the results. Asymmetries between the impact of increases and decreases of revenues on expenditure outcomes could explain part of the results: the estimated impact of non-tax revenues is averaged over increases and decreases in FPM revenues but the estimate of the impact of taxes comes from

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<sup>38</sup>This is in line with results in Corbi *et al.* (2014). Reliable data on municipal debt is not available for this period of study, total spending is defined in fiscal data as excluding debt payments.



only increases in tax revenues. This would lead to  $\beta_F$  being smaller than  $\beta_T$  even if the structural impact of increases in both types of revenues is the same if, for example, municipalities find it difficult to shut down classrooms when revenues fall. Excluding from the sample municipalities which drop to a lower population bracket at least once during the period (15% of municipalities) does not however affect the results.

Differences in the predictability of tax and transfer revenue could potentially explain the results. Politicians may be less willing to spend increases in non-tax revenues on items that require committing funds over time if they cannot predict how long they will last. This is unlikely to explain the results because in this context transfer revenues are not more volatile than taxes. On the contrary the within-municipality standard deviation is smaller relative to the mean for FPM revenues than for tax revenues, even after the tax capacity program. This is because FPM transfers only vary with population and the total amount allocated to FPM at the federal level whereas tax revenues react to changes in local economic conditions.<sup>39</sup> This being said, politicians are in direct control of tax revenues and have no control over municipal population estimates that determine transfers, so I cannot rule out that they think of tax revenues as a more reliable source of public funds than transfers.<sup>40</sup>

Finally, the tax capacity program itself could have made local governments spend *all* types of public revenues better, threatening the validity of program participation as an instrument for tax revenues. The re-organization of municipal tax departments may for example have had positive externalities on other departments through transfers of staff or sharing of information and best practices. The existence of exogenous variations in non-tax revenues allows for a test of this particular violation of the exclusion restriction: we can check whether PMAT municipalities spend FPM revenues better after the start of the program. The low number of observations means results must be treated with caution but the last panel of Table 6 shows the assumption seems to hold: there is no impact of increases in non-tax revenues amongst PMAT municipalities after the start of

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<sup>39</sup>Other types of non-tax revenues are probably more volatile so this mechanism may play a role in other contexts. The high volatility of aid revenues has for example been invoked to explain why aid is not necessarily spent on infrastructure investments (Bulir and Hamann, 2008).

<sup>40</sup>The Brazilian federation context could also explain part of the results. There is one institutional difference in the way FPM revenues and local tax revenues are monitored: state governments have the power to monitor the use of FPM revenues but not that of municipal tax revenues. Conversations in the field suggest this power is in practice hardly ever used, but municipalities could exert more caution in spending FPM resources if they fear state monitoring. It may be that spending on education infrastructure whilst abiding by state procurement rules takes more time, or is more difficult in particularly strict states. I find however no evidence that FPM revenues have an effect on outcomes with a one or two years lag or that there is an effect in some states but not others. This suggests state oversight cannot explain why FPM revenues have no impact on education infrastructure.

the program. This suggests the program itself did not directly affect how governments spend public revenues.<sup>41</sup>

### 6.2.2 Taxes vs non-tax revenues

As explained above we expect tax revenues to lead to more infrastructure spending than non-tax revenues if citizens are better informed about increases in taxes. Increases in tax revenues are necessarily observed by those that pay them but citizens may have to exert effort to know about increases in transfers: to compute the amount of transfers received by the government in a given year citizens would have to obtain information not only on the rule but on the total amount of revenues reserved for FPM transfers at the federal level and the precise population estimates used. If this hypothesis is correct we expect to see a smaller difference between how taxes and transfers are spent when citizens have more access to information on the public budget. Following Ferraz and Finan (2011a) I use the presence of a local radio station as a proxy for how much local information citizens can access and estimate equation (10) adding interaction terms between tax and transfer revenues and a time-invariant indicator for presence of a local radio station in the municipality in 1998. Results show that tax revenues have a significantly bigger impact on municipal education infrastructure than transfer revenues when there is no local radio station, but the difference is no longer statistically significant when the municipality has its own radio station (see web appendix). This is not a rigorous test of this mechanism as the presence of a local radio station is likely correlated with municipal characteristics that affect how revenues are spent. Nevertheless the evidence does not contradict the idea that information asymmetries explain part of the observed difference.

Two theories suggest that demand for public services or citizen's willingness to constrain politicians to meet this demand increases when tax revenue increase. Tiebout-style explanations suggest that an increase in taxes will attract different types of citizens, but mobility costs in Brazil are likely too high for this mechanism to bite (Timmins and Menezes, 2005). The literature in political science argues that paying more taxes (or starting to pay taxes) makes citizens demand more from their government and/or spend more time monitoring elected politicians; this idea came up often during interviews with taxpayers and politicians (see for example Paler (2013)). In the absence

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<sup>41</sup>Another possible channel through which the program could have affected outcomes directly is through citizen's political response to improved tax enforcement, as seen in Casaburi and Troiano (2015) who show that Italian citizens are more likely to re-elect mayors that implemented an anti tax-evasion program. I do not however find any impact of the PMAT program on the probability that the incumbent is re-elected - results are available in the online appendix.

of data on migration across municipalities over the period, citizens' preferences or endeavors to control their politicians these two mechanisms cannot be formally tested.<sup>42</sup> There is however evidence from the US that extra spending on school infrastructure has a large impact on housing prices, indicating that local residents value such increases highly (Cellini *et al.*, 2010). This suggests that increases in physical education infrastructure could be a particular visible use of public funds and hence the one politicians may choose to spend extra tax revenues on if indeed citizens demand more from their politicians when they pay more taxes.

Finally it is likely that the way in which tax revenues were increased explains how they were subsequently spent. Increases in tax revenues can come from higher tax rates or higher tax bases. Municipal data on tax rates is not available but evidence on actions financed by the program from a qualitative study of early participants (BNDES, 2002) and interviews with local officials (see web appendix) suggests that the main impact of the program was to widen the municipal tax base.<sup>43</sup> The program likely increased the number of individuals and firms paying local taxes and changed the characteristic of the median taxpayer, and may have forced governments to become more responsive to the demands of that median taxpayer. This would lead to tax revenues being spent more on education infrastructure if taxpayers favor education spending more than (poorer) non taxpayers, in line with evidence in Bursztyn (2013) that middle-income individuals demand more education spending than low-income individuals in Brazil. This qualifies the external validity of the results in this paper: they indicate that increases in tax revenues *due to improvements in tax capacity* are spent better than increases in non-tax revenues. This type of increases in tax revenues is probably the most relevant in developing countries in which future increases in public revenues are more likely to come from increases in governments' capacity to tax than from higher tax rates (Gordon and Li, 2009, Besley and Persson, 2013).

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<sup>42</sup>A proxy for citizens' willingness to monitor and hold accountable elected politicians could be voter turnout. Voting is compulsory in Brazil however, and the (small) variations in turnout at municipal elections are probably due to differences in enforcement of compulsory voting regulations.

<sup>43</sup>Several municipalities report decreasing their tax rates thanks to the PMAT program. In the city of Nova Iguaçu for example both the number of registered properties and the average registered property size had doubled after three years in the program and the municipality subsequently decreased the average property tax rate by nearly 50%, tax collection nevertheless increased substantially. See the web appendix for more details.

## 7 Conclusion

This paper takes advantage of a Brazilian local tax capacity program and discontinuities in the rule allocating federal transfers to municipalities to study how governments spend increases in public revenues from different sources. Results suggest that local governments use the increase in taxes thanks to the program to provide more education infrastructure than they do when faced with an increase in transfer revenues of the same amount.

This paper is admittedly limited in its capacity to test whether tax and transfer revenues would be spent differently by *all* governments - I can only directly compare the impact of the two types of revenues on a sub-sample of Brazilian municipalities - lending a cautionary note to drawing strong policy inference. Nevertheless, the results show that increasing the capacity to tax of Brazilian local governments that express an interest in raising tax revenues has a larger impact on locally provided public infrastructure than giving non-earmarked grants to the average government. As such, these results speak directly to considerations about the right form of decentralization. The existence of a large ‘fiscal gap’ between local expenditure responsibilities and local tax revenues is an ubiquitous characteristic of local governments around the developing world. The evidence presented here suggests that local tax collection is a necessary feature of successful decentralization. Moving up from the local government level, they support the idea that ‘tax capacity building’ is a form of development assistance that may lead to more public investments in human capital infrastructure than budget support assistance.

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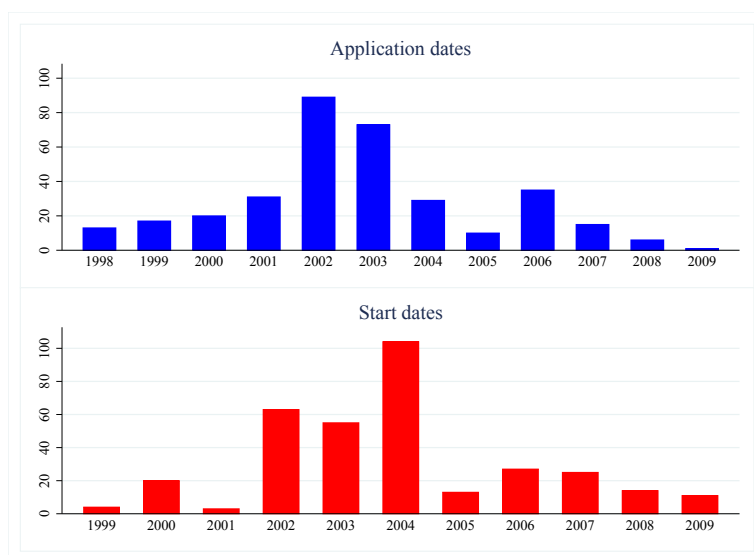
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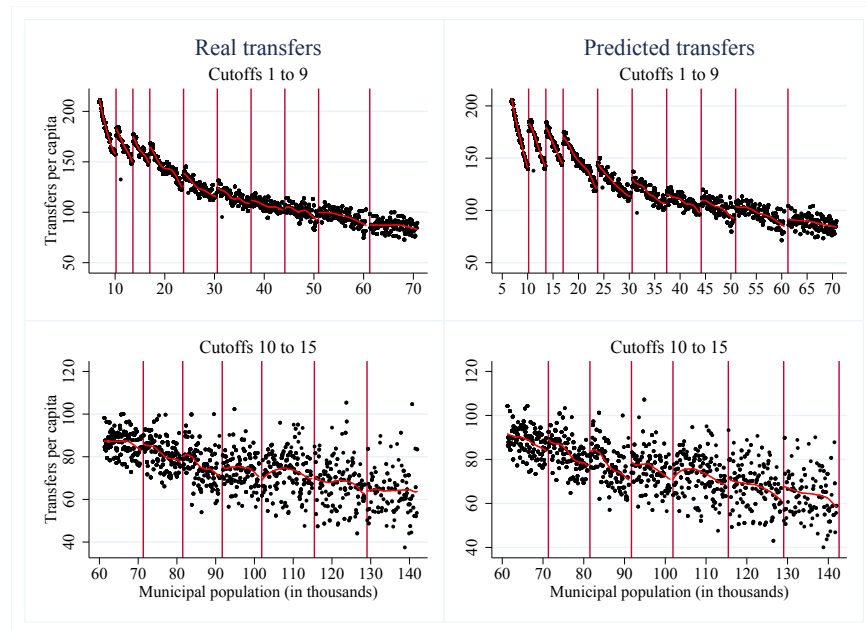
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Figure 1: Tax capacity (PMAT) program: distribution of application and start dates



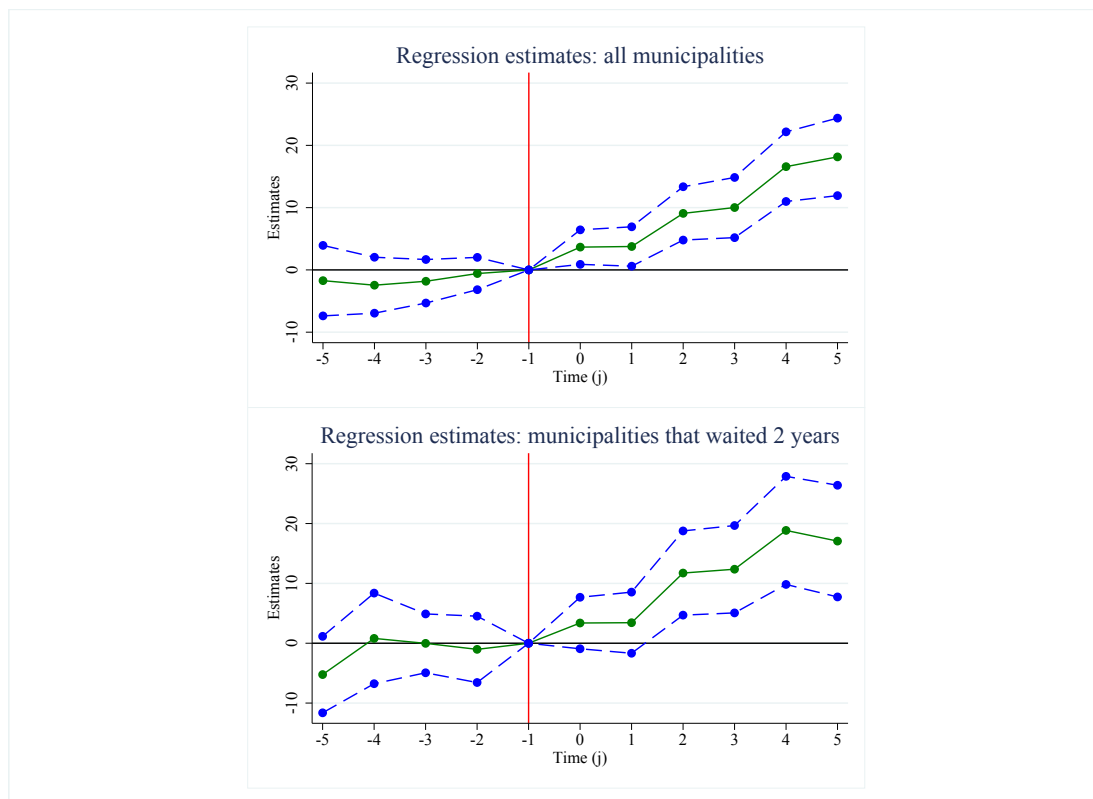
*Notes:* Each bar represents the number of municipalities applying to (top panel) or starting (bottom panel) the tax capacity program in a given year. The sample includes the 339 municipalities that take part in the program.

Figure 2: Real and predicted non-tax revenues per capita as a function of population



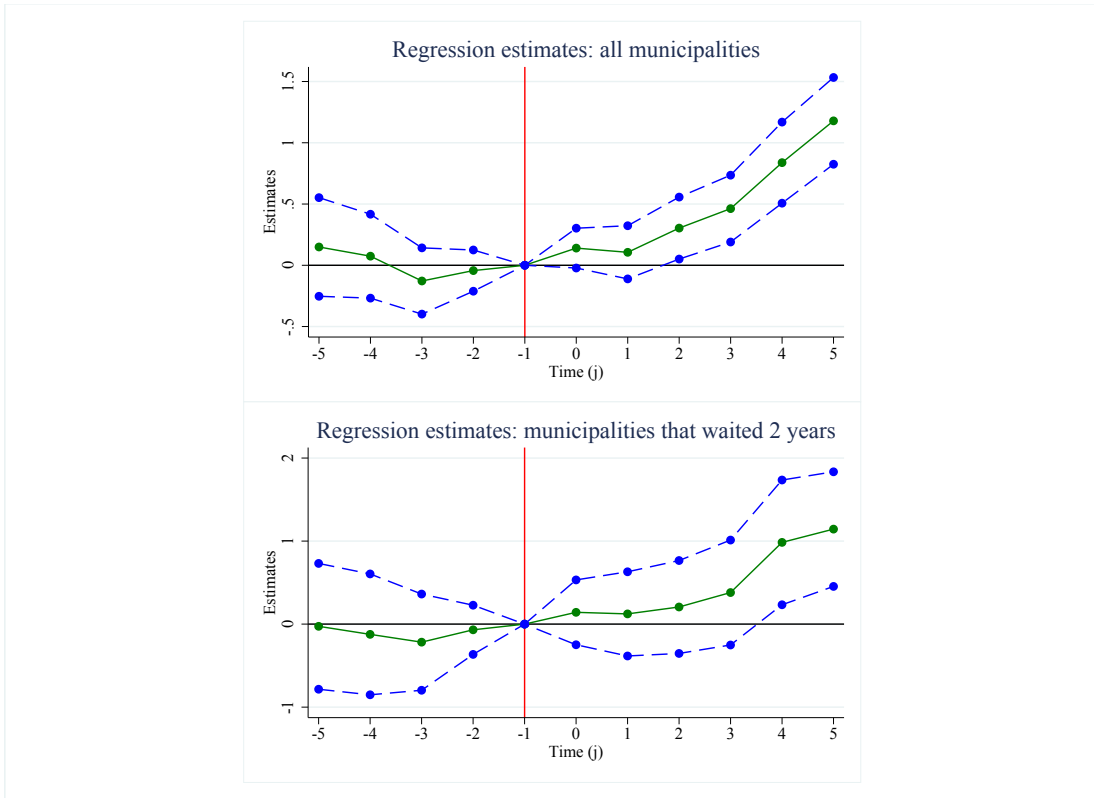
*Notes:* Real (left panel) and predicted (right panel) FPM transfer revenues per capita as a function of estimated municipal population in the previous year, local polynomial smoothing (in red) performed separately in each interval between two cutoffs. The sample includes all municipalities with less than 142,633 inhabitants and more than 6,792 inhabitants that are not state capitals over the period 1998-2011.

Figure 3: Evolution of municipal tax revenues in PMAT vs non-PMAT municipalities



*Notes:* Each point on the (solid) green line represents the impact on tax revenues per capita of having been in the program for  $j$  years (for  $j > 0$ ) or of starting the program in  $j$  years ( $j < 0$ ), estimated following equation (5). The vertical line at  $j = -1$  indicates the reference year. The points on the (dashed) blue lines represent the 95% interval for the estimates. The top panel compares all PMAT municipalities to non-PMAT municipalities, the bottom panel compares the 57 PMAT municipalities that waited two years between applying to and starting the program to non-PMAT municipalities.

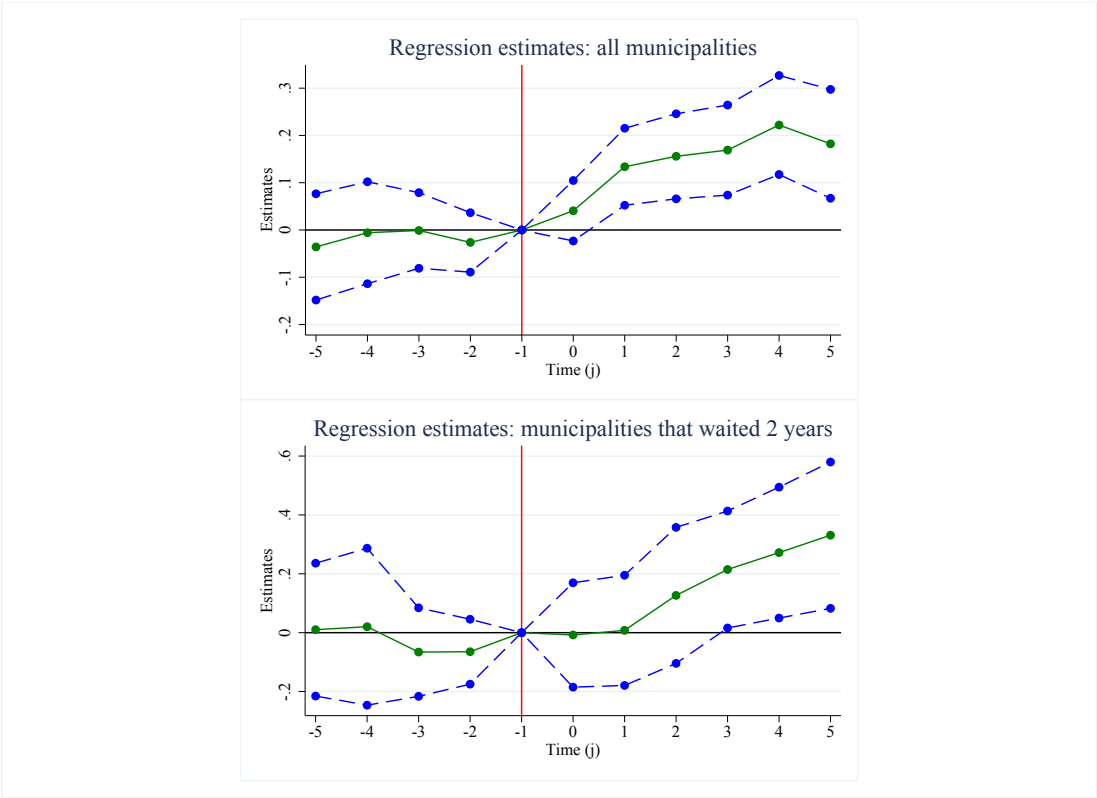
Figure 4: Evolution of municipal education infrastructure (quantity) in PMAT vs non-PMAT municipalities



*Notes:* See notes to Figure 3. The Figure plots estimates obtained by estimating equation (5) using the number of classrooms in use in municipal schools per thousand school-age inhabitants as dependent variable.

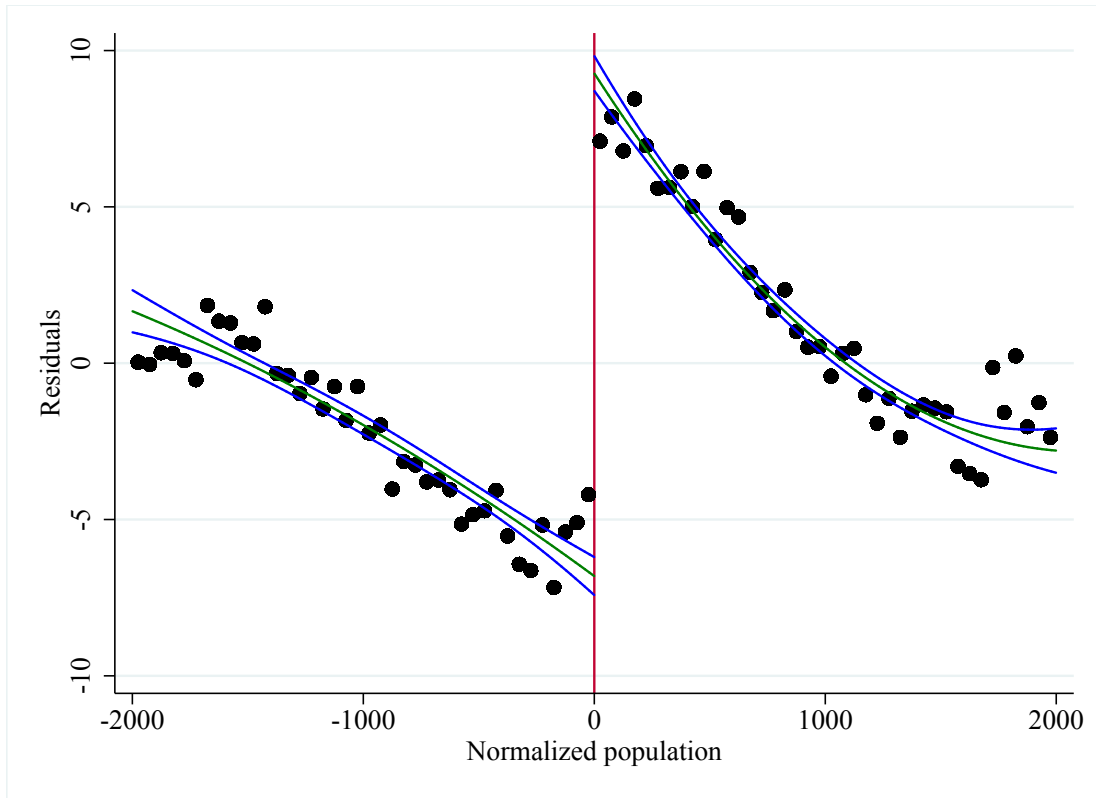
Figure 5: Evolution of municipal education infrastructure (quality) in PMAT vs non-PMAT municipalities

the



*Notes:* See notes to Figure 3. The Figure plots estimates obtained by estimating equation (5) using the index of municipal school quality as dependent variable.

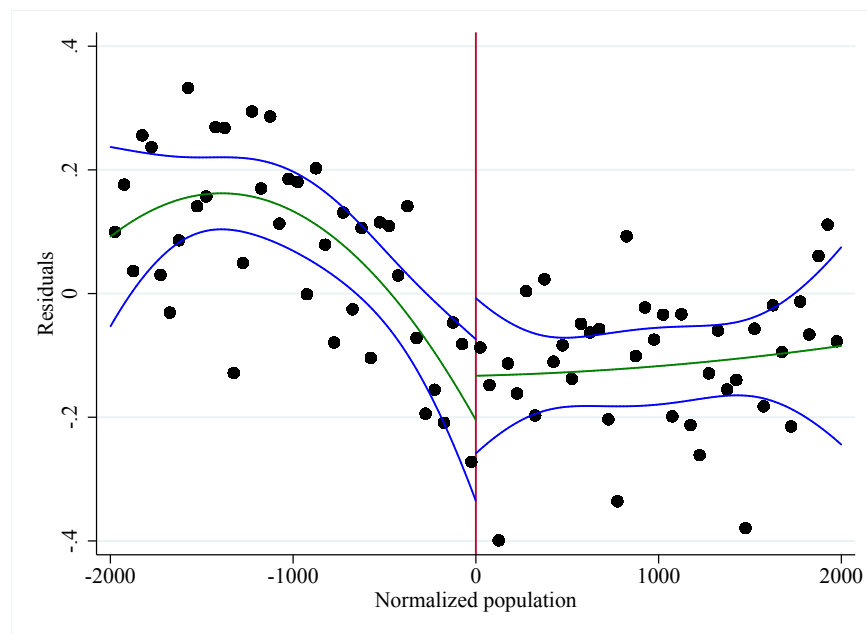
Figure 6: Transfer revenues per capita as a function of municipal population



*Notes:* Each point represents residual transfer revenues per capita as a function of normalized municipal population in the previous year averaged over 50 inhabitant bins (population size is normalized as the distance from the above or below threshold; symmetric intervals with no municipality in more than one interval). Residuals are obtained from a regression of transfer revenues per capita on municipality and year fixed effects and a set of 14 segment dummies. The central (green) line is a spline polynomial in population size fitted separately on each side of the pooled cutoff, the top and bottom (blue) lines are the 95% confidence intervals. The sample includes all municipalities that are not state capitals and with a population of less than 142,632 inhabitants and more than 6,792 inhabitants over the period 1998-2011.

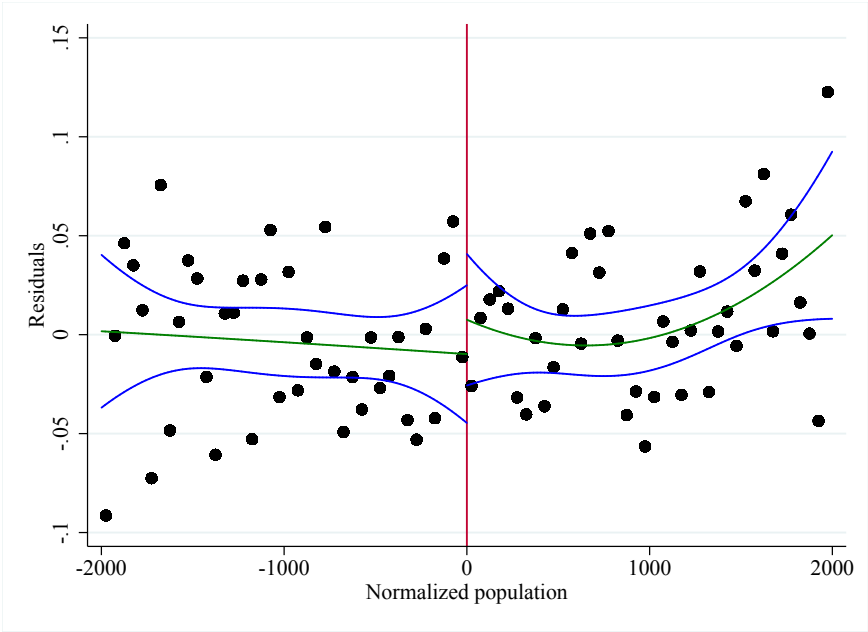


Figure 7: Municipal education infrastructure (quantity) as a function of municipal population



*Notes:* Each point represents the residual number of municipal classrooms divided by school age population as a function of normalized municipal population in the previous year averaged over 50 inhabitant bins. See notes to Figure 6.

Figure 8: Municipal education infrastructure (quality) as a function of municipal population



*Notes:* Each point represents the index of municipal school quality as a function of normalized municipal population in the previous year averaged over 50 inhabitant bins. See notes to Figure 6.



Table 1: Hazard model of the probability of applying to the program

	1	2	3	4
GDP per capita	0.187*** (0.036)	0.178*** (0.038)	0.186*** (0.036)	0.186*** (0.038)
Share services in GDP	-0.130 (0.170)	-0.157 (0.180)	-0.133 (0.171)	-0.098 (0.186)
Population	2.029*** (0.448)	3.076*** (1.130)	2.027*** (0.449)	-0.700 (2.269)
Urban population (%)	0.003* (0.002)	0.003 (0.002)	0.003* (0.002)	0.002 (0.002)
Taxes pc	-0.010 (0.031)	-0.020 (0.034)	-0.007 (0.031)	0.018 (0.027)
Distance to closest PMAT	-0.024** (0.012)	-0.021 (0.016)	-0.024** (0.012)	-0.022* (0.013)
Time	0.401*** (0.053)	0.282*** (0.071)	0.401*** (0.054)	0.415*** (0.059)
Municipal education infra: quantity	-0.005 (0.005)	-0.004 (0.005)	-0.005 (0.005)	-0.006 (0.005)
Municipal education infra: quality	0.004 (0.025)	0.001 (0.027)	0.004 (0.026)	-0.003 (0.026)
Municipal health infra	0.002** (0.001)	0.003*** (0.001)	0.002** (0.001)	0.003*** (0.001)
Inequality	-0.581 (0.474)	-0.945* (0.506)	-0.588 (0.476)	-1.123** (0.506)
Last term	-0.117* (0.060)	-0.147** (0.059)	-0.116* (0.060)	-0.126* (0.065)
Mayor's education		0.025*** (0.009)		
$\Delta$ Taxes pc $t - 1$			0.055 (0.116)	
$\Delta$ GDP pc $t - 1$			0.104 (0.262)	
$\Delta$ Population $t - 1$			-0.276 (0.861)	
$\Delta$ Taxes pc 96-98				-0.001 (0.001)
$\Delta$ Population 96-99				-0.000 (0.000)
$\Delta$ GDP pc 96-98				0.001 (0.008)
Observations	54502	42974	54502	42713
Municipalities	4565	4565	4565	3395

*Notes:* The dependent variable is an indicator equal to 0 as long as the municipality has not applied to the program and 1 the year it applies. Municipalities that have already applied are dropped. The sample starts in 1999 and excludes the 13 municipalities that applied in 1998. Observations prior to 2001 are excluded from column 2 because the mayor's education was not measured in the term 1996-2000, observations for which 1996 information is not available are excluded from column 4. All variables are measured in 1998 except municipal health infrastructure (1999) inequality and urban population (2000 Census), political variables (which change after each election) and lagged taxes, GDP and population. Tax and revenues are measured in 100 Rs. Standard errors are clustered at the municipality level. Statistical significance at the 10% level is represented by \*, at the 5% level by \*\*, and at the 1% level by \*\*\*.

Table 2: Descriptive statistics of municipalities by program participation status in year 1998

	PMAT	Non PMAT	Non PMAT, Common support
Tax revenues (Rs per capita)	94.35 (98.77)	36.24 (90.61)	105.9 (225.8)
Non-tax (FPM) revenues (Rs per capita)	85.25 (51.72)	180.4 (113.4)	90.07 (45.93)
Total municipal revenues (Rs per capita)	452.9 (203.9)	483.6 (1077.3)	482.8 (370.4)
Population	153.9 (583.9)	19.34 (44.11)	135.7 (196.4)
Municipal education infra: quantity	9.187 (4.744)	14.95 (7.698)	9.734 (5.381)
Municipal education infra: quality	-0.498 (1.295)	-1.269 (1.163)	-0.644 (1.198)
Municipal health infra (1999)	23.38 (18.07)	39.75 (30.07)	23.42 (18.10)
GDP (Rs per capita)	588.1 (431.2)	360.9 (343.6)	615.4 (661.6)
Share services in GDP (%) 64.4	61 (12.7)	63.9 (14.9)	(15.2)
Urban population (%) (2000)	84.19 (17.40)	60.19 (23.25)	83.93 (16.79)
Gini index (2000)	0.553 (0.0540)	0.557 (0.0572)	0.549 (0.0529)
Life expectancy (2000)	71.07 (3.182)	68.11 (4.778)	70.12 (3.840)
Median education level (2000)	5.624 (1.278)	4.076 (1.209)	5.370 (1.237)
Local radio station	0.720 (0.450)	0.294 (0.456)	0.695 (0.461)
Local judiciary seat	0.802 (0.399)	0.426 (0.495)	0.777 (0.417)
Municipalities	339	4239	3448
<i>Corruption data from Brollo et al. (2013)</i>			
Broad corruption index (2003)	0.625 (0.518)	0.663 (0.474)	0.561 (0.498)
Narrow corruption index (2003)	0.375 (0.518)	0.362 (0.483)	0.237 (0.429)
Municipalities	8	116	112
<i>Corruption data from Litschig and Zamboni (2012)</i>			
All irregularities index (2003)	20.45 (29.57)	41.5 (49.91)	17.6 (25.64)
Diversion irregularities index (2003)	0.358 (0.676)	1.477 (2.083)	0.499 (1.036)
Mismanagement irregularities index (2003)	7.541 (10.376)	17.032 (20.097)	7.379 (10.461)
Municipalities	15	205	174

*Notes:* Mean (standard error). The samples are 1) first column: all municipalities that start a PMAT program between 1998 and 2009 2) second column: all municipalities that never start a PMAT program 3) third column: all municipalities that never start a PMAT program and belong to the common support sample (see text). Observations in the third column are weighted by a function of their propensity score. All variables are measured in 1998 unless specified otherwise, when no 1998 data is available I use the first year for which the variable is available. The quantity of municipal education infrastructure is the number of classrooms per school-age inhabitants, the quality of education infrastructure the index constructed from school characteristics (see text) and municipal health infrastructure is the number of health units per 100,000 inhabitants.

Table 3: Impact of tax revenues

	(1) Whole sample	(2) Whole sample	(3) Common support	(4) Mayor fixed effect	(5) PMAT only
<b>A: First Stage - Impact of the program on tax revenues</b>					
Program	13.547*** (2.405)	11.630*** (2.558)	10.329*** (2.417)	9.953*** (2.501)	
Has applied	-0.695 (2.122)	0.417 (2.167)	0.501 (0.918)	0.499 (1.899)	-0.035 (0.150)
Years in the program					3.201*** (1.303)
Covariates	No	Yes	Yes	Yes	Yes
Observations	57507	57507	46661	57507	4600
Clusters	4578	4578	3724	13146	339
<b>B: Second stage - Impact of tax revenues on municipal education infrastructure</b>					
<i>Dependent variable: Quantity of municipal education infrastructure</i>					
Tax revenues	0.356*** (0.138)	0.470*** (0.181)	0.319* (0.179)	0.454** (0.182)	0.403*** (0.174)
Has applied	-0.084 (0.092)	-0.248 (0.169)	-0.155 (0.142)	0.142 (0.141)	0.067 (0.079)
Covariates	No	Yes	Yes	Yes	Yes
Observations	57507	57507	46661	57507	4600
Clusters	4578	4578	3724	13146	339
<i>Dependent variable: Quality of municipal education infrastructure</i>					
Tax revenues	0.114*** (0.040)	0.117** (0.048)	0.136** (0.055)	0.130** (0.058)	0.141** (0.062)
Has applied	-0.040 (0.053)	-0.042 (0.051)	0.041 (0.050)	0.012 (0.044)	-0.006 (0.051)
Covariates	No	Yes	Yes	Yes	Yes
Observations	57507	57507	46660	57507	4600
Clusters	4578	4578	3724	13146	339

*Notes:* Dependent variables are municipal tax revenues per capita (panel A), the number of classrooms in municipal schools per thousand school-age inhabitants (panel B.1) and the index of quality of municipal schools (panel B.2). In panels B.1 and B.2 the indicator for program participation is used as an instrument for tax revenues per capita in columns 1 to 4, and the number of years in the tax capacity program is the instrument in column 5. Tax revenues are per capita and in units of 10 Rs in the two bottom panels. All specifications include an indicator for having applied to the program and year fixed effects, columns 1-3 and 5 include municipality fixed effects, column 4 municipal administration fixed effects and columns 2-5 include time-varying controls. The sample in column 3 is the common support sample and non-PMAT municipalities are weighted by a function of their estimated propensity score. The sample in column 5 is only PMAT municipalities. Standard errors in parentheses are clustered at the municipality level. Statistical significance at the 10% level is represented by \*, at the 5% level by \*\*, and at the 1% level by \*\*\*.

Table 4: Impact of non-tax revenues

Polynomial specification	(1) 2% sample Linear	(2) 2% sample Linear	(3) 5% sample Linear	(4) 5% sample Linear	(5) Whole sample Third-order
<b>A: First Stage - Impact of the allocation rule on non-tax revenues</b>					
All cutoffs	12.501*** (0.991)	11.928*** (0.761)	13.632*** (0.668)	12.995*** (0.511)	14.382*** (0.710)
Covariates and municipality FE	No	Yes	No	Yes	Yes
Observations	5231	5231	13193	13193	35426
Clusters	1692	1692	2000	2000	2930
<b>B: Second Stage - Impact of non-tax revenues on municipal education infrastructure</b>					
<i>Dependent variable: Quantity of municipal education infrastructure</i>					
All cutoffs	0.195 (0.191)	-0.084 (0.085)	0.022 (0.127)	-0.045 (0.056)	-0.068 (0.071)
Covariates and municipality FE	No	Yes	No	Yes	Yes
Observations	5231	5231	13193	13193	35426
Clusters	1692	1692	2000	2000	2930
<i>Dependent variable: Quality of municipal education infrastructure</i>					
All cutoffs	0.050 (0.061)	0.036 (0.026)	-0.036 (0.038)	-0.011 (0.016)	-0.005 (0.019)
Covariates and municipality FE	No	Yes	No	Yes	Yes
Observations	5231	5231	13193	13193	35426
Clusters	1692	1692	2000	2000	2930

*Notes:* Dependent variables are municipal FPM revenues per capita (panel A), the number of classrooms in municipal schools per thousand school-age inhabitants (panel B.1) and the index of quality of municipal schools (panel B.2). All specifications include year fixed effects and control flexibly for population size, using local linear regressions in columns 1-4 and a spline third-order polynomial in the last column, and exclude municipalities with a population of more than 142,633 or less than 6,792 inhabitants over the period 1998-2011. Covariates are municipality fixed effects, GDP per capita, the share of agriculture and services in GDP, municipal population and political characteristics of the municipality. The sample includes all municipalities within a 2% bandwidth of a population cutoff in the first two columns, a 5% bandwidth in columns 3 and 4 and all municipalities within the bracket mid-points around a cutoff in the last column. Standard errors in parentheses are clustered at the municipality level. Statistical significance at the 10% level is represented by \*, at the 5% level by \*\*, and at the 1% level by \*\*\*.

Table 5: IV results - Impact of a 10 Rs increase in tax or non-tax revenues

	(1) Whole sample	(2) Mayor fixed effect	(3) Close to cutoffs only	(4) Common support
<b>A: Quantity of municipal education infrastructure</b>				
Non-tax revenue	-0.110 (0.084)	0.047 (0.086)	-0.089 (0.076)	-0.089 (0.078)
Tax revenue	0.509** (0.240)	0.543** (0.247)	0.403 (0.262)	0.395* (0.203)
Observations	35426	35426	13193	34747
Clusters	2930	8024	2000	2858
T-test p-value	0.02	0.07	0.11	0.03
<b>B: Quality of municipal education infrastructure</b>				
Non-tax revenue	0.018 (0.021)	-0.006 (0.024)	-0.021 (0.024)	-0.020 (0.022)
Tax revenue	0.124** (0.061)	0.133* (0.071)	0.133 (0.085)	0.114** (0.057)
Observations	35426	35426	13193	34747
Clusters	2930	8024	2000	2858
T-test p-value	0.07	0.06	0.11	0.04
<b>C: Total municipal spending</b>				
Non-tax revenue	10.001*** (0.354)	10.991*** (0.701)	10.108*** (0.411)	10.685*** (0.629)
Tax revenue	10.832*** (0.817)	12.306*** (1.236)	11.213*** (0.885)	12.356*** (1.121)
Observations	35426	35426	13193	34747
Clusters	2930	8024	2000	2858
T-test p-value	0.37	0.34	0.24	0.17

*Notes:* The dependent variables are the number of classrooms in municipal schools per thousand school-age inhabitants (panel A), the index of quality of municipal schools (panel B) and municipal spending per capita (panel C). Transfer and tax revenues are per capita and in units of 10 Rs when used as dependent variables (columns 3 and 4). Program participation and an indicator equal to one if lagged population is above a population cutoff are used as instruments for tax revenues and transfer revenues. All specifications include year and time-varying controls as well as an indicator equal to 1 if the municipality has applied to the program but not started yet, columns 1,3 and 4 include municipality fixed effects and column 2 municipal administration fixed effect. All specifications exclude municipalities not affected by the transfer allocation rule, ie observations with a population of more than 142,633 inhabitants or below 6,792. Columns 1 and 2 use the entire sample, column 3 all municipalities within a 5% bandwidth of the population thresholds and column 4 the common support sample. Column 3 controls for population linearly on both sides of each cutoff, other columns include spline cubic polynomial in population size which allow for different slopes on both sides of each cutoff. In Column 4 non-PMAT municipalities are weighted by a function of their estimated propensity score. Standard errors in parentheses are clustered at the municipality level. Statistical significance at the 10% level is represented by \*, at the 5% level by \*\*, and at the 1% level by \*\*\*.



Table 6: Results on municipalities that join the program only

Outcome variable	Tax revenues	Non-tax revenues	Quantity of educ. infra.	Quality of educ. infra.
<b>A: Impact of tax and non-tax revenues on PMAT municipalities only</b>				
<i>First stage: increases in tax and non-tax revenues</i>				
Years in the program	3.516*** (1.259)			
All cutoffs		10.104*** (1.451)		
<i>Second stage: impact of tax and non-tax revenues</i>				
Non-tax revenues			0.010 (0.500)	-0.039 (0.098)
Tax revenues per capita			0.409*** (0.163)	0.152** (0.072)
Observations	3448	3448	3448	3448
Clusters	257	257	257	257
<b>B: Impact of non-tax revenues on PMAT municipalities after the start of the program</b>				
Non-tax revenues			-0.245 (0.180)	0.001 (0.088)
Observations			1870	1870
Clusters			249	249

*Notes:* The dependent variables are non-tax revenues per capita (column 1), tax revenues per capita (column 2), the number of classrooms in municipal schools per thousand school-age inhabitants (column 3) and the index of quality of municipal schools (column 4). The sample includes all observations for municipalities that take part in the program at some point between 1998 and 2009, are not state capitals and have a population of less than 142,633 and more than 6,792 over the period. In Panel the sample includes all observations for these municipalities, in Panel B only observations after the start of the program. Non-tax and tax revenues are per capita and in units of 10 Rs when used as explanatory variables, and in units of 1 Rs when used as dependent variables, to facilitate comparison with the results above. The number of years in the program and an indicator equal to one if lagged population is above a population cutoff are used as instruments for tax revenues and non-tax revenues, specifications in the bottom panel control for the number of years a municipality has been taking part in the program. All specifications include year and time-varying controls as well as an indicator equal to 1 if the municipality has applied to the program but not started yet, municipality fixed effects and spline cubic polynomial in population size which allow for different slopes on both sides of each cutoff. Standard errors in parentheses are clustered at the municipality level. Statistical significance at the 10% level is represented by \*, at the 5% level by \*\*, and at the 1% level by \*\*\*.