Tax Me, But Spend Wisely
The Political Economy of Taxes, Theory and Evidence from Brazilian Local Governments

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Abstract

This paper shows that local governments are more accountable when a larger fraction of their resources comes from local taxes. I construct a principal-agent model of public finance in which public revenues come from taxes and inter-governmental transfers. An increase in taxes keeping transfers constant changes the equilibrium allocation of public revenues towards more public goods and less political rents because citizens have better information on taxes than on transfers. I then consider a program in Brazil that invests in the modernization of local tax administrations. Using 10 years of panel data and quasi-exogenous variations in the timing of program uptake I find that the program increases tax collection of local governments by 11% after four years. This increase in taxes is used to raise local public good provision but not corruption: the share of resources diverted by local politicians in total public revenues decreases as taxes increase. A discontinuity in the rule allocating federal transfers to local governments is used to compare the impact on spending outcomes of an exogenous increase in transfers to that of a raise in tax revenues thanks to the program. Results show, in line with the model's predictions, that local officials use extra tax revenues more to increase the supply of municipal education and less on private rents than they do with extra transfer revenues.

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"To increase responsiveness to local citizens, subnational governments need a local tax instrument and the freedom to set tax rates." (World Bank (2004), p. 189)

1 Introduction

Many developing countries have implemented decentralization policies over the last 30 years. One rationale for decentralization, first expressed by Tocqueville (1835-1840) and Tiebout (1956) argues that it leads to a better targeting of public good provision to suit local populations. More recently Seabright (1996) shows that decentralization makes it easier for citizens to hold politicians accountable. Yet studies point out the many imperfections in the local provision of services in developing countries, ranging from carelessness in public spending to outright diversion of public resources[1]. A common feature of local governments around the world is that generate very little of the revenue they spend (Bardhan and Mookherjee, 2006). The idea that how these governments are financed and in particular how much taxes they collect may affect the quality of their public spending has recently been flagged out in the policy and development studies literature[2]. Does this mean that local governments are more accountable when they collect more taxes?

In this paper I argue that the extent to which local governments are financed by tax revenues they collect as opposed to intra-governmental transfers affects the quality of their public spending. The more they rely on tax collection, the more local politicians have to respond to their constituents' demands when allocating public spending and the less rents they can extract for their private use. I construct a theoretical framework that predicts that marginal increases in taxes will be more *accountability-inducing* than marginal increases in transfers based on standard political economy mechanisms. Evidence supporting this prediction is found by comparing the marginal impact on provision of public services and corruption of an increase in taxes and in increase in transfers amongst Brazilian local governments.

My theoretical framework consists in a political agency model of public finance in which public revenues come from endogenous local taxes and exogenous transfers. A rent-seeking incumbent politician decides how to allocate the public budget between public good provision and diversion of funds for his private use (corruption). The key assumption is that tax revenues are perfectly observed by all players but transfers are a random variable whose realization is only fully observed by the politician. Information asymmetries lead to a difference in the extent to which citizens can control the allocation of tax and transfer revenues. The model's key prediction is that a policy that increases the efficiency of the

[1] Rose-Ackerman (1999). See Bardhan and Mookherjee (2006) for a review of the evidence on decentralization.

[2] See in particular Bird and Smart (2002), Brautigam *et al.* (2008) and Moore (2007) for a review of this literature.

tax administration and thus increases local tax revenues will lead to a bigger rise in local public good provision, and diversion of public revenues to private rents, than a policy that increases transfer revenues by the same amount. Increasing the government's capacity to tax its citizens therefore makes it more accountable as it leads to an allocation of public revenues towards more expenditures that benefit citizens, at the expense of corruption expenditures which benefit the incumbent politician.

I then test the model's predictions by evaluating the impact of a federal program that modernizes the tax administration of local governments (municipalities) in Brazil. The program offers municipalities subsidized loans to invest in the efficiency of their local tax administration. Selection into the program is purely voluntary. The challenge to identification is thus that those governments' choice to participate may not be orthogonal to unobservable factors that also affect tax collection and/or the allocation of public revenues. The richness of the data and specificities of the program's institutional design however allow me to explore and rule out the most likely alternative explanations for the empirical results.

My empirical strategy relies on a difference-in-differences estimator and a unique dataset of 10 years of panel data on municipal tax and transfer revenues, quantity and quality of municipal education supply, corruption of local politicians, and a large set of local economic, demographic and political characteristics. Data on corruption comes from the randomized audits of Brazilian local governments since 2003, from an index of the number of irregularities observed in local public spending is constructed[3] A key characteristic of the program is that municipalities decide when to *apply* to the program but the date at which they *start* one is determined by constraints faced by the supplier of the program. These create variations in the timing of program uptake that are unrelated to local characteristics. This specificity allows me to disentangle the impact of the program on tax revenues and public spending outcomes from that of (potential) time-varying determinants of selection that are unobserved. I also present propensity-score weighted estimates that are robust to the existence of unbalanced pretreatment characteristics correlated with the dynamics of the outcome variable and restrict the analysis to comparable municipalities.

I first consider how the program affects tax collection and then ask whether the increase in tax revenues benefits (corrupt) local politicians and/or citizens by evaluating whether the program leads to an increase in corruption and/or more provision of municipal education. From this I compute Wald estimates of the impact of an increase in taxes due to the program on how much local politicians decide to allocate to education provision and corruption. I compare these estimates with the effect of an increase in transfers to test the model's prediction that an increase in taxes leads to better public spending outcomes than an increase in transfers. This effect is identified using nonlinearities in the rule governing the

[3]This unique dataset on corruption is also used by Ferraz and Finan (2008), Ferraz and Finan (2011), Brollo *et al.* (2010) and Litschig and Zamboni (2008). I describe it in detail below.

allocation of the main federal transfer as a function of population which lead to several discrete jumps in the amounts of transfers local governments receive, following the research design used by Brollo *et al.* (2010), Litschig (2008a) and Litschig and Morrison (2010).

Results show that the tax modernization program raises local tax revenues by 11% after four years and that this increase persists over time. The cost of the investments in tax administration are on average recovered after only two years in the program. The rise in taxes is used to finance a 8% increase in municipal school infrastructure, an increase in school quality, but there is no increase in corruption. Comparing the impact of this increase in taxes to that of an increase in transfers of the same amount I find that an extra 10 Reais per capita of public revenues increases municipal education quantity and quality more when it comes from local taxes than when it comes from federal transfers. Such an increase in transfer revenues leads to a nearly 10% rise in the occurrence of corrupt practices as a share of revenues whereas higher taxes lead to a decrease in corruption. The impacts of taxes and of transfers are estimated on different sub-populations of Brazilian municipalities. I discuss to what extent these populations are comparable, and present evidence that suggests that the mechanism outlined in the model - that citizens have better information on taxes than on transfers - explains some of the observed differences in how taxes and transfers are spent.

This paper's first contribution is a model based on a fairly standard assumption in the political economy literature (asymmetries of information) which predicts that increases in tax capacity will make governments more accountable. The idea that relying on local taxes affects political officials' incentives dates back to at least Tiebout (1956). The more recent literature on market-preserving fiscal federalism argues that the more politicians depend on locally generated revenue the more they will invest in public goods that increase their local tax base (see Weingast (2009) for a review). Zhuravskaya (2000) provides evidence for this mechanism amongst Russian cities. This paper differs by relying on an explicit political economy mechanism to explain why taxes lead to more public good spending than transfers. The mechanism outlined here will hold even if local governments cannot finance growth-enhancing local public goods[4]. This model is also related to previous political agency models which argue that information asymmetries lead to more rent-taking opportunities by politicians (in particular Besley and Smart (2007)). Those do not however explore the possibility that public revenues are more or less well observed depending on their source and that this will affect elected officials' accountability to their constituents.[5].

[4]The type of public good provision this paper considers - education supply - is unlikely to have the type of short-run growth effect required for the mechanism outlined in Weingast (2009) and Zhuravskaya (2000) to be relevant amongst Brazilian local governments. I provide some evidence that this theory cannot explain the results found in this paper.

[5]See Besley (2006) for a review of political agency models. One exception is Strumpf (1998) who builds on the idea that citizens may have different information on different sources of local public funds to explain the fly-paper effect

The estimation of the tax returns to investments in fiscal administration contributes to the growing literature on state capacity and development. This literature argues that governments' investment in their capacity to tax constitutes an important covariate of economic development (Besley and Persson, 2009, 2010). To the best of my knowledge this paper is the first to consider specific innovations in tax collection and provide an empirical investigation of the returns to such innovations. Widely optimistic prognoses regarding the efficiency of investments in developing countries' tax administrations abound: the President of the African Tax Administration Forum, Oupa Magashula, for example claimed that investing in public resource mobilization can have up to "*a tenfold multiplier effect on states' resources*"(OECD, 2010b). I find an annual 'multiplier' effect of just over one (a one Real investment in tax administration leads to an extra one Real in tax revenue every year), far from tenfold but still very cost-effective.

This paper is also related to the large and growing literature on determinants of corruption and the quality of public expenditures at the local government level[6]. Fisman and Gatti (2002) establish a strong positive relationship in the US between the proportion of a state's expenditures derived from federal transfers and the number of convictions of public employees for abuse of public office[7]. Of particular relevance here are recent papers on the impact on public expenditures or corruption of an increase in public resources amongst Brazilian local governments. This literature tends to suggest that an increase in public revenues leads to wasteful or corrupt government spending in Brazil[8]: Brollo *et al.* (2010) find that higher grants from the federal government leads to more corruption whilst both Caselli and Michaels (2011) and Ferraz and Monteiro (2010) show that windfall from oil royalties lead to no improvement in local public good provision but to an increase in public employment (Ferraz and Monteiro, 2010). The literature on the 'natural resource curse' argues that governments become less accountable when they receive revenues from natural resources[9]. A similar argument has been made regarding how governments spend windfalls in aid revenues (Svensson, 2000).

This paper provides a framework that helps reconcile these findings with the fact that historically and cross-sectionally more government resources are associated with more efficient and accountable governments[10]. The model suggests that increases in public revenues coming from sources not directly observed by citizens, such as the ones considered in the literature described above, lead to worse public

[6]See for example Ferraz and Finan (2009) and Martinez-Bravo *et al.* (2011) on the role of elections, Olken (2005) and Litschig and Zamboni (2011) on bottom-up and top-down monitoring, or Reinikka and Svensson (2005) on the quality of the information available to users of public services.

[7]As the authors point out the causal interpretation of this relationship is limited by potential endogeneity problems. These problems are mitigated in this paper by the use of variations within municipalities over time and the use of corruption indexes from randomized audits.

[8]An exception is Litschig (2008a) who shows that more federal transfers leads to better education outcomes.

[9]see Van der Ploeg (2011) for a review.

[10]See Lindert (2003).

spending outcomes than increases in taxes, which generally constitute the bulk of government revenues. My research design allows for the first empirical comparison of how increases in tax revenues and non-tax revenues affect the accountability of governments. Finally, this paper contributes to the larger literature on the political economy of public good provision and corruption[11]. It focuses on the impact on local government accountability of one institutional characteristic - tax capacity - which has so far not been studied.

The outline of the paper is as follows. Section 2 provides an agency model of public finance that relates how governments are financed to how they allocate their budget between private rents and public good provision. Section 3 presents the institutional and economic context of the tax modernization program in Brazil and explains why some municipalities choose to join the program whilst many do not. Section 4 evaluates the impact of the program on local tax and public spending outcomes and discusses the identifying assumption whilst Section 5 contrasts the impact on education inputs and corruption of higher taxes thanks to the program to that of an increase in transfers. I conclude with Section 6.

2 Model

2.1 Set-Up

Structure

The model follows the political agency framework of Besley and Smart (2007) in which a representative citizen decides whether to re-elect an incumbent politician without observing part of this politician's actions. The budget of the government is representative of that of local governments throughout the world : public resources R come from local taxes T, endogenously determined, and intergovernmental-transfers F which are a exogenous and subject to some random variation. Transfers can take two values : F is equal to $F_H = \bar{F}(1+u)$ in the high state H with probability q and $F_L = \bar{F}(1-u)$ in the low state L, where $u,\ q \in [0,1]$[12].

The incumbent politician faces a budget constraint $T+F = R = G+S$, with G the level of public good maximizes and S the rents he diverts for himself ($S \geq 0$). He maximizes the sum of rents extracted from being in office $S + \sigma Z$, where Z is the value of re-election and σ the probability of re-election. He can choose to divert all public resources and forgoe re-election but institutional constraints limit maximal rent taking to $\bar{S} = \alpha R$ where $\alpha < 1$. Challengers in the election would behave in the same way as the incumbent once elected; the election is a way for the citizen to discipline the incumbent, not to choose the best type of candidate.

[11] See Banerjee *et al.* (2009) and Olken and Pande (2011) for recent reviews of this literature.

[12] One can alternatively think of F as any source of public revenues that is not directly extracted from citizens, such as revenues coming from the government's sale of natural resources or development aid. The predictions of the model are thus also relevant at the level of federal government.

The representative citizen derives utility from the provision of public good net of taxes. Her welfare is $W(G,T) = G - \phi C(T)$ where ϕ indexes the marginal utility cost to the citizen of paying taxes and $C(.)$ is increasing and strictly convex with $C(0) = C'(0) = 0$. I define $h(\cdot) = C'^{-1}(\cdot)$.

Full information equilibrium

The citizen chooses for each state $i = H, L$ the reelection rule $\sigma(G_i, T_i) = \sigma_i$ that will induce the politician to provide the policy menu (G_i, T_i) that maximizes her welfare. The maximum level of public good G_i she can obtain from the government when paying taxes T_i must be so that it leaves the government with enough rents today to make abiding by the re-election rule more attractive than running away with maximum rents and forgoing re-election. This *fiscal restraint* constraint takes the form:

$$T_i + F_i - G_i + \sigma_i Z \geq \alpha(T_i + F_i), \forall i = H, L \tag{1}$$

Re-electing the incumbent leads to an increase in the public good at no cost to the citizen so that in equilibrium she sets $\sigma_i^* = 1$ in each state i as long as the government provides the menu (G_i^*, T_i^*) such that:

$$G_i^* = (T_i^* + F_i)(1 - \alpha) + Z, \tag{2}$$

with T_i^* set such that the marginal value of the public good is equal to the marginal cost of taxation : $T_i^* = h(\frac{1-\alpha}{\phi})$. When the citizen fully observes all public revenues the way in which in the local government is financed does not matter. The marginal effect of an increase in taxes or transfers is to increase the public good by $(1 - \alpha)$ and rents by α. Note that even with perfect information the fact that the incumbent can threaten to run away with all public revenues means he diverts rents in equilibrium.

2.2 Equilibrium with asymmetric information

Assume now that the citizen does not perfectly observe transfer revenues: the realized value of F is known only to the incumbent[13]. The citizen perfectly observes the taxes she pays. Asymmetries of information increase the incumbent's capacity to extract rents from the public budget as he can now pretend to be in the low state when he receives high transfer revenues to capture the difference in revenues between the high and the low states to himself.

[13]This assumption is grounded in empirical evidence: Reinikka and Svensson (2005) for example show that local public funds coming from transfers are badly observed by citizens in Uganda and that improving information leads to less capture by local officials. What's new to this paper is the idea that taxes are better observed (here they are perfectly observed) because they come directly from the citizens' resources.

Note first that to deter the incumbent in state H from implementing the L state menu the menus offered by the citizen must now also respect the incentive constraint :

$$S_H + \sigma_H Z = T_H + \bar{F}(1+u) - G_H + \sigma_H Z \geq T_L + \bar{F}(1+u) - G_L + \sigma_L Z \quad (3)$$

And similarly for the incumbent in state L:

$$T_L + \bar{F}(1-u) - G_L + \sigma_L Z \geq T_H + \bar{F}(1-u) - G_H + \sigma_H Z \quad (4)$$

Putting together (3) and(4) there is only one situation in which both constraints are satisfied simultaneously : $G_H = G_L + T_H - T_L + Z(\sigma_H - \sigma_L)$.

Intuitively it is still optimal for the citizen to ask the incumbent in the low state to provide the maximal amount of the public good given the amount of taxes paid: state L's *fiscal restraint* constraint - equation (1)- is binding. This implies the following equilibrium levels of public good provision:

$$G_L^* = (T_L^* + \bar{F}(1-u))(1-\alpha) + \sigma_L^*\ Z \quad (5)$$

and

$$G_H^* = (T_H^* + \bar{F}((1-u))(1-\alpha) + \sigma_H^* Z + \alpha(T_H - T_L) \quad (6)$$

Re-election leads to an increase in the public good at no cost to the citizen whatever the state, so $\sigma_H^* = \sigma_L^* = 1$. Maximizing $W(G_H, G_L, T_H, T_L; q)$ subject to (5) and (6) determines the level of taxation in both states :

$$T_H^* = h(1/\phi) \quad (7)$$

and

$$T_L^* = \max\{0; h((1-q-\alpha)/\phi(1-q))\} \quad (8)$$

It is optimal for the citizen to pay less taxes in the low state as any increase in the level of taxes offered in the low state menu makes mimicking the low state equilibrium more attractive to the incumbent in the high state. This comes at the cost of less public good in the low state. The less likely the low state (the higher q) the more the citizen is willing to incur this cost, and the lower T_L^*[14]. The asymmetry of information leads to an equilibrium with lower public good provision (on average) than in the full information equilibrium due to the increase in rent-seeking obtained by the incumbent in state H.

The structure of public finance now affects the way in which the incumbent allocates the budget. Using equations (5) and (6) we can write the average level of the public good as:

$$E(G^*) = (1-\alpha)(E(T^*) + \bar{F})) - \bar{F}u(1-\alpha) + Z + (1-q)\alpha(T_H^* - T_L^*) \quad (9)$$

[14]For high values of q the theoretical appendix shows that it will be optimal for the citizen to pay no taxes in the L state.

A marginal increase in taxes increases public good provision by $(1-\alpha)$ (assuming for simplicity that the increase does not affect the spread $T_H^* - T_L^*$) compared to $(1-\alpha)(1-u)$ for a marginal increase in average transfers, keeping everything else constant[15]. The term $u(1-\alpha)\bar{F}$ corresponds to the informational rents the incumbent can appropriate in state H by 'hiding away' the extra transfer revenues. The last term in equation (9) simply says that the more the citizen can provide the incumbent in the high state with high powered incentives relative to the low state (the bigger the difference between taxes in both states) the lower the informational rents. Finally, note that the higher the asymmetry of information (higher u) the bigger the difference between taxes and transfers. At the limit when $u = 1$ any increase in transfers is spent fully on rents, and when $u = 0$ the equilibrium is a full information one.

The equilibrium share of rents in public revenues s^* is increasing in the share of transfers in the budget proxied by $\bar{f}^* = \bar{F}/E(R)$:

$$E(s^*) = \alpha + E(\bar{f}^*)2u(1-\alpha)(1-q) - Z/E(R) - (1-q)\alpha(T_H^* - T_L^*)/E(R) \quad (10)$$

This is the *accountability effect of taxes on the allocation of public spending* =: as the share of taxes in revenue increases, so does the share of revenues that is spent towards public good provisions. Intuitively increasing the share of taxes increases the amount of information the citizen has on her government's budget and thus limits the extent to which a rent-seeking politician can capture public funds by 'hiding' them. This leads to an allocation of the budget that is more favorable to the citizen.

2.3 Impact of a tax capacity program

Consider now the impact of a program that makes the tax administration more efficient. This takes the form of a smaller difference between the cost of taxation borne by taxpayers $\phi C(T)$ and how much taxes go in the government budget T : the program decreases ϕ[16]. This makes the citizen more willing to pay taxes in order to get more public good. Using equations (7) and (8) the impact of a program that lowers the efficiency cost by $d\phi < 0$ on taxes is given by :

$$\frac{\partial E(T^*)}{\partial \phi} d\phi > 0 \quad (11)$$

[15] I assume throughout that any increase in transfers *ceteris paribus* comes from an increase in $\bar{F}$ and not a change in the probability q of the high state. This is consistent with the type of increase in transfers considered in the empirical strategy which are a consequence of a local government moving to a higher transfer bracket, not a random shock to transfers.

[16] One could also model the efficiency of the tax administration by introducing a cost to the government of levying taxes. The reform would then lower that cost, leaving the results of this model unaffected.

The program will also lead to an increase in public good provision proportional to the increase in taxes :

$$\frac{\partial E(G^*)}{\partial \phi} d\phi = (1-\alpha)\frac{\partial E(T^*)}{\partial \phi} d\phi + (1-q)\alpha \frac{\partial (T_H^* - T_L^*)}{\partial \phi} d\phi > 0. \qquad (12)$$

Because it decreases the share of transfers in total revenues f^* the reform also lowers the share of rents s^* (equation (10)). This leads to a first testable proposition regarding the impact of a tax administration reform:

Proposition 1 *A tax capacity program leads to an increase in taxes, an increase in public good provision and a decrease in the share of rents in total public revenues.*

Two other propositions follow from comparing the impact of a marginal rise in taxes thanks to the program with that of an exogenous marginal increase in the average value of transfer revenues($E(F)$) of the same amount:

Proposition 2 *The rise in taxes due to the reform leads to more increase in public good provision than a rise in transfer revenues of the same amount* $\frac{\partial G^*}{\partial E(T^*)} > \frac{\partial G^*}{\partial E(F)}$.

Proposition 3 *The rise in taxes due to the reform leads to a fall in the share of rents in public revenues. An increase in transfer revenues increases the share of rents in public revenues :* $\frac{\partial s^*}{\partial E(T^*)} dE(T) < 0 < \frac{\partial s^*}{\partial E(F)} dE(T)$.

A final proposition comes from observing that an increase in the information the citizen has on the budget lowers the equilibrium information rents and thus mitigates the relative accountability effect of taxes and the difference between taxes and transfers:

Proposition 4 *The higher the information the citizen has on the level of transfers (the lower the u) the more similar the impact of an increase in taxes thanks to the program and the impact of an equivalent increase in transfers :* $\frac{\partial G^*}{\partial E(T^*)} - \frac{\partial G^*}{\partial E(F)}$ *is increasing in u.*

Formal proofs of these propositions and all the results in this section are in the theoretical appendix.

The first part of the empirical section of this paper offers a test of proposition 1 by evaluating the impact of a tax modernization program on public good provision and the share of rents (corruption) diverted by politicians in total public revenues. The second part tests propositions 2 and 3 by comparing the impact on public goods and corruption of an increase in taxes thanks to the tax modernization program to the impact of an exogenous increase in transfers. It then provides some evidence regarding proposition 4 by considering how these impacts vary when citizens have better access to information about the budget thanks to the presence of local media.

3 Context : Brazilian local governments and the PMAT tax capacity program

3.1 The public finances of Brazilian local governments and the PMAT program

The 1988 Brazilian constitution devolves substantial expenditure responsibility and tax autonomy to the country's more than 5000 local governments[17]. The rates and bases of three main local taxes (a service tax, a property tax and a property sales tax) as well as the method of tax assessment and collection are decided by local elected officials. This leads to a great diversity in tax revenues: in 2008 local governments collected anything from 2 to 2000 Reais per capita in taxes, with an average of 45 Rs per capita (median income in Brazil was round 6300 Rs over the period)[18] . In total local taxes represent nearly 2% of GDP.

Municipalities' *de facto* tax collection is small. They collect less than 13% of their total revenue themselves. The spiralling of local debts in the early 1990s has directed much policy attention in Brazil towards the low tax efforts of local governments with commentators pointing out the poor quality of local tax administrations (**?**). The few studies of Brazilian tax administrations available paint a dire picture of unskilled and overworked staff with outdated tax registers, no institutional memory and a lack of methods to accurately assess tax liabilities[19]. High costs of understanding and paying taxes likely push many citizens into non-compliance and local government and officials have admitted to tolerating a situation of ongoing tax amnesty where tax arrears are rarely recovered (Afonso and Araujo (2006), BNDES (2002)).

The *Programa de Modernizacao de Administracao Tributaria* (PMAT) was launched in 1998 by the Brazilian Development Bank (BNDES) to increase municipalities' capacity to tax their citizens. It provides local governments with subsidized loans to invest in modernizing their tax administration and is available to all municipalities in Brazil. To apply local administrations must prepare a detailed tax modernization project which is then assessed by the BNDES to check it qualifies with the program's requirement - in practice the BNDES accepts all projects. 331 municipalities started a program between 1998 and 2008, covering 40% of the Brazilian population.

The program's loan can be used to fund only investment expenses related to the tax administration - other budget items were explicitly not eligible. The BNDES staff checks the receipts for all expenditures made in relation to the program

[17]Bardhan and Mookherjee (2006) classify Brazil as one of the few developing countries in which local governments have been given substantial tax autonomy.

[18]All statistics in this section are computed using the FINBRA database on local governments' public finances and program data from the BNDES and are in 2000 Reais. One Brazilian Real is equal to roughly 0.56 dollars.

[19]An extensive study of the property tax collection in Brazil's largest metropolitan areas estimates that less than 60% of urban property is registered on any tax administration's files (de Carvalho Jr. (2006)).

but otherwise exerts no control on the public finance processes of participating municipalities. Each local government is left free to choose the type of actions to take to modernize its tax administration but the same firms or civil servants were involved in the development of several PMAT projects[20]. Overall the program consisted mostly in 1)the creation or updating of tax registers 2)decreasing the costs of paying taxes through the multiplication of tax offices, means and frequency of payments 3) facilitating controls of tax payers through the recovery of tax arrears or the development of build-in cross-checking mechanisms. Virtually all participants launched their PMAT program with a change in their tax registers. Table 1 shows that in 2003 municipalities that had already started a PMAT program were much more likely to have updated their property tax register since 1998 than both the average municipality and municipalities that joined PMAT since.

Table 1: Share of municipalities who updated their property tax register between 1998 and 2003 (number of municipalities)

	% updated 1998-2003	Number municipalities
Control	72%	4723
Started after 2003	70%	122
Started before 2003	83%	146

Source : Perfil dos Municipios Brasileiros, 1998, 2004.

The timing of application to and entry in the PMAT program is of particular interest. The amount of time between the date at which a municipality applies to a program and its start lies between one and 4 years. Table 5 shows that the time between applying and starting a program varied over the period.This is due to the changing situation of the suppliers of the program. The BNDES processed all applications itself for the first 3 years of the program's existence, this took it more than two years and a half on average. It contracted the public bank *Banco do Brasil* in 2002 to take in charge most of the application process[21]. *Banco do Brasil*'s involvement initially accelerated the application process until the bank decided to cut down the resources allocated to PMAT in 2006. It administered most of the projects from 2002 to 2005, and substantially shortened the waiting period. In 2007 the BNDES signed a similar agreement with another public bank, the *Caixa General* and we see another decrease in waiting time for the 2007 cohort. The BNDES' own devotion of resources to the program varied over the years : the federal government's initial push for the policy was short-lived and in 2001-2002

[20]For more on the program and the context of its creation see Santos *et al.* (2008), BNDES (2002) and Afonso and Serra (1999).

[21]The BNDES is based in Rio de Janeiro but the *Banco do Brasil* has branches around the country, allowing for more geographic outreach. The contract stipulates that *Banco do Brasil* help municipalities design a project that respects legal and financial rules and then transmit the application to the BNDES which is the only institution that can decide to start a program.

only one BNDES official was working on PMAT, the idea being that *Banco do Brasil* would take charge of most of the administrative work. The swearing into office of a new President in 2003 put the project back up high on the BNDES agenda and today the staff team has stabilized to around 12 individuals.

Table 2: Average Time Between Program Application and Start

Application Year	Years to Program Start	Nb Municipalities
1997	2.6	11
1998	2.7	10
1999	2.5	18
2000	2.3	13
2001	1.5	90
2002	1.1	29
2003	1	10
2004	0.7	33
2005	1.4	12
2006	1.8	11
2007	0.7	11
All	1.2	330

Whilst the possibility that the most eager municipalities pressure the BNDES to start the program soon after applying cannot be ruled out, in the overwhelming majority of cases (95%) the order in which municipalities apply to the program corresponds to the order in which they start a program. This particularity of the program's timing suggests that whilst municipalities choose when to apply the precise date at which they start a program is out of their control. It also provides, for each municipalities, two dates of interest for identifying the program's impact. I return to this below.

Local revenues which are not locally levied come from transfers from the state or federal governments. I focus on the transfer *Fundo de Participacao dos Municipios* (FPM) which is constitutionally mandated and the largest single source of local revenues (40%). This transfer has attracted the attention of researchers in the past because of nonlinearities in the rule allocating the distribution of *FPM* resources that provide exogenous variations which can be used to identify the impact of an increase in transfer revenues on outcomes of interest. Federal law defines 18 population brackets inside which the amount of *FPM* transfers a municipality receives is fixed, at each population threshold between brackets the amount received jumps by 20% on average. This *FPM* allocation rule has been used by two recent studies - Brollo *et al.* (2010) and Litschig (2008a). Using it is thus not a novelty of this paper, the interested reader is referred to these studies and the appendix for more details.

What is novel to this study is the comparison of how local tax revenues and transfers are spent. In this respect the key advantage of considering *FPM* transfers

is the fact that, unlike all other transfers received by municipalities, their use is virtually unrestricted[22].Local politicians therefore have the same discretion in deciding how to spend tax and *FPM* revenues, any observed difference in how marginal increases in these two revenues are spent cannot be due to any formal spending rule.

Data on participation to the PMAT program, date of application, program start, and amount borrowed through the program have been collected by the author at the BNDES. I use public finance data for the years 1999-2008 from the FINBRA dataset of the *Tesouro Nacional* to get the detailed revenue sources of local governments, in particular tax and *FPM* transfer revenues. All revenue variables used in the analysis are per capita[23]

3.2 Expenditure responsibilities of Brazilian local governments

I focus on two potential uses of local public budgets: providing education in municipal schools and diversion of public revenues by the administration. This is justified by data availability, the large share of both in local public resources, and their salience in Brazilian political debates.

The Brazilian constitution stipulates that states and municipal governments share the responsibility for the provision of primary and secondary education. In practice state governments manage secondary schools and municipal governments are mostly in charge of primary schools (*ensino fundamental*)[24] Education is the largest budget item of local governments, representing nearly 30% of expenditures. I use panel data on municipal education inputs from the annual census (*Censos Escolar*) of all Brazilian schools to measure the quality and quantity of municipal education infrastructure. The number of classrooms in use in municipal schools per thousand inhabitants is a good indicator of how much education inputs local governments are providing to use in this context. First, classrooms are a component of school infrastructure which municipalities can easily adjust if they choose to spend more on education by furbishing existing unused rooms or renting extra office space. Second, local governments receive substantial federal transfers directed towards education expenditure but those generally come with rules specifying that they must be spent on staff, school lunches of school transport and not on physical teaching infrastructure[25]. Classrooms are therefore the education

[22]Brazilian law requires that 15% of *FPM* revenues be spent on education and health. It is highly unlikely that this rule ever binds however, given the importance of education and health as a share of total public budgets and the fact that there is no federal guidelines regarding what expenditure items can be labeled as health or education.

[23]Per capita variables are computed using annual population estimates provided by the Brazilian statistical institute *IBGE*.

[24]By 2005, approximately 85% of all grade 1 to 4 schools were run by local governments (the remainder being private or state primary schools) who are responsible for providing school infrastructure, school lunches and transportation, and hiring, training, and paying teachers (see Ferraz *et al.* (2009)).

[25]For example 60% of the largest of those education transfers, FUNDEB, must fund teacher's salaries. All my results are unaffected when I control for the amount of education-specific transfers

input most likely to be under-funded.

Several variables are available to proxy for the quality of municipal education infrastructure: the number of schools with computers, with internet, with a sports facility, and with tv/video equipment. I use principal components analysis (PCA) to combine these four measures into an index of infrastructure quality. The first principal component explains 80% of the variation in the data; this suggests that using PCA reduces the dimensionality of the data with little loss of information.

The second public expenditure outcome this paper considers is corruption. There is considerable information on how local governments divert public resources away from public uses in Brazil thanks to an anti-corruption program launched by the federal government: since 2003 over 1800 local governments have been randomly chosen by lottery to be audited by staff of the independent audit agency *Corregedoria Geral da Uniao* (CGU). These staff audit the use of discretionary federal transfers by local governments over the last two years by collecting administrative documents and interviewing citizens and administrative staff[26]. They check for example whether spending can be accounted for by receipts, whether program rules are met, and whether procurement of public works is done competitively. The results of those audits are publicly available records. Ferraz and Finan (2011) estimate using this data that approximately 550 million US dollars per year were diverted in the period 2001-2003, or 8% of audited transfer revenues[27].

The corruption data I use comes from the coding of the CGU audits for the years 2003-2006 provided by Litschig and Zamboni (2008). It is available for a small sample of 971 municipalities, 54 of which join the program. Following the existing literature I construct a corruption index from this data by scaling the number of irregularities by the number of potential 'offenders'- civil servants in the local government administration[28] - and the total amount of government revenue audited. This provides a proxy for the share of diverted revenues in total government revenues, the theoretical outcome considered in the model above.

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received by the municipality.

[26]The utilization of the two types of local revenues considered in this paper- taxes and *FPM* transfers - is not directly audited. Most discretionary federal transfers require that municipalities contribute some of their 'own revenues' (defined as *FPM* transfers or taxes) on the programs they fund so we can think of the audits as reflecting the overall quality of government spending.

[27]For more on the anti-corruption program and analysis using data from the audits see Ferraz and Finan (2008), Ferraz and Finan (2011), Ferraz *et al.* (2009) Ferraz and Finan (2009),Litschig and Zamboni (2008) and Brollo *et al.* (2010).

[28]This is obtained from the dataset *Perfil dos Municipios Brasileiros* 1998, published by the IBGE.

3.3 Why did municipalities join the program?

331 municipalities choose to join the PMAT program between 1998 and 2008[29]. Interviews conducted with BNDES staff and local officials suggest the program is not of interest to a large share of Brazilian local governments whose small economic and population size make for a weak tax base and very low potential returns to investments in tax administrations. Participants often say that they joined the program because they were dissatisfied with their current level of tax collection compared to what they thought was their tax potential. Low take up is also partially explained by the fact that the BNDES did very little promotion of its program : most participants said they heard about the program because they knew someone who worked at BNDES, or because one of the municipalities in their neighborhood had already joined.

Given the large number of municipalities that did not join the program I choose to exclude from my analysis those which fieldwork and inspection of summary statistics suggest have no interest in the program and constitute a very poor counterfactual for the evolution of outcomes in treated municipalities. Those municipalities are those whose population, tax collection or GDP are below the minimum value of those variables amongst the sample of treated municipalities. More specifically I take out municipalities with a population of less than 35,000 (16% of municipalities which did not join the program), income per capita below 750 Rs (8%) and tax per capita in 1998 below 3.6 Rs (10%). Over 3,000 control municipalities remain to be included in the analysis.

To better understand why some local governments chose to join the program I estimate a discrete time hazard model of the probability that a given municipality at a given period of time applies to the program as a function of both pre-treatment characteristics of municipalities and time-varying covariates[30]. I consider the role played by pre-treatment values of GDP per capita, population (both estimated annually by the Brazilian statistical institute *IBGE*), and tax revenues and consider key demographic characteristics (median education, inequality and share of urban population, all from the 2000 Census) which could affect local political economy outcomes. The possibility that municipalities hear about the program from their participating neighbors is considered by including the average distance between a municipality and its 5 closest neighbors who have already joined a PMAT program and allowing for time dependence. Mayors with specific political ideologies and political ties may be more likely to join the program, so I include political party affiliation, alignment with the state governor's party and a measure of political competition to proxy for the type of political context mayors

[29]Program candidates were never directly rejected by the BNDES which only requires that funds are spend on modernizing the tax administration. Lack of proof of compliance to some federal regulations (for example the existence of overdue debt payments to a federal agency) did make some local governments ineligible.

[30]See Jenkins (1995) for a description of the method and Galiani *et al.* (2005) for a similar application to privatization of local water provision in Argentina.

face[31]. I test the hypothesis that political or economic shocks influenced program uptake by including lagged changes in GDP per capita, tax revenues and whether a new mayor was elected in the previous election.

Results confirm the intuition gathered from field interviews: municipalities that join the program are rich and populated compared to the average Brazilian municipality but once these variables are controlled for they collect less taxes in 1998. They are also more educated, less agricultural and more politically competitive - all these characteristics are highly serially correlated so the identifying variation used for these estimates is mostly cross-sectional. Political characteristics of the mayor do not play a role (a full set of 26 dummies for political parties do not come out as jointly or individually significant) and neither does alignment with the governor's party. This provides some reassurance that the program's loans were not directed towards politically favored mayors. Municipalities which have neighbors who have already joined the program are more likely to apply, possibly because they hear about the program from them, and the coefficients for time dependance suggests information about the program spread gradually over time.

In the second column I test whether past shocks determined program uptake, and find no evidence of an 'Ashenfelter dip' in tax revenues or that selection in the program is driven by specific economic, demographic or political shocks. Results in the third column suggest that treated municipalities followed similar trends to the control ones in the 1996-1999 period. The fact that no observable shocks determine selection in the program motivates the use of the difference in differences methodology described in the next section.

[31] All political variables are from the *Tribunal Superior Eleitoral*.

Table 3: Determinants of Program Uptake

	(1)	(2)	(3)
Income	0.1252***	0.1203***	0.1435***
	(0.0406)	(0.0454)	(0.0465)
Population	0.2141	0.2852	0.2164
	(0.1973)	(0.2431)	(0.2151)
Taxes	-0.4123*	-0.5924*	-0.2544
	(0.2466)	(0.3275)	(0.2530)
Agr\ GDP	-0.0873***	-0.1087***	-0.1104***
	(0.0211)	(0.0235)	(0.0250)
Serv\ GDP	-0.0005	0.0048	-0.0090
	(0.0146)	(0.0168)	(0.0177)
Education	0.9979**	1.1272**	0.7256
	(0.4659)	(0.5188)	(0.5423)
Urban pop.	0.0058***	0.0063***	0.0063***
	(0.0017)	(0.0019)	(0.0020)
Inequality	0.0028	0.0052	0.0038
	(0.0044)	(0.0051)	(0.0050)
Distance to closest PMAT	-0.0032**	-0.0032*	-0.0035**
	(0.0015)	(0.0019)	(0.0017)
Time	0.0020**	0.0066***	0.0023**
	(0.0010)	(0.0011)	(0.0011)
$Time^2$	-0.0003***	-0.0006***	-0.0003***
	(0.0001)	(0.0001)	(0.0001)
Governor's party (d)	-0.0003	-0.0005	-0.0004
	(0.0006)	(0.0006)	(0.0006)
Pol. competition	0.0048**	0.0050**	0.0052**
	(0.0020)	(0.0023)	(0.0023)
Growth in GDP		0.0303	
		(0.0873)	
Growth in population		-5.7851	
		(5.7434)	
Growth in taxes		-0.0000	
		(0.0000)	
Change in mayor (d)	-0.0012	-0.0008	
	(0.0008)	(0.0011)	
Growth in GDP 96-99			-0.7615
			(0.5008)
Growth in population 96-99			-0.3873
			(2.3233)
Growth in taxes 96-99			-0.4153
			(0.2858)
Observations	27845	23721	25040
Municipalities	3370	3349	3043

Hazard model of the probability of applying to the program: observations corresponding to municipalities which have applied to the program at least a year ago are dropped from the sample. The dependent variable is a dummy equal to 0 for municipalities which have not applied to the program yet and 1 the year in which they apply. Cluster-robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Municipalities which joined in the first two years and observations for 1999 are excluded because the variable 'distance to the 5 closest municipalities which have already joined a PMAT program' is not available for those. Results are very similar when that variable is taken out of the specification and the sample is not restricted.

4 Impact of the tax modernization program on local tax revenues and spending outcomes

4.1 Empirical Strategy

I evaluate the impact of the tax modernization program on 1) local tax collection and 2) local expenditures outcomes that can potentially be financed from an increase in taxes, education inputs and corruption. In principle one would like to randomly assign treatment (program participation) to some municipalities and compare the average outcomes in the treated and control groups. Tax policies are hardly ever the subject of randomized trials[32] so I turn to non-experimental methods that create a credible counterfactual from the control municipalities under a reasonable set of assumptions.

The biggest identification concern is that treated and control municipalities could be different along dimensions which correlate with outcomes. For example richer municipalities join the program more often and they also collect more taxes and provide more public goods. Many of the unobservable characteristics that may confound identification are however likely to be fixed over time; I use panel data and estimate a difference in differences model to control for such time-invariant unobserved heterogeneity.

Formally, I estimate the model :

$$Y_{i,t} = \beta P_{i,t} + \delta X_{i,t} + \gamma_t + \mu_i + \epsilon_{i,t} \tag{13}$$

where $Y_{i,t}$ is either tax collected per capita in municipality i in year t or a measure of education inputs, $P_{i,t}$ is a dummy equal to 1 if municipality i is taking part in the program at year t and γ_t and μ_i a set of year and municipality fixed effect. I control for the key determinants of both tax collection and public expenditure outcomes, namely local population, GDP, political characteristics of the mayor (party, term limit) and the competitiveness of the last local election.

Panel data is not available for the corruption index so I estimate :

$$C_{i,t} = \beta P_{it} + \delta X_{i,t} + \delta_2 S_i + \delta_3 Z_i + \gamma_t + \epsilon_{i,t} \tag{14}$$

where $C_{i,t}$ is the corruption index, S_i is equal to 1 if municipality i joins the program between 1998 and 2008 and Z_i is a set of time invariant controls that includes state fixed effects[33]. All specifications allow for arbitrary covariance structure within municipalities by computing standard errors clustered at the municipality

[32] An exception is Pomeranz (2010) which studies randomized enforcement of the VAT in Chile.

[33] I control for characteristics which are likely to affect corruption levels and/or program uptake: median education level, inequality and life expectancy from the 2000 census, whether the municipality is a state capital, whether tourism is a major industry, existence of a local radio stations and local judiciary presence.

level[34].

The key identifying assumption required for the interpretation of β in (13) and (14) as the average effect of the PMAT program is that the evolution of outcomes Y and C in treated municipalities would have been the same in the absence of the program as the evolution in control municipalities once the impact of time-varying covariates is controlled for (common trend assumption conditional on X). One can use variations in outcomes in the pre-treatment period to get a sense of whether this assumption is likely to hold. If treated and control municipalities are not following similar trends before the program starts it is unlikely that they would have in the absence of the program.

Table 3 already suggested that treated municipalities had been following trends similar to control ones before 1999, and that they did not experience shocks before applying to the program. A more rigorous test that the pre-intervention time trends are the same is obtained by running a modified version of equation (13) using only observations for the pre-treatment period (1999-2008 for control municipalities, and years before joining for treated ones). Excluding the program dummy and including separate year fixed effects for (future) treated and controls I cannot statistically reject the hypothesis that pre-treatment year effects are the same for both groups[35]. This implies that tax revenues and public expenditure outcomes (education inputs and corruption) were following the same time trend in the pre-treatment period and is reassuring for the internal validity of my identification strategy. I present other tests which would detect violations of the identifying assumption in the robustness section below.

I also estimate a time-varying impact of the program :

$$Y_{i,t} = \sum_{j=-9}^{9} \beta_j P_{jit} + \delta X_{i,t} + \gamma_t + \mu_i + \epsilon_{i,t} \tag{15}$$

where P_{jit} equals 1 if municipality i is in the jth year of the program in year t when $j \geq 0$ or will start the program in j years if $j < 0$. The β_j estimates are of interest for two reasons. First, they identify the dynamic impact of the program and estimate to what extent effects are sustainable over time. Second, estimates for $j < 0$ allow me for a test of an impact of the program 'before it happens'[36]. I

[34]Error correlation in the cross-section dimension of the panel could also be a concern if local governments adjust their tax policies to the actions taken by neighboring governments. Clustering at the state-year level allows for correlation amongst municipalities affected by similar shocks. Standard errors computed using this specification are slightly smaller suggesting spatial correlation is less of a concern than correlation over time within municipalities. Results using state-year clusters are available from the author upon request.

[35]Results available from the author upon request.

[36]Because of variation in the year in which the program started the fact that pre-treated year fixed effects are the same for treated and control groups could be overlooking pre-treatment trends that occur at different times for different cohorts.

run a test that the β_j are equal to zero for $j < 0$ to complement the test on year fixed effects explained above.

A final concern is that pretreatment characteristics that are thought to be associated with the dynamics of the outcome variable are unbalanced between treated and control municipalities. Convergence in tax revenues over time may, for example, lead to different dynamics between treated municipalities and controls. This could be addressed by interacting pre-treatment covariates with a time trend, but restricting their effect to be linear may not be suitable if the treatment effect is heterogenous (Meyer (1995)). In this case simple difference-in-differences estimates may suffer from two additional sources of bias (Heckman *et al.* (1998)). The first occurs when there are no comparable control municipalities for some of the municipalities that join the program. The second source of bias arises from different distributions of observable covariates in the control and treated groups. Treated municipalities are different from control ones along several observable dimensions so both these types of bias are here a concern.

I therefore complement my empirical analysis by estimating a propensity score-weighted version of equation (13) following Hirano and Imbens (2001) (see also Hirano *et al.* (2003)). Propensity score-weighted regression methods eliminate both sources of bias by 1) restricting the sample to observations of common support in the distribution of covariates, and 2) obtaining balance of covariates by re-weighting the control group observations. In practice this is done by estimating a model of the probability that a municipality joins the program as a function of the set of covariates W used in Table 3, obtaining the predicted probability $\hat{P}(W)$ and then estimating (13) 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. More detail on the model used to estimate the probability, the distribution of the propensity score and the selection of the common support sample are in the Appendix[37]

4.2 Summary statistics and graphical evidence

Table 4 presents summary statistics of key characteristics of treated and control municipalities. Treated municipalities levy 95 Reais per capita in 1998, nearly three times more than control municipalities. They are also much bigger (180,000 inhabitants on average) and as a consequence receive less *FPM* transfers per capita. Average municipal revenues per capita are however very similar across the two groups. Municipalities that eventually join the program have in 1998 roughly the same amount of school infrastructure but better school quality than control municipalities, and the randomized audits reveal a lot less irregularities

[37]Wooldridge (2007) notes that implementing this method whilst ignoring the impact of the first-stage estimation of the selection probabilities on standard errors obtains conservative inference. All results below therefore present standard errors non-adjusted for first stage estimation. The use of a bootstrap method yields very similar standard errors.

for them on average over the period. Other municipality characteristics are as expected given the results for the selection model presented in Table 3. The last column of the table shows that weighting control observations by a function of their propensity score obtains a reasonable balance of pre-treatment characteristics across the treated and control groups.

The evolution of tax revenues in control and treated municipalities from 1998 to 2008 is presented in Figure 1 in which tax revenues in treated municipalities are scaled so that the year in which the program starts is assigned to 2002. We see a clear increase in tax collection in the treated municipalities once the program has started, however the most striking element in Figure 1 is probably the great difference in tax collection between the treated group and the group composed of all control municipalities. This difference in levels makes it difficult to assess the validity of the common trend assumption. The evolution of the common support, or 'small' control group, constructed as explained above and in which the observations are weighted according to their propensity to join the program, appears to provide a more credible counterfactual. Tax collection levels are very similar for this group and the treated municipalities in 1998 and both groups follow similar trends until 2001. Comparing their evolution post 2002 suggests the program increased tax collection by 10-15% after 3 years. They diverge increasingly over time but part of this divergence may be due to a sample composition effect, as only municipalities which started the program early are used to compute average tax collection towards the end of the period. Regression analysis gets rid of this composition effect, and controls for time-varying covariates that differ across treated and control municipalities.

4.3 Results : Impact of the program

Table 5 reports results from the estimation of equations (13) and (15) using tax revenues per capita as the dependent variable. The first column presents results for a model using the whole sample and including no covariates except for municipality fixed effects and year dummies. I find that the program leads to a 10.5 Rs increase in tax revenues, which amounts to a 11% increase in tax collection compared to the baseline 1998 level in treated municipalities. Controlling for time-varying covariates in column 2 and restricting the sample to the common support in column 3 decreases the estimate slightly. The estimated impact of the program is an extra 7.7 Rs of taxes per capita (a 8% increase) using the preferred propensity score weighted regression method. This suggests that the difference in pre-treatment characteristics between treated and control municipalities leads to some over-estimation of the impact of the program. Results in all the following tables are obtained using the propensity score weighted method, estimates from alternative specifications are presented in the appendix.

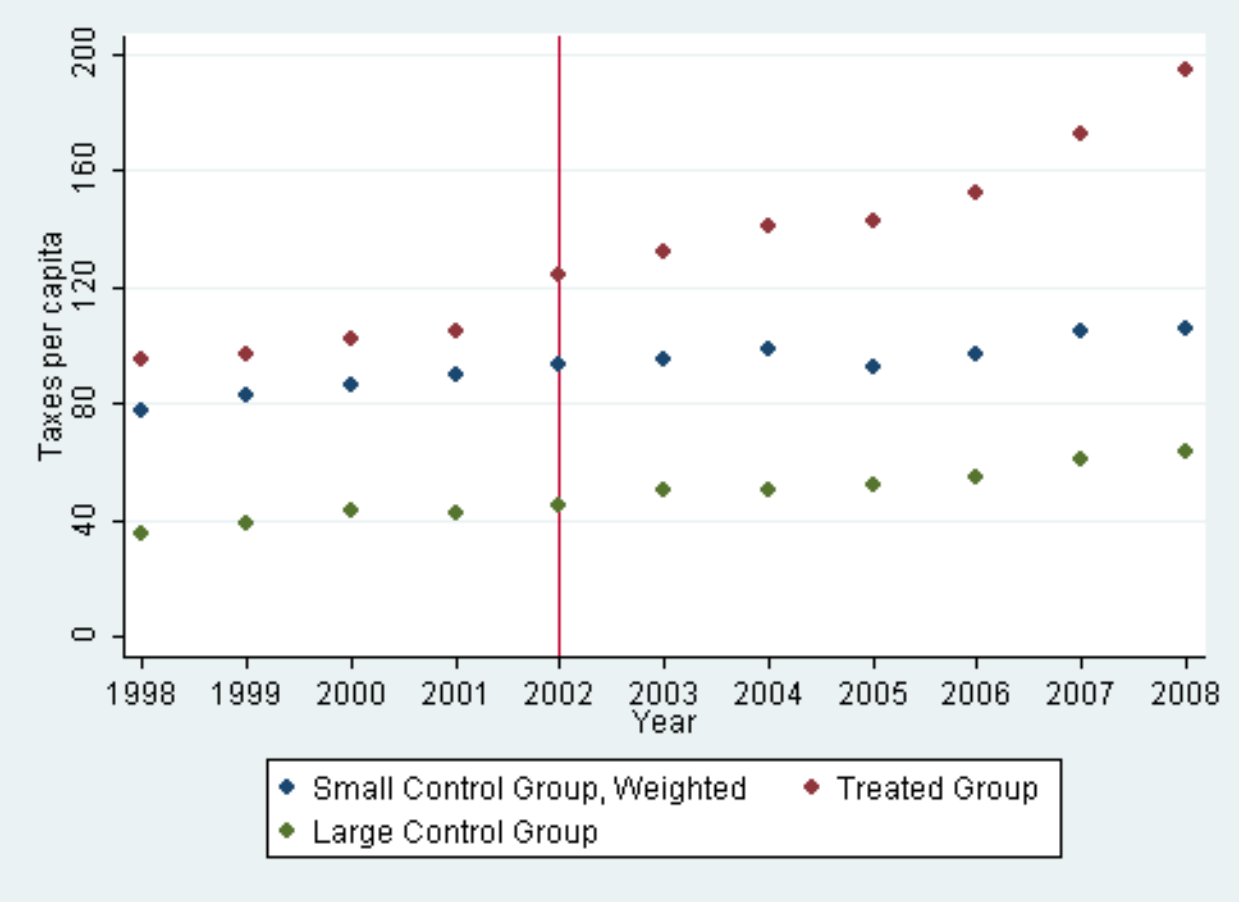
Columns 5 6 and 7 show results for the dynamic impact of the program using the weighted difference in differences model in equation (15). I restrict for now

Table 4: Summary Statistics

		Large sample		Restricted sample	
		Control Municipalities	**All municipalities**	**Control Municipalities**	**All municipalities**
	(1)	(2)	(3)	(4)	(5)
Pre-treatment characteristics					
Taxes (1998)	95.17 (97.49)	38.92 (79.46)	44.11(82.89)	78.40(66.65)	81.73(74.08)
FPM Transfers (1998)	93.9 (63.9)	167.2(76.1)	160 (78.04)	120.1 (56.5)	116.9(62.6)
Total revenues (1998)	449 (193)	440 (208)	441 (222)	565 (192)	538(197)
School infrastructure (1998)	4.64 (2.54)	4.18 (2.13)	4.04 (2.13)	5.01(2.24)	4.54(2.31)
School quality (1998)	0.4 (1.2)	-0.28 (0.4)	-0.22 (1.6)	0.2(1.5)	0.3(1.4)
Population (1998, thousands)	180 (70)	24 (57)	40 (23)	150 (46)	160 (52)
GDP (1999)	7.09 (5.14)	4.45 (4.12)	4.71 (4.30)	8.23(3.77)	8.02(4.09)
Agr\ GDP (1999)	10.15 (12.24)	23.74(15.75)	22.43 (15.96)	36.94(23.54)	31.86(24.24)
Serv\ GDP (1999)	63.39 (12.61)	59.48 (14.13)	59.85 (14.04)	45.81(21.12)	49.15(20.95)
Income	2.34 (1.16)	2.21 (1.05)	3.56 (1.48)	3.20 (1.09)	3.27(1.19)
Census variables (2000)					
Inequality	0.55 (0.05)	0.55(0.05)	0.55 (0.05)	0.56(0.03)	0.56(0.04)
Urban pop.	0.84(0.12)	0.64 (0.22)	0.66 (0.22)	0.8(0.11)	0.80(0.12)
Education	5.65 (1.27)	4.25 (1.17)	4.38 (1.25)	5.07 (1.13)	5.18(1.19)
Average for whole period					
Corruption	74.50 (171.86)	115.31(159.23)	112.05(160.53)	46.13(84.46)	59.13(129.05)
Has at least one AM radio in 1998	0.67 (0.47)	0.23(0.42)	0.27(0.45)	0.23 (0.42)	0.31(0.46)
Change in mayor	0.14(0.35)	0.15(0.35)	0.15(0.36)	0.11(0.32)	0.12(0.33)
Governor's party	0.23(0.42)	0.22(0.41)	0.23(0.42)	0.11(0.31)	0.13(0.34)
Pol. competition	0.57(0.10)	0.52(0.11)	0.53(0.12)	0.58(0.11)	0.57 (0.11)
Distance to closest PMAT	311.70 (449.48)	356.08 (413.82)	351.95(417.46)	291.54(372.62)	295.93(387.18)
Number of municipalities	331	3654	3985	2462	2760

Columns 4 and 5 restrict the sample to the common support sample and weight observations according to their propensity score. All revenue variables are per capita. School infrastructure is the number of classrooms in use in municipal schools per 1000 inhabitants, school quality is an index constructed from several indicators of school quality using principal component analysis, the political competition variable is the Herfindahl index of the concentration of votes during the last municipal elections, and distance to closest PMAT is the average distance between the municipality and the 5 closest municipalities who have already joined a PMAT program.

Figure 1: Evolution of Tax Revenues in Treated and Control Municipalities



the β_j to be equal to 0 for $j < 0$ to obtain more efficient estimates. To avoid confounding the estimation of the program's dynamic impact with that of potential heterogenous effects depending on time of entry in the program[38]the sample used to provide the estimates in column 5 excludes municipalities that start the program after 2005 because those are observed less than 4 years after they join. In column 6 I similarly exclude municipalities that start after 2004, and in column 7 those which starte after 2003. The immediate impact of the program is small (5% increase) but it reaches 10-11 Rs per capita after 4 years. This 11% increase in tax collection seems sustainable : estimates vary little from the 4th year onwards. On average municipalities borrow 9 Rs per capita at a real interest rate of 5% through the program and reimburse the loan after 6 years, leading to a total cost of 12 Rs per capita. The program is highly cost-effective: on average a municipality recovered invested funds after 2 years in the program; after 4 years one Real invested in modernizing the tax administration leads to slightly more than one extra Real in tax revenue in each year.

Estimates of the impact of the program on local expenditure outcomes are presented in Table 6. The table shows that it leads to the opening of an extra 0.2 classrooms per 1000 inhabitants on average. The dynamic impact of the program mirrors that of the increase in tax revenues described above, with an extra 0.4 classrooms after 5 years in the program, an 8% increase with respect to the baseline. The school quality index increases by around one-tenth of a standard deviation amongst the treated group. Finally column 6 suggests that the corruption index which proxies for the share of resources diverted in total public revenues decreases thanks to the program. The estimate is very large (it corresponds to a 50% decrease) but less precisely estimated.

It is not a priori clear how to interpret Table 6 as the increase in the quantity and quality of municipal education supply may have been financed by either the extra tax revenues generated by the program, or by the loosening of the budget constraint allowed by the program loan itself. If treated municipalities would have invested in their tax administration without the program, the program money could have allowed them to allocate extra funds to education expenditures. A rough back of the envelope calculation suggests the latter explanation does not hold. Both treated and control municipalities had local public revenues of around 450 Rs per capita in 1998, and financed the use of around 4.6 classrooms per 1000 inhabitants. Assuming that the average and marginal propensity to spend on opening new classrooms out of an increase in municipal revenue are equal, one would expect municipalities to have to obtain an extra 100 Rs per capita before they open one new classroom. The average loan amount, 9 Rs per capita, clearly would not suffice to open up to 0.4 extra classrooms unless we're willing to assume extremely large differences between the marginal and the average willingness to spend on classrooms. On the other hand the program's cumulated impact over

[38]The specifications includes a separate dummy for each year in the program.

Table 5: Impact of the Program on Tax Revenues

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	DiD		DiD on restricted sample	Weighted DiD			
All years	10.523***	9.946***	9.204***	7.243***			
	(1.945)	(2.093)	(2.600)	(2.589)			
1th year					5.170*	4.978*	4.783
					(2.671)	(2.744)	(3.021)
2th year					8.148**	8.321**	6.443
					(3.488)	(3.677)	(3.923)
3th year					10.065***	9.907***	9.407**
					(3.264)	(3.431)	(4.299)
4th year					11.164***	11.628***	11.063**
					(3.330)	(3.435)	(4.427)
5th year						10.500***	10.589**
						(3.835)	(4.533)
6th year							11.435**
							(5.400)
Controls		X	X	X	X	X	X
Observations	35600	35562	24049	24049	24049	24049	24049
Municipalities	3654	3654	2462	2462	2462	2462	2462

Cluster-robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Tax revenues are measured per capita. All regressions include municipality and year fixed effects, controls in columns 2 to 5 are GDP per capita, population size, share of agriculture and services in GDP, political competition in the previous election, mayor's party affiliation and whether the mayor is a facing a term limit.

5 years is to increase local revenues by 50 Rs. The idea that this amount was enough to open 0.4 extra classrooms seems reasonable. The same logic applies to investments in school quality. Overall Tables 5 and 6 indicate that the program increased the amount of tax revenues available to treated municipalities and also increased the availability of the main type of public good financed with local revenues - education - without leading to more corruption.

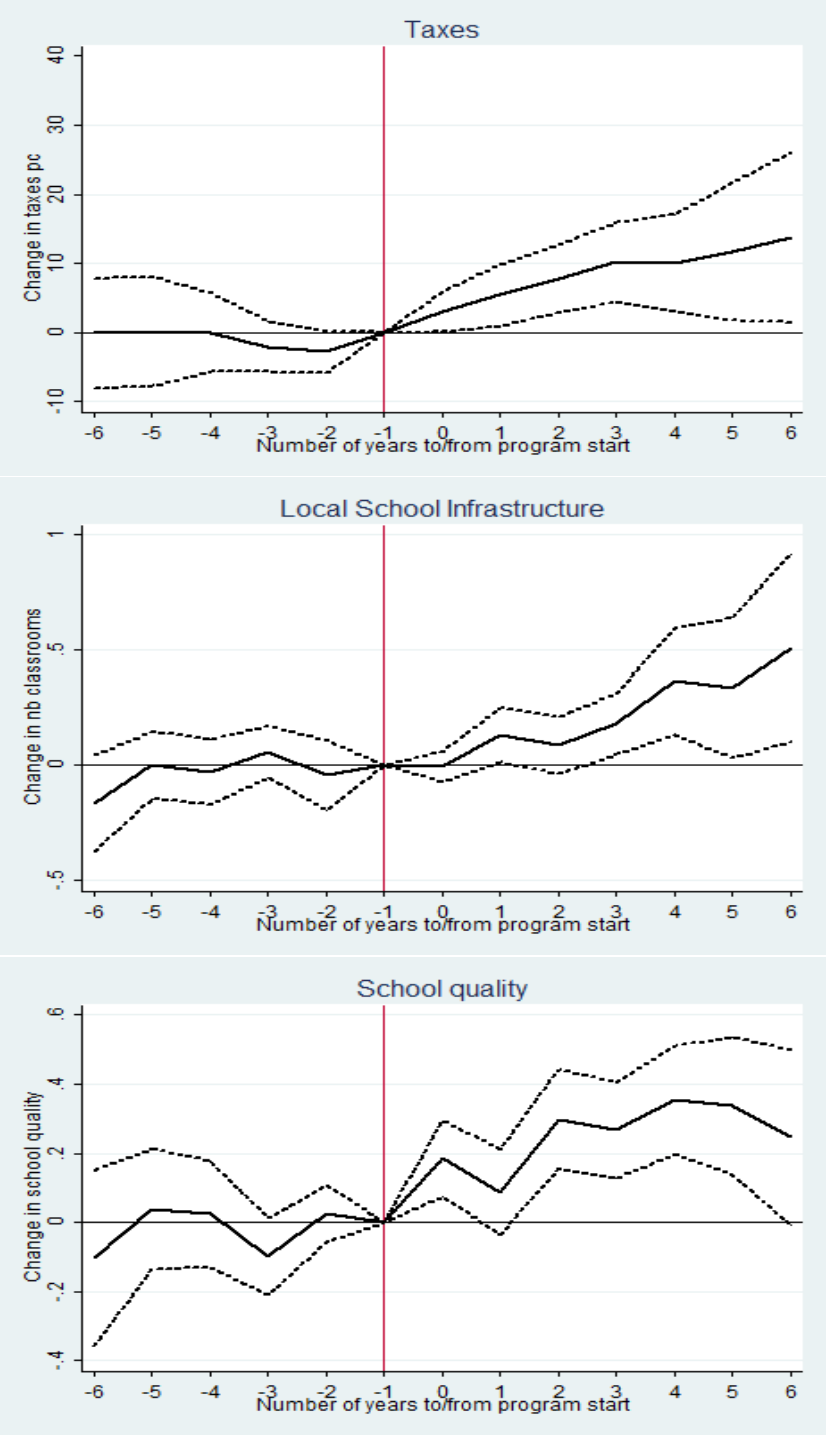
Finally Figure 2 provides some reassuring evidence regarding the validity of the common trend assumption necessary for identification by graphing the unrestricted dynamic estimates obtained from estimating equation 15. Each point on the solid lines summarizes the effect of having been in the program for j years (for j positive ordinate values) or of starting the program in j years (for j negative ordinate values) compared to the year just before the program started. The figure shows that these effects are never statistically different from zero before the program starts. This confirms that treated municipalities did not join the program at a time when they had already started to increase tax revenues and public goods.

Table 6: Impact of the Program on Local Expenditure Outcomes

Dependent variable :	Infrastructure				School quality				Corruption
All years	0.206***				0.142**				-35.232*
	(0.069)				(0.059)				(20.546)
1th year		0.047	0.040	-0.095		0.081	0.085	0.125	
		(0.040)	(0.043)	(0.093)		(0.058)	(0.063)	(0.078)	
2dyear		0.101*	0.109*	0.181***		0.117**	0.124**	0.117**	
		(0.058)	(0.064)	(0.089)		(0.058)	(0.061)	(0.087)	
3d year		0.152	0.127	0.159**		0.158**	0.169**	0.188***	
		(0.091)	(0.077)	(0.075)		(0.065)	(0.067)	(0.096)	
4th year		0.202***	0.160**	0.230**		0.173**	0.186**	0.186***	
		(0.072)	(0.072)	(0.111)		(0.073)	(0.073)	(0.096)	
5th year		0.311***	0.405***	0.270***		0.189*	0.192*	0.264***	
		(0.134)	(0.134)	(0.099)		(0.098)	(0.099)	(0.104)	
6th year		0.425***	0.419***	0.391***		0.240*	0.243*	0.301**	
		(0.174)	(0.173)	(0.153)		(0.130)	(0.131)	(0.130)	
Observations	24049	24049	24049	24049	24049	24049	24049	24049	483
Municipalities	2462	2462	2462	2462	2462	2462	2462	2462	483

Cluster-robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Infrastructure is number of classrooms in use in municipal schools per 1000 inhabitants, school quality is the first principal component constructed from the school quality variables described above, and corruption is the misgovernance index compiled from the CGU audits and Litschig and Zamboni (2008).All regressions include municipality and year fixed effects, controls are GDP per capita, population size, share of agriculture and services in GDP, and changes in political competition, mayor's party affiliation and mayor's term limit in the previous election. The specification used in the last column also controls for state fixed effects, lottery fixed effects, median education level, inequality and life expectancy, whether the municipality is a state capital, whether tourism is a major industry, existence of a local radio station and local judiciary presence.

Figure 2: Year by Year Impact of the Program



4.4 Robustness checks

The above analysis uses the evolution of outcomes in municipalities that have not joined the program yet, including those who are never observed starting the program, as a counterfactual outcome to identify the impact of the program on the treated municipality. An alternative is to use only the evolution of outcomes in municipalities who have not joined yet at time t, but will later at a time $t+s$ observed in the data, as those are arguably very similar to the municipalities who have already joined at time t. Unfortunately the bunching of municipalities' program start date around a few main years makes it impossible to estimate equation (13) on a sample consisting only of the 330 municipalities who enter the program before 2009 and identify separately year fixed effects and the program's impact (see appendix Table 5). Table 7 nevertheless reports estimates obtained using this sample which approximate what one would ideally like to do.

In the first column I present estimates obtained from estimating equation (13) using the sample of treated municipalities only and keeping the years up to 2003 only. In this sample the 183 municipalities which start the program after 2003 are never in the program, and are therefore used as control municipalities for the 147 who are treated up to 2003. The estimated impact of the program on this smaller sample is close to what the results obtained using the baseline specification above, though less precisely estimated. The second column shows results from estimating equation (13) on the sample of treated municipalities only and using all the years available but without year fixed effects. The estimated 'program impact' confounds the true program impact and the increase in outcomes over time that is common to all municipalities the comparison of these estimates with those obtained from estimating the same specification (equation (13) without year fixed effects) on the main sample used for analysis in column 3 is of interest. The estimates in columns 2 and 3 could differ for two reasons which would be a concern for the identification strategy used above. First the evolution of outcomes before 2003 could be different in control municipalities and municipalities treated after 2003. Second the estimates obtained using equation (13) could be mis-specifying the impact of time-varying covariates that are still unbalanced between treated and control municipalities despite the re-weighting. We have already seen evidence that the first cause for concern does not hold in the data, but the fact that the estimates in columns 2 and 3 are remarkably similar provides further reassurance regarding the validity of the identification strategy used.

The remaining concern regarding the interpretation of the estimates as impact of the program is that of an unobservable shock occurring at the same time as the program and affecting local outcomes. For example the characteristic 'government capability and honesty' could increase for unobserved reasons in one year, leading local officials to simultaneously apply for the program, become more efficient at collecting taxes and more accountable in their allocation of the budget. The assumption that this does not happen is essential to my identification strategy and

Table 7: Impact of the Program, Alternative Specifications

	Treated only, until 2003	Treated only, no year FE	All, no year FE
Dep. var: taxes			
Program : all years	7.746*	16.802***	16.846***
	(4.630)	(1.747)	(2.509)
Observations	959	3188	24049
Municipalities	330	325	2462
Dep. var: school infrastructure			
Program : all years	0.161***	0.208***	0.204***
	(0.051)	(0.037)	(0.051)
Dep. var: school quality			
Program : all years	0.113*	0.385***	0.367***
	(0.064)	(0.052)	(0.061)
Observations	960	3195	24053
Municipalities	330	325	2462

Cluster-robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Tax revenues are per capita, infrastructure is the number of classrooms in use in municipal schools per 1000 inhabitants and school quality is the first principal component constructed from the school quality variables described above. All regressions include municipality fixed effects as well as controls for GDP per capita, population size, share of agriculture and services in GDP, and changes in political competition, mayor's party affiliation and mayor's term limit in the previous election; year fixed effects are used in column 1.

cannot be tested. I can however use the arguably exogenous time lag between program application and program start to offer some evidence that such unobservable shocks do not explain the observed changes in outcomes after the start of the program. If municipalities apply to the program because their local officials become more capable and honest and this has an impact on outcomes we should see a change in outcomes at the time a municipality applies *even if the program itself does not start for a couple of years.* Table 5 presents estimates of the average impact of the program and the variation in local outcomes for the 3 years prior to the start of the program for municipalities which apply and start a program in the same year (63 municipalities), those that wait one year (174) and those that wait 2 or 3 years (81).

We see that for all these sub-groups and all outcome variables the variation in outcomes prior to start of the program is small and not statistically significant. It is in particular impossible to see a jump in outcomes at the date of application when municipalities apply 2 to 3 years before joining (third column). Of course we expect to see no such pattern if the municipalities whose files were processed slowly are also the ones who were the less eager to join the program and hence experienced a smaller and more delayed impact of the program. The average impact of the program is similar for all groups however; this suggests that any heterogeneity in the treatment effect is not correlated with the speed of application.

Finally Appendix Table 14 presents results for the dynamic impact of the program using different specifications and sample size. When the whole control group

Table 8: Impact of the Program by Time between Application and Program Start

Time between application and program start (years)	0	1	2-3
Dep. var: taxes			
3 years before	3.254	-3.985	-0.524
	(4.855)	(3.078)	(3.081)
2 years before	0.466	-4.792	-0.405
	(5.728)	(3.585)	(3.358)
1 year before	-0.035	0.276	0.467
	(5.947)	(4.425)	(3.958)
Program : all years	7.979*	6.794*	8.114*
	(4.129)	(3.388)	(4.674)
Dep. var: school infrastructure			
3 years before	0.219	-0.070	0.130
	(0.159)	(0.064)	(0.115)
2 years before	-0.041	-0.138	-0.004
	(0.165)	(0.096)	(0.129)
1 year before	-0.002	0.026	-0.061
	(0.187)	(0.185)	(0.131)
Program : all years	0.290*	0.155*	0.248*
	(0.172)	(0.092)	(0.128)
Dep. var: school quality			
3 years before	0.031	-0.017	0.009
	(0.082)	(0.073)	(0.110)
2 years before	0.060	0.106	0.055
	(0.095)	(0.111)	(0.122)
1 year before	0.127	0.042	0.071
	(0.108)	(0.081)	(0.114)
Program : all years	0.190*	0.116*	0.213**
	(0.110)	(0.069)	(0.074)
Observations	21619	22642	21710
Clusters	2214	2318	2224

Cluster-robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Tax revenues are per capita, infrastructure is the number of classrooms in use in municipal schools per 1000 inhabitants and school quality is the first principal component constructed from the school quality variables described above. All regressions include municipality and year fixed effects as well as controls for GDP per capita, population size, share of agriculture and services in GDP, and changes in political competition, mayor's party affiliation and mayor's term limit in the previous election.

is used and observations are not weighted according to their propensity score the program seems to have a larger impact on taxes, municipal school infrastructure and quality. Though estimates remain close to the ones obtained using the preferred specification this confirms that restricting the sample to construct a more credible counterfactual matters, as suggested by Figure 1. A recurring criticism of the difference-in-differences methodology is that it is strongly functional form dependent (Heckman (1996)). I present results for the natural logarithm of the dependent variables using the preferred propensity-score weighted method which paint a similar picture of the dynamic impact of the program[39].

The availability of rich panel data and the exploitation of the program's timing thus allows me to rule out the most likely alternative hypothesis that could explain the observed changes in outcomes once the program has started. This allows me to argue that the program itself has a causal impact on tax revenues, which are then used to finance extra education provision but no increase in corruption. Confidence with respect to causality however does not imply that the program would lead to such outcomes if applied to all Brazilian municipalities. Two things are required to increase tax collection: the program's money and technical knowledge, and motivation of local politicians and administrations. The last set of results suggest that local motivation (proxied by the timing of application to the program) is not a sufficient condition for the observed change in outcomes. It is however likely to be a necessary one: imposing the program on municipalities in which local officials are not interested in increasing tax collections probably would not work.

All the above estimates must therefore be understood as impact of the program *on the treated*. To provide estimates of the impact of the program were it imposed on the average Brazilian municipality one would need to assume knowledge of the local motivation to increase in tax capacity in this municipality, an assumption that I am not willing to make. This interpretation of the estimates as 'treatment on the treated' must be kept in mind when comparing the marginal impact of an increase in taxation to that of an increase in transfers in the next section.

[39]Results differ when using a log specification and the large sample. This is a consequence of the large difference in levels between the treated and the control groups when the latter is not restricted to the group on which there is common support, which motivates the combination of a difference in differences specification and propensity score matching.

5 Are tax revenues better spent than transfer revenues?

This section compares the impact on local expenditure outcomes of an increase in tax revenues thanks to the program to the impact of an exogenous increase in transfer revenues. It directly tests proposition 2 of the model which states that an increase in taxes thanks to the program will lead to a higher increase in public good provision and a smaller increase in corruption than an increase in transfers of the same amount.

As explained above the impact of an exogenous increase in transfers on corruption and education provision at the local government level in Brazil has already been studied in the literature by studies that exploit discontinuities in the allocation rule of the largest unrestricted transfer to local governments, the *FPM* transfer. Brollo *et al.* (2010) find that an exogenous increase in transfers of 10% raises incidence of corruption by 12 percentage points. Litschig (2008a) estimates that a 1000 Rs increase leads to 0.42 more years of elementary schooling and a 5.6% increase in student literacy rate.

In contrast the results in the previous section suggest, using a simple Wald estimate, that a 10% increase in taxes thanks to the program leads to a fall in the incidence of corruption. I cannot compare my findings with those in Litschig (2008a) because his data on education outcomes is not available for my period of study. I therefore replicate both studies' estimation strategy in attempt to directly compare the impact of tax and transfer revenues on corruption and education inputs.

5.1 Empirical Strategy

I evaluate the impact of an increase in taxes thanks to the program by estimating the following equations :

$$E_{i,t} = \pi_{TE} T_{it} + \eta X_{i,t} + \gamma_t + \mu_i + \epsilon_{i,t} \tag{16}$$

and

$$C_{i,t} = \pi_{TC} T_{it} + \eta X_{i,t} + \eta_2 Z_i + \eta_3 S_i + \gamma_t + \epsilon_{i,t} \tag{17}$$

where $E_{i,t}$ is a measure of education inputs and $C_{i,t}$ the corruption index, $T_{i,t}$ is instrumented for using program participation and all covariates are as above. The identifying assumption required to make a causal interpretation of the IV parameters π valid is the same as the one used to interpret the difference-in-differences estimates : there must be no unobserved time-varying municipal characteristics that affect program uptake, tax revenues and local expenditure outcomes simultaneously. The discussion above of those assumptions is therefore still relevant in this section.

The impact of intergovernmental transfers is identified using exogenous variations in the amount of FPM grants received generated by the transfer allocation

rule outlined in the appendix. This rule specifies that all municipalities in the same state and in a given population bracket receive the same amount of transfers. Appendix Figure 3 shows that, although there are multiple cases of mis-assignments around the population thresholds, the amount of FPM transfers received by municipal governments displays clear jumps at each threshold. Following Brollo *et al.* (2010) I therefore use a fuzzy regression discontinuity approach where the amount that the rule predicts each municipality should receive (theoretical transfers) is an instrument for the transfer actually received. The estimated equation for transfers is:

$$Y_{i,t} = \pi_{FY} F_{i,t} + \eta_{FY} X_{i,t} + \gamma_t + \mu_i + v_{i,t}, \tag{18}$$

where $Y_{i,t}$ is education inputs or the corruption index[40], $F_{i,t}$ is FPM resources per capita, instrumented for using theoretical FPM per capita and I flexibly control for population size by allowing for different slope and curvature around the thresholds. The identification of the impact of transfers in equation (18) comes from both cross-sectional variations (municipalities just below and just above the thresholds) and within-municipalities variations (municipalities who cross the thresholds over time). Because the variation used to identify the impact of taxes in equation (16) is within municipalities I present estimates of the transfer equation with and without municipality fixed effects and show that results are similar. More details on the methodology used is in the Appendix.

Finally equations 17 and 18 using the corruption index as the dependent variable are estimated using the two-sample instrumental variable (TSIV) method developped by Angrist and Kruger (1992, 1995) (see also Inoue and Solon (2010)). TSIV is appropriate in situations where the outcomes are available in one data set, the endogenous regressor is available in a second data set, both data sets contain the instrumental variable and the other exogenous variables included in the model, and the distribution of variables is the same in both datasets. In my case the indicator for participation in the PMAT program and the tax variable are present in the large sample of nearly 3000 municipalities but the corruption index is only available for a random subsample. The TSIV method increases the precision of the first stage by first estimating the impact of the PMAT program and theoretical transfers on tax and transfer revenues on the large sample and predicting tax and transfer using these estimates. These predicted variables as finally used regressors in the second stage equation on the small sample for which the corruption index is available. Standard errors taking into account the fact that these second stage regressors are estimated are obtained using bootstrap.

5.2 Results

Table 9 presents estimates of the impact of a one Real increase in taxes and transfers on local education infrastructure and corruption. F statistics for the

[40]When $Y_{i,t}$ is the corruption index municipality level fixed effect is replaced by a treated fixed effect and the set of time-invariant covariates used above.

corresponding first stage estimates are reported to assess the strength of the (conditional) correlation between the instruments and the endogenous variables. The first two columns reports results from estimating equations (18) on small municipalities with less than 28,000 inhabitants first and on municipalities with more than 28,000 inhabitants. Small municipalities seem to use the increase in transfers to fund a small increase in classrooms and quality of school infrastructure. This is coherent with the results in Litschig (2008a) which show that an increase in transfers leads to better education outcomes amongst small municipalities. There is however no such impact in bigger municipalities. The impact of transfers on corruption is very similar in small and large municipalities - a 1% increase compared to the baseline, though imprecisely estimated. The third column presents estimates of the impact of transfers in the restricted sample of municipalities, reweighted to be similar to the municipalities in the PMAT program. This gives more weight to large municipalities. In this sample an increase in transfers has no impact on education infrastructure, but increases corruption significantly.

The last column in table 9 estimates the impact of an increase in tax and transfer revenues simultaneously using the preferred fixed-effect and propensity score weighted specification. Increases in taxes thanks to the program lead to more investment in school infrastructure and a higher increase in school enrollment than the increase in transfer revenues that occurs thanks to the *FPM* allocation rule, in line with the model's proposition 2. Notice that the impact of taxes on education infrastructure is much larger than the impact of transfers in small municipalities.

The extent to which the estimates of the impact of extra transfers or taxes on local expenditure outcomes are comparable needs to be discussed. An ideal test of the model's predictions 2 and 3 that increase in taxes lead to a more favorable allocation of public revenues than increases in transfers would require the existence of two perfectly identical municipalities A and B, one in which local taxes are exogenously increased, and one in which transfer revenues are exogenously increased by the same amount. The assumption discussed in Section 4 are sufficient to interpret coefficients in Table 9 as the causal impact of an increase in taxes on local expenditure outcomes but to compare the impact of increases in taxes and transfers we need to make an additional assumption. Because both estimates are *local* this comparison is only valid if we assume that the marginal propensity to spend out of taxes and transfers is the same in municipalities that participate to the program and in the average municipality affected by the instrument for transfers. This is equivalent to assuming that reweighting the control group ensures that the average municipality is comparable to the municipalities in the program along all characteristics which influence these marginal propensities to spend.

Table 10 offers a test of the model's prediction that the marginal impact of tax and transfer revenues on public good provision and corruption is more similar the more information citizens have about the public budget (proposition 4). Following Ferraz and Finan (2011) I use the presence of a local radio station as a proxy for how much information citizens can access about local public budgets. Table 10

Table 9: Impact of Increases in Tax or Transfer Revenues on Local Expenditure Outcomes : IV Estimates

	Small municipalities	Large municipalities	All municipalities	All municipalities
Dep. var : school infrastructure				
Taxes				0.029**
				(0.013)
Transfers	0.003***	0.004	-0.000	0.007
	(0.001)	(0.006)	(0.004)	(0.006)
Dep. var : school quality				
Taxes				0.016***
				(0.012)
Transfers	0.002***	0.002	-0.002	0.006
	(0.000)	(0.006)	(0.002)	(0.004)
F stat (Taxes)				13.2
F stat (Transfers)	62.4	35.3	48.2	48.2
Observations	24049	9264	14785	24049
Municipalities	2462	934	1528	2462
Dep. var : corruption				
Taxes				-2.882
				(3.048)
Transfers	0.649	0.656	0.627**	0.665**
	(0.402)	(0.466)	(0.303)	(0.345)
F stat (Taxes)				11.5
F stat (Transfers)	452	382	450	450
Observations	418	259	677	677

Cluster-robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.Infrastructure is number of classrooms in use in municipal schools per 1000 inhabitants, school quality is the first principal component constructed from the school quality variables described above and corruption is the misgovernance index compiled from the CGU audits and obtained from Litschig and Zamboni (2008). All regressions include year fixed effects, controls are GDP per capita, population size, share of agriculture and services in GDP, and changes in political competition, mayor's party affiliation and mayor's term limit in the previous election. The specifications in which municipality fixed effects are not included also control for state fixed effects, median education level, inequality and life expectancy, whether the municipality is a state capital, whether tourism is a major industry, existence of a local radio station and local judiciary presence. When the corruption index is the dependent variable the estimates are obtained using TSIV, and standard errors are estimated using bootstrap.

reports the p-values of t tests for the hypothesis that the impact of taxes is equal to the impact of transfers in municipalities with and without a radio station. The difference between the two sources of revenue becomes statistically insignificant when there is a local radio in the municipality, in line with the model's predictions. The results for the corruption index are however inconclusive.

Appendix Tables 15 and 16 explore two alternative mechanisms which could lead to a difference in how increases in tax and transfer revenues are spent. Governments which rely more on local tax revenues may have better incentives to invest in public goods if this increases the local tax base. The program has however no impact on local GDP or population, suggesting this mechanism is not relevant in the context of Brazilian local governments. This could be because the types of investments local governments can make in Brazil (in education or health) are unlikely to affect local growth fast enough to be a relevant factor for politicians and be detected in the data. Another difference between taxes and transfers could be that tax revenues are more stable than transfer revenues. This could explain the results in Table 9 if local governments only invest in education quantity and quality when they experience an increase in revenues that they believe is stable over time, and divert increases in revenues that are short lived. The within municipality standard deviation is however always smaller relative to the mean for transfer revenues than for tax revenues. This is unsurprising given that the transfers considered - *FPM* transfers - only vary if the total amount allocated to *FPM* transfers at the federal level changes (or if the municipality's population reaches a threshold). This mechanism may however be relevant if one considered discretionary transfers which are more volatile.

Table 10: Impact of Taxes and Transfers with and without local media

	Infrastructure	Quality	Corruption
Taxes	0.040*	0.014*	-3.060
	(0.024)	(0.008)	(32.967)
Transfers	0.004	-0.001	7.681
	(0.007)	(0.002)	(19.459)
Taxes*radio	-0.024	0.001	-17.219
	(0.020)	(0.007)	(39.199)
Transfers*radio	0.013	0.009	-5.851
	(0.010)	(0.008)	(75.583)
T-test p value (no radio)	0.06	0.02	0.8
T-test p value (with radio)	0.91	0.45	0.9
Observations	22673	22673	462
Municipalities	2331	2331	462

Cluster-robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Infrastructure is number of classrooms in use in municipal schools per 1000 inhabitants, school quality is the first principal component constructed from the school quality variables described above and corruption is the misgovernance index compiled from the CGU audits and obtained from Litschig and Zamboni (2008). All regressions include year fixed effects, controls are GDP per capita, population size, share of agriculture and services in GDP, and changes in political competition, mayor's party affiliation and mayor's term limit in the previous election. The specifications in which municipality fixed effects are not included also control for state fixed effects, median education level, inequality and life expectancy, whether the municipality is a state capital, whether tourism is a major industry, existence of a local radio station and local judiciary presence. When the corruption index is the dependent variable the estimates are obtained using TSIV, and standard errors are estimated using bootstrap. The variable 'radio' is an indicator equal to 1 if the municipality has at least one local radio station.

6 Conclusion

This paper provides evidence on the impact of a tax modernization program amongst Brazilian local governments - a permanent 11% increase in taxes per capita, an increase in the quantity of education inputs provided by the municipalities, and no increase in the incidence of a broad measure of corruption. I take advantage of the variation in taxes induced by the program and discontinuities in the rule allocating federal transfers to test a theoretical prediction that taxes are more *accountability inducing* than transfers. Results show that local governments use the increase in taxes thanks to the program to provide more education inputs than they do when faced with an increase in transfer revenues of the same amount. More transfers lead to more corruption, more taxes do not.

These results speak directly to debates about the right form of decentralization. The existence of a large 'fiscal gap' between the expenditure responsibilities and the tax collection of local governments is an ubiquitous characteristic of local governments around the world. In developing countries in particular local governments have been granted substantial expenditure responsibilities but local capacity to tax generally lags behind. My results suggest that narrowing this fiscal gap by empowering local governments to levy more tax revenue will make them more accountable to their constituents. Substantial local tax collection - complemented by intergovernmental transfers for revenue equalization purposes - may be a necessary feature of successful decentralization.

Moving up from the local government level the mechanisms explored in this paper also contribute to debates on how to finance development. One of the central recommendation of the 2005 report on achievement of the Millenium Development Goals is that developing countries should mobilize increased domestic resources by up to four percentage points by 2015 (UnitedNations (2005)), yet there is very little research on how this aim could be achieved. What's more, technical aid on public sector financial management has always been the poor parent of official development aid[41] This paper shows that one type of resource mobilization program in place in Brazil for more than a decade has been successful in providing long term sources of funds to local governments. The theoretical mechanism developed in this paper also applies to a federal government financed by tax and non-tax revenues (such as aid or revenues from natural resources). It suggests that technical help in tax capacity building may lead to an increase in public resources which is more conducive to the type of public spending that benefits citizens than traditional development aid. To understand whether increasing tax capacity at the national level does indeed improve the accountability of national governments remains an important topic for future research.

[41] See OECD (2010a) for a discussion of the different forms of aid in public sector financial management.

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A Theoretical appendix

A.1 Proof that the participation constraint is binding in the L case

The representative citizen's maximizes :

$$W(G_L, T_L, T_H, \sigma_H, \sigma_L) = (1-q)(G_L - \phi C(T_L)) + q(G_L + T_H - T_L + Z(\sigma_H - \sigma_L) - \phi C(T_H)) \tag{19}$$

subject to the following constraints, where λ_i is the lagrange multiplier associated with constraint i

$$\begin{cases} 1: \ G_L \leq (\bar{F}(1-u) + T_L)(1-\alpha) + \sigma_L Z \ (\lambda_1) \\ 2: \ G_L \leq \bar{F}(1+u)(1-\alpha) - \alpha T_H + T_L + \sigma_L Z \ (\lambda_2) \\ 3: \ T_H \geq 0 \ (\lambda_3) \\ 4: \ T_L \geq 0 \ (\lambda_4) \\ \sigma_H \in [0,1] \\ \sigma_L \in [0,1] \end{cases}.$$

where I am using the fact that $G_H = G_L + T_H - T_L + Z(\sigma_H - \sigma_L)$ to rewrite the participation constraint in case H (constraint 2).
The first order conditions for maximization are:

$$\frac{\partial W}{\partial \sigma_H} = Zq \tag{20}$$

$$\frac{\partial W}{\partial \sigma_L} = Z(\lambda_1 + \lambda_2 - q) \tag{21}$$

$$\frac{\partial W}{\partial G_L} = 0 \Leftrightarrow \lambda_2 + \lambda_1 = 1 \tag{22}$$

$$\frac{\partial W}{\partial T_L} = 0 \Leftrightarrow \lambda_4 = (1-q)\phi C_{T_l} + q - \lambda_2 - \lambda_1(1-\alpha) \tag{23}$$

$$\frac{\partial W}{\partial T_H} = 0 \Leftrightarrow \lambda_3 = q(\phi C_{T_H} - 1) + \alpha + \lambda_2) \tag{24}$$

Note first that the citizen will always set the probability of re-election equal to 1 to maximize the level of public good provided. Trivially, equation (20) implies that $\frac{\partial W}{\partial \sigma_H} > 0$ and $\sigma_H = 1$. Combining equations (21) and (22) similarly gives $\frac{\partial W}{\partial \sigma_L} = Z(1-q) > 0$ and $\sigma_L = 1$. Equation (22) shows that one of constraints 1 and 2 must bind. Intuitively one the participation constraints must bind - if not, public good in one of the states could be increased whilst keeping taxes constant.

Suppose the participation constraint in the L case (constraint 1) does not bind. This implies that $\lambda_2 = 1$ so that the participation constraint in the H case binds and G_L is set such that $G_L = (\bar{F}(1+u) + T_L)(1-\alpha) + \alpha(T_L + T_H) + \sigma_L Z$. The

participation constraint in the L case implies that the optimal tax levels must respect:

$$(\bar{F}(1+u)+T_L^*)(1-\alpha)+\alpha(T_L^*-T_H)+\sigma_L Z \leq (\bar{F}(1-u)+T_L^*)(1-\alpha)+\sigma_L Z \quad (25)$$

$$\Leftrightarrow \alpha(T_L^*-T_H) \leq -(1-\alpha)\bar{F}2u \leq 0 \quad (26)$$

Intuitively we must have $T_H^* > T_L^*$ to ensure that the politician in the L case does not find it profitable to pretend he is in the H case. However $\lambda_2 = 1$ and $\lambda_1 = 0$ implies that

$$\lambda_4 = (1-q)\phi C_{T_l} + q - 1 \Leftrightarrow T_L^* = h(1/\phi),\ \lambda_4 = 0 \quad (27)$$

and

$$\lambda_3 = q(\phi C_{T_H} - 1) + \alpha \quad (28)$$

$$\Leftrightarrow T_H^* = h(q-\alpha)/\phi q),\ \lambda_3 = 0,\ q > \alpha \text{ or } \lambda_3 > 0, T_H^* = 0 \quad (29)$$

this implies that $T_H^* < T_L^*$ and violates (26). This completes the proof

A.2 Proof of propositions 1-4

The program lowers ϕ by $d\phi < 0$ and therefore increases taxes in both states :

$$\frac{\partial T_H^*}{\partial \phi} = -\frac{h'(1/\phi)}{\phi^2} < 0 \quad (30)$$

and

$$\frac{\partial T_L^*}{\partial \phi} = \min\{0, -\frac{h'(1-q-\alpha/\phi(1-q))}{\phi^2}\frac{1-q-\alpha}{1-q}\} \quad (31)$$

I write

$$\frac{\partial E(T^*)}{\partial \phi} = -\omega_1 < 0 \quad (32)$$

It also increases the spread between T_H^* and T_L^*[42] :

$$\frac{\partial T_H^*}{\partial \phi} - \frac{\partial T_L^*}{\partial \phi} = -\frac{h'(1/\phi)}{\phi^2} - (1-\alpha)(E(T^*)+\bar{F}) - \bar{F}u(1-\alpha) = -\omega_2 < 0 \quad (33)$$

From equations (30), (31) and (33) we can write the increase in average public good provision thanks to the program:

$$-\frac{\partial E(G^*)}{\partial \phi} = (1-\alpha)\omega_1 + (1-q)\alpha\omega_2 > (1-\alpha)\omega_1 \quad (34)$$

Consider an increase in $\bar{F}$ of the same amount ω_1. It leads to an increase in average public good provision such that:

$$\frac{\partial E(G^*)}{\partial \bar{F}}\omega_1 = (1-\alpha)(1-u)\omega_1 < -\frac{\partial E(G^*)}{\partial \phi} \quad (35)$$

[42] Provided $h(.)$ is not too concave.

This completes the proof of propositions 1 and 2.

Proposition 3 follows from observing that equation (35) implies that:

$$\frac{\partial E(S^*)}{\partial \bar{F}}\omega_1 > \frac{\partial E(S^*)}{\partial \phi} \tag{36}$$

as $E(S^*) = E(T^*) + E(F) - E(G^*)$. This gives proposition 3.

Proposition 4 follows from observing that as u increases $\frac{\partial E(G^*)}{\partial \bar{F}}\omega_1$ increases but $\frac{\partial E(G^*)}{\partial \phi}$ is unchanged.

B The Transfer Allocation Rule

The most important source of municipal revenue is the *Fundo de Participacao dos Municipios* (FPM), an automatic federal transfer established by the Brazilian Constitution. The FPM allocation mechanism divides local governments into population brackets which determine the share of their state's total FPM resources they will receive. Smaller population brackets are allocated lower shares. Each of the 26 states receives a different share of the total FPM resources in the federal budget, so two municipalities will receive the same amount only if they are in the same population bracket and state. The revenue sharing mechanism determining the amount $FPM^s_{i,t}$ received by government i in state s is

$$FPM^s_{i,t} = \frac{f(pop_{i,t})}{\sum_{j \in s} f(pop_{j,t})} FPM^s \tag{37}$$

where $f(pop_{i,t})$ is the coefficient corresponding to the population bracket in which the local government's population is found. Table 11 presents the population brackets and associated coefficients[43] in its first two columns. The *Tribunal de Contas Uniao* (TCU) determines how much each municipality will receive each year using the population estimates calculated by the Brazilian Statistical Institute (IBGE). I construct the amounts of theoretical FPM grants each municipality is allocated according to the above rule depending on its state and population size for each year. Table 11 reports the average of those theoretical grants as well as the average actual grants received by municipalities in each population bracket.

It is clear from the table that population and state do not perfectly predict the FPM grants each municipalities receives, due to several reasons. Various law amendments during the 1990s froze the FPM allocations for some municipalities (in particular, the ones that split over the period). Even for municipalities not affected by those amendments the rule is not perfectly enforced : Litschig (2008b) presents some evidence of manipulative sorting above the FPM thresholds for the years 1989 and 1991, evident from the official TCU population estimates used to calculate the allocation of FPM resources each year. The official TCU estimates and the IBGE population estimates used in this paper indeed do not coincide, suggesting potential manipulation at the TCU level. Nonetheless, real FPM grants received do increase substantially at each population threshold. Figure 3 displays the scaterplot of received and theoretical FPM transfers received by municipalities in the state of Minas Gerais in 2008[44]; the vertical lines represent the population thresholds. Both figures display visible jumps at the thresholds, though there are cases of misassignment around the cutoffs in the graph for actual FPM transfers.

[43] Set by Decree No. 1881/81 and unchanged since 1981.

[44] The sample is restricted to one year and state (Minas Gerais, which contains the most municipalities) to limit the variation in grants received and make the graphs easier to understand. All year/state combinations provide similar graphs.

Figure 3: Real and Theoretical FPM Transfers for the state of Minas Gerais in 2008

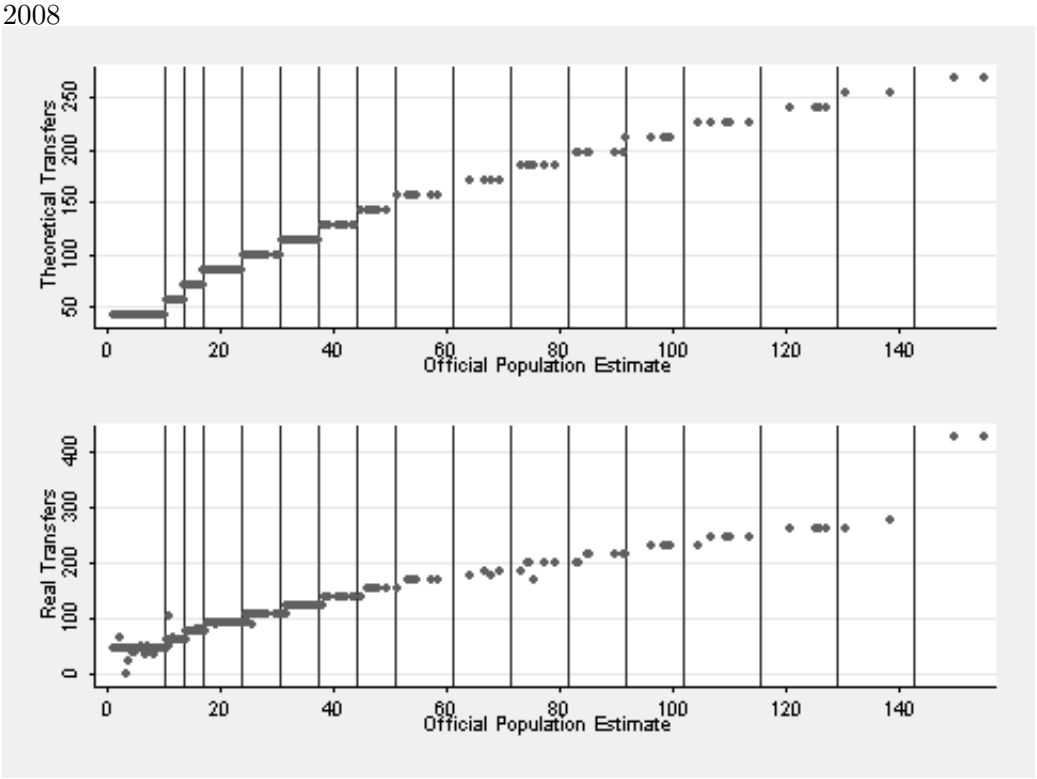


Table 11: Real and Theoretical FPM Transfers per capita and Coefficients

Population	Coefficient	Real Transfer	Theoretical Transfer
<10,189	0.6	385	341
10,189-13,584	0.8	192	170
13,585-16,980	1	175	166
16,981-23,772	1.2	160	47
23,773-30,564	1.4	142	54.6
30,565-37,356	1.6	131	63.7
37,357-44,148	1.8	122	71.7
44,149-50,940	2	117	80.6
50,941-61,128	2.2	108	87.8
61,129-71,316	2.4	99	94.8
71,317-81,504	2.6	92	103.7
81,505-91,692	2.8	85	107.5
91,693-101,880	3	84	125.6
101,881-115,464	82	127.8	134.8
115,465-129,047	76	132.6	135.3
129,048-142,632	68	144.4	146.1

Population is the official population estimate from the IBGE. The coefficient are obtained from official documents of the *Tribunal de Contas Uniao* and used to estimate the theoretical FPM transfer allocated to each municipality. Real FPM transfers received are from the *FINBRA* database.

To identify the causal impact of an increase in FPM transfers on local spending outcomes I use variations in the amounts of theoretical grants municipalities should have received, controlling for any impact of the variables determining the allocation by using state fixed effects and a high-order polynomial in population size. Table 12 presents the first stage of this identification strategy. We see that the actual amount of FPM transfers received increases one for one with the theoretical amounts. None of the control variables have any impact on the amounts received, suggesting that manipulations of the rule are rare and/or unrelated to the variables which affect the dependent variables of interest in this paper.

Table 12: First Stage Regression for FPM Transfers

	Dep. var : Real FPM Transfer pc
Theoretical FPM pc	1.046***
	(0.074)
GDP pc	0.000
	(0.001)
Population	0.550
	(2.815)
Agr\ GDP	-40.937
	(36.303)
Serv\ GDP	2.319
	(32.428)
Density	0.003*
	(0.002)
Income pc	0.003
	(0.003)
Inequality	12.010
	(12.195)
[1em] Education	-9.097
	(6.218)
Pol. competition	20.313
	(15.806)
Term limit	-4.982
	(4.837)
Observations	48460
Municipalities	5087

Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The regression includes state and year fixed effects as well as a high order polynomial in population size.

C Estimation of the Propensity Score

The propensity score used to implement the weighted-difference in differences methodology is calculated by estimating a probit model of the probability that a municipality started a PMAT program sometime between 1998 and 2008 as a function of the pre-intervention characteristics used in the first column of Table 3. Table 13 presents the results of this estimation.This model is then used to predict the propensity (probability) that a municipality will privatize.

Table 13: Determinants of the probability of joining a program

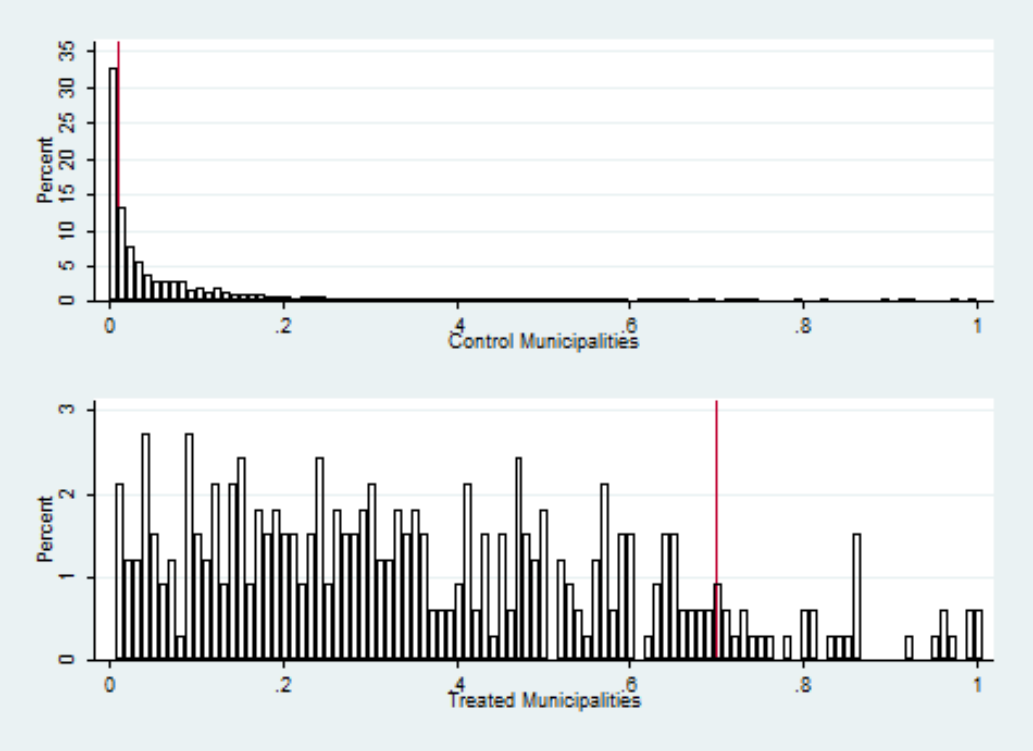
	eq1
=1 if PMAT pgm wasn't cancelled, 0 if it was	
Income	0.1586**
	(0.0757)
Population	0.1069**
	(0.0468)
Taxes in 1998	0.0000
	(0.0005)
Agr\ GDP	-0.0048
	(0.0042)
Serv\ GDP	0.0006
	(0.0045)
Education	0.0203
	(0.0875)
Urban pop.	0.8583***
	(0.2942)
Inequality	-0.8831
	(0.9148)
Governor's party	-0.0827
	(0.1073)
Pol. competition	0.7736**
	(0.3786)
esample() from estimates store	
Distance to closest PMAT	
Observations	3560

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* p¡0.10, ** p¡0.05, *** p¡0.01

Cluster-robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each coefficient represents a marginal effect and the regression includes state fixed effects.

I identify control and treatment observations on a common support as follows. I exclude all control observations whose propensity scores are less than the propensity score of the treated municipality at the mid-point of the first percentile of the treatment propensity score distribution, and exclude all treated observations whose propensity score is greater than the propensity of the control observation at the mid-point of the 99th percentile of the control distribution. This eliminates 33% of control observations and 10% of treated. Figure 4 graphs the distribution of the propensity score in the treated and control groups. The red lines indicate the limit of the common support.

Figure 4: Distribution of the propensity score



D Table Appendix

Table 14: Impact of the Program, Alternative Specifications : (1) All sample (2) Logs

	Taxes	Taxes	Infrastructure	Infrastructure	Quality	Quality	Corruption	Corruption
1th year	5.138***	0.048**	0.066**	0.022*	0.178***	0.122***		
	(1.716)	(0.024)	(0.032)	(0.012)	(0.036)	(0.037)		
2th year	6.738***	0.097**	0.102***	0.038**	0.192***	0.079*		
	(2.061)	(0.039)	(0.036)	(0.018)	(0.068)	(0.044)		
3th year	10.042***	0.099***	0.138***	0.042**	0.440***	0.069		
	(2.294)	(0.035)	(0.042)	(0.017)	(0.118)	(0.051)		
4th year	12.668***	0.087***	0.193***	0.061***	0.293***	0.097		
	(2.685)	(0.031)	(0.047)	(0.020)	(0.068)	(0.059)		
5th year	16.251***	0.063**	0.264***	0.100***	0.432***	0.095		
	(3.357)	(0.031)	(0.058)	(0.034)	(0.092)	(0.062)		
All years							-56.282	-0.467**
							(42.152)	(0.216)
Observations	35562	24049	35569	24047	35569	8992	705	483
Clusters	3654	2462	3654	2462	3654	1444	705	483

Standard errors in parentheses
* p¡0.10, ** p¡0.05, *** p¡0.01

Cluster-robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions include municipality and year fixed effects as well as controls for GDP per capita, population size, share of agriculture and services in GDP, and changes in political competition, mayor's party affiliation and mayor's term limit in the previous election. The results for the log specification (2) are obtained from running propensity-score weighted versions of equations (13) and (14) on the common support sample using the natural logarithm of taxes, infrastructure, enrollment and the corruption index as dependent variables.

Table 15: Impact of the program on GDP and population

	GDP	Population
Program : all years	0.742	-0.094
	(0.902)	(0.127)
Observations	24070	24070
Municipalities	2462	2462

Cluster-robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions include year fixed effects, controls are GDP per capita, population size, share of agriculture and services in GDP, and changes in political competition, mayor's party affiliation and mayor's term limit in the previous election.

Figure 5: Distribution of Application and Start Dates

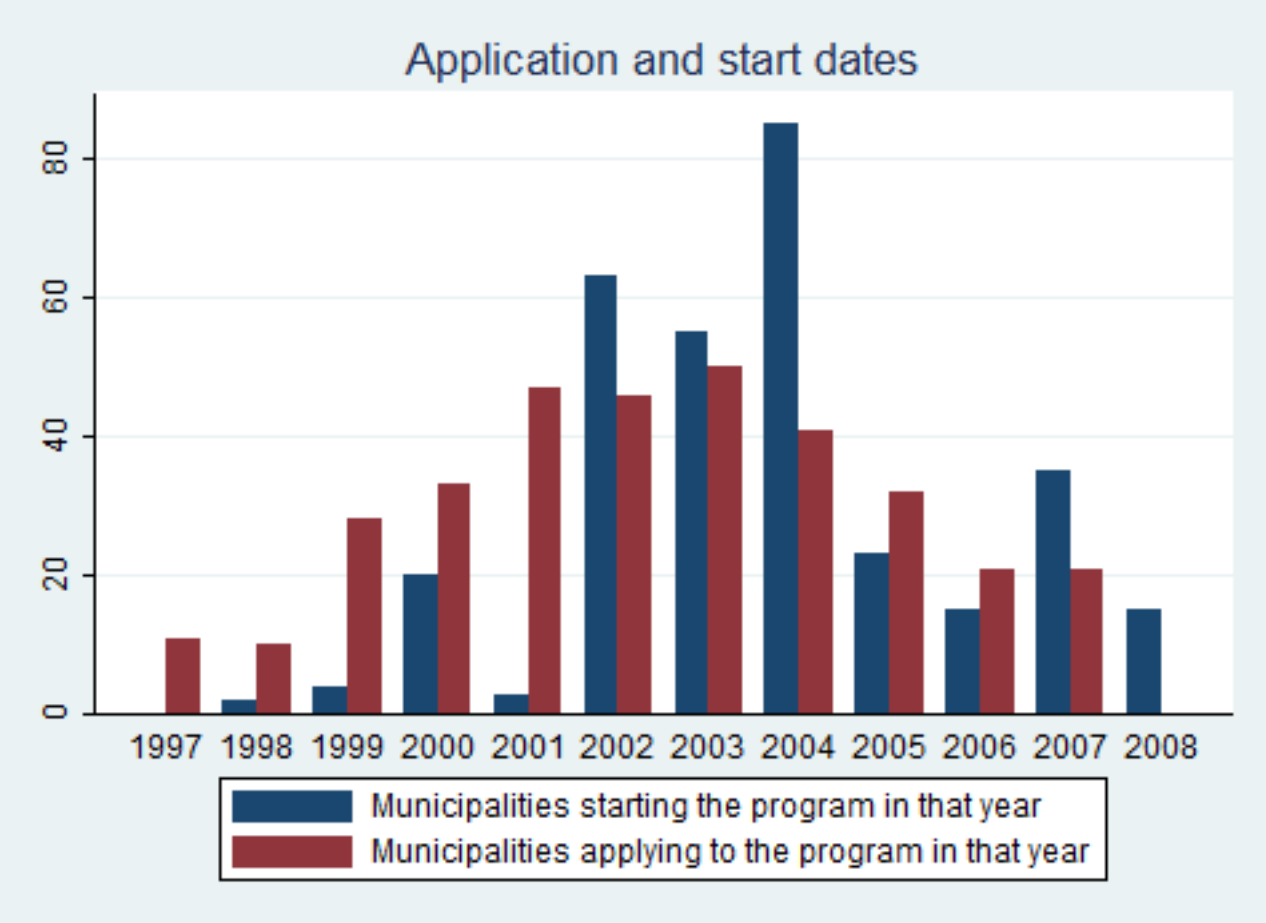


Table 16: Within municipality mean (standard deviation) in taxes and transfers, unweighted sample

	All	Controls	Treated before PMAT	Treated after PMAT
Taxes	70.1 (74)	65.1 (78)	90 (19)	142.9 (25)
[1em] Transfers	174.8 (48)	182 (77)	106 (17)	129 (22)