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LOSING TO GAIN: REVENUE SHORTFALLS AND FISCAL CAPACITY IN BRAZIL

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ABSTRACT

This paper investigates how shocks to non-tax revenues impact tax collection in Brazilian municipalities, exploiting an exogenous shift in intergovernmental transfers driven by population updates. Our analysis reveals asymmetric effects of shocks: revenue gains lead to increased spending without tax reductions, while losses in transfers prompt investments in fiscal capacity and boost tax revenues. Enhancing fiscal capacity entails adjusting tax bureaucrat payments, improving property registries, and cracking down on delinquency, with heterogeneous responses based on political competition and the educational levels of local leaders and the bureaucracy. These findings emphasize the importance of rules that reduce the reliance on non-tax revenues and promote effective tax collection.

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1 Introduction

The effective collection of taxes by states and their allocation towards public services are widely recognized as fundamental drivers of economic development (Kaldor, 1963; Besley & Persson, 2011). Nevertheless, many developing nations encounter challenges in garnering substantial tax revenues from their economic activities. Existing literature attributes this phenomenon to a significant reliance on foreign aid and natural resource rents, which, as non-tax revenues, diminish the impetus for governments to modernize their tax administrations, enforce taxation laws, and leverage third-party information sources (Zhuravskaya, 2000; Ross, 2001; Bornhorst *et al.*, 2009; Knack, 2009; Jensen, 2011; Moore *et al.*, 2018). Despite these insights, empirical evidence supporting this assertion remains limited.

In this paper, we examine whether the reliance on non-tax revenues influences the incentives for local tax collection, with a specific focus on the Brazilian context. Local governments in Brazil receive substantial intergovernmental transfers from the central government to fund public services and possess autonomy in determining their local taxes. Figure 1 depicts the relationship between tax revenues per capita and formula-based transfers per capita for Brazil's local governments. Similar to what is observed in cross-country comparisons, local governments in Brazil that receive larger intergovernmental transfers tend to collect lower taxes. However, interpreting this relationship as causal is challenging due to the endogeneity of transfers to local institutions, the quality of the bureaucracy, and political alliances.[1]

We leverage significant and unexpected changes in intergovernmental transfers resulting from population updates to shed light on the causal impact of non-tax revenues on local tax collection. Like many countries, Brazil uses formula-based intergovernmental transfers to finance local governments. The amount transferred from the federal government to municipalities varies discontinuously at specific population thresholds.[2] While population censuses are used to determine intergovernmental transfers, they are only conducted every ten years. In between census years, formula-based transfers are determined using projected population figures. However, Brazil's statistical office occasionally conducts population counts between censuses, resulting in updates to local population estimates that may push municipalities below or above the discontinuous thresholds. Consequently, some local governments unexpectedly gain resources, while others experience reductions in fiscal transfers. We exploit this variation to examine whether such changes affect local tax collection efforts.

Our empirical strategy builds upon Suárez Serrato & Wingender (2016), utilizing the outcome of the 2007 population count and the resulting discontinuity in formula-based transfers as an exogenous source of variation. We designate municipalities that crossed a population-based threshold and consequently received different levels of transfers starting in 2008 as the treatment group. We instrument the transfer shock with the interaction between the treatment indicator and

[1] For additional insights, see Knight (2002), Johansson (2003), Dahlberg *et al.* (2008), and Brollo & Nannicini (2012).

[2] For other applications of such formula-based discontinuities, see Brollo *et al.* (2013), Litschig & Morrison (2013), Gadenne (2017), and Corbi *et al.* (2019).

a dummy variable indicating the post-updated period. To examine whether treated and control localities have similar pre-trends, we also estimate reduced-form models using an event-study design that compares localities that changed their population bracket after the population count with localities that stayed in the same population bracket. While most previous studies look at the effects of transfer changes, we differentiate between positive and negative revenue shocks because the impact of winning versus losing fiscal revenues can be asymmetric.

We use this research design to answer three questions: 1) how changes in intergovernmental transfers affect tax revenues, 2) whether losses in revenues lead municipalities to invest in fiscal capacity (e.g., crackdown on delinquency, hiring bureaucrats, improving cadasters), and 3) what is the role of mayor, bureaucracy and municipal characteristics in allowing for the response in fiscal capacity.

We begin our analysis by showing that population updates affect the value of transfers received by municipalities after the shock. On average, municipalities that move to lower population brackets experience a 9 percent reduction in transfers compared to those that remain in the same brackets. Conversely, municipalities moving to higher population brackets witness an average increase of 18 percent in transfers. Event-study figures reveal a sharp and sudden decline (rise) in intergovernmental transfers for municipalities hit by a negative (positive) shock starting in 2008, the year following the population count, with no observable trends before that.

Next, we investigate whether governments alter their fiscal capacity in response to unexpected changes in the transfers received from the federal government. Municipalities facing adverse shocks experience a significant increase in tax revenues in subsequent periods, nearly offsetting the losses through local tax collection efforts. In our instrumental-variable specification, which employs the shock indicator as an instrumental variable for transfers, a loss of R$ 1 in transfers is compensated by a gain of R$ 0.97 in tax revenues. This implies an approximate 43 percent increase in tax revenues compared to pre-shock levels. Local governments respond by augmenting real estate and service tax revenues. The results differ considerably for localities experiencing positive shocks, as they tend to increase government spending without reducing tax collection. This asymmetry further reinforces previous findings regarding governments' asymmetric response to intergovernmental transfers (Hines & Thaler, 1995). It is also consistent with how fiscal adjustment episodes are implemented in Latin America by increasing tax revenues instead of cutting government spending, especially when politicians must consolidate their budgets under severe economic circumstances (Ardanaz *et al.*, 2020).

There are multiple avenues for governments to enhance their tax revenues, including cracking down on delinquency rates, investing in monitoring to improve compliance, adopting new technologies, or adjusting tax rates. In many municipalities across Brazil, property tax delinquency rates are high. In some municipalities, 30 to 50 percent of urban property owners do not pay the property tax (Carvalho Jr, 2017). There is also evidence that cadasters are not up to date in many localities, and the use of new technologies is limited.

Although comprehensive data on taxpayer compliance is unavailable, we use data on the quantity of registered properties and service taxpayers. We find that municipalities with a more extensive initial tax base for property and service taxes are more successful in increasing tax revenues after the negative revenue shock. In contrast, those with a limited tax base do not exhibit similar improvements. This suggests that local governments actively target high delinquency rates to boost tax revenues following economic downturns.[3]

We also test whether municipalities invest in tax infrastructure to improve fiscal revenues. Leveraging data from Brazil's Statistical Office (IBGE) annual municipal government survey, we use information about tax collection tools and technologies before and after the revenue shock. We find that adverse fiscal shocks positively affect municipalities' probability of using a property price register. However, we do not observe a corresponding increase in adopting more advanced technologies, such as a digitized register, perhaps because most municipalities already had their records digitized.

To assess whether municipalities increase tax revenues by hiring new bureaucrats such as tax auditors or introducing financial incentives, we use Brazil's employer-employee dataset. We measure the number and the wage bill of bureaucrats employed in occupations associated with tax collection: tax auditors and tax technicians. While we do not find strong evidence indicating that municipalities adjust taxes by hiring tax workers following a negative revenue shock, we find a positive and significant effect on the wage bill of tax auditors. This suggests that municipal governments use monetary incentives to enhance tax enforcement by bureaucrats.[4]

Previous research has highlighted the significance of political incentives, social structures, and institutional quality in shaping fiscal capacity (Besley & Persson, 2011). We examine the impact of political incentives on the response to budgetary shocks by considering two measures: term limits and political competition. Mayors facing term limits have less to gain from investing in fiscal capacity, thus leading us to anticipate a weaker response to sudden revenue losses. Conversely, mayors operating in more politically competitive environments allocate resources to secure reelection, potentially resulting in increased efforts in tax collection. To test these hypotheses, we compare the reactions of mayors with and without term limits and those elected in more versus less competitive districts in response to the loss of federal transfers.[5] Our analysis reveals no significant differences in tax revenues in localities where mayors face term limits. However, we observe that municipalities with higher political competition intensify their tax collection efforts following a revenue slump.

Another factor that may impact the capacity of local governments to respond is the quality of the mayor and the local bureaucracy. Mayors with higher levels of education are likely to possess enhanced problem-solving abilities in devising various tax collection strategies. We use education as a proxy for the mayor's and the bureaucracy's quality (Besley *et al.*, 2011). Our findings

[3]We cannot rule out the possibility that municipalities adjust their tax rates to increase revenues, but qualitative evidence suggests that local governments rarely use tax rate increases as a fiscal adjustment tool due to the potential political costs associated with such actions (Oliveira Junior, 2014).

[4]For insights on performance pay for tax collectors in Brazil, refer to Kahn *et al.* (2001).

[5]For evidence on the influence of term limits on policies in Brazil, refer to Ferraz & Finan (2011).

indicate that following an adverse fiscal shock, mayors with a college degree and bureaucracies comprising more educated public servants exhibit increased tax revenues. Finally, we investigate whether social structures affect the government's ability to generate higher tax revenues. It has been previously argued that higher inequality and social fragmentation make incumbent politicians unwilling to invest in the fiscal capacity that can be used to redistribute to other groups in the future Alesina *et al.* (1999). It has also been argued that informality makes it harder for governments to tax workers and firms (Gordon & Li, 2009; Besley & Persson, 2014). However, we do not find significant differences in the capacity to increase taxes across municipalities characterized by varying levels of inequality or the proportion of informal sector workers.

This study adds to the growing literature that seeks to understand how tax revenues can be increased in low-income countries. Recent research has highlighted factors such as the identity and assignment of tax collectors and their monetary incentives (Khan *et al.*, 2016; Balán *et al.*, 2022; Bergeron *et al.*, 2022). Furthermore, policies that foster third-party reporting incentivize the adoption of new technologies, or expand taxpayer offices also contribute to higher tax revenues (Pomeranz, 2015; Carrillo *et al.*, 2017; Gadenne, 2017; Brockmeyer *et al.*, 2019; Naritomi, 2019; Basri *et al.*, 2021; Dzansi *et al.*, 2022; Bergolo *et al.*, 2023; Okunogbe & Santoro, 2023).[6] Our results suggest that many of these efforts might not materialize if governments have access to large flows of aid or transfers.

Our results are also closely related to work that examines how local governments adjust their budgets after revenue shocks or changes in fiscal rules. Helm & Stuhler (2023) shows that a negative revenue shock for local governments in Germany triggers an immediate adjustment in government spending, primarily through a reduction in public investment.[7] Carreri & Martinez (2024) examine the effects of imposing a fiscal rule on Colombian municipalities and find that municipalities comply with the fiscal rule primarily by cutting expenditures. Our results, in contrast, show that municipalities adjust their budget after a negative revenue shock by increasing tax revenues rather than cutting spending, similar to what has been found by Grembi *et al.* (2016) and Alpino *et al.* (2022) for Italy. However, unlike those papers, we document how adverse shocks trigger local governments to invest in efforts to collect taxes. These results differ from Corbi *et al.* (2019), who use the discontinuity thresholds and find that localities that receive more revenues collect more taxes. Their estimates, however, do not distinguish between positive and adverse fiscal shocks, nor do they use unexpected shocks to examine how local governments adapt.

Our paper also builds upon the work of Gillitzer (2017), who investigates the role of macroeconomic income shocks in prompting changes to tax structures by U.S. state governments during the Great Depression. He examines variations in income shocks across states. He finds that a decline in income between 1929 and 1933 is associated with a higher likelihood of a state introducing a retail sales tax. Nevertheless, his study is limited to a cross-section of 48 U.S. states. In contrast, our research expands on this work by examining the effects of an exogenous revenue shock using

[6] See Okunogbe & Tourek (2024) for a review on the use of new information technology tools and the role of tax officials.

[7] They also find effects on taxes in the short-run that fade away in the longer-run. This suggests that for the case of Germany, the adjustment does not occur through changes in fiscal capacity.

an event-study design on approximately 2,500 municipalities in Brazil.

Finally, our study also contributes to the work that links fiscal capacity to political incentives (Besley & Persson, 2013). Our findings indicate that, under normal circumstances, politicians with access to substantial intergovernmental transfers may resist imposing taxes on the local economy. However, when faced with a significant fiscal downturn, local governments actively invest in strengthening their tax capacity. Our research reveals heterogeneous responses based on the characteristics of mayors and bureaucracies and political competition, which align with some of the theoretical predictions of Besley & Persson (2011). Our finding also supports the literature pioneered by Drazen & Grilli (1993), who argue that crises can serve as catalysts for reforms as inefficient policies become increasingly costly during turbulent times.

The rest of the paper is organized as follows. Section 2 discusses Brazil's institutional background and provides some stylized facts. Section 3 presents the data and describes the identification strategy. Results are discussed in Section 4, and Section 5 concludes.

2 Public Finance in Local Governments

The municipal government structure in Brazil comprises over five thousand municipalities across 26 states, each managed by an elected mayor and a city council. Mayors serve four-year terms with the possibility of two consecutive terms, while city councilors also serve four-year terms without term limits. Brazilian local governments are responsible for providing essential public goods and services such as education, infrastructure, and healthcare. The funding for these services primarily comes from intergovernmental transfers from states and the federal government, supplemented by local tax revenues.

Under the Brazilian federal constitution, municipalities have significant autonomy in collecting revenues. With city council approval, they have the authority to implement and set rates for three tax instruments and various public fees. The tax instruments include a tax on services (ISS), an urban property tax (IPTU), and a tax on the transaction of real estate properties (ITBI), while public fees cover activities such as road conservation, cleaning, lighting, and issuing construction licenses. Additionally, municipalities retain a portion of the federal income tax levied on the remuneration of local public servants (IRRF).

The tax on services (ISS) contributes almost half of the local tax revenues and is levied based on the service price. The municipal level determines the tax rate, but federal laws impose 2 to 5 percent limits. On the other hand, municipalities have full autonomy to set the rates for the urban property tax (IPTU), which typically ranges from 0.3 to 1.5 percent for built properties and constitutes approximately 25 percent of total municipal tax revenues.[8] The remaining tax instruments, ITBI and public fees, account for approximately 16 to 20 percent of tax revenues. Unlike the ISS, no federal laws govern the ITBI rates and public fees. The executive branch proposes and negotiates

[8]This is below the average for other Latin American countries in terms of rates and relative importance for local revenues (Carvalho Jr, 2006).

these rates with the local legislative.

The effectiveness of tax instrument design and tax payment enforcement relies significantly on the mayor's office. However, local administrations often do not fully utilize their tax powers. For instance, non-compliance with the IPTU is estimated to be over 20 percent in around 85 percent of municipalities, mainly due to factors such as a lack of registered taxpayers, limited tax audits, and lenient penalties imposed by municipalities.[9] Therefore, rather than relying on increasing tax rates, Brazilian local governments have substantial potential to enhance tax collection by improving tax administration.

The primary source of revenue for Brazilian municipalities is a federal transfer called the *Fundo de Participação dos Municípios* (FPM). This transfer is formula-based and financed by the income and industrialized products taxes. The allocation of FPM funds to municipalities is determined by a formula that considers local population estimates and the resources allocated to the state where the municipality is located. The allocation mechanism consists of two steps. First, a fixed share (unchanged since 1991) of the total FPM funds is assigned to each Brazilian state. Second, 18 population brackets are defined, each with an associated coefficient that varies non-linearly between 0.6 and 4. Municipalities are assigned to a population bracket based on their number of inhabitants, and all municipalities within the same bracket in the same state receive the same amount of FPM transfers, regardless of the precise population size.

The population coefficients are defined by Brazil's Statistical Office *Instituto Brasileiro de Geografia e Estatística* (IBGE) using population counts. However, considering that Brazilian censuses are conducted every ten years, in the inter-censuses period, the IBGE constructs local population estimates taking into account past censuses data on birth, mortality, and immigration rates. Once the population estimates are produced, the IBGE sends the estimates to the *Tribunal de Contas da União* (TCU), which is the federal agency in charge of determining the population bracket and, therefore, the associated coefficient that each municipality will have. As a general rule, the TCU announces the FPM coefficients for all municipalities by November of year *t-1*, municipal legislatures vote their budget by December of year *t-1*, and the FPM funds are transferred to municipalities along the following year *t*.[10]

Combining the local population estimates with the share of total funds that each state has, the allocation mechanism can be defined as:

$$FPM_{i,t}^{k} = \frac{FPM_{k,t} \times \lambda_{i,t}}{\sum_{i \epsilon k} \lambda_{i,t}}$$

Where $FPM_{i,t}^{k}$ is the amount of FPM transfers received by municipality *i* in year *t* in state *k*; $FPM_{k,t}$ is the total amount of resources allocated to state *k* in year *t*; and $\lambda_{i,t}$ is the FPM coefficient of municipality *i*. The formula above highlights an important characteristic that we exploit in our research design: municipalities in the same population bracket and same state receive the same

[9]See De Cesare & Smolka (2004); Afonso *et al.* (2013).

[10]See Appendix Table A.1 for a description of the population brackets, the coefficients associated, and the percentage variation of the related coefficients between consecutive brackets.

amount of FPM transfers independently of the precise number of inhabitants.

Figure 2 illustrates the relationship between FPM transfers and population from 2005 to 2012. The vertical dashed lines represent the population thresholds of the FPM allocation formula. Gray dots represent FPM transfers averaged overpopulation bins of 500 inhabitants, while red lines depict the smoothed mean of transfers for each population bracket. Notably, there are clear jumps in transfers at each population threshold, with variability within brackets due to different shares of FPM funds received by each state and overall growth in FPM funds over time. It is worth mentioning that total FPM resources increased in 2008 through a federal amendment, and Brazilian GDP per capita grew by an annual rate of 3 percent from 2005 to 2012.

Since 1992, national law has mandated population counts by the IBGE in inter-census periods to update population estimates more frequently. These counts are conducted every five years, but the implementation year can be adjusted for organizational or financial reasons. The timing of these counts significantly impacts municipalities' finances, as the population figures determine population brackets and associated coefficients. We focus on the changes resulting from the population count conducted in 2007, which falls within the inter-census period of 2000-2010.[11] In 2007, the IBGE conducted a population count in all municipalities with fewer than 170,000 inhabitants and 21 municipalities above that threshold, accounting for 97 percent of Brazil's municipalities. Households within these municipalities were surveyed from March 31 to April 1, 2007. The new population figure did not affect municipalities above the 170,000 population threshold as they were already in the highest FPM population bracket. In 2008, reflecting the new population figures from the 2007 count, 443 municipalities were switched to a lower population bracket while 403 municipalities changed to a higher bracket (see the negative and positive jumps across population brackets in Figure 2). In contrast, during non-census years, an average of 22 municipalities move to lower brackets and 190 to higher. With more than 5,000 municipalities in total, this implies that bracket changes are rarer in non-census years (more on this below). Figure A.1 shows the spatial distribution of shocks, which are widely spread across states.

3 Empirical Strategy and Data

3.1 Estimation Strategy

To examine the relationship between intergovernmental transfers and tax collection, it is crucial to address the potential sources of bias that might compromise the analysis. The first potential source of endogeneity is related to political factors. Certain jurisdictions may receive more favorable treatment in grant negotiations due to political factors. This could result in higher levels of intergovernmental transfers for these jurisdictions, which may influence their tax collection behavior. Similarly, political considerations might come into play during the design of the grant schedule, leading to biased allocation of transfers. For example, some jurisdictions could receive

[11] The 2000 census results became available in December 2001, impacting the 2002 FPM transfers, while the 2010 census results were released within the same year, thus affecting the 2011 FPM transfers.

more generous grants due to political favoritism (Knight, 2002; Johansson, 2003; Dahlberg *et al.*, 2008).

The second potential source of endogeneity occurs if local jurisdictions have specific socioeconomic attributes that influence their tax collection capacity and the allocation of intergovernmental transfers. For instance, jurisdictions with a higher tax base or greater fiscal capacity may be more likely to receive lower levels of transfers as they are perceived to have a lower need for financial assistance. Conversely, jurisdictions with lower socioeconomic indicators may receive higher transfers to compensate for their limited revenue-raising capacity (Gordon, 2004).

In Brazil, political favoritism plays an important role in the allocation of federal transfers to municipalities (Brollo & Nannicini, 2012; Litschig, 2012). Hence, previous studies have used the discontinuities in the FPM formula in a regression discontinuity setting to circumvent endogeneity issues (Brollo *et al.*, 2013; Litschig & Morrison, 2013; Gadenne, 2017; Corbi *et al.*, 2019). We adopt an alternative empirical strategy that relies on a difference-in-differences strategy and unforeseen positive and negative revenue shocks.

We build on the research design of Suárez Serrato & Wingender (2016), who use updates in the U.S. population census to estimate local fiscal multipliers. In our context, the results of the 2007 population count are used to update FPM coefficients in 2008. We assign a treatment status to municipalities that cross a threshold after the update of local population. We estimate separate effects for municipalities that were reshuffled negatively—crossed to a lower population bracket—and received fewer transfers, and municipalities that were reshuffled positively—crossed to a higher population bracket—and received larger transfers. Municipalities that have not crossed a threshold since 2002—when the previous census became effective for the FPM—are used as the control group. This research design allows us to estimate asymmetric treatment effects of winning versus losing fiscal transfers, which is not possible under a Regression Discontinuity Design that just considers the treatment as crossing a cut-off. Also, since this approach leverages a population count as a source of variation, it measures unanticipated shocks to intergovernmental transfers and mitigates concerns regarding manipulating population estimates around the discontinuity.

Due to data availability and to avoid incomplete mayoral terms, we limit the analysis to 2005-2012 since those eight years coincide with two full electoral terms of local governments. We hold the set of treatment municipalities fixed for the entire analysis period, independent of crossing another threshold, including municipalities that jumped back to their previous bracket after treatment. We follow this intention-to-treat approach as investments in fiscal capacity will still pay off, even though the effect of an increase or decrease in transfers vanishes due to another shift across population brackets.[12]

We begin our analysis by estimating the following difference-in-differences event study speci-

[12] In Brazil, municipalities are subject to strict fiscal regulations that limit their ability to run a fiscal deficit or incur debt. Under the Fiscal Responsibility Law (Complementary Law 101 of 2000), municipalities must maintain a balanced budget. For municipalities experiencing a negative shock, this requirement means cutting expenditures or finding alternative revenue sources, even when the shock is temporary. Appendix Table A.2 indicates that fewer than 35 percent of municipalities returned to their previous FPM bracket one year after the 2008 negative shock, underscoring the need for multi-year revenue compensation for most of them.

fication:

$$Y_{i,t} = T_i \times [\sum_{p=-2}^{-1} \pi_j \mathbf{1}(t - t^* = p) + \sum_{p=1}^{5} \beta_j \mathbf{1}(t - t^* = p)] + \omega_{i,t} + \gamma_t + \delta_i + \epsilon_{i,t} \quad (1)$$

Where T_i is an indicator that equals 1 if a municipality changed its FPM bracket due to the updated population in the 2007 population count and zero otherwise. Indicator variables $\mathbf{1}(t - t^* = p)$ represent the time since the population count was conducted in $t^* = 2007$. As such, coefficients π_j show the evolution of various budgetary outcomes Y before the shock (2005-2006), and the β_j show the evolution after the shock from 2008 to 2012. Coefficients are normalized to the omitted year 2007, which is the last period before the shock became effective.

We also estimate an alternative set of models where the budgetary outcomes Y are regressed on the value of the FPM transfers. We instrument the value of the FPM transfers with the interaction between the treatment indicator discussed above and a dummy variable indicating the post-updated period. In this Instrumental Variables approach, the first stage is given by:

$$FPM_{i,t} = \rho \cdot T_{i,t} + \omega_{i,t} + \gamma_t + \delta_i + \epsilon_{i,t} \quad (2)$$

The second stage is estimated using the following specification:

$$Y_{i,t} = \theta \cdot \widehat{FPM}_{i,t} + \omega_{i,t} + \gamma_t + \delta_i + \varepsilon_{i,t} \quad (3)$$

The estimated coefficient θ indicates how many Brazilian Reais taxes and other budgetary items react to one Real reduction (increase) in transfers. All specifications define the dependent variables in absolute values measured in millions of 2012 Brazilian Reais (R$). This accounts for the fact that the transfer shock (i.e., the first stage) should be specified in levels (rather than in logs), as this follows the allocation formula of the FPM law (Corbi *et al.*, 2019). Furthermore, given that treatment depends on changes in population counts, the effect could be confounded by population changes rather than the transfer shock if we used per capita values based on annual population.

All specifications include municipality fixed effects δ_i to account for time-invariant factors that determine political and economic conditions at the local level and a complete set of year fixed effects γ_t to control for national developments that could affect outcome variables, such as federal tax policies and the business cycle. Furthermore, we follow Corbi *et al.* (2019) and include cutoff-year fixed effects $\omega_{i,t}$ in all our specifications, which account for different trends across municipalities of different sizes.

We subsequently add further controls and fixed effects to this baseline specification. First, our preferred specification includes a trend variable for jumps experienced by municipalities in the treatment group before and after the shock. This variable is the accumulation of positive and negative shifts across thresholds before and after 2008. For example, for a treated municipality that jumped positively in 2005 and negatively in 2010, this variable will be one from 2005 to 2007, zero in 2008 and 2009, and -1 from 2010 to 2012. In addition, we show that results are robust to the inclusion of mayors' characteristics, state-year fixed effects, and population figures. Standard errors are clustered two ways at the micro-region and state-by-year level.[13] This clustering considers that nearby municipalities may share economic and migration connections. Note that this first cluster also nests the municipality level, allowing for arbitrary correlation of errors over time. The state-by-year level allows for the correlation between standard errors by municipalities in the same state for a year. This is essential as the total sum of annual transfers is divided between all municipalities within one state.[14] The Appendix shows results are robust to clustering standard errors at the municipality, micro-region, or state level among several alternative levels.

3.2 Data and Sample

We combine data from various sources. First, we use the population estimates that determine the intergovernmental transfers. These estimates are obtained from the Brazilian Institute of Geography and Statistics (IBGE) and the Federal Court of Accounts (TCU). Data on municipal public finances, including FPM transfers, are available from *Finanças do Brasil* database (FINBRA). FINBRA is a dataset compiled by the National Treasury that contains self-reported information about local budgets. It has yearly accounting records from all Brazilian municipalities and includes disaggregated data on revenues and expenditures.

Appendix Table A.3 reports statistics for municipal population and public finances, grouped by population brackets. Almost 90 percent of Brazilian municipalities are in the first eight population brackets, which include cities with less than 50,000 inhabitants. As previously discussed, this group highly depends on other government-level resources. The federal transfer FPM is the most critical source of local revenues, accounting on average for more than 35 percent, followed by state transfers and other federal transfers, representing on average 24 percent and 13 percent, respectively. Unlike large municipalities (9-18 brackets), in which tax revenues represent, on average, up to 20 percent of local revenues, in most Brazilian municipalities, local tax revenues only represent a small fraction of total revenues (10 percent or less).

We exclude three groups of municipalities from the primary analysis: state capitals, considering that they are subject to a different formula of the FPM transfer; municipalities below 3,972 inhabitants and above 179,060 inhabitants, considering that within this population range none of the municipalities ever crossed a threshold; and municipalities with missing values in the main regressor of interest and outcome variables.[15] Table 1 presents summary statistics for the budgetary variables of the sample we use in the analysis. Note that expenditures and revenues are, on aver-

[13] Brazilian states are geographically divided into meso-regions and micro-regions. According to the Brazilian Institute of Geography and Statistics, meso-regions consider municipalities that share similarities in social characteristics and geography. Micro-regions add to this criterion, similarities in economic characteristics and spatial interaction. On average, there are 21 (5) micro-regions (meso-regions) per state and 9 (36) municipalities per micro-region (meso-region).

[14] Note, however, that treatment status is defined at the municipality level and is independent of the state in which a municipality is located.

[15] In this way 32.5 percent of Brazilian municipalities are excluded from the main analysis: 0.5 percent are state capitals; 19 percent are municipalities outside the aforementioned population range; and 13 percent are municipalities with missing values. We present a set of robustness checks in the Appendix that include all municipalities except for state capitals. Results are robust to their inclusion.

age, very similar as municipalities do not have access to borrowing. Furthermore, the breakdown of revenues highlights the importance of the FPM formula transfer, which is about a third of total revenues. While taxes represent a much smaller share of total revenues, we observe substantial variation in this variable. The standard deviation is about three times the mean. This indicates that the degree to which municipalities rely on their tax revenues varies substantially.

We merge our budgetary data with detailed information related to several variables that may affect the fiscal capacity of local jurisdictions, either via technology or additional tax auditors and tax technicians, by using data from the *Relação Anual de Informações Sociais* (RAIS) and the *Pesquisa de Informações Básicas Municipais* (MUNIC). RAIS is a yearly administrative dataset compiled by the Ministry of Labor that includes labor market contractual information, and it covers roughly all formally employed workers from the private and public sectors. We aggregate employment information at the municipal level, considering only local government employees. The detailed occupation data from RAIS allows us to identify two categories we define as tax workers: tax auditors and tax technicians.[16]

Table 1 shows the average number of tax workers per municipality. The intensive margin is around 2.3, and at the extensive margin, the table documents that almost half of the municipalities do not report employing any tax workers. The substantial variation in the number of tax workers and the total salaries paid by local governments to these employees—wage bill—is consistent with the variation in tax revenues described above.

Regarding tax collection technology, we use the IBGE MUNIC database which contains information on municipal public institutions' structure, technology, and operation. MUNIC allows us to examine three tax collection tools. We study the local administration's use of a real estate cadastre, a property price register, and a service provider register. The first two tools are relevant for collecting property-related taxes (IPTU and ITBI), while the latter is mainly used for the service tax (ISS). Between 2006 and 2009, the years for which we have information, approximately 95 percent of municipalities in our sample report a real estate cadastre that covers legal properties, 78 percent a property price register that values legal properties, and 89 percent a service provider register that contains a list of legal self-employed professionals and service-related businesses.

Table 1 also presents general summary characteristics of the local context. We have characteristics of the mayors and information about elections from Brazil's Superior Electoral Court (TSE). We computed the education level of bureaucrats from RAIS, and municipality socioeconomic characteristics were obtained from the IBGE censuses (2000 and 2010). We observe that mayors are, on average, 49 years old, almost 50 percent of them have a college degree, approximately 58 percent are in their first term in office, and 10 percent are affiliated with the same political party as the president at the time (PT—Labor Party). The average municipality has 12 percent of its local

[16]Tax auditors and tax technicians are described by the CBO2002 (*Classificacão Brasileira de Ocupacões)* of the Brazilian Ministry of Labor and Employment as: "They supervise compliance with tax legislation; constitute the tax credit through launching; control the collection and promote the collection of taxes, applying penalties; analyze and make decisions on administrative and tax proceedings; control the transaction of goods and services; assist and guide taxpayers, and also plan, coordinate and direct tax administration offices." ISCO-88 equivalent classification: Tax Collector; Inspector of Taxation; Excise Officer; Tax Examiner.

bureaucrats with higher education, less than 3 candidates in mayoral elections, and about 18,000 inhabitants.

The budget data is characterized by substantial outliers in some periods. Some might be data errors, as municipalities make mistakes when reporting their budgets to the statistical office. Others might be actual values, as in some rare cases, municipalities receive exceptionally high revenues. For instance, the municipality of Manoel Urbano, from the state of Acre, had a massive increase in local tax revenues due to the construction of the BR-364 national road. Before the construction, the average yearly collection of tax on services (ISS) was around 70 thousand Reais. During the primary years of construction, however, the taxes levied on paving services increased the ISS revenues to an average of 2 million Reais (Ferrer, 2013). For observations where revenues increased or decreased by more than 100 percent yearly, we interpolate the values of these extreme outliers. Under this definition, for example, 1.04 percent of tax revenue observations are classified as outliers. The Appendix reports all results without adjusting for outliers, showing that the findings remain robust but become less precise when these outliers are retained.

3.3 Exogeneity of the Population Update

The validity of our identification strategy relies on the exogeneity of the population update, i.e., the update should not be anticipated by the local administration. This identification assumption would be violated if time-invariant characteristics of certain municipalities drive measurement errors in population figures—which have to be corrected by the census—and might be determinants of budgetary outcomes (Suárez Serrato & Wingender, 2016). For example, specific local characteristics could systematically attract individuals, which yields larger measurement errors as long as population projections do not adequately account for internal migration. If those individuals are high-skilled workers, the tax base might increase and drive revenues upwards. If they are low-skilled, expenditures for social assistance might increase instead. We follow Suárez Serrato & Wingender (2016) to provide evidence that this is not likely to be the case for census updates in our context. If those time-invariant features were to drive the census shock, then the population updates of two consecutive censuses should be serially correlated. Figure 3 shows a scatter plot of the population update in 2007—that was used to define the FPM population bracket in 2008—versus the previous one in 2001—that was used to define the FPM population bracket in 2002—after controlling for cutoff and state fixed effects. The flat slope confirms that there is no serial correlation between the two censuses and that, indeed, a random measurement error is the source of variation, which we exploit for identification.

To provide further evidence for the validity of our identification strategy, we run the following linear probability model:

$$T_i = \beta\, \mathbf{Z_i} + \omega_i + \sigma_s + \varepsilon_i \tag{4}$$

where the outcome variable T_i is a dummy equal to one if a municipality changed its 2008 FPM bracket due to the 2007 population count and zero otherwise; and $\mathbf{Z_i}$ represents a large set of observable characteristics of the municipality. We use the same sample as in our main analysis and differentiate between the two types of treatment. Given the cross-sectional nature of this exercise, we include cutoff (ω_i) and state (σ_s) fixed effects as in all subsequent models in the main part of the analysis.[17]

Appendix Figure A.2 presents the results. The first set of variables are related to the local population and are expected to be significant as they mechanically determine the treatment status. We find that a larger (projected) pre-census population in 2006 and a population growth rate decreased the probability of having a positive shock and increased the probability of receiving a negative shock. We also find that being close to a threshold increases the probability of belonging to either treatment group. All these variables indicate that the error in population projections correlates with the shock. While this is expected, more relevant for our design is that most other variables do not show significant correlations with the treatment indicators or are small in magnitude. We do not find strong evidence that political alignment (with both the federal or state government), electoral term, education, gender, or party affiliation of the mayor are correlated with the positive or negative shock. Significant determinants of the positive shock are the mayor's age, the share of adults with college degrees, and the tax share. For the negative shock, only the presence of a municipal radio station and a zoning law decrease slightly the probability of receiving it. Since this large set of observables does not exhibit strong correlations with treatment status, we assume that unobservable factors are also unlikely to do so. Furthermore, the explanatory power of the models is primarily driven by fixed effects, while budgetary and political variables, along with other characteristics, contribute minimally (Appendix Figure A.3). Population, yet, has some predictive power. One possible threat to our identification is that mayors might be able to anticipate actual population changes by accessing reliable and up-to-date demographic information about their municipalities. However, in a setting where over 50 percent of the urban population lacks property rights to their homes (Christensen & Garfias, 2021), tax non-compliance among registered properties exceeds 20 percent (De Cesare & Smolka, 2004; Carvalho Jr, 2017), and more than 25 percent of the employed population works informally, it is unlikely that mayors can accurately anticipate population updates.

4 Results

4.1 First Stage Results: Census Updates and Changes in Intergovernmental Transfers

We begin our analysis by examining the impact of an unexpected population update on FPM transfers, as illustrated in Figure 4 and detailed in Table 2. Figures 4(a) and 4(b) present the coefficients derived from estimating Equation 1, separating the results for negative and positive population updates. These figures distinctly highlight the shift in FPM revenues occurring in 2008, coinciding

[17] State fixed effects are nested in municipality fixed effects, which, due to the cross-sectional nature of this analysis, cannot be included here.

with the implementation of the population update, with no discernible trends observed before the event.

Table 2 reports the average effect estimated through a difference-in-difference model. Panel A showcases a reduction in transfers within jurisdictions affected by a negative shock, amounting to approximately R$ 700,000 relative to the control group. This reduction translates to a per capita decrease of R$ 40—equivalent to 2.8 percent of pre-shock total revenues—or approximately 9 percent of pre-shock transfers. Conversely, Panel B shows an increase in FPM formula transfers by approximately R$ 1.3 million in response to a positive population shock. This increment corresponds to a per capita rise of R$ 86—representing 5.9 percent of pre-shock total revenues—or 18 percent of pre-shock transfers.

In addition, Table 2 presents these first-stage effects across various specifications. The initial column incorporates fixed effects for municipality, year, and cutoff-year. In Column 2, adjustments are made to account for discontinuities around thresholds in years other than 2008, resulting in a slight reduction in the observed impact. This model serves as our baseline for subsequent analysis. Moving to Column 3, we introduce controls for mayors' characteristics, including gender, age, schooling, term in office, political party affiliation, and alignment with higher-level governments. Notably, these additional controls leave the coefficients largely unaffected. Finally, Column 4 incorporates state-year fixed effects providing further control for state-year-specific characteristics. Despite these adjustments, the qualitative findings remain consistent across all specifications, with minimal changes observed in the magnitudes of the coefficients.

4.2 Responses of Local Tax Revenues

The response of local tax revenues to the transfer shock is documented in Table 3. Panel A presents negative shock results and reports a surprisingly significant increase in tax revenues, which almost offsets the loss in the FPM formula transfers. The IV results of our preferred specification (cell 2a) indicate that a R$ 1 reduction in transfers translates into an additional R$ 0.97 of tax revenues. This implies an increase in tax revenues of approximately 43 percent, which indicates a substantial response of this variable in the adjustment process after a negative shock. Instead, panel B indicates a precisely estimated zero effect for the positive shock.

These results document two important findings. First, positive and negative shocks create asymmetric reactions; second, adverse shocks trigger tax revenue increases to offset the loss of transfer revenues. The zero effect for the positive shock is consistent with a flypaper effect. From a representative citizen perspective, the increase in transfers should partially be rebated to citizens through tax decreases (Inman, 2008), but several empirical studies document spending increases (see Dahlberg *et al.* 2008 for an example) of similar magnitude as the transfer windfall. We document this effect in Appendix Table A.4.[18] This table also shows further results for the negative

[18]The IV estimate of model 1b in Panel B of Table A.4 suggests that R$ 1 increase in transfers yields extra spending of approximately R$ 1.5 or 7 percent in pre-shock expenditures, indicating that additional transfers are used to expand the budget on the expenditure side.

shock: Panel A of Table A.4 indicates positive changes in other transfers—albeit statistically non-significant, which can be driven by either local initiatives to apply for more discretionary funding or bail-out policies of higher levels of government.[19]

Besides the aggregate estimates, the estimated event studies provide insights about the adjustment process. Figure 5(a) presents the coefficients obtained from the event study analysis of total tax revenues after the negative shock. Results indicate an increase in impact during the year after the shock came into effect, and a further increase two years after. By 2010, tax revenues had grown by approximately R$ 900 thousand. This pattern is consistent with investments in fiscal capacity, which materialize with some delay into additional tax revenues. The event study also confirms the absence of pre-trends, as all coefficients before the shock are close to zero.

The same figure documents effects for individual tax instruments by disaggregating total tax revenues in panels 5(b) to 5(f).[20] Property-related taxes, such as the urban property tax (IPTU) and the real estate transaction tax (ITBI), exhibit a consistent upward trajectory in their contribution to the overall effect. The observed increase in revenue follows a gradual pattern, suggesting potential growth in the tax base rather than rate effects. On average, a reduction of R$ 1 in transfers predicts R$ 0.16 increase in property tax revenues, representing a 27 percent rise compared to pre-shock levels. As for the real estate transaction tax, the results show a more considerable change of R$ 0.19, accounting for approximately 93 percent of the 2007 revenues, but remains at low absolute levels given the total tax revenues.

The service tax (ISS) makes the most considerable contribution to the overall tax effect, with an increase of approximately R$ 0.51 for every one-unit reduction in transfers. Initially, the additional service tax revenues contribute the most to the overall effect immediately after the shock. However, they account for only half of the extra tax revenues after five years. Additionally, public fees experience a 40 percent increase, while the local share of the federal income tax charged from public employees (IRRF) shows a positive but insignificant result. This outcome is expected since the local jurisdiction has no control over this tax instrument.

Appendix Figure A.4 provides robustness checks for the tax results using IV estimates. Firstly, we consider specifications that control for different combinations of threshold jumps in years other than 2008, mayors' characteristics, state-year fixed effects, and municipalities' population size. Secondly, we assess the precision of the estimates using different methods for clustering the standard errors. Thirdly, we address concerns regarding outliers and sample composition. Specifically, we report results for the baseline sample with outlier correction (referred to as "main analysis"), the raw data without outlier correction (referred to as "raw data"), an unbalanced panel that incorporates municipalities with missing values in either FPM transfers or tax revenues, and a sample that incorporates municipalities outside the population range of 3,972 to 179,060. Appendix Figure A.4 demonstrates that the results remain consistent across all model specifications, choices of standard

[19]This explains the increase in overall revenues reported in model 2a, which is partially attributed to the positive changes in other transfers and the gradual reshuffling of municipalities back to the pre-shock population bracket (see Appendix Table A.2).

[20]Average effects over the five years following the treatment are documented in Table 4.

error clustering, and sample variations. The lowest point estimate reported is 0.522.

Furthermore, Appendix Figures A.5 and A.6 present the primary event studies using the raw data without outlier correction. The main results obtained using data corrected for outliers are also included in a lighter color for comparison. These figures show that while the results may appear noisier using the raw data, on average, they remain qualitatively and quantitatively unchanged.

After establishing that municipalities compensate for the loss of intergovernmental transfers by adjusting their tax revenues rather than cutting spending, the remainder of the paper focuses on the instruments by which additional tax revenues are generated.

4.3 How Do Governments Increase Tax Revenues?

In this section, we examine different channels through which a municipality can increase its fiscal capacity to illuminate the mechanisms driving the increase in tax revenues after a negative shock.

Crackdown on delinquency rates In many municipalities across Brazil, property tax delinquency rates are high. In some municipalities, 30 to 50 percent of urban property owners do not pay the property tax (Carvalho Jr, 2017). Although comprehensive data on taxpayer compliance are lacking, we utilize information regarding the tax base to investigate whether municipalities that experience an increase in tax revenues are characterized by a significant number of registered properties and service taxpayers relative to their population. Should the uptick in tax revenues be primarily driven by regions with a substantial base of registered taxpayers, this would suggest that enhanced enforcement of tax payments is a key driver. Conversely, if increases are observed in municipalities with a smaller proportion of taxpayers, it is likely due to proactive measures to register additional taxpayers.

To test this hypothesis, we employ data from the annual municipal surveys conducted by IBGE, focusing on the quantity of registered properties and service taxpayers. We categorize municipalities based on the per capita number of registered properties and service taxpayers and assess the extent to which those with an extensive pre-existing tax base have successfully enhanced tax collection post-revenue shock. The results, illustrated in Figures 6(a) and 6(b), indicate that municipalities with a more extensive initial tax base for property and service taxes are indeed more successful in increasing tax revenues after the negative revenue shock. In contrast, those with a limited tax base do not exhibit similar improvements. This finding suggests that local governments actively target high delinquency rates to boost tax revenues following economic downturns.

Tax bureaucrats The capacity of local governments to reduce delinquency rates and increase their tax base depends on the local bureaucratic capacity and the motivation of public workers to increase tax collection efforts. We use Brazil's employer-employee data and occupation codes to examine how municipal governments employ bureaucrats in tax collection activities. Specifically, we construct measures to assess whether municipal governments have tax auditors and tax technicians among their employees. We then quantify the number of employees engaged in tax-related roles and analyze their wages.

Table 5 presents results using as dependent variables an indicator for whether municipalities have a tax worker, the number of tax workers, and the wage bill. We do not find clear evidence that the average municipality hires a tax worker (Panel A) or increases the number of tax workers after a negative revenue shock (Panel B). However, we find a positive and significant increase in the tax administration wage bill (p-value = 0.084). The IV estimate shown in Panel C suggests that R$ 1 reduction in transfers translates into an increase of about R$ 0.016 in the wage bill of tax workers—an increase of around 47 percent relative to pre-shock figures. The event study results (Figure 7) show no distinct trends before the shock and a rapid increase after 2008. Point estimates for the wage bill are significant at (or just above) 10 percent for most post-shock years but are generally imprecise for the number of tax workers. The findings suggest an upward adjustment of salaries or, more generally, monetary incentives to tax bureaucrats to improve tax enforcement. This result might reflect the use of performance pay for tax collectors, which is widely used across Brazil (see Kahn *et al.*, 2001 for example).

We conduct robustness checks of the wage bill results in Appendix Figure A.7. As before, we show the robustness of the IV estimates to different combinations of fixed effects and control variables, to clustering standard errors at different levels, and to various samples of municipalities. Apart from some specifications that include combinations of state-year fixed effects, most of the estimated coefficients are significant at (or just above) 10 percent.

Tax collection tools and technologies. Another potential mechanism to increase tax collection is to invest in information and technology. We examine the existence of administrative tools crucial in municipal tax collection in Brazil (Afonso *et al.*, 2013). One such tool is the real estate cadastre, which serves as the municipal administration's means of registering and documenting the characteristics and ownership details of real estate properties within its jurisdiction. Additionally, the tax administration utilizes a register of land and housing prices to estimate the market value of all municipal properties, employing an individual valuation model for each property. Another essential administrative measure is registering self-employed professionals and legal entities engaged in service-related activities, who must register as local service providers and fulfill their tax obligations accordingly. Before the revenue shock, approximately 95 percent of municipalities had a real estate cadastre, most utilizing IT-based technology (92 percent). In contrast, 75 percent had a property price register (79 percent utilizing IT-based), and 86 percent had a provider register (85 percent utilizing IT-based).

We gathered data from IBGE's annual municipal surveys. The waves of 2006 and 2009 include specific questions about tax collection tools and their technology (paper- or IT-based). We built a series of indicator variables for whether municipalities had a real estate cadastre, a register of land and house prices, or a register of local service providers. Since the information is unavailable annually, we collapse our data into one pre-shock and one post-shock period. In other words, we set the outcome variables equal to zero in the pre-shock (post-shock) period if the wave 2006 (2009) indicates that the respective tool was unavailable and equal to one otherwise. Likewise, we calculate the 2006-2007 and 2008-2009 federal formula transfer means for the instrumental variable estimates and impute the values to the pre- and post-shock periods, respectively.

Table 6 presents the results. In odd columns, we report estimates on having the register independent of the technology used, and in even columns, we consider exclusively IT-based registers (conditional on having the register). We do not find any clear evidence for fiscal capacity building by investing in IT-based infrastructure. However, we find that municipalities hit by a negative shock increase the probability of using a property price register by about 9 percentage points, or 12 percent, relative to pre-shock adoption. This result is consistent with the trend-break after 2009 previously observed in Figures 5(b) and 5(c), as the property price register is employed to define the tax base of the urban property tax and of the tax on the transaction of real estate properties. Last, Appendix Figure A.8 reports the robustness of the property price register results and shows that the effects are similar across estimations, except when the model does not consider past threshold jumps.

To sum up, municipalities affected negatively by the population update invest in fiscal capacity through monetary compensations to their tax employees, by adopting a system to assess the market value of local properties, and by cracking down on registered tax evaders. Different from Helm & Stuhler (2023), who studies German municipalities that are not directly in charge of the administration of local tax bases, a significant part of the adjustment in our context occurs through improvements in tax capacity. In many local governments across Latin America, most spending is earmarked for salaries of public employees and pensions. Thus, municipalities must crack down on bureaucratic inefficiencies and tax evasion (Ardanaz *et al.*, 2020).

4.4 Do Political Incentives, Institutional Quality, and Social Structure Matter?

A growing literature on fiscal capacity emphasizes the role played by political incentives, the quality of institutions, and the social structure (Besley & Persson, 2011, 2013). We now examine whether local characteristics affect municipalities' response to unexpected federal transfer reductions.

We begin by looking at political incentives and consider two measures: term limits and political competition. Mayors facing term limits have less to gain from investing in fiscal capacity because they will not be around to obtain the return on their investment. Also, mayors operating in more politically competitive environments want more resources available to secure re-election and thus put more effort into tax collection. To test these hypotheses, we compare the reactions of mayors with and without term limits and those elected in more versus less competitive districts in response to the loss of federal transfers. We measure political competition in mayoral races by the average number of candidates in the previous two elections to the shock.[21] In Figures 8(a) and 8(b), we show the coefficients estimated from our event-study specification. We do not find significant differences in the tax collection behavior after a negative shock for municipalities with or without a term-limited mayor. In both types of municipalities, mayors collect more taxes. We do, however, find significant differences based on political competition. In places where political competition has been historically high, tax revenues increase significantly after a negative revenue shock compared to places with low political competition.

[21] For evidence on the influence of term limits on policies in Brazil, refer to Ferraz & Finan (2011).

We next examine whether the quality of the mayor and the local bureaucracy matter in response to lost revenues. Mayors and bureaucracies with higher levels of education are likely to possess enhanced problem-solving abilities in devising various tax collection strategies. We use education as a proxy for the mayor's and the bureaucracy's quality following Ferraz & Finan (2009) and Besley *et al.* (2011). We use an indicator to determine whether the mayor has at least a college degree.[22] For bureaucrats, we use the share of municipal employees with a college degree. The estimated coefficients plotted in Figures 8(c) and 8(d) indicate that, following an adverse transfer shock, mayors with a college degree and highly-educated bureaucracies exhibit a significant increase in tax revenues while places with less educated mayors and bureaucracies do not change tax revenues.

At last, we examine whether social structures affect the government's ability to generate higher tax revenues. It has been previously argued that higher inequality and social fragmentation make incumbent politicians unwilling to invest in the fiscal capacity that can be used to redistribute to other groups in the future Alesina *et al.* (1999). It has also been argued that informality makes it harder for governments to tax workers and firms (Gordon & Li, 2009; Besley & Persson, 2014). In Figures 8(e) and 8(f) we show the event-study coefficients by different levels of inequality and informality. Places with lower inequality and a lower share of informality seem to be able to generate a higher increase in their tax revenues after the negative shock. However, the point estimates are not statistically different between high and low inequality and informality localities.

We complement the event-study figures with Table A.5, where we report the 2SLS estimates over the entire post-treatment. Columns (1) to (6) show the heterogeneous effects of the unexpected transfer change on tax collection. Similar to what we show in the event-study figures, localities with more political competition and a more educated mayor and bureaucracy respond more strongly to an unexpected change in the FPM transfers.

One might be concerned that some characteristics used to estimate the heterogeneous effects are not randomly assigned across municipalities. Thus, the analyzed characteristics could capture other features of the municipality that lead to a differential impact of the transfer shock on tax revenues. For instance, the mayor's education could serve as a proxy for the municipality's education level. If, after the shock, municipalities appeal to tax morale to increase tax revenues, places with more educated citizens might respond more to the message.

To test for potential confounds and to assess whether the differences in responses are jointly significant, we estimate a model that allows the shock to vary with all the characteristics previously reported and, in addition, with the political party alignment between the mayor and the Brazilian president, the average margin of victory in mayoral elections, and with the municipality share of adults with college education, literacy rates, per capita income, and urban population. Results are reported in column 7 of Appendix Table A.5. When the model allows the shock to vary with multiple characteristics, some of the results become weaker. However, they support the previous finding that electoral competition and the education level of the mayor seem to be essential elements behind the increase in after-shock tax revenues.

[22] Approximately 56 percent of treatment and 48 percent of control municipalities were run by mayors with a college degree during this period.

5 Conclusion

This paper examines whether municipalities that depend too much on non-tax revenues exert less effort to collect taxes. We use unexpected changes in intergovernmental revenues in Brazil, driven by changes in population estimates, to examine whether local governments that lose access to non-tax revenues have incentives to invest in fiscal capacity. We first document asymmetric effects of budgetary shocks: municipalities that unexpectedly gain intergovernmental transfers increase spending without tax reductions. In contrast, municipalities that lose transfers invest in fiscal capacity and boost tax revenues.

Our results shed light on the mechanisms driving fiscal capacity enhancement. We provide suggestive evidence that unexpected losses in non-tax revenues induce more considerable tax revenue gains in localities that already had a large tax base as a proportion of the population, suggesting that cracking down on tax delinquency is probably an important channel to boost tax revenues in a setting where changing tax rates is politically costly. We also provide indicative evidence that the increased effort in tax collection comes not from hiring and growing the tax bureaucracy, but instead incentivizing tax auditors through higher salaries.

One question that emerges is if municipalities can crack down on delinquency and incentivize tax bureaucrats, why weren't they doing this before the fiscal shock? One explanation could be that local governments are inefficient in tax collection, and crises can serve as catalysts for reforms, as Drazen & Grilli (1993) suggested. One piece of evidence that supports this theory is that the investments in fiscal capacity and tax revenues are driven by localities where political competition is high. Hence, the incumbent government would have more incentive to react to budgetary losses. Another explanation is that learning how to collect tax revenues and changing the bureaucracy is difficult and costly. Our results show that mayors with more education are the ones who can increase tax collection, which would support an explanation based on knowledge and managerial skills. While we cannot precisely test some of these explanations, future work should focus on understanding whether inefficient policies, such as low tax revenue, are driven by a lack of bureaucratic capacity, knowledge about policies, or misaligned electoral incentives.

Our findings also have important policy implications related to discussions of the optimal level of decentralization and how to implement transfer schemes that redistribute resources across localities. Our findings suggest that intergovernmental transfers may disincentivize governments from enhancing tax collection efforts. While formula-based transfers, designed to redistribute resources among diverse regions, serve a crucial role in mitigating regional disparities, they must be structured considering potential crowding-out effects on local fiscal capacity-building efforts.

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TABLE 1: SUMMARY STATISTICS

Variables	Mean	SD	Min	Max
Budgetary Items (R$)				
Expenditures	28,583.7	41,047.6	2,936.7	1,177,458.0
Revenues	29,183.2	42,154.5	2,905.6	1,004,522.0
– Transfer (FPM)	8,520.6	5,705.0	1,207.1	77,878.59
– Federal transfers (net of FPM)	3,981.0	10,075.6	0.0	445,793.9
– State transfers	7,189.2	13,951.9	0.0	375,314.8
– Tax revenues	2,733.1	8,978.5	0.0	315,664.3
– IPTU	645.1	2,852.8	0.0	95,773.7
– ITBI	253.1	728.6	0.0	23,143.6
– ISS	1,146.3	4,517.6	0.0	162,521.0
– Fees	300.4	1,179.1	0.0	56,839.9
– IRRF	321.9	822.7	-5.8	32,551.5
Tax Workers				
Number	2.3	5.3	0.0	213.0
Wage Bill (R$)	42.1	136.3	0.0	4992.4
Extensive Margin (%)	49.6	50.0	0.0	100.0
Tax Infrastructure (%)				
Real Estate Cadastre	94.8	22.2	0.0	100.0
Property Price Register	77.9	41.5	0.0	100.0
Service Provider Register	89.1	31.2	0.0	100.0
Local Government Characteristics				
Mayor Age (years)	48.7	9.4	21.0	90.0
Mayor Male (%)	91.4	28.1	0.0	100.0
Mayor College (%)	49.1	50.0	0.0	100.0
Mayor First Term (%)	57.5	49.4	0.0	100.0
Mayor Party Alignment Federal (%)	10.1	30.2	0.0	100.0
Mayor Party Alignment State (%)	21.3	41.0	0.0	100.0
Bureaucrats College (%)	12.2	8.9	0.0	88.6
Number of Candidates in Mayoral Elections	2.2	0.5	1.0	5.8
Margin of Victory in Mayoral Elections (p.p.)	15.5	16.6	0.0	100.0
Municipality Characteristics				
Population	18,557.4	21,887.2	3,168.0	193,134.0
Population College (%)	5.5	2.9	0.3	31.2
Population Literacy (%)	79.3	12.8	45.2	98.9
Gini index	55.0	6.5	33.0	87.0
Labor Market Formality (%)	73.5	10.1	26.4	99.1
Monthly Income pc (R$)	.422	.223	.078	2.042
Urban Population (%)	58.9	22.5	4.0	100.0

Notes: The table covers the positive shock and negative shock samples of municipalities used in the main analysis and includes observations of 2,950 municipalities. The categories budgetary items and tax workers show yearly statistics over the period 2005-2012. The category tax infrastructure shows yearly statistics over the years 2006 and 2009. The variables of the mayor show the characteristics of the mayors that sat in office during the 2009-2012 term (the winning candidates of the 2008 election). Bureaucrats college shows yearly statistics over the period 2008-2012. Number of candidates and margin of victory in mayoral elections show the average statistics of the 2000 and 2004 elections. Population shows yearly statistics over the period 2005-2012. Population college and population literacy show statistics from the 2010 census. Gini index, labor market formality (measured as 1 minus shared of employed individuals without a formal contract), monthly income pc, and urban population show statistics from the 2000 census. All monetary values are in thousands of Brazilian Reais, expressed in 2012 constant prices.

Table 2: FPM formula transfer adjustment
Negative & positive shock

Panel A	(1a)	(2a)	(3a)	(4a)
Outcome	Transfer (FPM)	Transfer (FPM)	Transfer (FPM)	Transfer (FPM)
Shock (negative)	-0.888***	-0.694***	-0.699***	-0.628***
	(0.098)	(0.073)	(0.072)	(0.073)
Observations	21,056	21,056	21,056	21,056
Municipalities	2,632	2,632	2,632	2,632
Mean of dep. var. (2007)	7.970			
Panel B	(1b)	(2b)	(3b)	(4b)
Outcome	Transfer (FPM)	Transfer (FPM)	Transfer (FPM)	Transfer (FPM)
Shock (positive)	1.344***	1.267***	1.266***	1.342***
	(0.058)	(0.060)	(0.060)	(0.055)
Observations	20,704	20,704	20,704	20,704
Municipalities	2,588	2,588	2,588	2,588
Mean of dep. var. (2007)	7.150			
Municipality & Year FE	Yes	Yes	Yes	Yes
Cutoff X Year FE	Yes	Yes	Yes	Yes
Jumps Trend	No	Yes	Yes	Yes
Mayor Characteristics	No	No	Yes	Yes
State X Year FE	No	No	No	Yes

Notes: The table shows results for the effect of the negative population bracket update (Panel A) and positive population bracket update (Panel B) on reported FPM formula transfer. The dependent variable is measured in millions of R$. Each cell reports the average effect for the entire post-treatment period of a difference-in-difference model. All specifications include municipality and year fixed effects, and cutoff X year fixed effects. Jumps trend is a variable defined as the accumulation of positive and negative jumps across thresholds before and after the shock took place in 2008. For a treated municipality that jumped negatively in 2006 and positively in 2009, for instance, this variable will be 0 in 2005, -1 from 2006 to 2007, zero in 2008, and 1 from 2009 to 2012. Mayor characteristics are gender, age, educational level, term in office, political party alignment with the federal government, political party alignment with the state government, and political party. Robust standard errors, two-way clustered by state X year and micro-region level, in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 3: TAX REVENUES
NEGATIVE & POSITIVE SHOCK

Panel A	(1a)	(2a)	(3a)	(4a)
Outcome	Taxes	Taxes	Taxes	Taxes
Reduced Form				
Shock (negative)	0.614***	0.672***	0.685***	0.660***
	(0.222)	(0.240)	(0.245)	(0.241)
2SLS				
Transfer (FPM)	0.692**	0.968**	0.980**	1.052**
	(0.280)	(0.383)	(0.388)	(0.421)
F-statistic (1st stage)	81.7	89.9	93.5	73.1
Observations	21,056	21,056	21,056	21,056
Municipalities	2,632	2,632	2,632	2,632
Mean of dep. var. (2007)		2.244		
Panel B	(1b)	(2b)	(3b)	(4b)
Outcome	Taxes	Taxes	Taxes	Taxes
Reduced Form				
Shock (positive)	0.054	0.019	0.015	0.069
	(0.074)	(0.080)	(0.079)	(0.066)
2SLS				
Transfer (FPM)	0.040	0.015	0.012	0.051
	(0.055)	(0.063)	(0.062)	(0.049)
F-statistic (1st stage)	536.6	450.1	447.0	587.5
Observations	20,704	20,704	20,704	20,704
Municipalities	2,588	2,588	2,588	2,588
Mean of dep. var. (2007)		1.655		
Municipality & Year FE	Yes	Yes	Yes	Yes
Cutoff X Year FE	Yes	Yes	Yes	Yes
Jumps Trend	No	Yes	Yes	Yes
Mayor Characteristics	No	No	Yes	Yes
State X Year FE	No	No	No	Yes

Notes: The table shows results for the effect of the negative population bracket update (Panel A) and positive population bracket update (Panel B) on reported local tax revenues. The dependent variable is measured in millions of R$. Each cell reports the average effect for the entire post-treatment period of a difference-in-difference model. The 2SLS regressions use the population bracket update as an instrument of the FPM formula transfer shock. In the row below the 2SLS estimates, F-statistic (1st Stage) reports the Kleibergen-Paap rk Wald F-statistic of weak instruments. All specifications include municipality and year fixed effects, and cutoff X year fixed effects. Jumps trend is a variable defined as the accumulation of positive and negative jumps across thresholds before and after the shock took place in 2008. For a treated municipality that jumped negatively in 2006 and positively in 2009, for instance, this variable will be 0 in 2005, -1 from 2006 to 2007, zero in 2008, and 1 from 2009 to 2012. Mayor characteristics are gender, age, educational level, term in office, political party alignment with the federal government, political party alignment with the state government, and political party. Robust standard errors, two-way clustered by state X year and micro-region level, in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 4: TAX COLLECTION - INDIVIDUAL TAXES NEGATIVE SHOCK

Outcome	(1) IPTU	(2) ITBI	(3) ISS	(4) Fees	(5) IRRF
Reduced Form					
Shock (negative)	0.109**	0.129***	0.355**	0.076***	0.025
	(0.047)	(0.039)	(0.150)	(0.025)	(0.020)
2SLS					
Transfer (FPM)	0.158**	0.185***	0.511**	0.109***	0.035
	(0.073)	(0.066)	(0.232)	(0.041)	(0.030)
F-statistic (1st stage)	89.9	89.9	89.9	89.9	89.9
Mean of dep. var. (2007)	0.596	0.198	0.852	0.273	0.264
Observations	21,056	21,056	21,056	21,056	21,056
Municipalities	2,632	2,632	2,632	2,632	2,632
Municipality & Year FE	Yes	Yes	Yes	Yes	Yes
Cutoff X Year FE	Yes	Yes	Yes	Yes	Yes
Jumps Trend	Yes	Yes	Yes	Yes	Yes

Notes: The table shows results for the effect of the negative population bracket update on reported revenues from the IPTU—property tax (Column 1), ITBI—tax on the transaction of properties (Column 2), ISS—service tax (Column 3), Fees—public service fees (Column 4), and IRRF—federal income tax retained by the municipality (Column 5). The dependent variables are measured in millions of R$. Each cell reports the average effect for the entire post-treatment period of a difference-in-difference model. The 2SLS regressions use the negative population bracket update as an instrument of the FPM formula transfer shock. In the row below the 2SLS estimates, F-statistic (1st Stage) reports the Kleibergen-Paap rk Wald F-statistic of weak instruments. All specifications include municipality and year fixed effects, and cutoff X year fixed effects. Jumps trend is a variable defined as the accumulation of positive and negative jumps across thresholds before and after the shock took place in 2008. For a treated municipality that jumped negatively in 2006 and positively in 2009, for instance, this variable will be 0 in 2005, -1 from 2006 to 2007, zero in 2008, and 1 from 2009 to 2012. Robust standard errors, two-way clustered by state X year and micro-region level, in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 5: TAX WORKERS
NEGATIVE SHOCK

Panel A	(1a)	(2a)	(3a)	(4a)
Outcome	Extensive Margin	Extensive Margin	Extensive Margin	Extensive Margin
Reduced Form				
Shock (negative)	-0.018	-0.017	-0.016	-0.023
	(0.021)	(0.021)	(0.021)	(0.021)
2SLS				
Transfer (FPM)	-0.021	-0.025	-0.023	-0.037
	(0.024)	(0.031)	(0.030)	(0.033)
F-statistic (1st Stage)	76.0	82.0	85.1	70.4
Mean of dep. var. (2007)		0.471		
Panel B	(1b)	(2b)	(3b)	(4b)
Outcome	Number	Number	Number	Number
Reduced Form				
Shock (negative)	0.379	0.362	0.361	0.267
	(0.239)	(0.244)	(0.244)	(0.244)
2SLS				
Transfer (FPM)	0.444	0.532	0.526	0.428
	(0.289)	(0.369)	(0.367)	(0.400)
F-statistic (1st Stage)	76.0	82.0	85.1	70.4
Mean of dep. var. (2007)		2.095		
Panel C	(1c)	(2c)	(3c)	(4c)
Outcome	Wage Bill	Wage Bill	Wage Bill	Wage Bill
Reduced Form				
Shock (negative)	0.011*	0.011*	0.011*	0.009
	(0.006)	(0.006)	(0.006)	(0.006)
2SLS				
Transfer (FPM)	0.013*	0.016*	0.015*	0.015
	(0.007)	(0.009)	(0.009)	(0.010)
F-statistic (1st Stage)	76.0	82.0	85.1	70.4
Mean of dep. var. (2007)		0.034		
Observations	20,120	20,120	20,120	20,120
Municipalities	2,515	2,515	2,515	2,515
Municipality & Year FE	Yes	Yes	Yes	Yes
Cutoff X Year FE	Yes	Yes	Yes	Yes
Jumps Trend	No	Yes	Yes	Yes
Mayor Characteristics	No	No	Yes	Yes
State X Year FE	No	No	No	Yes

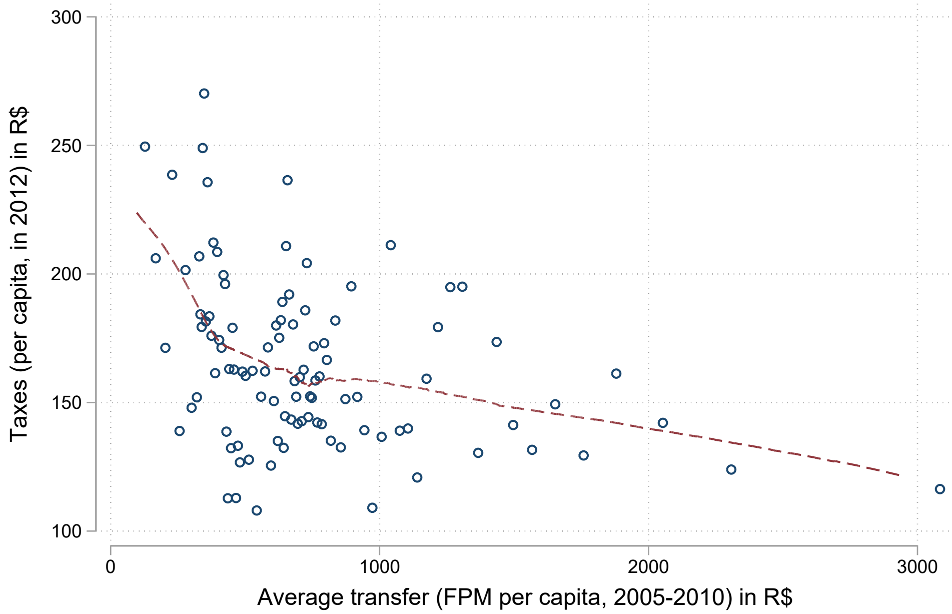
Notes: The table shows results for the effect of the negative population bracket update on hiring tax workers on the extensive margin (Panel A), reported number of tax workers—intensive margin (Panel B), and wage bill measured in millions of R$ (Panel C). Each cell reports the average effect for the entire post-treatment period of a difference-in-difference model. The 2SLS regressions use the negative population bracket update as an instrument of the FPM formula transfer shock. In the row below the 2SLS estimates, F-statistic (1st Stage) reports the Kleibergen-Paap rk Wald F-statistic of weak instruments. All specifications include municipality and year fixed effects, and cutoff X year fixed effects. Jumps trend is a variable defined as the accumulation of positive and negative jumps across thresholds before and after the shock took place in 2008. For a treated municipality that jumped negatively in 2006 and positively in 2009, for instance, this variable will be 0 in 2005, -1 from 2006 to 2007, zero in 2008, and 1 from 2009 to 2012. Mayor characteristics are gender, age, educational level, term in office, political party alignment with the federal government, political party alignment with the state government, and political party. Robust standard errors, two-way clustered by state X year and micro-region level, in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 6: TAX INFRASTRUCTURE
NEGATIVE SHOCK

	Real Estate Cadastre		Property Price Register		Provider Register	
Outcome	(1) Any Technology	(2) Digital Technology	(3) Any Technology	(4) Digital Technology	(5) Any Technology	(6) Digital Technology
Reduced Form						
Shock (negative)	0.015	-0.034	0.096**	-0.032	0.015	0.005
	(0.014)	(0.032)	(0.036)	(0.050)	(0.034)	(0.039)
2SLS						
Transfer (FPM)	0.013	-0.033	0.087***	-0.031	0.013	0.004
	(0.012)	(0.029)	(0.032)	(0.048)	(0.031)	(0.035)
F-statistic (1st Stage)	113.0	109.8	113.0	72.3	113.0	92.8
Mean of dep. var. (2007)	0.950	0.917	0.748	0.789	0.857	0.846
Observations	5,260	4,880	5,260	3,680	5,260	4,376
Municipalities	2,630	2,440	2,630	1,840	2,630	2,188
Municipality & Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cutoff X Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Jumps Trend	Yes	Yes	Yes	Yes	Yes	Yes

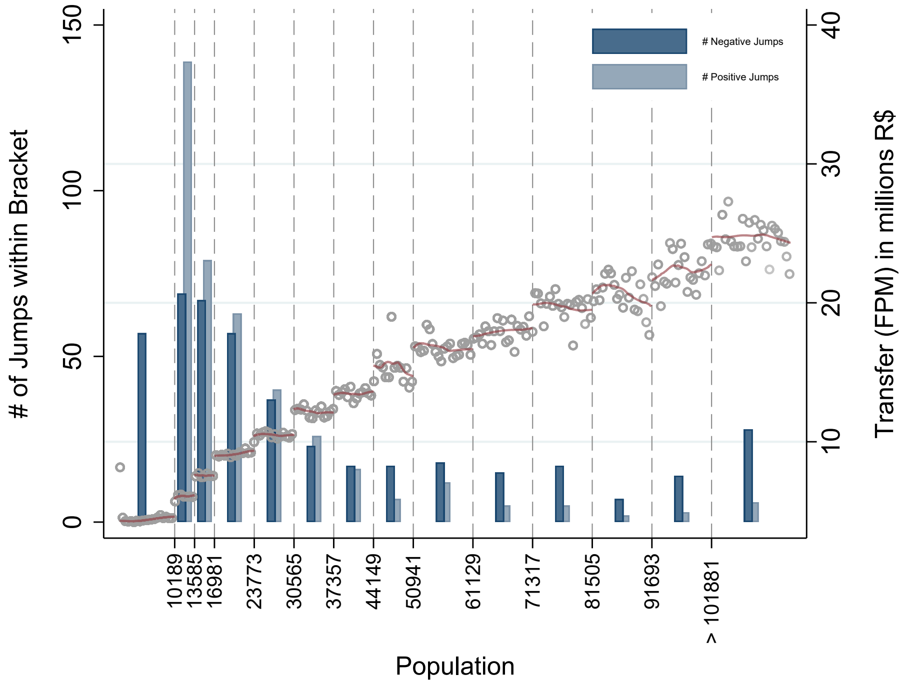
Notes: The table shows results for the effect of the negative population bracket update on reported adoption of a real estate cadastre (Columns 1 and 2), of a property price register (Columns 3 and 4), and of a provider register (Columns 5 and 6). Any technology refers to the existence of the tax collection tool, regardless of its format (e.g., paper-based or digital), while digital technology refers to tools that are digital-based. Columns 2, 4, and 6 restrict the sample to municipalities that report having the tool, regardless of its type. The dependent variables are dummy variables = 1 if the municipality adopted the tax collection tool in 2006 and/or 2009—the two years for which we have information, 0 otherwise. Each cell reports the average effect for a single post-treatment period of a difference-in-difference model. The 2SLS regressions use the negative population bracket update as an instrument of the FPM formula transfer shock. In the row below the 2SLS estimates, F-statistic (1st Stage) reports the Kleibergen-Paap rk Wald F-statistic of weak instruments. All specifications include municipality and year fixed effects, and cutoff X year fixed effects. Jumps trend is a variable defined as the accumulation of positive and negative jumps across thresholds before and after the shock took place in 2008. For a treated municipality that jumped negatively in 2006 and positively in 2009, for instance, this variable will be 0 in 2005, -1 from 2006 to 2007, zero in 2008, and 1 from 2009 to 2012. Robust standard errors, two-way clustered by state X year and micro-region level, in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

FIGURE 1: TAX REVENUES AND TRANSFER DEPENDENCE



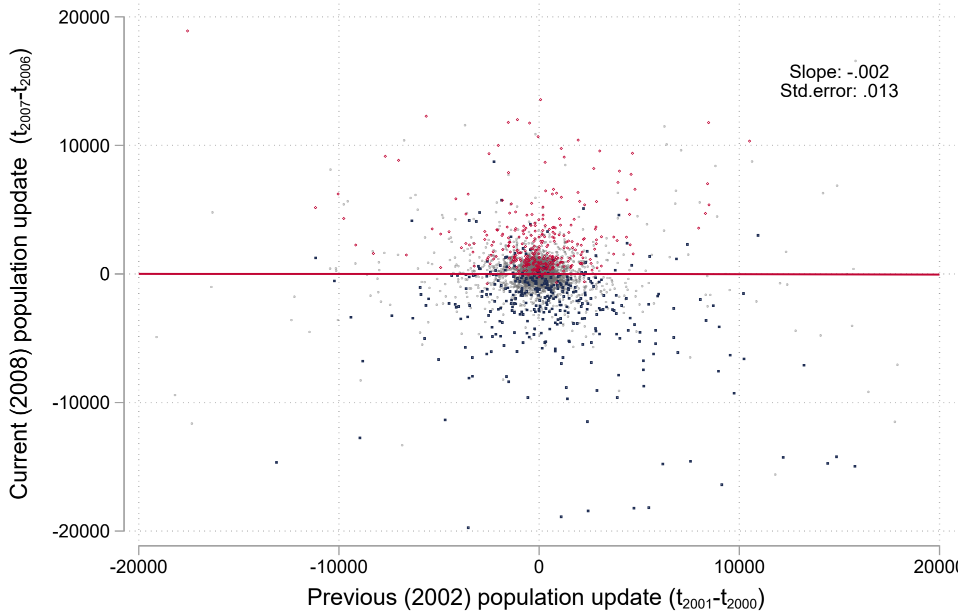
Notes: This figure plots per capita tax revenues in 2012 against the average amount of FPM formula transfer per capita received between 2005 and 2010 in hundred equally sized bins. The dashed line is a smoothed lowess with kernel $k = 0.8$ of the data. Both series are residualized on 2012 population and state dummies.

Figure 2: Population brackets and FPM formula transfer



Notes: The figure shows the 2005-2012 scatter plot of FPM formula transfer averaged over 500-inhabitants bins and running-mean smoothing between population brackets (red lines). The bars indicate the number of municipalities that crossed a population bracket in 2008. The last category (>101,881) includes the count for all municipalities above that cut-off for readability.

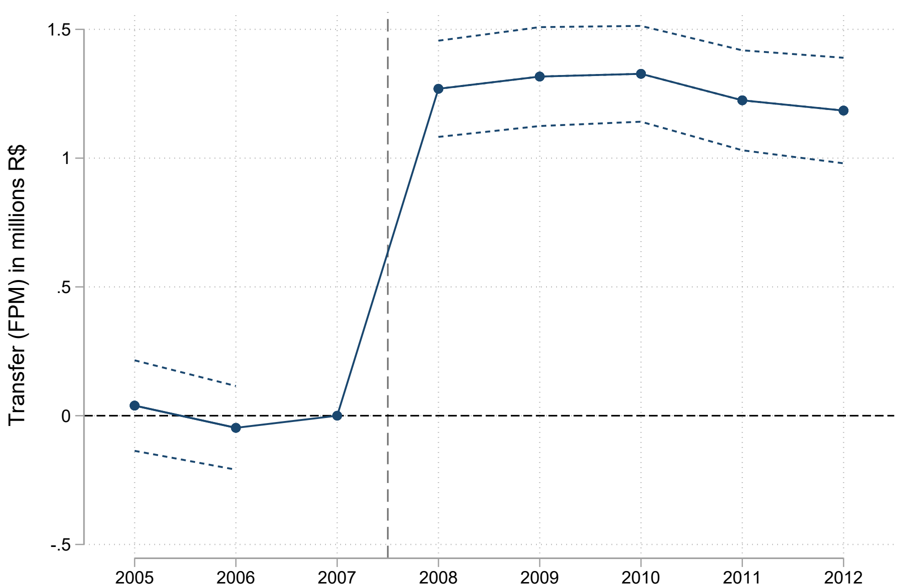
FIGURE 3: CORRELATION BETWEEN DIFFERENT CENSUS SHOCKS



Notes: The figure shows the scatter plot and linear prediction between the census shock in t (2008) and the previous census shock in 2002 (t-6) after controlling for cutoff and state fixed effects. Bins outside of the [-20,000 20,000] range are omitted for readability, but included in the fit of the line. Blue (red) dots represent municipalities included in the negative (positive) treatment group.

FIGURE 4: FPM FORMULA TRANSFER ADJUSTMENT
NEGATIVE & POSITIVE SHOCK

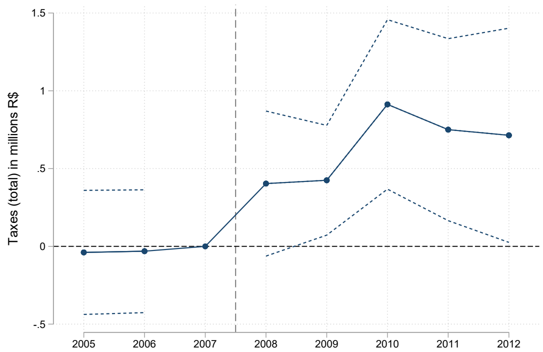
(a) Negative shock



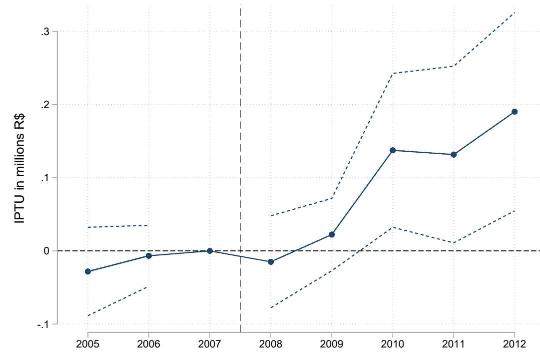
(b) Positive shock

Notes: The figures show the results from Equation 1 for reported FPM formula transfer measured in millions of R$, including in the model municipality and year fixed effects, cutoff X year fixed effects, and a trend variable for jumps across thresholds experienced by municipalities in the treatment group before and after the shock took place in 2008. For a treated municipality that jumped negatively in 2006 and positively in 2009, for instance, this variable will be 0 in 2005, -1 from 2006 to 2007, zero in 2008, and 1 from 2009 to 2012. Panel (a) shows effects for the negative shock and Panel (b) shows effects for the positive shock. 95% confidence intervals of standard errors two-way clustered by state X year and micro-region level are indicated around the point estimates.

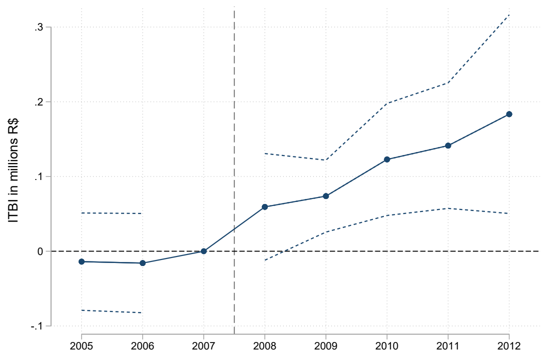
FIGURE 5: TAX COLLECTION
NEGATIVE SHOCK



(a) Total

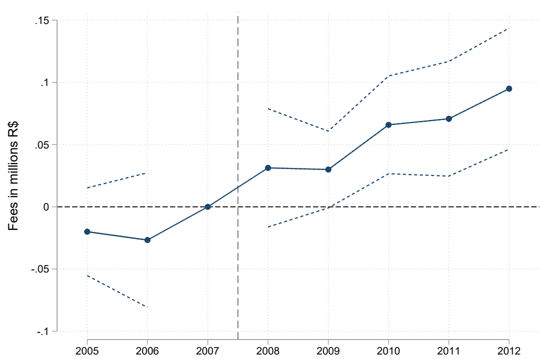


(b) Property tax (IPTU)

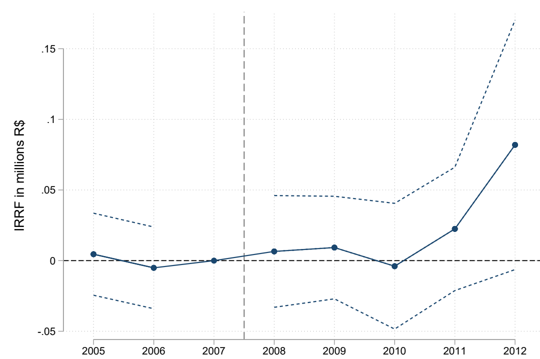


(c) Property transaction tax (ITBI)

(d) Service tax (ISS)



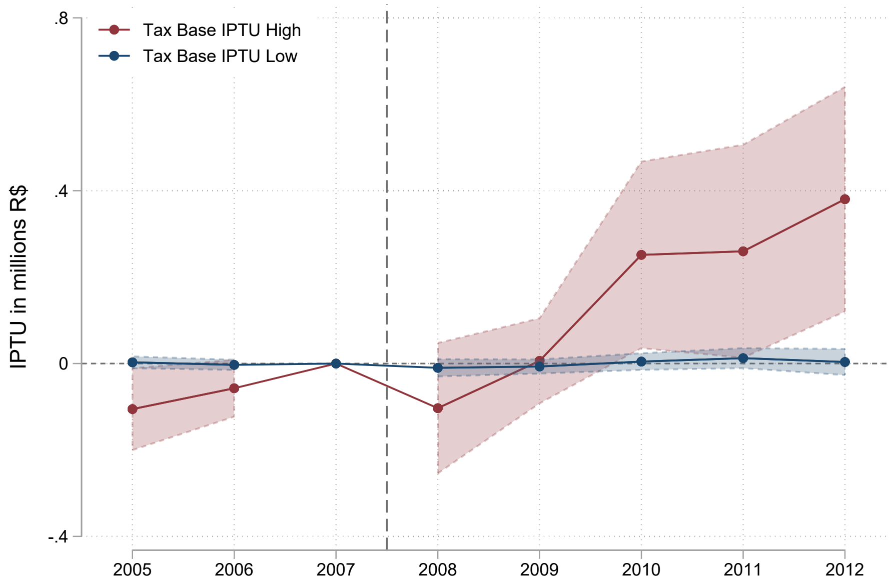
(e) Public service fees



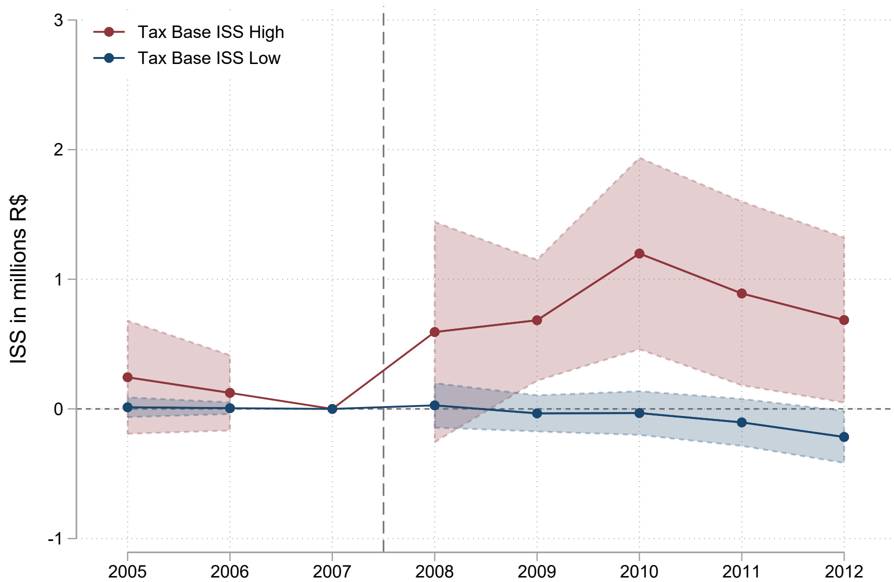
(f) Income tax retained (IRRF)

Notes: The figures show the results from Equation 1 for reported total tax revenues in Panel (a), property tax in Panel (b), tax on the transaction of properties in Panel (c), service tax in Panel (d), public service fees in Panel (e), and federal income tax retained by the municipality in Panel (f). The dependent variables are measured in millions of R$. All specifications include municipality and year fixed effects, cutoff X year fixed effects, and a trend variable for jumps across thresholds experienced by municipalities in the treatment group before and after the shock took place in 2008. For a treated municipality that jumped negatively in 2006 and positively in 2009, for instance, this variable will be 0 in 2005, -1 from 2006 to 2007, zero in 2008, and 1 from 2009 to 2012. 95% confidence intervals of standard errors two-way clustered by state X year and micro-region level are indicated around the point estimates.

FIGURE 6: TAX COLLECTION - TAX BASE HETEROGENEITY NEGATIVE SHOCK



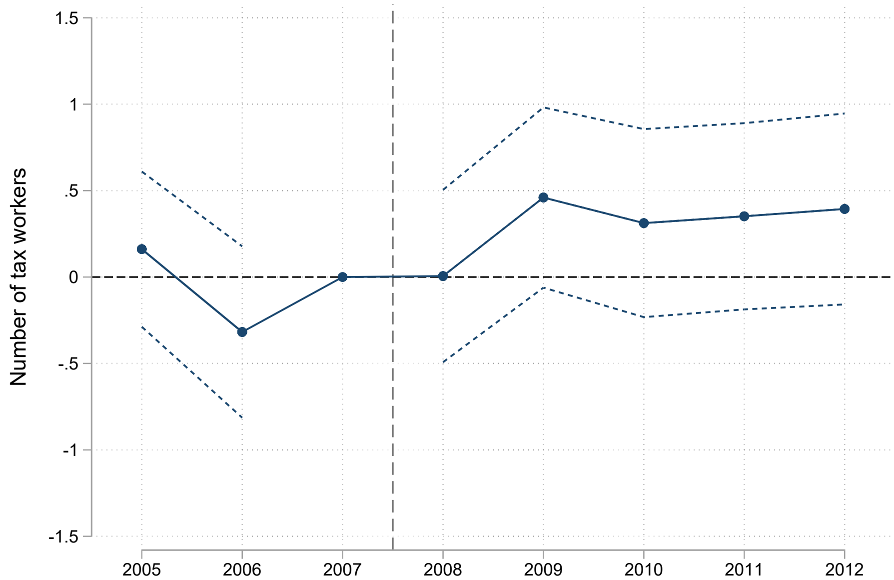
(a) Tax base of property tax (IPTU)



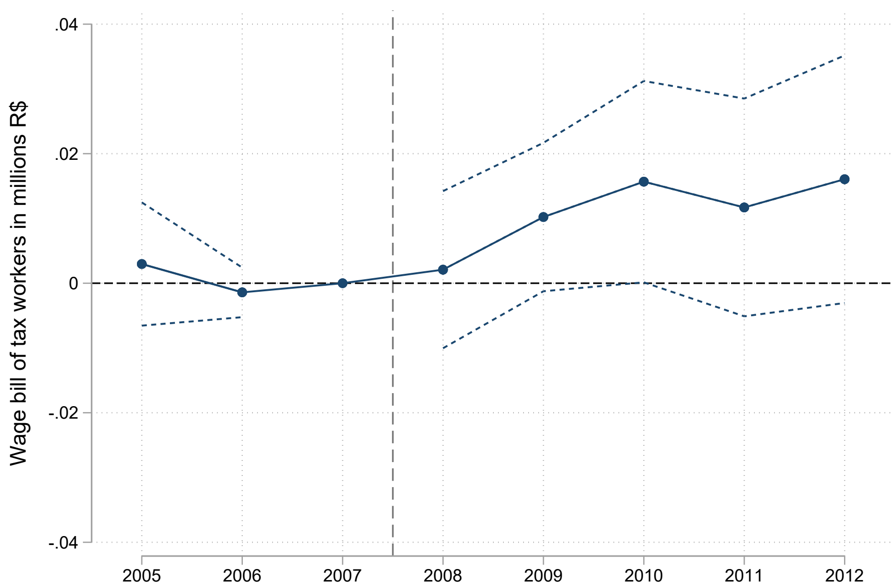
(b) Tax base of service tax (ISS)

Notes: The figures show the results from Equation 1 for reported property tax revenues in Panel (a) and service tax revenues in Panel (b). In Panel (a), red (blue) illustrates municipalities where the pre-shock total number of registered properties per capita subject to the property tax is above (below) the median of the distribution of sample municipalities. In Panel (b), red (blue) illustrates municipalities where the pre-shock total number of registered service taxpayers per capita subject to the service tax is above (below) the median of the distribution of sample municipalities. The dependent variables are measured in millions of R$. All specifications include municipality and year fixed effects, cutoff X year fixed effects, and a trend variable for jumps across thresholds experienced by municipalities in the treatment group before and after the shock took place in 2008. For a treated municipality that jumped negatively in 2006 and positively in 2009, for instance, this variable will be 0 in 2005, -1 from 2006 to 2007, zero in 2008, and 1 from 2009 to 2012. 95% confidence intervals of standard errors two-way clustered by state X year and micro-region level are indicated around the point estimates.

FIGURE 7: TAX WORKERS
NEGATIVE SHOCK



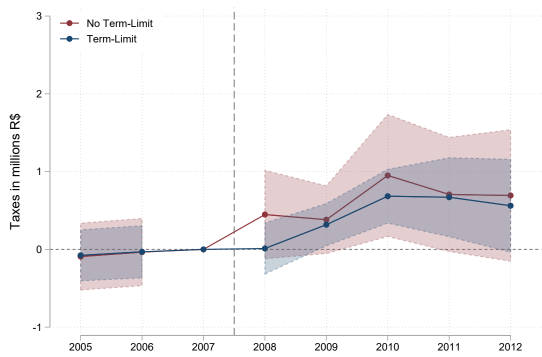
(a) Number



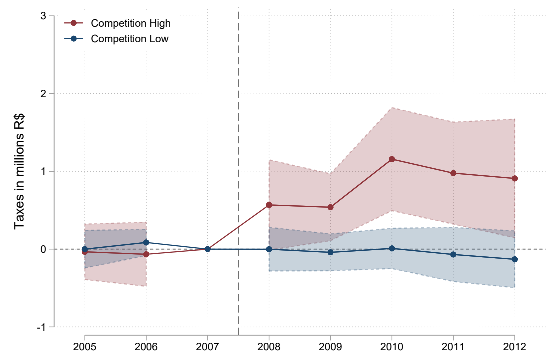
(b) Wage bill

Notes: The figures show the results from Equation 1 for tax workers reported number of employees—intensive margin—in Panel (a), and wage bill measured in millions of R$ in Panel (b). All specifications include municipality and year fixed effects, cutoff X year fixed effects, and a trend variable for jumps across thresholds experienced by municipalities in the treatment group before and after the shock took place in 2008. For a treated municipality that jumped negatively in 2006 and positively in 2009, for instance, this variable will be 0 in 2005, -1 from 2006 to 2007, zero in 2008, and 1 from 2009 to 2012. 95% confidence intervals of standard errors two-way clustered by state X year and micro-region level are indicated around the point estimates.

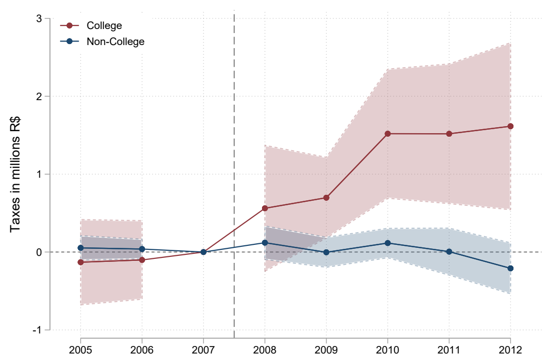
FIGURE 8: TAX COLLECTION - HETEROGENEITIES
NEGATIVE SHOCK



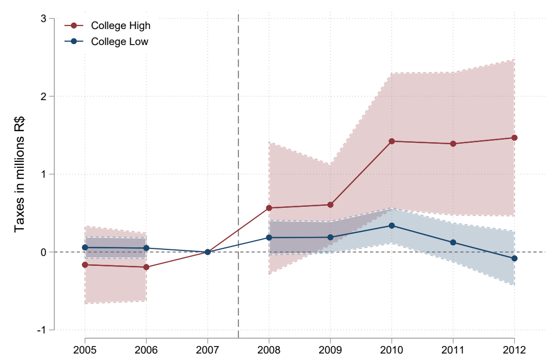
(a) Term in office



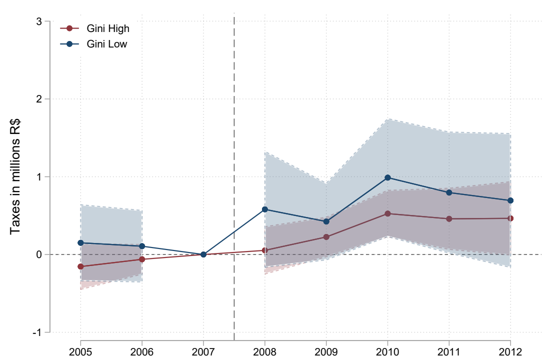
(b) Political competition



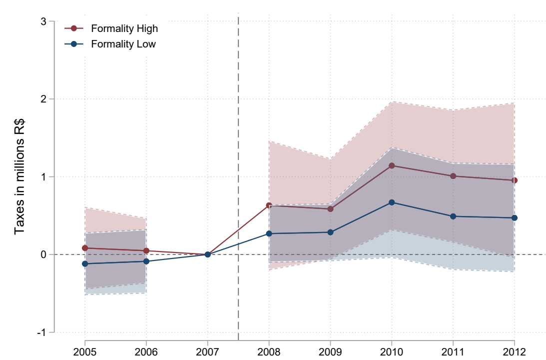
(c) Mayor education



(d) Bureaucrats education



(e) Gini index



(f) Labor market formality

Notes: The figures show the results from Equation 1 for reported tax revenues measured in millions of R$ splitting the sample municipalities into two groups by variable. In panel (a), red (blue) illustrates municipalities where the post-shock mayor does not face (face) term limits. In panel (b), red (blue) illustrates municipalities where the pre-shock average number of candidates in mayoral elections is above (below) the median of the distribution of sample municipalities. In panel (c), red (blue) illustrates municipalities where the post-shock mayor has at least (at most) a college degree (high school diploma). In panel (d), red (blue) illustrates municipalities where the post-shock bureaucracy has an above- (below-) median share of college-educated employees considering the distribution of sample municipalities. In panel (e), red (blue) illustrates municipalities where the pre-shock Gini index is above (below) the median of the distribution of sample municipalities. In panel (f), red (blue) illustrates municipalities where the pre-shock formality level of the local labor market is above (below) the median of the distribution of sample municipalities. All specifications include municipality and year fixed effects, cutoff X year fixed effects, and a trend variable for jumps across thresholds experienced by municipalities in the treatment group before and after the shock took place in 2008. For a treated municipality that jumped negatively in 2006 and positively in 2009, for instance, this variable will be 0 in 2005, -1 from 2006 to 2007, zero in 2008, and 1 from 2009 to 2012. 95% confidence intervals of standard errors two-way clustered by state X year and micro-region level are indicated around the point estimates.

Appendix

Table A.1: FPM formula transfer coefficients

Bracket	Population	Coefficient	% Variation
1	0-10,188	0.6	-
2	10,189-13,584	0.8	33.3
3	13,585-16,980	1	25.0
4	16,981-23,772	1.2	20.0
5	23,773-30,564	1.4	16.7
6	30,565-37,356	1.6	14.3
7	37,357-44,148	1.8	12.5
8	44,149-50,940	2	11.1
9	50,941-61,128	2.2	10.0
10	61,129-71,316	2.4	9.1
11	71,317-81,504	2.6	8.3
12	81,505-91,692	2.8	7.7
13	91,693-101,880	3	7.1
14	101,881-115,464	3.2	6.7
15	115,465-129,048	3.4	6.2
16	129,049-142,632	3.6	5.9
17	142,633-156,216	3.8	5.6
18	156,217-	4	5.3

Notes: The table shows the 18 population brackets of the FPM formula transfer, the associated coefficient to each bracket, and the percentage variation between the coefficients of consecutive brackets.

TABLE A.2: NUMBER OF MUNICIPALITIES THAT CROSSED A POPULATION BRACKET

	Year										
Jumps	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012
Negative	291	29	23	48	19	13	**443**	4	9	171	31
- 2008 FPM Shock Reversal	-	-	-	-	-	-	-	148	23	115	14
Positive	682	152	159	340	191	147	**403**	344	119	382	73
- 2008 FPM Shock Reversal	-	-	-	-	-	-	-	1	1	51	4

Notes: The table shows the yearly number of municipalities that crossed any FPM formula transfer population bracket in the period 2002-2012; where 2002 (2011) indicates the year in which the population census held in 2000 (2010) became effective, and 2008 indicates the year in which the population count held in 2007 became effective. The jumps that took place between the years 2003-2007, and in 2009, 2010, and 2012, were determined by the local population estimates produced by the IBGE and TCU. 2008 FPM Shock Reversal shows the annual number of municipalities that were reshuffled in 2008 and subsequently returned to their pre-shock population bracket in the following years.

TABLE A.3: POPULATION AND BUDGET ITEMS

		Population			Revenues (% of Total)				Tax Revenues (% of Taxes)				
Bracket	Share	Mean	Mean Growth	SD Growth	FPM Trans.	Fed. Trans.	State Trans.	Taxes	IPTU	ITBI	ISS	Fees	IRRF
1	0.47	5,255	0.01	0.07	0.45	0.11	0.24	0.05	0.14	0.13	0.42	0.08	0.18
2-4	0.29	15,574	0.01	0.06	0.35	0.14	0.23	0.07	0.18	0.10	0.43	0.10	0.15
5-8	0.14	33,363	0.01	0.05	0.26	0.15	0.24	0.10	0.24	0.09	0.43	0.10	0.11
9-13	0.06	70,746	0.01	0.04	0.19	0.15	0.26	0.13	0.26	0.08	0.43	0.11	0.10
14-18	0.04	239,689	0.01	0.03	0.10	0.14	0.29	0.20	0.29	0.07	0.44	0.09	0.09
1-18	1.00	26,021	0.01	0.06	0.22	0.14	0.26	0.13	0.26	0.08	0.44	0.10	0.10

Notes: The table covers the population of Brazilian municipalities (does not include state capitals) and includes 44,300 observations from 5,538 municipalities over the period 2005-2012. The table reports by groups of population brackets: proportion of municipalities, municipal population mean, municipal population growth mean and standard deviation, and main municipal sources of revenues as a share of total revenues. Main sources of municipal revenue comprise i) FPM transfer; ii) federal transfers net of FPM transfer; iii) state transfers; and iv) local taxes which include IPTU (property tax), ITBI (tax on the transaction of properties), ISS (service tax), public service fees, and IRRF (federal income tax retained by the municipality).

TABLE A.4: OTHER BUDGETARY ADJUSTMENTS

Panel A	(1a)	(2a)	(3a)
Outcome	Expenditures	Revenues	Other Transfers
Reduced Form			
Shock (negative)	0.839	1.385*	0.402
	(0.682)	(0.745)	(0.323)
2SLS			
Transfer (FPM)	1.208	1.995*	0.579
	(1.031)	(1.167)	(0.485)
F-statistic (1st stage)	89.9	89.9	89.9
Observations	21,056	21,056	21,056
Municipalities	2,632	2,632	2,632
Mean of dep. var. (2007)	24.262	24.977	9.222
Panel B	(1b)	(2b)	(3b)
Outcome	Expenditures	Revenues	Other Transfers
Reduced Form			
Shock (positive)	1.933***	2.362***	0.404
	(0.489)	(0.625)	(0.397)
2SLS			
Transfer (FPM)	1.525***	1.864***	0.319
	(0.382)	(0.485)	(0.311)
F-statistic (1st stage)	450.1	450.1	450.1
Observations	20,704	20,704	20,704
Municipalities	2,588	2,588	2,588
Mean of dep. var. (2007)	20.918	21.439	8.070
Municipality & Year FE	Yes	Yes	Yes
Cutoff X Year FE	Yes	Yes	Yes
Jumps Trend	Yes	Yes	Yes
Mayor Characteristics	No	No	No
State X Year FE	No	No	No

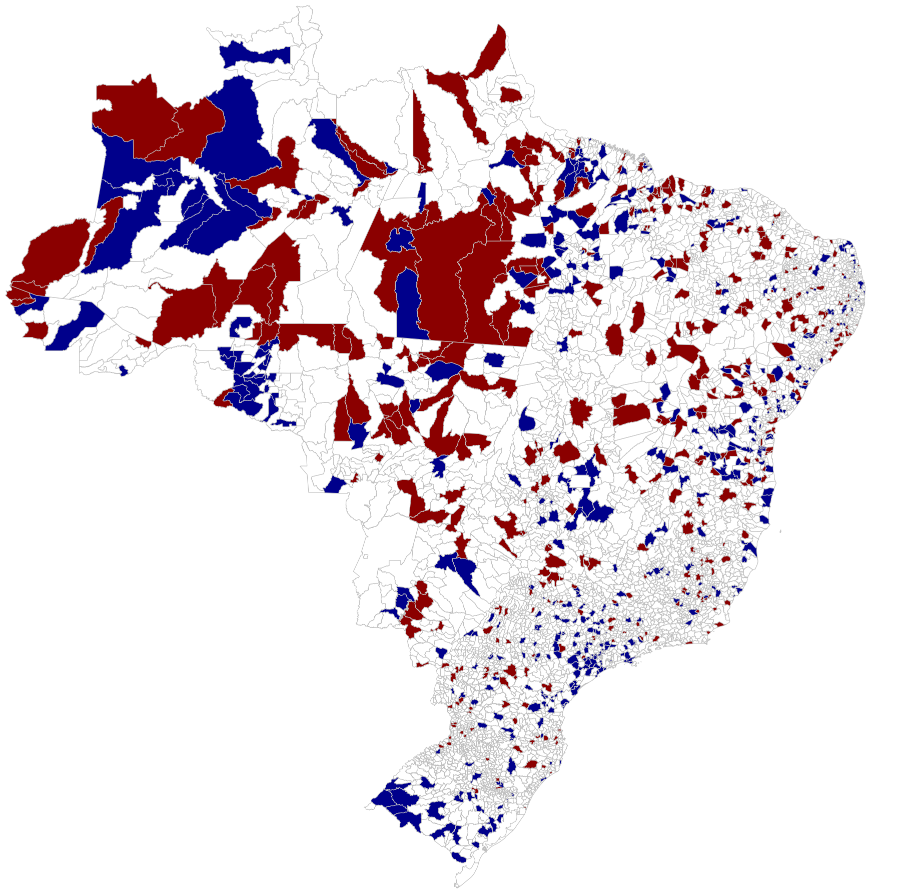
Notes: The table shows results for the effect of the negative population bracket update (Panel A) and positive population bracket update (Panel B) on reported total expenditures (Column 1), total revenues (Column 2), and transfers net of FPM (Column 3). The dependent variables are measured in millions of R$. Each cell reports the average effect for the entire post-treatment period of a difference-in-difference model. The 2SLS regressions use the population bracket update as an instrument of the FPM formula transfer shock. In the row below the 2SLS estimates, F-statistic (1st Stage) reports the Kleibergen-Paap rk Wald F-statistic of weak instruments. All specifications include municipality and year fixed effects, and cutoff X year fixed effects and jumps trend defined as the accumulation of positive and negative jumps across thresholds before and after the shock took place in 2008. For a treated municipality that jumped negatively in 2006 and positively in 2009, for instance, this variable will be 0 in 2005, -1 from 2006 to 2007, zero in 2008, and 1 from 2009 to 2012. Mayor characteristics are gender, age, educational level, term in office, political party alignment with the federal government, political party alignment with the state government, and political party. Robust standard errors, two-way clustered by state X year and micro-region level, in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A.5: TAX COLLECTION - HETEROGENEITIES (2SLS) NEGATIVE SHOCK

Outcome	(1) Taxes	(2) Taxes	(3) Taxes	(4) Taxes	(5) Taxes	(6) Taxes	(7) Taxes
Transfer (FPM)	0.827**	-0.092	-0.040	0.127	0.894*	0.643	-0.395
	(0.379)	(0.137)	(0.134)	(0.122)	(0.507)	(0.399)	(0.529)
Transfer (FPM) X No Term-Limit	0.037						-0.700
	(0.497)						(0.678)
Transfer (FPM) X Political Competition		1.451***					1.107**
		(0.479)					(0.501)
Transfer (FPM) X Mayor College			1.884***				1.522**
			(0.648)				(0.609)
Transfer (FPM) X Bureaucrats College				2.135**			-0.047
				(0.917)			(0.420)
Transfer (FPM) X Gini					-0.292		-0.574
					(0.529)		(0.412)
Transfer (FPM) X Labor Market Formality						0.644	-0.439
						(0.615)	(0.448)
F-statistic (1st Stage)	19.4	32.1	24.7	17.6	37.2	28.7	1.1
Observations	21,056	21,056	21,056	21,032	21,056	21,056	21,024
Municipalities	2,632	2,632	2,632	2,629	2,632	2,632	2,628
Municipality & Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cutoff X Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Jumps Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows results for the effect of the FPM formula transfer shock on reported tax revenues measured in millions of R$, using a 2SLS regression model where the FPM formula transfer shock is instrumented with the negative population bracket update. Each cell reports the average effect for the entire post-treatment period of a difference-in-difference model, in which the effect of the FPM formula transfer shock varies with the indicated characteristic. All continuous independent variables used in the analysis of heterogeneous effects are expressed as indicators of above-below the median of the distribution of sample municipalities. No term-limit is = 1 if the post-shock mayor does not face term limits, 0 otherwise. Political competition = 1 if the pre-shock average number of candidates in mayoral elections is above the median of the distribution of sample municipalities, 0 otherwise. Mayor college = 1 if the post-shock mayor has at least a college degree, 0 otherwise. Bureaucrats college = 1 if the post-shock bureaucracy has an above-median share of college-educated employees considering the distribution of sample municipalities, 0 otherwise. Gini = 1 if the pre-shock Gini index is above the median of the distribution of sample municipalities, 0 otherwise. Labor Market Formality = 1 if the pre-shock formality level of the local labor market is above the median of the distribution of sample municipalities, 0 otherwise. In addition, the model of Column 7 includes as interactions: post-shock political party alignment between the mayor and the Brazilian president of the time, pre-shock average margin of victory in mayoral elections (above-below median), post-shock share of the population with a college degree (above-below median), post-shock population literacy rate (above-below median), pre-shock average monthly income per capita (above-below median), and pre-shock share of urban population (above-below median)—(results not shown). In the row below the 2SLS estimates, F-statistic (1st Stage) reports the Kleibergen-Paap rk Wald F-statistic of weak instruments. All specifications include municipality and year fixed effects, cutoff X year fixed effects, and a trend variable for jumps across thresholds experienced by municipalities in the treatment group before and after the shock took place in 2008. For a treated municipality that jumped negatively in 2006 and positively in 2009, for instance, this variable will be 0 in 2005, -1 from 2006 to 2007, zero in 2008, and 1 from 2009 to 2012. In addition, all specifications include interactions of the indicated local characteristic with the set of stated controls in order to ensure that the model identifies only the differential effect of the local characteristic of interest. Robust standard errors, two-way clustered by state X year and micro-region level, in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

FIGURE A.1: SPATIAL DISTRIBUTION OF SHOCKS



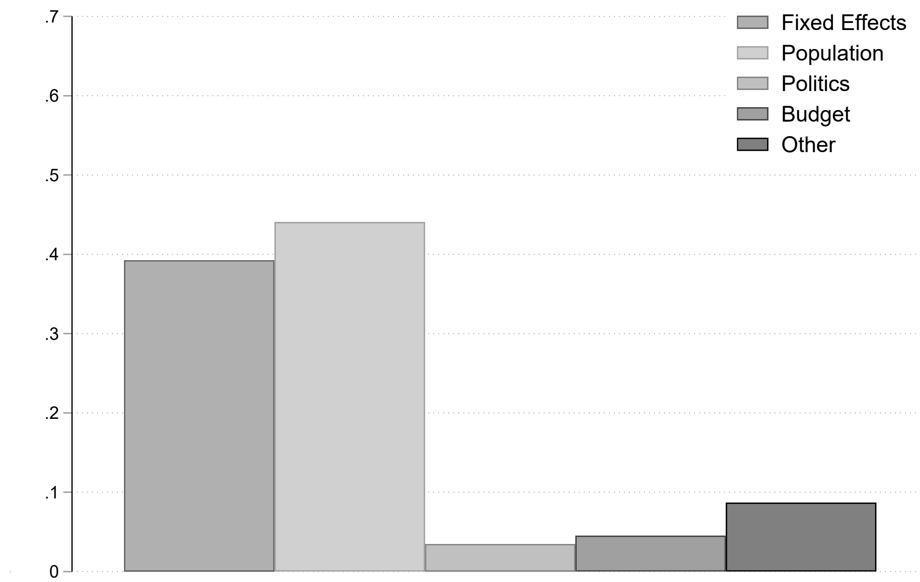
Notes: The figure shows municipalities that shifted to a higher (blue) or lower (red) population bracket in 2008 as a result of the 2007 population count.

Figure A.2: Probability of receiving a shock
Negative & positive shock

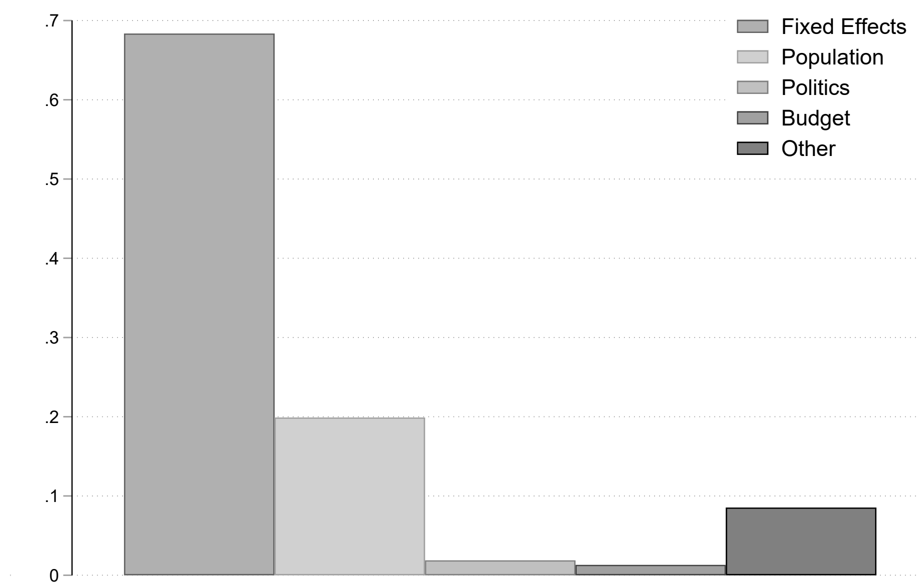
(a) Negative shock (b) Positive shock

Notes: The figure shows the estimated coefficients from the linear probability model $T_i = \beta \mathbf{Z_i} + \varepsilon_i$. In Panel (a) the dependent variable is a dummy = 1 if the municipality received a negative shock in 2008, 0 otherwise. In Panel (b) the dependent variable is a dummy = 1 if the municipality received a positive shock in 2008, 0 otherwise. The models use the respective sample of municipalities of the main analysis. All specifications include cutoff and state fixed effects. 95% confidence intervals of standard errors clustered at the micro-region level are indicated around the point estimates. Filled dots (red) indicate coefficients significant at the 5% level.

FIGURE A.3: PROBABILITY OF RECEIVING A SHOCK - CONTRIBUTION TO ADJUSTED R^2
NEGATIVE & POSITIVE SHOCK



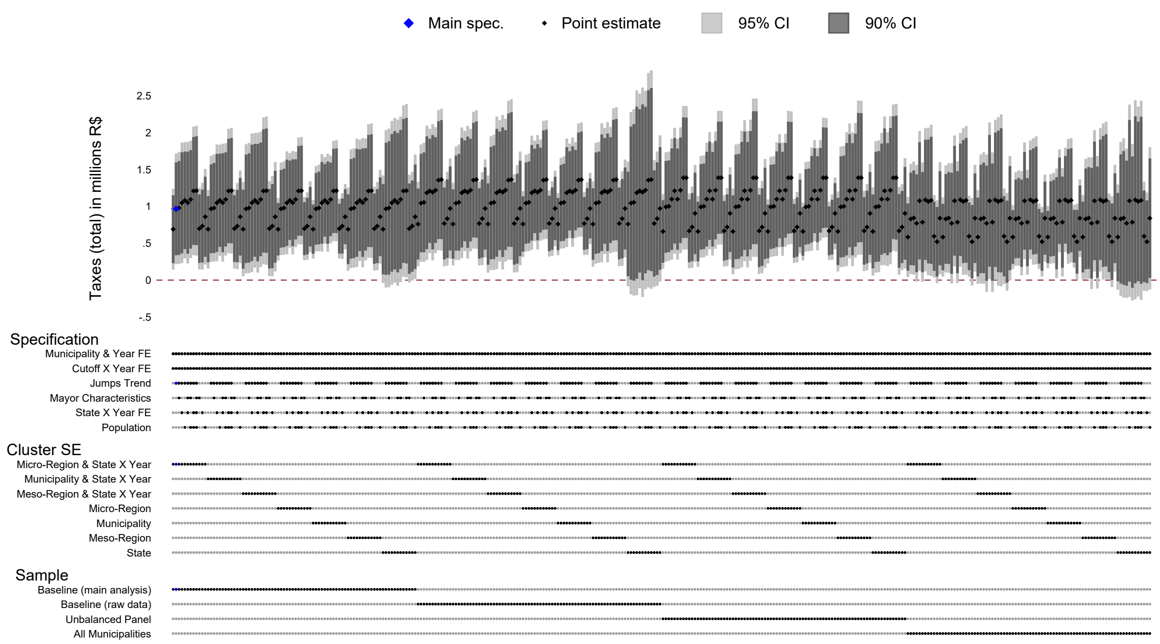
(a) Negative shock



(b) Positive Shock

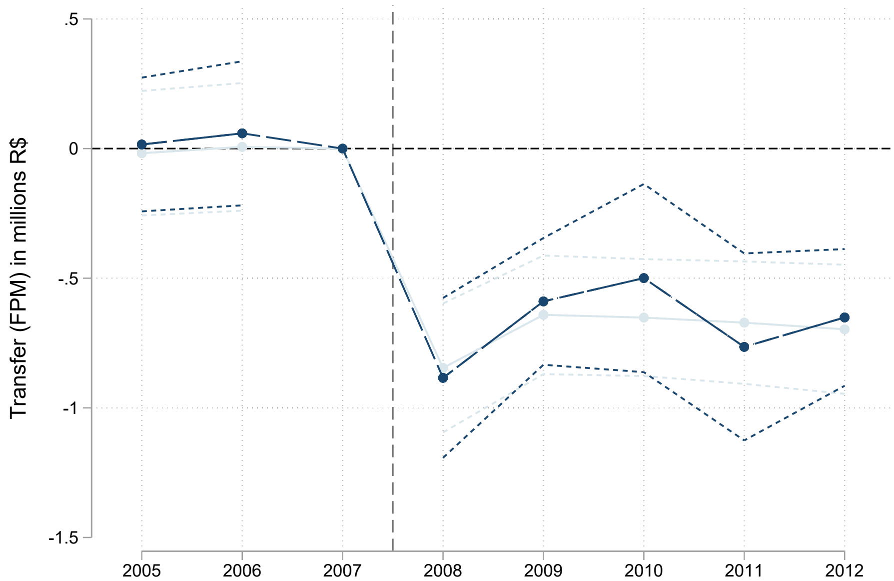
Notes: The figure shows the relative contribution of different set of variables on the explanatory power of the linear probability models depicted in Appendix Figure A.2. Panel (a) shows results for the negative shock, while Panel (b) shows results for the positive shock.

FIGURE A.4: TAX COLLECTION - ROBUSTNESS NEGATIVE SHOCK

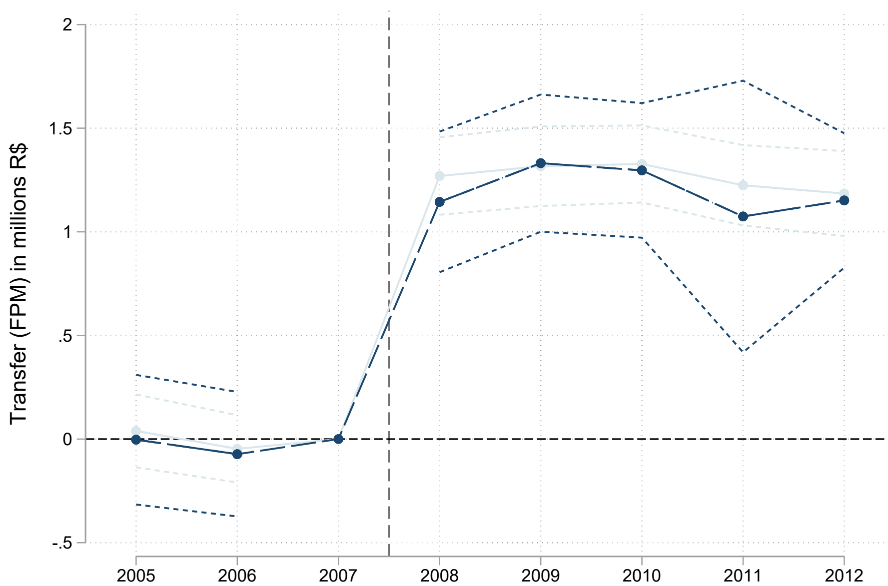


Notes: The figure shows robustness for the effect of the FPM formula transfer shock on reported tax revenues using a 2SLS regression model where the FPM formula transfer shock is instrumented with the negative population bracket update. The dependent variable is measured in millions of R$. Each point estimate reports the average effect for the entire post-treatment period of a difference-in-difference model. In total, 336 models were run that combines i) controls for municipality and year fixed effects, cutoff X year fixed effects, a trend variable for jumps across thresholds experienced by municipalities in the treatment group before and after the shock took place in 2008, mayors' characteristics (gender, age, educational level, term in office, political party alignment with the federal government, political party alignment with the state government, and political party), state X year fixed effects, and municipalities' number of inhabitants (second order polynomial); ii) two-way clustering of standard errors at the micro-region and state X year level, at the municipality and state X year level, and at the meso-region and state X year level, one-way clustering at the micro-region level, at the municipality level, at the meso-region level, and at the state level; iii) baseline sample correcting the data for outliers, baseline sample without correcting the data for outliers, an unbalanced sample that includes municipalities with missing values in either FPM transfers or tax revenues and corrects the data for outliers, and a sample that includes all municipalities with fewer than 300,000 inhabitants and corrects the data for outliers. Our preferred specification (column 2b of Table 3) is colored in blue. 95% (90%) confidence intervals are indicated in light-gray (dark-gray) around the point estimates. 85% (96%) of the estimates are significant at the $p < 0.05$ (0.10) level.

Figure A.5: FPM formula transfer adjustment (raw data)
Negative & positive shock



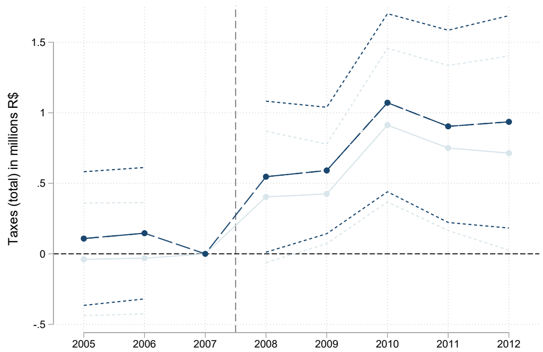
(a) Negative shock



(b) Positive shock

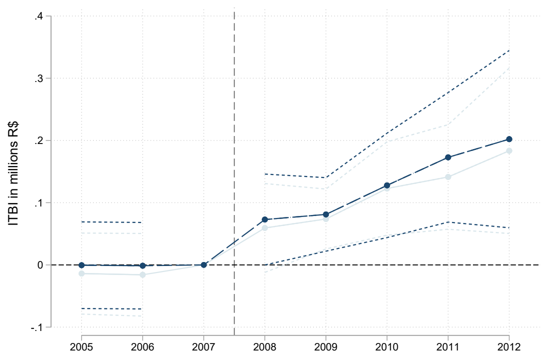
Notes: The figures show the results from Equation 1 for reported FPM formula transfer measured in millions of R$, including in the model municipality and year fixed effects, cutoff X year fixed effects, and a trend variable for jumps across thresholds experienced by municipalities in the treatment group before and after the shock took place in 2008. For a treated municipality that jumped negatively in 2006 and positively in 2009, for instance, this variable will be 0 in 2005, -1 from 2006 to 2007, zero in 2008, and 1 from 2009 to 2012. In blue, the figures present estimates without correcting the data for outliers; while in light-blue the figures present estimates correcting for outliers. Panel (a) shows effects for the negative shock and Panel (b) shows effects for the positive shock. 95% confidence intervals of standard errors two-way clustered by state X year and micro-region level are indicated around the point estimates.

Figure A.6: Tax collection (raw data) Negative shock

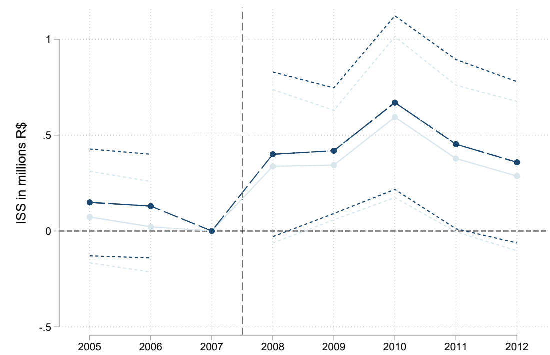


(a) Total

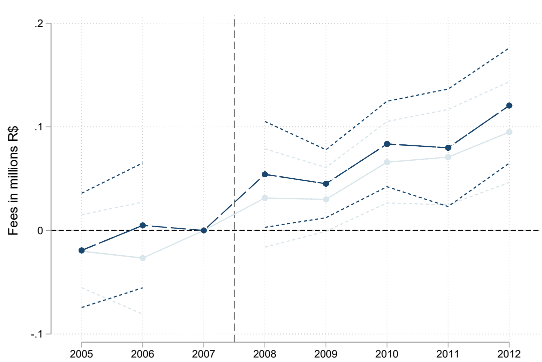
(b) Property tax (IPTU)



(c) Property transaction tax (ITBI)



(d) Service tax (ISS)

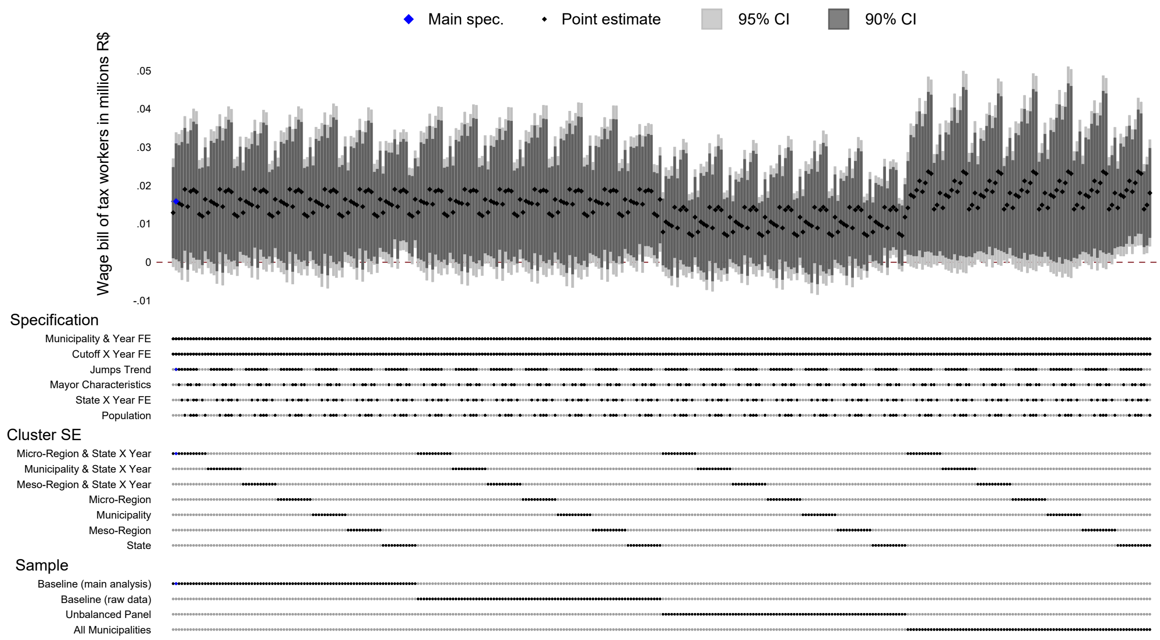


(e) Public service fees

(f) Income tax retained (IRRF)

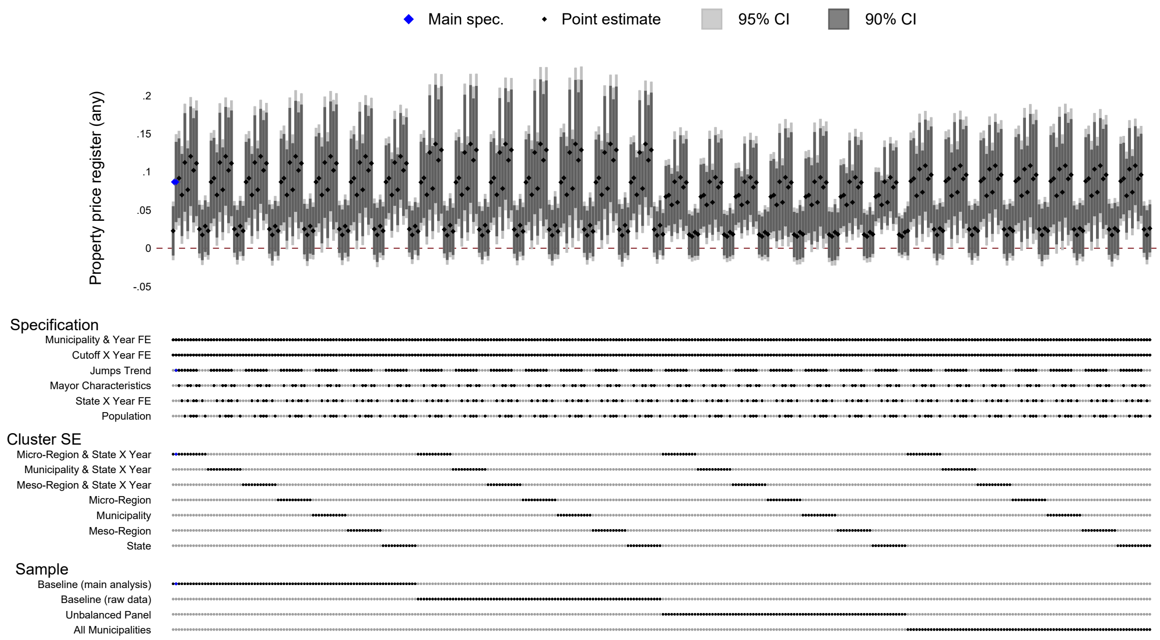
Notes: The figures show the results from Equation 1 for reported total tax revenues in Panel (a), property tax in Panel (b), tax on the transaction of properties in Panel (c), service tax in Panel (d), public service fees in Panel (e), and federal income tax retained by the municipality in Panel (f). The dependent variables are measured in millions of R$. All specifications include municipality and year fixed effects, cutoff X year fixed effects, and a trend variable for jumps across thresholds experienced by municipalities in the treatment group before and after the shock took place in 2008. For a treated municipality that jumped negatively in 2006 and positively in 2009, for instance, this variable will be 0 in 2005, -1 from 2006 to 2007, zero in 2008, and 1 from 2009 to 2012. In blue, the figures present estimates without correcting the data for outliers; while in light-blue the figures present estimates correcting for outliers. 95% confidence intervals of standard errors two-way clustered by state X year and micro-region level are indicated around the point estimates.

FIGURE A.7: TAX WORKERS WAGE BILL - ROBUSTNESS
NEGATIVE SHOCK



Notes: The figure shows robustness for the effect of the FPM formula transfer shock on tax workers reported wage bill using a 2SLS regression model where the FPM formula transfer shock is instrumented with the negative population bracket update. The dependent variable is measured in millions of R$. Each point estimate reports the average effect for the entire post-treatment period of a difference-in-difference model. In total, 336 models were run that combines i) controls for municipality and year fixed effects, cutoff X year fixed effects, a trend variable for jumps across thresholds experienced by municipalities in the treatment group before and after the shock took place in 2008, mayors' characteristics (gender, age, educational level, term in office, political party alignment with the federal government, political party alignment with the state government, and political party), state X year fixed effects, and municipalities' number of inhabitants (second order polynomial); ii) two-way clustering of standard errors at the micro-region and state X year level, at the municipality and state X year level, and at the meso-region and state X year level, one-way clustering at the micro-region level, at the municipality level, at the meso-region level, and at the state level; iii) baseline sample correcting the data for outliers, baseline sample without correcting the data for outliers, an unbalanced sample that includes municipalities with missing values in either FPM transfers or tax workers wage bill and corrects the data for outliers, and a sample that includes all municipalities with fewer than 300,000 inhabitants and corrects the data for outliers. Our preferred specification (column 2b of Table 5) is colored in blue. 95% (90%) confidence intervals are indicated in light-gray (dark-gray) around the point estimates. 66% (87%) of the estimates are significant at the $p < 0.10$ (0.15) level.

Figure A.8: Property price register - Robustness Negative shock



Notes: The figure shows robustness for the effect of the FPM formula transfer shock on reported adoption of a property price register using a 2SLS regression model where the FPM formula transfer shock is instrumented with the negative population bracket update. The dependent variable is a dummy variable = 1 if the municipality adopted a property price register in 2006 and/or 2009—the two years for which we have information, 0 otherwise. Each point estimate reports the average effect for a single post-treatment period of a difference-in-difference model. In total, 336 models were run that combines i) controls for municipality and year fixed effects, cutoff X year fixed effects, a trend variable for jumps across thresholds experienced by municipalities in the treatment group before and after the shock took place in 2008, mayors' characteristics (gender, age, educational level, term in office, political party alignment with the federal government, political party alignment with the state government, and political party), state X year fixed effects, and municipalities' number of inhabitants (second order polynomial); ii) two-way clustering of standard errors at the micro-region and state X year level, at the municipality and state X year level, and at the meso-region and state X year level, one-way clustering at the micro-region level, at the municipality level, at the meso-region level, and at the state level; iii) baseline sample correcting the FPM transfer data for outliers, baseline sample without correcting the FPM transfer data for outliers, an unbalanced sample that includes municipalities with missing values in either FPM transfers or adoption of a property price register and corrects the FPM transfer data for outliers, and and a sample that includes all municipalities with fewer than 300,000 inhabitants and corrects the FPM transfer data for outliers. Our preferred specification (column 3 of Table 6) is colored in blue. 95% (90%) confidence intervals are indicated in light-gray (dark-gray) around the point estimates. 63% (67%) of the estimates are significant at the $p < 0.05$ (0.10) level.