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Urban Planning and Local State Capacity: Evidence from Brazil's City Statute

Guillermo Alves¹

¹Economista Principal. Dirección de
Investigaciones Socioeconómicas.
CAF - Development Bank of Latin
America and the Caribbean
galves@caf.com

This paper studies the effects of a national policy that mandates and supports municipal urban planning. In 2006 Brazil required municipalities above 20,000 inhabitants to adopt a master plan, and the federal government provided technical support for its preparation. I exploit this threshold in a fuzzy regression discontinuity design, combining data on urban regulations, public finances, population censuses, and urban footprint over nearly two decades. Adopting a master plan led municipalities to expand their planning and land-management instruments, invest more in housing and urban infrastructure, and durably raise property tax revenue. The revenue gains operated through stronger tax administration rather than higher statutory rates or a larger base. These actions delivered sustained and narrowly targeted gains in housing, sanitation, and water access for the poorest residents, with no detectable effects on population, economic activity, or the extent and shape of the urban footprint. Mandating and supporting local planning thus strengthened the urban and fiscal dimensions of local state capacity and produced targeted welfare gains.

KEYWORDS

Urbanization, State capacity, Property tax, Housing.

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Planificación Urbana y Capacidad Estatal Local: evidencia del Estatuto de la Ciudad en Brasil

Guillermo Alves¹

Resumen

Este trabajo estudia los efectos de una política nacional que exige y apoya la planificación urbana municipal. El Estatuto de la Ciudad de Brasil de 2001 exigió a los municipios con más de 20.000 habitantes adoptar un plan director, y el gobierno federal brindó apoyo técnico para su elaboración. Se explota este umbral en un diseño de regresión discontinua difusa, combinando datos sobre regulaciones urbanas, finanzas públicas, censos de población y huella urbana a lo largo de casi dos décadas. La adopción de un plan director llevó a los municipios a ampliar sus instrumentos de planificación y gestión del suelo, a invertir más en vivienda e infraestructura urbana y a aumentar de forma duradera la recaudación del impuesto sobre bienes inmuebles. El aumento de la recaudación provino de una administración tributaria más fuerte, y no de mayores alícuotas legales ni de una base imponible más amplia. Estas acciones generaron mejoras sostenidas y focalizadas en vivienda, saneamiento y acceso al agua para los residentes más pobres, sin efectos detectables sobre la población, la actividad económica ni la extensión y la forma de la huella urbana. Exigir y apoyar la planificación local fortaleció así las dimensiones urbana y fiscal de la capacidad estatal local y produjo mejoras de bienestar focalizadas.

Palabras clave: Urbanización. Vivienda. Impuesto predial. Capacidad estatal.

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1 | INTRODUCTION

A central question in the economics of development is how governments acquire the capacity to provide public goods and raise revenue (Besley and Persson, 2009, 2011). In developing countries, this question is increasingly an urban one. Cities concentrate a large and growing share of the population and must confront congestion externalities with limited resources and administrative capacity (Glaeser, 2013; Glaeser and Henderson, 2017; Lall et al., 2017). Managing congestion requires local governments to provide infrastructure and services, regulate land use, and raise the revenue to finance these functions. Whether and how local governments acquire this urban dimension of state capacity remains largely unstudied, despite its consequences for the welfare of much of the developing-world population.

The master plan is the basic legal instrument through which local governments organize urban land use. By defining the urban perimeter, height limits, and floor area ratios, the plan regulates the supply of formal urban land and thereby affects housing informality and affordability. It coordinates the location of private investment, with consequences for the efficiency of the urban layout and access to basic services. Formal urban property, in turn, constitutes the base of the property tax, a local revenue source with well-documented efficiency advantages and persistently low collection in developing countries (Norregaard, 2013).

Producing a master plan is institutionally demanding. It requires technical analysis, negotiation among competing claims on urban land, and enactment as municipal law. This process can build local state capacity beyond what the plan itself contains, but its difficulty also limits adoption. Only 15 percent of all Brazilian municipalities had a master plan in 2005, and fewer than half of Mexican municipalities had one in 2023 (Alves et al., 2025a). Several national governments, including Colombia and India, as well as multilateral organizations, support municipal planning (Government of Colombia, 1997; Government of India, 2005; IDB, 2016), yet the effects of these efforts, and of adopting a master plan more generally, remain undocumented.

Studying the impacts of urban planning institutions in the developing world poses three main challenges. First, economic development and institutional quality are strongly correlated, complicating the identification of causal effects. A set of studies have exploited natural experiments to examine changes in land-use institutions within specific areas of cities (Field, 2007; Galiani and Schargrodsky, 2010; Michaels et al., 2021; Harari and Wong, 2025), but none examines the causal effects of citywide interventions in urban planning capacity. Second, systematic data on the existence of master plans and other local urban regulations across cities are scarce in developing countries. Third, many effects of urban planning materialize only in the long run, so that their evaluation requires outcome data spanning at least a decade or two.

This paper addresses these challenges by examining a federal law that mandated Brazilian municipalities with 20,000 or more inhabitants to implement a master plan. The population threshold enables a regression discontinuity design that addresses the identification challenge. I leverage the high-quality and diverse public data available for Brazilian municipalities to evaluate the effects of approving a master plan on urban laws and regulations, local public finances, the extent and shape of the urban footprint using satellite imagery, and measures of urban infrastructure and quality of life from population censuses. These effects are measured from the very short run to nearly two decades after the mandate was introduced.

The mandate was introduced by the 2001 City Statute (*Estatuto da Cidade*), a federal law that required a set of municipalities to approve a master plan by 2006. The federal government supported compliance through a national campaign, run by the newly created

Ministry of Cities, that provided guidance, training, and technical assistance for plan preparation. In addition to the population rule, the mandate also applied to municipalities located in officially designated metropolitan or tourist areas. I therefore focus on the 3,353 out of 5,564 municipalities that did not meet these additional criteria, representing roughly one-third of Brazil's population. The first-stage estimate indicates that the mandate increased the probability of having an approved master plan in 2008 by 42 percentage points.

The master plan mandate affected Brazilian municipalities along several dimensions. First, the approval of the plan expanded the set of urban management laws and regulations, including participatory democracy institutions central to the spirit of the City Statute. By 2012, treated municipalities exhibited a higher prevalence of urban councils, urban perimeter laws, and instruments for active land use management and land value capture. This institutional upgrading continued over time. By 2018, it included a broad set of zoning laws and land management instruments, notably the designation of zones of special social interest, directed at formalizing informal housing. This first set of results indicates that implementing a plan strengthened the formal institutional dimension of urban state capacity. The second and third sets of results show that these formal changes translated into concrete state actions.

Second, master plans made municipalities more active in implementing housing policies, including the construction of new units and the upgrading of existing ones, and led to increases in expenditure on housing and urban infrastructure. These expenditure effects were large relative to the control mean but small in absolute value, and concentrated in the first years after the mandate. The largest magnitudes appear between 2009 and 2012, then effects remain positive but smaller through 2015 and are zero thereafter. In the peak years, the plan increased annual per capita expenditure by 56 reais on housing policy and 141 reais on urban infrastructure, compared to control group averages of 9 and 129 reais, respectively. Although this period coincided with the large federal public housing program *Minha Casa Minha Vida* (Belchior et al., 2023), I find no effects of having a master plan on the number of units or the expenditure associated with this program, suggesting that the expenditure effects were locally driven and funded.

Third, master plans raised collections from the two local taxes linked to real estate: the urban property tax (IPTU) and the tax on real estate transactions (ITBI). Effects on both taxes emerge around 2009 and persist through the end of the sample period, in 2023. Revenue from these taxes is small in Brazilian municipalities, which are heavily dependent on federal transfers. As with expenditure, the effects on tax revenue were large relative to the control mean but small in absolute value. Several results identify improved collection capacity as the mechanism: master plans increased the likelihood that the property cadastre was computerized and made publicly accessible, and raised revenue from the collection of tax arrears, suggesting successful efforts to reduce delinquency. The interpretation that planning drove these increases is reinforced by a null effect on the main local tax, a sales tax on services (ISS).

Fourth, the improved urban state capacity documented in legislation, expenditure, and tax collection translated into better quality of life in dimensions related to housing and urban infrastructure. These improvements were not broad but narrowly targeted at situations of deprivation, and they connect to the preceding results on laws and expenditure. In the 2010 Census, only a few years after the mandate deadline, treated municipalities exhibited nearly a 70 percent reduction in a self-reported measure of housing informality. Improvements in housing quality persisted more than a decade after the mandate took effect, as shown by the 2022 Census. By that year, municipalities with master plans had roughly tripled the share of households with septic tanks connected to the sewer network, a mid-tier sanitation solution

between stand-alone septic systems and full sewer connections. The plan also eliminated some extreme forms of housing deprivation related to urban infrastructure and services, such as dependence on rainwater and the absence of formal waste disposal. Overall, these targeted improvements are consistent with the relatively modest expenditure effects, which were additionally concentrated in a limited number of years.

Finally, I document a set of informative null effects that complete the characterization of the scope of the impacts of the policy. Leveraging the MapBiomas dataset, which identifies urban land use in Brazil at 30-by-30 meter resolution, I find that having a master plan did not affect city size measured by the total spatial footprint of the urbanized area. There are also no detectable effects on various measures of urban form such as sprawl (Burchfield et al., 2006) or disconnection (Harari, 2020) of the municipalities' capital town or fragmentation of the urban footprint of the whole municipal territory (Alves et al., 2025b). In the already highly urbanized context of Brazil, the plans did not affect the extensive margin of city growth but instead improved the quality of the existing urban territory. The policy likewise had no effects on total population or on measures of local economic activity, including wages, household incomes, rents, or the number of firms.

In sum, the results indicate that master plans improved urban living conditions for the most deprived households by strengthening multiple dimensions of municipal state capacity. Approving a plan induced municipalities to adopt a broader set of planning, participatory, and land management regulations, expanding their ability to manage existing urban areas and mobilize revenues tied to urban land. The resulting increases in own-source revenue partly financed higher investment in housing and urban infrastructure, which in turn translated into improvements in housing formality and access to basic services. The plans had no detectable effects on city size, urban form, or aggregate economic activity. Over the period studied, the regulatory content of the plans thus did not detectably reshape the built environment. The institutional, administrative, and fiscal effects point instead to the process of producing and adopting the plan as the source of the documented gains.

This paper contributes to three strands of literature. First, the paper provides causal evidence on the effects of citywide planning institutions. A large literature documents the negative effects of land-use regulation on housing supply and affordability in developed countries (Saiz, 2010; Turner et al., 2014; Baum-Snow and Han, 2023), with a smaller but growing set of contributions for developing countries (Brueckner et al., 2017; Chen et al., 2024; Nagpal and Gandhi, 2024; Anagol et al., 2026). In settings of weak state capacity, planning interventions can instead have positive effects (Lall et al., 2017). Henderson et al. (2025) document positive effects of greenfield planning on land values in Dar es Salaam, operating through improved property rights and road access, and Michaels et al. (2021) document long-run gains from planned greenfield development in Tanzanian cities. These studies identify the effects of localized interventions within or at the edge of particular cities. The present paper complements them by studying planning as a citywide institution. Additionally, I examine an increasingly relevant setting in which rapid urban growth has already occurred, so that the policy problem is the management of existing urban areas rather than the planning of new land.

Second, the results contribute to the literature on state fiscal capacity in developing countries by identifying an institutional channel through which local governments expand their ability to tax immobile bases. A large body of work emphasizes that fiscal capacity depends on administrative and informational investments rather than statutory tax rates alone (Besley and Persson, 2009, 2011; Ferraz et al., 2024). Taxes on land and real property are widely regarded as among the most efficient and least distortionary sources of local revenue (Oates, 1972; Gordon and Li, 2009), yet they have historically underperformed in developing economies. The findings here suggest that improvements in urban planning

capacity can translate into sustained gains in property tax revenue by strengthening tax collection, specifically through the computerization of the property cadastre, the expansion of public access to cadastre information, and improved collection of tax arrears. The paper confirms that increases in local tax revenue translate into improved infrastructure and welfare outcomes (Gadenne, 2017; Martínez, 2025).

Finally, the paper connects to work on the causes and consequences of slum formation as an extreme form of urban housing deprivation (Brueckner and Selod, 2009; Cavalcanti et al., 2018; Alves, 2018, 2021; Henderson et al., 2020; Rojas Ampuero and Carreras, 2022; Tsivanidis and Gechter, 2023; Harari and Wong, 2025). I find that master plans reduced several extreme forms of housing deprivation in Brazilian municipalities, including informality and inadequate sanitation and water provision. A substantial body of research shows that improvements along these dimensions have large positive effects on residents' welfare (Field, 2005, 2007; Galiani and Schargrodsky, 2010; Galiani et al., 2017; Coury et al., 2024). In particular, extensive evidence documents the severe health consequences of inadequate water and sanitation (Kesztenbaum and Rosenthal, 2017; Alsan and Goldin, 2019), both of which improved following the adoption of master plans in the Brazilian municipalities studied here.

2 | CONTEXT

2.1 | Finances and responsibilities of Brazilian Municipalities

Brazil is a federal state with two levels of subnational government: 27 states, including the federal district, and 5,569 municipalities as of March 2025 (Alves et al., 2025a). Municipal responsibilities include the typical tasks of local territorial management, along with the provision of primary and secondary education and of primary healthcare. Education and health are by far the main expenditure categories. The categories on which I find effects of master plans, housing and urban infrastructure, accounted for only 0.7 and 2.8 percent of total municipal expenditure in 2004 in the sample I study (Table 2).

Transfers from the federal government are the main source of municipal revenue, especially for the small municipalities that constitute the bulk of my sample. The largest single source, accounting for roughly 30 percent of total revenue, is the Fundo de Participação dos Municípios (FPM), a constitutionally mandated federal transfer. Beyond transfers, municipalities rely on three main taxes. In order of revenue importance, these are a tax on services (ISS), an urban property tax (IPTU), and a tax on real estate transactions (ITBI). The IPTU applies a rate set by the municipality to an assessed property value, also determined by the municipality. The ITBI is levied on the registered transaction value rather than on an assessed value. Noncompliance with the IPTU is estimated to be high, although systematic data are not available (Afonso et al., 2013).

2.2 | The 2001 Estatuto da Cidade

In July 2001, the Brazilian parliament approved the City Statute (*Estatuto da Cidade*). The law introduced a set of new urban management instruments and imposed the obligation of having a master plan on a subset of municipalities: those with more than 20,000 inhabitants, those located within officially designated metropolitan regions or areas of special tourist interest, and those influenced by large economic projects. These municipalities were given five years to approve their master plan.

The City Statute was shaped by three principles introduced by the 1988 federal constitution. These are the "right to the city," the "social function of urban property," and expanded

participatory governance. In line with these ideas, the law created several new urban management instruments, including the progressive property tax, joint urban operations (*operações urbanas consorciadas*) and zones of special social interest (ZEIS). These last two tools allow municipalities to apply exceptions to the land use rules (e.g. height limits) for specific areas of the city, the former usually applied in areas of intense demand for private development and the latter to informal settlements. The law established the existence of a municipal master plan as a prerequisite for using these instruments. It also required social participation both in the formulation of the plan and in the governance of the associated instruments. Beyond the participation mandate and the requirement that the plan cover the entire municipal territory rather than only its urban areas, the Statute did not impose detailed requirements on the plan's substantive content, leaving considerable discretion to municipalities. The Federal Ministry of Cities, created in 2003, launched a national campaign in 2005 that provided training, technical guidance, and a network of state-level hubs to support municipalities in meeting the 2006 deadline. These features imply that complying with the mandate involved substantially more than producing a document. Municipalities had to organize participatory processes, develop or contract technical capabilities, in many cases with support from the federal campaign, and secure the approval of the plan as municipal law by the local legislature.

The three panels in Figure 1 show how the mandate generated a sharp increase in the number of municipalities with a master plan. All three panels focus on the 3,353 municipalities in the paper's main sample. Using retrospective data from the 2021 MUNIC survey, panel (a) shows a spike in 2006 in the year of plan approval, with more than 700 municipalities approving their plan that year. High approval rates continued in the two following years, with around 300 municipalities per year. Panel (b) shows the cumulative share of municipalities with a master plan. This share was below 10 percent for the municipalities in the sample in 2005, illustrating a general deficit of urban planning capacity that is common to many developing world cities. The share of municipalities with a master plan jumped discontinuously in 2006 and continued to grow at a decreasing rate, reaching approximately 55 percent by 2021.

Panel (c) of Figure 1 provides further evidence of the Statute's impact through a sharp reduction in the average age of existing master plans in 2006. The figure also reveals that the average age of master plans has risen steadily since 2008, exceeding 12 years by 2021. Although the Statute requires revision at least every ten years, this requirement is not reflected in the data.

3 | DATA AND SAMPLE

The paper employs five main data sources. First, the Brazilian national statistical agency (IBGE) provides annual municipal population estimates. These figures are used to calculate major federal transfers that depend on population brackets (Corbi et al., 2019) and to determine whether a municipality is legally required to prepare a master plan. I use the 2006 IBGE population estimate as the running variable for the main regression discontinuity specification, since 2006 was the statutory deadline and the year in which the largest number of new master plans were approved, as shown in Figure 1.

Second, I use IBGE's MUNIC survey on local public policies, which covers all municipalities. I draw on the 2005, 2008, 2009, 2012, 2015, 2018, 2020, and 2021 editions. In all of these years the MUNIC reports whether each municipality has a master plan, the treatment variable. The 2021 edition additionally reports the year in which the current plan was adopted. I combine these sources into an annual indicator for each year between 2005

and 2021. Because the master plan is a permanent legal instrument, I treat plan status as absorbing, so that a municipality reporting a plan retains one thereafter. A small share of municipalities nonetheless report a plan in one wave and none in a later wave. I resolve these cases by dating adoption to the year most consistent with the municipality's full sequence of reports, namely the single adoption year that minimizes the number of waves it contradicts, breaking ties toward later adoption. I then use the retrospectively reported adoption year to place the transition within the implied window and to fill the years between surveys. For example, a municipality reporting a plan in 2009 and 2012, none in 2015, and a plan again in 2018 and 2021 is coded as having a plan from 2009, since the two early reports outweigh the isolated 2015 zero. As a second example, a municipality reporting no plan in 2009 and a plan in 2012, and whose 2021 record gives 2010 as the adoption year, is coded as having a plan from 2010, filling the unsurveyed years 2010 and 2011. When the retrospective year is unavailable or inconsistent with the surveys, adoption is dated to the first survey in which the plan is observed. 21 percent of municipalities in the sample have at least one inconsistency in their plan existence data and are then subject to this adjustment. Table A.2 in the Appendix reproduces the first stage using the unadjusted plan indicator and shows that the discontinuity is essentially unchanged.

The MUNIC survey additionally reports the existence of a set of policies central to the analysis, including housing construction, titling of informal settlements, the adoption of the new urban management instruments created by the *Estatuto da Cidade*, and indicators of tax collection capacity, such as whether the IPTU cadastre is digitized and made public.

Third, I use the 2010 and 2022 population censuses to measure socioeconomic outcomes at the municipal level, with particular attention to housing quality and access to basic urban services. I also use the 2000 census to obtain pre-treatment measures for validation of the regression discontinuity design and for inclusion as controls in some of the regressions. Table 1 reports municipality-level descriptive statistics from the three censuses for the municipalities in the sample. These data exhibit the strong improvement in housing quality and urban services during the two-decade period between the censuses.

The fourth data source is municipal public finance data from the National Treasury Secretariat (STN) of the Ministry of Finance. Accounts through 2012 come from the FINBRA database, and accounts from 2013 onward come from the annual filings (DCA) collected through the SICONFI system. Both report detailed annual revenues and expenditures, including transfers received from state and federal governments. The data cover 2000 to 2024, although some expenditure categories are available only for a subset of years. The highly detailed categories allow me to separately measure expenditure in housing policies and urban infrastructure and revenue from IPTU and ITBI. Table 2 provides averages and standard deviations for these expenditure and revenue categories. The table illustrates the high dependence of these municipalities on transfers and the small share of total and even own revenue that comes from IPTU and ITBI. The data also allow me to measure the revenue obtained from collection of past-due taxes, a proxy for collection effort. In order to study the financing of housing policies by the federal government, I complement these fiscal data with municipality-level data on the number of housing units built and local expenditure under the Minha Casa Minha Vida federal public housing program.

Finally, I use the 30-meter resolution urban classification from the MapBiomas dataset for each year between 2000 and 2023. I compute several measures of the evolution of each municipality's urban footprint, both for the urban polygon around the municipal capital and for the municipality as a whole. To delineate the urban footprint of each capital I apply a seeded version of the city clustering algorithm of Rozenfeld et al. (2011) to built-up land. Starting from the pixel containing the capital point reported in official maps, I iteratively absorb every built-up pixel located within 2 km of the cluster and stop once no further

built-up pixel lies within that distance. This connectivity criterion mirrors the gap-filling logic used to define urban areas from gridded data (Dijkstra et al., 2021). For this set of pixels I compute Burchfield et al. (2006)'s sprawl measure for urban growth over 2000–2023. Figure A.1 in the Appendix illustrates the resulting measure for the two municipalities with the lowest and highest sprawl values. Using the same set of pixels around the capital, I construct a convex hull containing them and compute its area, perimeter, and Harari (2020)'s disconnection measure in 2023. Figure A.2 illustrates the municipalities with the lowest and highest disconnection indices. Finally, following Alves et al. (2025b), I compute the average share of undeveloped land surrounding each urban pixel in the municipality in 2023. Figure A.3 illustrates this fragmentation index.

The main sample includes all municipalities that existed in 2006 and for which the obligation to prepare a master plan depended only on the 20,000-inhabitant population threshold. I therefore exclude municipalities located in metropolitan regions or urban agglomerations in 2001, the year the Statute was approved, as well as those designated as tourist areas or influenced by large economic projects according to the 2005 MUNIC survey, the edition closest to the deadline. This leaves 3,353 of the 5,564 municipalities that existed in 2006. The municipalities in the sample were home to approximately 70 million residents in 2022, roughly one-third of Brazil's population.

4 | METHODOLOGY

4.1 | Estimation

The City Statute imposed on each municipality i a discontinuous obligation $Z_i = \mathbf{1}[X_i \geq c]$, with running variable $X_i = \text{Pop}_{i,2006}$ and cutoff $c = 20,000$. The obligation was to adopt a master plan, which I denote by the indicator $D_{i,t}$ for each year t . Because plans are absorbing, $D_{i,t}$ is non-decreasing in t within a municipality, so the monotonicity condition required for the instrumental variable interpretation below holds by construction (Imbens and Angrist, 1994).

There are two causal effects of interest. The first is the intention to treat τ^{ITT} , the average effect on an outcome $Y_{i,t}$ of being subject to the mandate at the cutoff, regardless of whether the municipality adopted a plan. I recover it estimating the following regression equation:

$$Y_{i,t} = \alpha + \tau^{\text{ITT}} Z_i + f(X_i - c) + \varepsilon_{i,t}. \quad (1)$$

The second causal effect is the local average treatment effect τ^{LATE} of implementing a master plan, which I recover with a fuzzy design that instruments plan status with the discontinuous mandate:

$$D_{i,t-1} = \pi_0 + \pi Z_i + g(X_i - c) + u_{i,t}, \quad (2)$$

$$Y_{i,t} = \alpha + \tau^{\text{LATE}} D_{i,t-1} + f(X_i - c) + v_{i,t}. \quad (3)$$

In every specification f and g are local polynomials estimated separately on each side of the cutoff. I implement the regression discontinuity design with the `rdplot` and `rdrobust` commands of Calonico et al. (2017) and report robust bias corrected inference. For each outcome with a claimed discontinuity I present binned graphical evidence together with regression estimates under five combinations of kernel, polynomial order, and the inclusion of baseline controls.

The two coefficients speak to distinct policy questions. The object τ^{ITT} is the effect of the obligation itself, which is what the federal legislator controls in this and potentially other contexts in which a higher-than-local government level (national or subnational above local) aims at increasing plan adoption in its jurisdiction. The object τ^{LATE} is the effect of holding a plan in year $t - 1$ for the compliers, the municipalities whose plan status in $t - 1$ is determined by their position relative to the 2006 cutoff. This second object is of interest for thinking about the effect of having a plan and, more broadly, thinking about other types of policies that promote planning adoption at the local level.

The choice of $t - 1$ in $D_{i,t-1}$ deserves a note. As discussed in the previous section, 2006 was the year the mandate became effective and also the year of strongest adoption. However, adoption was not complete in that year, the mandate stayed, and the cutoff kept raising adoption well after the deadline (see the first stage results below). Using plan status in $t - 1$ absorbs every adoption channel through which the mandate operates and in this way respects the exclusion restriction.

Because compliers adopt plans in different years, a complier observed in year t has been exposed to its plan for a duration that varies across the complier population. The coefficient τ^{LATE} therefore averages the effect of the plan across exposure durations, with weights determined by the distribution of adoption dates among compliers (Angrist and Imbens, 1995). Panel (a) of Figure 1 shows that adoption is heavily concentrated between 2006 and 2008, so that for outcomes measured late in the sample period most compliers have been exposed to the plan for most of the years since the deadline, while for outcomes measured shortly after the deadline exposure is correspondingly short. The interpretation of τ^{LATE} thus varies with the outcome year, and I make the implied exposure explicit when discussing each result.

For per capita property tax revenue, which I observe annually over the full period, I complement the static design with a dynamic regression discontinuity specification in the spirit of Cellini et al. (2010) that identifies the average causal response to an additional year of plan exposure. I stack outcome years from 2007 to 2023 and exploit the full set of population-year crossings of the 20,000 threshold from 2006 to 2022. The specification is

$$Y_{i,t} = \theta E_{i,t-1} + \mathbf{W}_{i,k,t}^T \beta + v_{i,k,t}, \quad (4)$$

$$E_{i,t-1} = \pi Z_{i,k} + \mathbf{W}_{i,k,t}^T \gamma + u_{i,k,t}, \quad (5)$$

where a stacked observation is a triple (i, k, t) formed by municipality i , the population year $k \in \{2006, \dots, 2022\}$ whose crossing of the 20,000 threshold serves as the instrument, and the outcome year $t \in \{2007, \dots, 2023\}$, with $t > k$. The instrument $Z_{i,k} = \mathbf{1}[\text{Pop}_{i,k} \geq c]$ shifts cumulative years of plan exposure $E_{i,t-1}$, so that θ identifies the average causal response per year of exposure. The control vector $\mathbf{W}_{i,k,t}$ includes fixed effects defined by the cohort and the outcome year, together with a local linear polynomial in $\text{Pop}_{i,k} - c$ with slopes specific to each cohort and to each side of the cutoff. The static and dynamic designs carry a testable implication that links them. The static τ^{LATE} for a given outcome year should approximate the per-year coefficient θ multiplied by the average exposure of compliers in that year, as implied by the distribution of adoption dates. I assess this implication in the results section.

4.2 | Validity of the regression discontinuity design

I provide three validity checks for the regression discontinuity design. The first two are the standard ones, and the third is specific to the institutional context of Brazilian municipal

finance. The first check examines the continuity of the density of the running variable, the 2006 population, around the 20,000 threshold, which would be violated under sorting or manipulation. The histogram in Figure A.4 in the Appendix shows no visible discontinuity. The formal density manipulation test of Cattaneo et al. (2020) yields a p-value of 0.24, failing to reject continuity.

The second check verifies the absence of discontinuities at the threshold in relevant pre-treatment outcomes. Table A.1 in the Appendix reports the corresponding estimates. Using 2000 census data, I find no discontinuities in average household income or in measures of sanitation, water access, trash collection, and housing informality, including the specific outcomes for which I later find post-treatment effects in the 2010 and 2022 censuses. The table also shows no discontinuities in 2000 in a series of fiscal variables: per capita total expenditure, expenditure on housing and urbanism, and revenue from the IPTU and ITBI. These expenditure and revenue categories, except for total expenditure, display clear post-treatment discontinuities, as shown below.

The third check addresses a potential confounder in this setting. Municipalities rely heavily on federal transfers, in particular the Municipal Participation Fund (FPM), whose allocation is largely determined by population thresholds. A series of papers have found positive effects of these transfers on development outcomes (Litschig and Morrison, 2013; Gadenne, 2017; Corbi et al., 2019). If any FPM threshold coincided with the 20,000 cutoff used in the master plan mandate, the FPM discontinuity could confound the estimated effects. This is not the case. The two closest FPM thresholds are at 16,980 and 23,772 inhabitants. Figure A.5 in the Appendix plots local averages of FPM transfers as a function of the 2006 population. The figure shows clear jumps at the FPM thresholds, indicated by dashed vertical lines, but no discontinuity at 20,000. The absence of a discontinuity at the 2006 population 20,000 threshold holds not only for transfers received in 2006 but also for those received in 2010 and 2014. The only other federal mandate that shares the 20,000 threshold is the 2012 urban mobility plan requirement, which had its deadlines postponed to 2024 and 2025, and is itself conditioned on the master-plan obligation.

4.3 | First stage

Table 3 and Figure 2 document a discontinuous increase in plan adoption at the 20,000-inhabitant threshold of the 2006 population, across several years. In 2008, the earliest year for which data on plan adoption are available after the 2006 deadline, Table 3 shows that the mandate increased the probability of having a master plan by 42 percentage points. The corresponding graphical evidence in Figure 2 displays a clear discontinuity at the threshold, indicating that the mandate roughly tripled the share of municipalities with a plan.

Although municipal population and the other mandate criteria (for example, metropolitan or tourist area status) change over time, the 2006 population cutoff continues to have an effect on plan adoption in later years. Both the table and the figure show clear discontinuities at the 20,000 threshold in the share of municipalities with a master plan in 2012, 2015, and 2021, with magnitudes declining over time. This persistence highlights two features of the natural experiment. First, it is consistent with adoption being nonreversible. Second, because population and the other mandate criteria change over time, the 2006 discontinuity becomes fuzzier in later years.

5 | RESULTS

The mandate affected municipalities along several dimensions, which I group into five sets of results. The first set documents how the mandate generated a substantially richer set of urban regulations and management instruments, many of which were introduced by the City Statute and incorporate participatory governance. These results draw on the 2008, 2012, and especially the 2018 editions of the MUNIC survey, the years with the most detailed information on municipal urban regulations. The mandate increased the adoption of a wide range of laws.

Figure 3 illustrates the discontinuities for three representative regulations in different years: joint urban operations in 2008, the urban perimeter in 2012, and zones of special social interest (ZEIS) in 2018. Table A.3 in the Appendix reports regression estimates for the effect of having a master plan on the adoption of 18 municipal laws, most of them measured in 2018. The effects are generally large, exceeding 20 percentage points for all but one variable, and can be grouped into four categories. First, the mandate increased the use of core planning tools, including laws defining the urban perimeter (Column 2), regular zoning (Column 9), environmental zoning (Column 15), building codes (Column 14), and requirements for impact studies (Columns 4 and 13). Second, it raised the prevalence of participatory institutions, such as urban councils (Column 1). Third, it expanded the use of instruments aimed at the upgrading and formalization of informal settlements, including ZEIS (Column 5), the concession of the real right of use (Column 3), and urban adverse possession (*usucapião*, Column 19). Fourth, it increased the adoption of land value capture instruments, such as the onerous grant of the right to build (*Outorga Onerosa do Direito de Construir*, Column 10), special assessments (*Contribuição de Melhoria*, Column 11), and joint urban operations (*Operação Urbana Consorciada*, Column 12).

The second set of results shows that master plans led to higher investment in housing policy and urban infrastructure. Table A.4 in the Appendix and Figure 4 document positive effects on the share of municipalities reporting, in the MUNIC surveys, the construction of new public housing units in 2008, the existence of a municipal housing fund in 2009, and the implementation of a housing improvement program in 2020. These binary measures reported by municipalities are corroborated by fiscal data from the federal finance ministry. Figure 5 presents regression discontinuity estimates for the effect of the master plan on per capita expenditure on housing (Panel a) and urban infrastructure (Panel b) for each year between 2004 and 2023.¹ The effect on housing expenditure begins in 2009 and persists through 2015. The effect on urban infrastructure is evident as early as 2006, peaks between 2008 and 2013, and persists, though with a smaller point estimate, through 2020. Table 4 reports regression estimates for average per capita expenditure over 2010–2012, the period in which effects are strongest for both categories. Figure A.6 in the Appendix presents the corresponding graphical evidence, showing clear discontinuities in housing and urban infrastructure expenditure in those years.

Table 2 showed that expenditure on housing and urban infrastructure represents a small share of municipal budgets. The estimated effects in Table 4 are very large relative to the control group mean, implying that the plan raised per capita housing expenditure to roughly seven times the control mean and more than doubled per capita urban infrastructure expenditure in those few years when the impact reached its maximum. However, the effects are rather small in absolute value, reaching 56 and 141 reais per capita, respectively, in the preferred specification controlling for the value of the dependent variables in 2004. Combined with the fact that the effects were concentrated in the first years of the mandate,

¹I find no effect of the plan on population, so effects on per capita variables are effectively driven by the numerator.

and that these yearly flows operate on highly durable stocks of housing and infrastructure, these magnitudes imply that aggregate effects on municipal housing and infrastructure outcomes should be moderate.

A natural candidate for explaining the expenditure effect on housing is *Minha Casa Minha Vida*, the large federal housing program that expanded over the same years (Belchior et al., 2026). I find no effect of the master plan on the number of units built or on local expenditure under that program, which indicates that the housing and infrastructure spending documented here was locally driven and locally funded.

The third set of results examines municipal revenues. The mandate raised collections from taxes on real estate. Figure 6 reports regression discontinuity estimates for per capita revenue from IPTU (Panel a), the urban property tax, and ITBI (Panel b), the tax on real estate transactions, for each year between 2004 and 2023. The effect on IPTU begins in 2008, two years after the bulk of plans were approved, and persists through 2023. Figure A.7 in the Appendix presents conventional RDD plots with local averages of per capita IPTU for 2000, 2009, 2013, and 2021 against the 2006 population. The plots confirm the absence of a discontinuity at baseline and the emergence of a clear discontinuity in subsequent years. The effect on ITBI in Figure 6 also begins in 2009 but becomes noisy after 2015, with positive point estimates in some years and null effects in others. Table 5 reports regression estimates for average IPTU and ITBI revenue over 2011–2013, the period of the largest effects. The coefficients are again large relative to the control mean but rather small in absolute value. The preferred point estimates in column 5, which controls for baseline revenue, reach 117 percent of the mean for IPTU and 44 percent for ITBI. As a comparison, the PMAT program studied by Gadenne (2017) increased per capita IPTU by 10 percent. The absolute magnitudes are modest in both settings, amounting here to annual totals of only 34 and 11 reais per capita for IPTU and ITBI, respectively. The mandate did not transform municipal finances in the aggregate in these heavily transfer-dependent municipalities, but it persistently strengthened the property tax.

Table A.5 in the Appendix presents the effect of planning on IPTU pooling several years of outcomes together and clustering standard errors at the municipality level. Column 1 employs only the 2006 population discontinuity, and as such its coefficient is an average of those in Figure A.7, and Column 2 expands the number of observations by pooling all the population years from 2006 to 2022 with outcome-year fixed effects. The results of this alternative specification confirm the magnitudes and significance of the previous ones. Columns 3 and 4 implement the dynamic specification of Equation 4, replacing the binary plan indicator with cumulative years of exposure at $t - 1$. Each additional year of plan exposure increases per capita IPTU revenue by 3.1 and 3.5 reais, respectively. These estimates satisfy the implication linking the static and dynamic designs stated in Section 4. The distribution of adoption dates implies that compliers observed in 2023 have approximately fifteen years of exposure, and the per-year coefficient is approximately one fifteenth of the static estimate for 2023 IPTU revenue. The two designs therefore yield mutually consistent measures of how plan exposure accumulates into revenue, which supports interpreting the static estimate as the cumulated effect of sustained exposure rather than as an artifact of a particular specification.

The differential timing of the effects on IPTU and ITBI is informative about the underlying mechanism. IPTU is an annual tax on the stock of formal properties, whereas ITBI is a transaction tax on the flow of formal real estate transfers. The second set of results above documented the construction of new units and the adoption of laws favoring property formalization, a pattern that reappears in the 2010 Census results below. The transitory ITBI response mirrors the timing of the expenditure effects on housing and urban infrastructure, consistent with a temporary surge in formal transactions driven by new housing units built

or formalized during the first years of plan implementation. Once that wave subsides, ITBI revenue reverts. The IPTU response, by contrast, is permanent, pointing to a durable mechanism rather than a one-time flow effect.

I first rule out the existence of IPTU collection itself as the most basic permanent mechanism: in 2009, 93 percent of sample municipalities already collected the tax according to the MUNIC, and I find no effect of the plan on this proportion in subsequent MUNIC surveys. Once the tax already exists, a permanent increase can arise through three channels: a larger real tax base, higher statutory rates, or stronger administration. The evidence rules out the first two and supports the third. Regarding the quantity side of the tax base, there are no effects of the plan on city expansion, whether measured by urban area in the MapBiomass dataset or by the number of households in the population censuses, and the 2019 MUNIC shows no effect on the number of properties registered in the cadastre. On the price side, there are no effects on average rents paid by renting households in the 2010 Census, by which point the IPTU effect is already present. Although I do not have data on housing prices or rents in later years, the 2022 Census shows no effects on the main fundamentals behind these variables, such as population and wages.

Although ITBI and IPTU rates are set by municipal law, and the City Statute explicitly created the progressive IPTU as an instrument tied to the plan, I find no effect on the adoption of a specific law regulating the progressive IPTU in the 2012 MUNIC. As in [Ferraz et al. \(2024\)](#), I cannot formally rule out changes in ordinary statutory rates in the absence of municipality-level data on these rates. However, as those authors note, rate changes are rare events that require legislative action, and there is no statutory mechanism linking the plan to ITBI rates.

A set of several positive results point instead to improvements in local tax administrative capacity as the main mechanism behind the permanent higher IPTU revenue. Table A.6 in the Appendix shows that the plan substantially raised the probability that the IPTU cadastre was computerized by 2015 and made publicly accessible by 2019. The 2019 MUNIC shows no effect on the updating of property valuations. [Ferraz et al. \(2024\)](#) suggest the reduction of delinquency rates as a mechanism behind the tax revenue effects they document, showing that municipalities with a larger initial tax base are more successful at increasing revenue after a negative shock. I measure this channel directly using revenue from past-due tax collections. Table A.7 in the Appendix shows that the plan roughly doubled per capita collections from *dívida ativa tributária* by 2015, consistent with stronger enforcement against delinquent taxpayers. These findings connect to a growing literature showing that investments in local tax administration can generate large revenue responses without changes in statutory rates, including [Khan et al. \(2016\)](#) on property tax collection in Pakistan and [Basri et al. \(2021\)](#) on corporate taxation in Indonesia.

Two informative null results on the revenue side deserve mention. First, I find no effects on various intergovernmental transfers received by these municipalities, both from the federal and the state governments and across several categories, including funds for housing and infrastructure. This suggests that the observed increase in expenditure was not driven by differential access to federal or state resources. Second, the plan did not affect ISS, the main local tax, reinforcing the interpretation that the revenue effects are specifically tied to the property tax instruments activated by the master plan.

The fourth set of results examines whether the institutional, fiscal, and expenditure changes documented above translated into improvements in living conditions. I find evidence of specific, narrowly targeted effects on housing outcomes in both the 2010 and 2022 population censuses. The results show improvements in situations of more extreme deprivation, affecting a small share of households, rather than broad effects on citywide infrastructure conditions. Importantly, these targeted impacts are consistent with the small

absolute magnitudes of the expenditure effects documented above, thus forming a consistent picture. Figure 7 presents regression discontinuity plots for census outcomes in both years. Table 6 reports regression estimates under alternative specifications, confirming robustness to the choice of kernel, polynomial order, and the inclusion of baseline controls.

In the 2010 Census, only a few years after the mandate deadline, treated municipalities exhibited two specific improvements. First, the mandate reduced the share of households in irregular occupancy. These are households that report not owning, renting, or having permission to use the dwelling they occupy. Only 0.5 percent of households in the sample report this condition, a number that substantially understates the true prevalence of housing informality in Brazil.² The point estimate of 0.35 percentage points thus represents a large proportional reduction in the share of households who self-declare as informal. This result is consistent with the adoption of instruments aimed at tenure formalization, such as zones of special social interest and grants of the real right of use, documented in the first set of results above. Second, the mandate reduced the share of households relying on rainwater as their primary water source by 4.7 percentage points. This effect persists and becomes slightly larger in the 2022 Census. Importantly, both effects in the 2010 census are small, targeted and simple enough that they could have been achieved after the first four years of the mandate coming into effect.

Effects on more complex infrastructure outcomes take more time and emerge over a longer horizon, as shown by the 2022 Census. I find that master plans increased the share of households with septic tanks connected to the sewer network by approximately 6 percentage points by that year, raising it from a control group mean of 2.9 percent to roughly 9 percent. This category captures an intermediate sanitation solution common in Brazilian municipalities where the sewer network was extended to areas with pre-existing septic systems. Rather than a full direct connection, the household's wastewater receives primary treatment in the on-site tank before entering the public network. This outcome requires both household-level investment and municipal infrastructure extension, making it a direct indicator of active sanitation planning. In addition, treated municipalities show a reduction of 0.45 percentage points in the share of households reporting irregular waste disposal in 2022, defined as disposal methods other than collection services, burning, or burying. This represents about four fifths of the control group mean of 0.55 percent and is an additional example of improvements in situations of more extreme deprivation.

These census results indicate that master plans produced targeted, specific improvements at the margins of urban service provision. The effects are concentrated in outcomes that require active municipal intervention but not large amounts of resources, whether through regulatory instruments that formalize tenure or through infrastructure investments that extend sanitation and waste collection networks. The improvements are meaningful in proportional terms but small in absolute magnitude, consistent with a policy that strengthened the capacity of local governments to address well-defined deficits rather than transforming the overall level of urban service provision.

The fifth and final set of results documents several informative null effects. Master plans had no detectable effects on average household income, wages, or rents in either the 2010 or 2022 Census. Using urban land classifications from MapBiomas, I also find no effects on several measures of urban form: the total urban area of the municipality or of the capital, Burchfield et al. (2006)'s sprawl measure of 2000–2023 urban growth, the disconnection measure of Harari (2020), or the fragmentation measure of Alves et al. (2025b).

²For instance, the share of households in officially identified informal neighborhoods (*aglomerados subnormais*) reached 5.6 percent in the 2010 Census (IBGE, 2011). I do not use this official measure because the IBGE explicitly acknowledges that the identification of informal neighborhoods depends on successful collaboration with local authorities.

The mandate thus did not alter the extensive margin of city growth or its spatial structure. These null effects reinforce the interpretation that master plans improved urban outcomes by strengthening local capacity to address targeted deficits, rather than by reshaping the city's size, form, or economic base.

Taken together, the results document effects along the full chain running from institutions to public finances and living conditions. The mandate induced municipalities to adopt a broader set of planning, participatory, and land management regulations. It durably raised property tax revenue through improvements in tax administration, and this additional own-source revenue partly financed higher investment in housing and urban infrastructure, which translated into small but effectively targeted improvements in housing formality and access to basic services. Over the same period, the plans had no detectable effects on city size, urban form, population, or aggregate economic activity. This combination of results is consistent with the effects of the mandate originating in the process of producing and adopting the plan, which developed the institutional and administrative capabilities of municipal governments, rather than in the plan's content as a regulatory document, which produced no detectable changes in the built environment within the horizon studied.

6 | CONCLUDING REMARKS

Approving a master plan under the City Statute mandate strengthened the urban and fiscal dimensions of local state capacity in Brazilian municipalities. The plans generated targeted improvements in housing and basic urban services, financed in part by a persistent increase in property tax revenue that originated in stronger tax administration rather than in higher statutory rates or a larger tax base.

These findings have implications for the design of urban planning policy in developing countries. The results show that the process of producing a plan generates measurable changes in the set of urban management instruments that municipalities adopt, in their fiscal capacity to tax urban land, and in the extent to which they address extreme forms of housing deprivation. The mechanism is not the plan itself as a document, but the institutional development generated by the obligation to produce and approve the plan.

Several limitations of the analysis point to directions for future research. The regression discontinuity design identifies the effect of having a plan for municipalities near the 20,000-inhabitant threshold, outside metropolitan areas. These are small and mid-sized municipalities that together house roughly one-third of Brazil's population. The almost two-decade period of the analysis allows me to document persistent effects on fiscal capacity and targeted effects on housing deprivation, with no effects on measures of city size and shape. However, changes in the built urban structure are durable and path-dependent, so effects on city size and form may materialize only over horizons longer than the one studied here.

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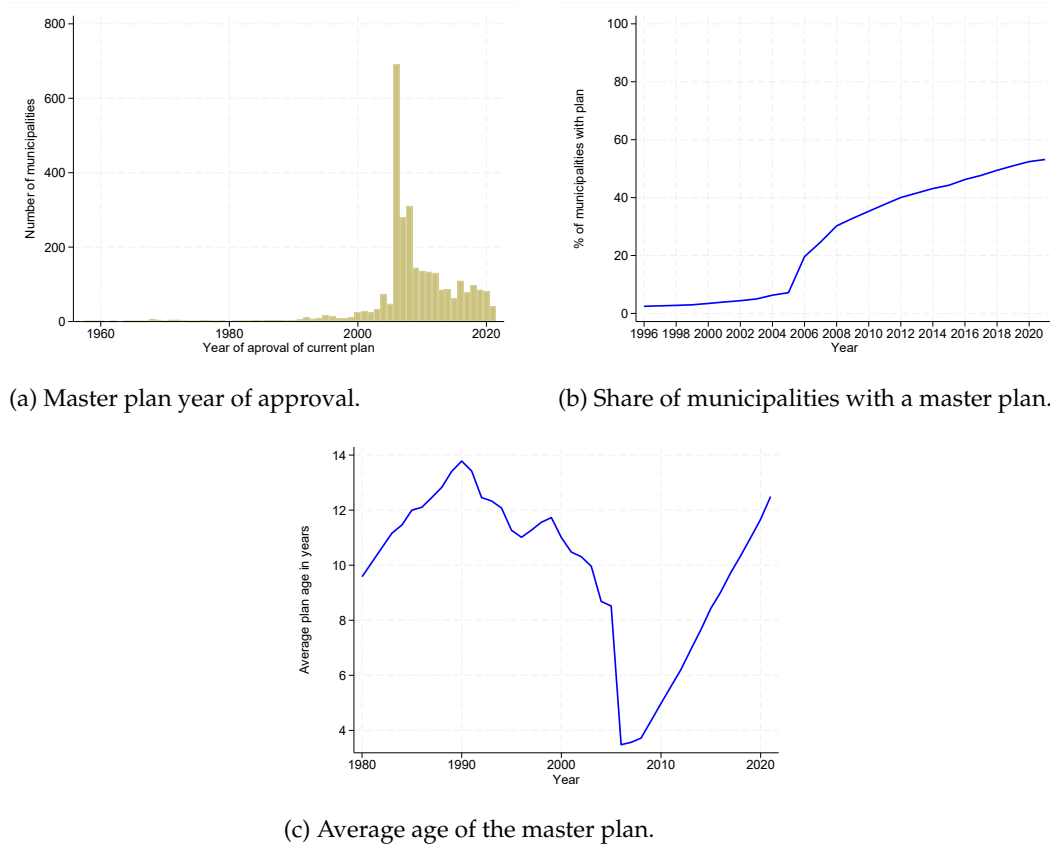
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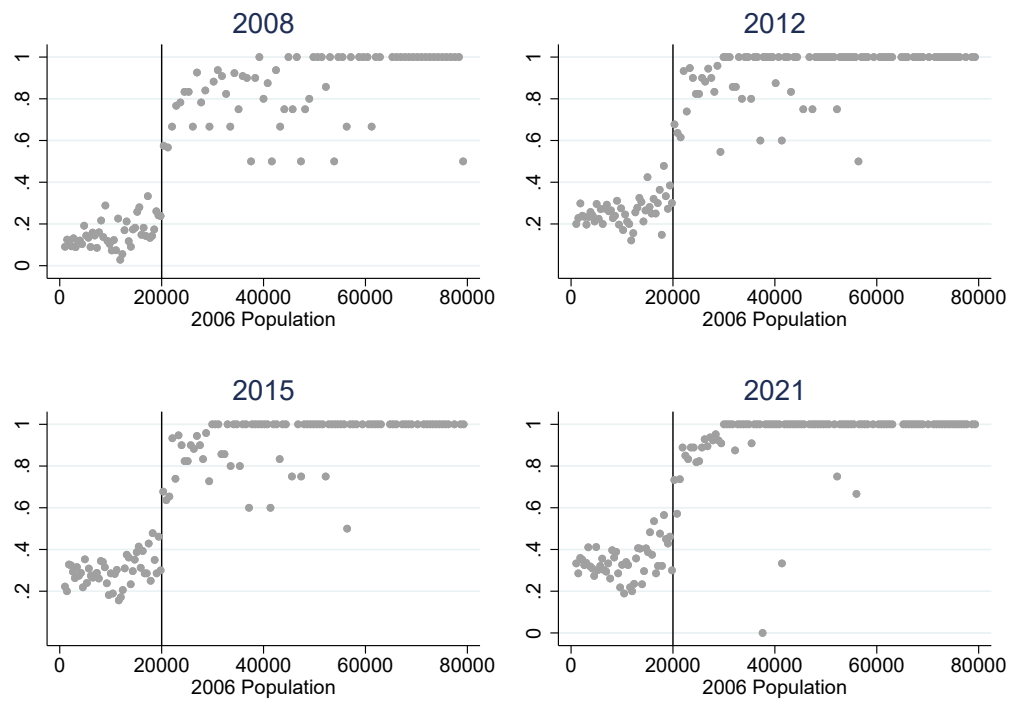
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TABLES AND FIGURES

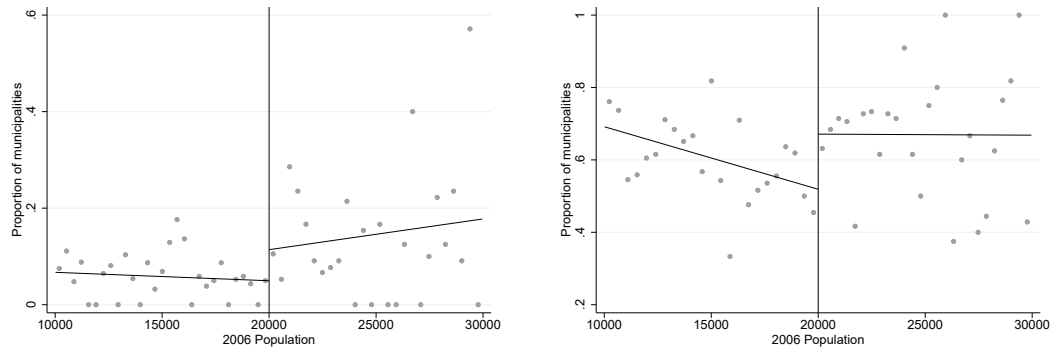
FIGURE 1 Master plans in Brazilian municipalities

Notes: All panels are constructed with data from the 2021 edition of the MUNIC survey. In panel (a), the approval year refers to the municipality's current plan at the time of that survey. Panel (b) plots the cumulative share of municipalities with an adopted master plan by year. Panel (c) reports the average age in years of the current master plan as of 2021.

FIGURE 2 Impact on the probability of having a master plan.

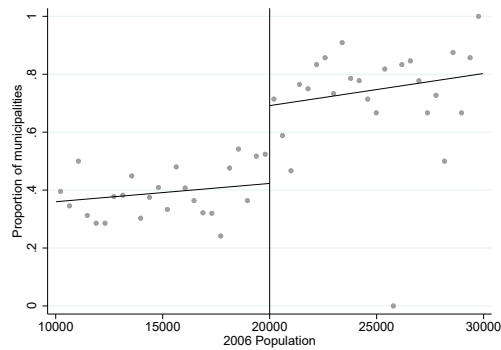


Notes: The y-axis variables are binary indicators of whether the municipality reported having a master plan in each MUNIC edition. The dots are local averages in bins of the same size. This size corresponds to the mimicking variance evenly-spaced method using spacing estimators provided by the `rdplot` Stata command by [Calonico et al. \(2017\)](#). I restrict the sample to municipalities with a population of less than 80,000 in 2006.

FIGURE 3 Impact on various urban laws.

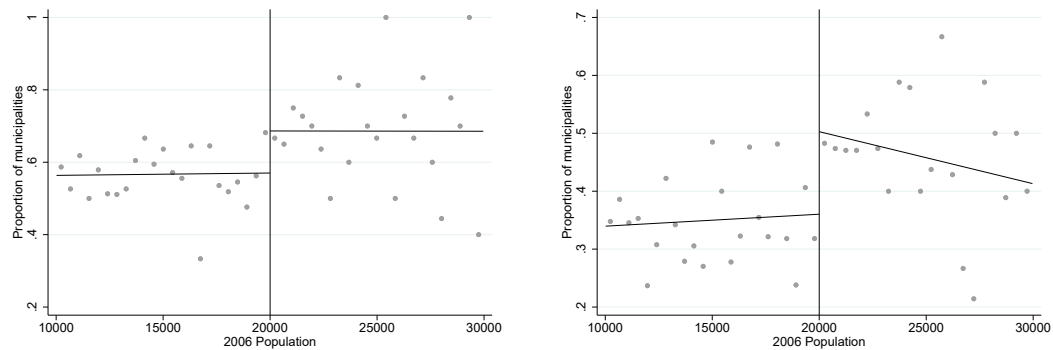
(a) Joint development. 2008.

(b) Urban perimeter. 2012.



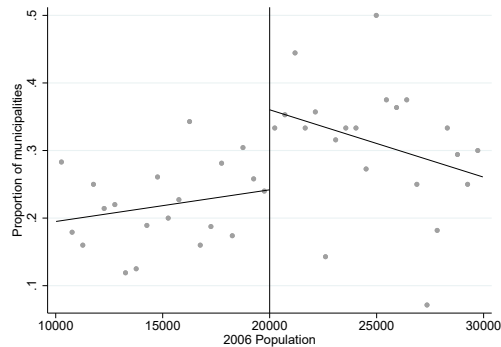
(c) Special zones of social interest. 2018.

Notes: The y-axis variables are binary indicators of whether the municipality declared to have each law in different editions of the MUNIC survey. The dots are local averages in bins of the same size. This size corresponds to the mimicking variance evenly-spaced method using spacing estimators provided by the `rdplot` Stata command by [Calonico et al. \(2017\)](#). I restrict the sample to municipalities with a population of more than 10,000 and less than 30,000 in 2006.

FIGURE 4 Impact on housing interventions.

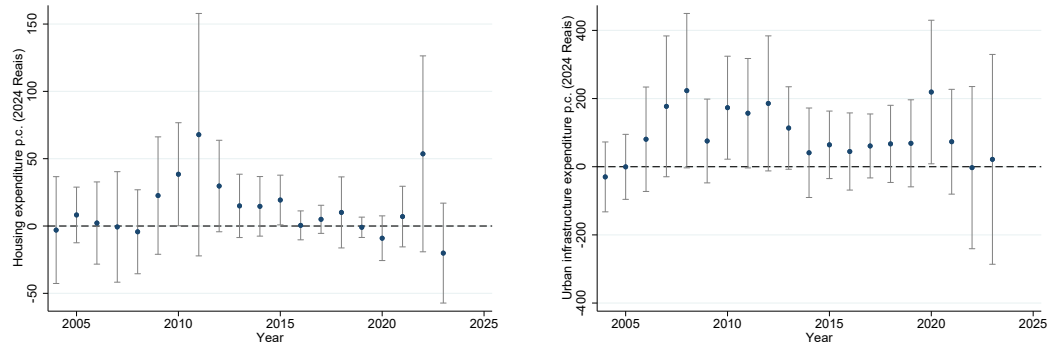
(a) Public construction of new housing units by the municipality. 2007 and 2008.

(b) Existence of municipal public housing fund. 2009.



(c) Improvement of existing housing units. 2018 and 2020.

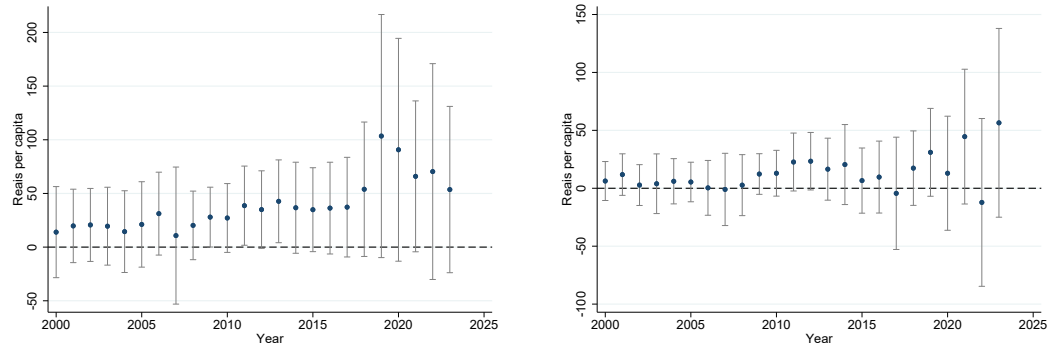
Notes: The dots are local averages in bins of the same size. This size corresponds to the mimicking variance evenly-spaced method using spacing estimators provided by the `rdplot` Stata command by [Calonico et al. \(2017\)](#). The lines are polynomials of degree one estimated separately on each side of the threshold. Variables represented on the y-axis are binary outcomes obtained from different editions of the MUNIC survey. The existence of public construction of new housing units is obtained from the 2008 edition, the existence of a municipal housing fund from the 2009 edition, and the existence of actions improving existing housing units from the 2020 MUNIC. The years of reference of each outcome may differ from the year of the survey, as indicated in each graph's title. For instance, the 2020 MUNIC edition asked for improvement actions between 2018 and 2020. I restrict the sample to municipalities with a population of more than 10,000 and less than 30,000 in 2006.

FIGURE 5 Impact on per capita public expenditure in housing and urban infrastructure.

(a) Housing

(b) Urban infrastructure

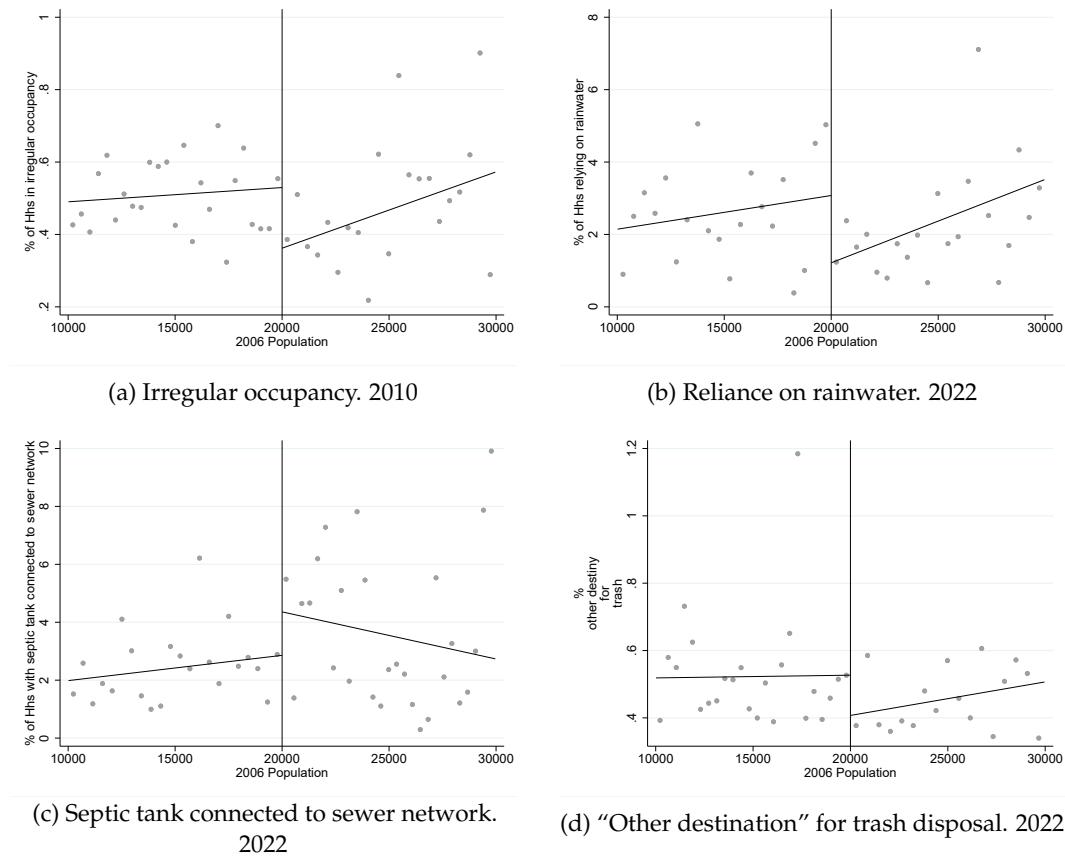
Notes: Each point is the year-specific estimate from a single stacked two-stage least squares regression. The endogenous treatment is plan status in the year before the outcome interacted with outcome-year indicators, instrumented by the interaction of the 20,000 cutoff indicator (2006 population as the running variable) with outcome-year indicators. The specification includes outcome-year fixed effects and running-variable slopes specific to each year and to each side of the cutoff, weights observations with a triangular kernel within a common bandwidth, and clusters standard errors by municipality. The dependent variable is per capita expenditure in the corresponding year, in 2024 reais. Bands are 95 percent confidence intervals.

FIGURE 6 Effect on per capita revenue from IPTU and ITBI taxes.

(a) IPTU

(b) ITBI

Notes: Each point is the year-specific estimate from a single stacked two-stage least squares regression. The endogenous treatment is plan status in the year before the outcome interacted with outcome-year indicators, instrumented by the interaction of the 20,000 cutoff indicator (2006 population as the running variable) with outcome-year indicators. The specification includes outcome-year fixed effects and running-variable slopes specific to each year and to each side of the cutoff, weights observations with a triangular kernel within a common bandwidth, and clusters standard errors by municipality. The dependent variable is per capita revenue from the corresponding tax in the corresponding year, in 2024 reais. Bands are 95 percent confidence intervals.

FIGURE 7 Impact on housing outcomes in the 2010 and 2022 censuses.

Notes: The dots are local averages in bins of the same size. This size corresponds to the mimicking-variance evenly-spaced method using spacing estimators provided by the `rdplot` Stata command by [Calonico et al. \(2017\)](#). The lines are polynomials of degree one estimated separately on each side of the threshold. Variables represented on the y-axis are obtained from the 2010 and 2022 Brazilian Censuses. I restrict the sample to municipalities with a population of more than 10,000 and less than 30,000 in 2006.

TABLE 1 Descriptive statistics from the population censuses.

	2000		2010		2022	
	Mean	sd	Mean	sd	Mean	sd
Population	16,750	49,506	18,270	60,143	20,847	49,722
Occupancy status (%)						
Owner	78.9	7.8	76.9	9.2	76.8	9.0
Renter	12.4	6.1	11.3	6.6	14.9	7.5
Informal occupant	0.52	1.10	0.50	0.81	0.62	1.16
Other tenure status	8.2	4.5	11.3	5.5	7.6	3.7
Trash disposal (%)						
Collected	49.5	27.0	67.8	21.8	78.6	17.6
Burned or buried	29.7	19.0	26.8	18.2	20.1	16.8
Dumped on vacant land	15.3	16.9	4.6	6.9	0.8	1.4
Other destination	4.2	10.4	0.9	1.5	0.6	1.2
Water provision (%)						
Public network	55.7	24.8	68.4	19.9	71.2	19.6
Well or spring	28.9	20.6	23.9	18.4	22.3	17.5
Rainwater	—	—	2.1	6.4	1.6	5.4
Other form	14.1	19.1	5.6	9.9	4.3	9.6
Sanitation (%)						
Sewer network	31.8	37.8	28.7	32.0	32.6	33.1
Septic tank	9.3	15.6	12.1	16.0	19.0	22.0
connected to sewer	—	—	—	—	2.9	6.8
Rudimentary cesspit	41.2	28.8	45.3	29.1	41.1	30.9
Other	7.0	10.5	7.9	10.4	5.3	8.5
No bathroom/sanitation	19.8	22.4	6.9	10.7	1.4	3.4
N	3293		3353		3353	

Notes: The sample includes municipalities not belonging to metropolitan or tourist areas in 2005. All variables except population are expressed as a share of households (%). Rainwater data are not available in the 2000 Census; the water category for that year includes rainwater. The 2022 Census distinguishes septic tanks connected to the sewer network from those not connected; this distinction is not available in earlier censuses. sanitation groups ditch, river/lake/sea, and other forms.

TABLE 2 Descriptive statistics: per capita municipal expenditure and income variables in 2024 Brazilian reais (BRL).

	2004		2013		2023	
	Mean	sd	Mean	sd	Mean	sd
Panel A: Municipal Expenditure						
Total	2564.1	1358.6	4391.3	2022.5	7118.7	3308.3
Housing	18.9	73.1	12.4	110.6	13.5	70.9
Urban infrastructure	72.6	131.2	113.9	181.6	311.7	441.5
Panel B: Municipal Revenue						
Total	2621.2	1389.1	4578.9	2235.0	7294.6	3538.7
Own Revenue	50.6	354.3	530.7	611.9	1237.7	1079.0
IPTU	25.8	60.8	38.0	75.1	78.0	129.7
ITBI	17.1	30.3	35.3	55.3	67.0	124.7
N	3206		3298		3325	

Notes: Revenues are current or effective. In some years, they differ slightly from budgetary revenue. The sample is those municipalities not belonging to metropolitan or tourist areas in 2005.

TABLE 3 First stage

	(8)	(9)	(12)	(15)	(18)	(21)
RD Effect	0.43*** (0.05)	0.42*** (0.05)	0.39*** (0.06)	0.34*** (0.06)	0.29*** (0.06)	0.29*** (0.06)
Control mean	0.14	0.18	0.25	0.29	0.32	0.34
Bandw.	19261	18571	16896	15774	14788	14645
Eff. Obs.	3077	3052	2600	2247	1944	1912
Obs.	3353	3353	3353	3353	3353	3353

Notes: Each column reports the first stage, the discontinuity at the 20,000 population cutoff (2006 population as the running variable) in the probability of having an adopted master plan in the year shown in the column header. Point estimates are conventional; robust bias-corrected standard errors in parentheses (*rdrobust*, Calonico et al., 2017). Local linear regression with a triangular kernel in a symmetric, MSE-optimal window. Control mean is the average of the dependent variable for municipalities below the cutoff within the respective bandwidth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 4 Effects on Per Capita Expenditure in Housing and Urban Infrastructure. 2010–2012 average.

	Housing					Infrastructure				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Effect	53.2** (24.4)	32.8** (17.8)	55.3* (26.1)	25.2** (15.9)	56.3** (24.1)	176.0* (79.3)	167.8** (106.2)	179.4* (74.2)	172.6** (109.8)	140.8 (71.4)
Control mean	9.1	6.3	8.2	7.7	9.4	129.6	115.4	126.5	115.1	129.2
Bandw.	7028	2258	5819	1383	7124	11726	2560	9190	1711	12198
Eff. Obs.	703	221	560	140	667	1329	245	974	171	1292
Obs.	3321	3321	3321	3321	3086	3321	3321	3321	3321	3086
Pol. Order	1	0	1	0	1	1	0	1	0	1
Kernel	Triang	Triang	Unif	Unif	Triang	Triang	Triang	Unif	Unif	Triang
Controls	No	No	No	No	Yes	No	No	No	No	Yes

Notes: Estimates correspond to a fuzzy regression discontinuity design with the 2006 population as the running variable, 20,000 as the cutoff, and having a master plan in the year preceding the outcome as the treatment. Point estimates are obtained with a local polynomial regression with a triangular or uniform kernel, a polynomial of degree 0 or 1, and symmetric Mean Squared Error-optimal windows using the `rdrobust` Stata command by Calonico et al. (2017). Column (5) controls for the 2004 value of per capita housing expenditure; column (10) controls for the 2004 value of per capita urban infrastructure expenditure. Control mean is the average of the dependent variable for municipalities below the 20,000 cutoff within the respective bandwidth.

TABLE 5 Effects on IPTU and ITBI Per Capita Revenue

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: IPTU per capita, 2011–2013 average</i>					
RD Effect	40.1* (20.1)	53.7*** (24.5)	25.2 (20.4)	47.6** (25.6)	33.8** (15.9)
Control mean	28.7	28.8	29.8	30.3	28.9
Bandw.	16497	3937	13152	2494	16424
Eff. Obs.	2457	380	1561	241	2331
Obs.	3298	3298	3298	3298	3146
<i>Panel B: ITBI per capita, 2011–2013 average</i>					
RD Effect	22.4 (13.5)	18.3 (16.7)	22.1 (14.0)	22.9 (19.1)	11.4 (8.8)
Control mean	25.2	21.0	23.6	20.7	26.0
Bandw.	11385	2608	9009	1632	13893
Eff. Obs.	1296	252	958	162	1659
Obs.	3342	3342	3342	3342	3185
Pol. Order	1	0	1	0	1
Kernel	Triang	Triang	Unif	Unif	Triang
Controls	No	No	No	No	Yes

Notes: Estimates correspond to a fuzzy regression discontinuity design with the 2006 population as the running variable, 20,000 as the cutoff, and having a master plan in the year preceding the outcome as the treatment. The dependent variable is per capita IPTU revenue in 2024 Brazilian reais. Point estimates are conventional; robust bias-corrected standard errors in parentheses (`rdrobust`, Calonico et al., 2017). Column 5 controls for the baseline (2000) value of per capita IPTU revenue. Control mean is the average of the dependent variable for municipalities below the cutoff within the respective bandwidth. *p < 0.10, **p < 0.05, ***p < 0.01.

TABLE 6 Effects on Census Outcomes

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Informal occupancy (2010 Census)</i>					
RD Effect	-0.38** (0.18)	-0.24** (0.16)	-0.31* (0.17)	-0.24** (0.15)	-0.35* (0.18)
Control mean	0.51	0.51	0.51	0.50	0.50
Bandw.	15622	4743	12221	3340	14987
Eff. Obs.	2191	457	1419	328	1976
Obs.	3353	3353	3353	3353	3294
<i>Panel B: Rain dependent (2010 Census)</i>					
RD Effect	-4.71** (1.87)	-3.04*** (1.66)	-4.22* (1.92)	-2.91*** (1.75)	-4.74* (1.92)
Control mean	2.54	3.03	2.92	3.02	2.68
Bandw.	15391	6241	12040	3927	14326
Eff. Obs.	2108	614	1401	382	1834
Obs.	3353	3353	3353	3353	3353
<i>Panel C: Rain dependent (2022 Census)</i>					
RD Effect	-5.59** (2.53)	-4.24*** (2.34)	-5.17** (2.62)	-4.44*** (2.43)	-6.59** (3.04)
Control mean	2.10	2.57	2.41	2.96	2.38
Bandw.	14580	5065	11684	3161	11784
Eff. Obs.	1902	490	1343	310	1359
Obs.	3353	3353	3353	3353	3353
<i>Panel D: Septic tank connected to sewer network (2022 Census)</i>					
RD Effect	5.93** (3.34)	6.86** (4.16)	4.70* (3.25)	7.25** (4.28)	6.01** (3.38)
Control mean	2.74	2.43	2.62	2.52	2.71
Bandw.	17146	3784	13569	2857	17064
Eff. Obs.	2697	366	1682	276	2638
Obs.	3353	3353	3353	3353	3293
<i>Panel E: Other trash (2022 Census)</i>					
RD Effect	-0.45* (0.34)	-0.35* (0.26)	-0.47* (0.38)	-0.50** (0.31)	-0.45* (0.34)
Control mean	0.54	0.54	0.52	0.57	0.55
Bandw.	11680	4502	9016	3252	11510
Eff. Obs.	1343	430	962	321	1317
Obs.	3353	3353	3353	3353	3353
Pol. Order	1	0	1	0	1
Kernel	Triang	Triang	Unif	Unif	Triang
Controls	No	No	No	No	Yes

Notes: Estimates correspond to a fuzzy regression discontinuity design with the 2006 population as the running variable, 20,000 as the cutoff, and having a master plan in the year preceding the outcome as the treatment. Point estimates are conventional; robust bias-corrected standard errors in parentheses (*rdrobust*, Calonico et al., 2017). Column 5 controls for the baseline value of the outcome in the 2000 Census. Control mean is the average of the dependent variable for municipalities below the cutoff within the respective bandwidth.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A | ONLINE APPENDIX

FIGURE A.1 Low and high 2000–2023 urban sprawl of municipalities' capitals.

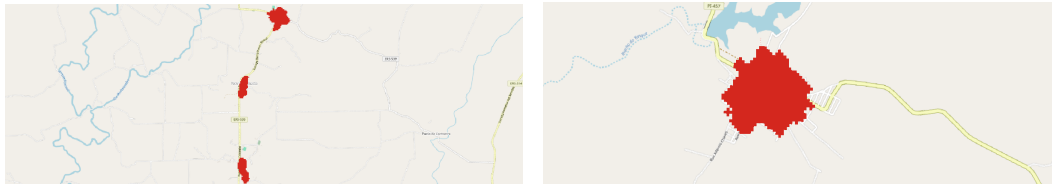


Brasília de Minas. Index = 0.219

Porto de Pedras. Index = 0.861

Notes: The two municipalities are the ones with the lowest (left) and highest (right) 2000–2023 sprawl using [Burchfield et al. \(2006\)](#)'s measure.

FIGURE A.2 Low and high disconnection in 2023. Urban footprint of municipalities' capitals.

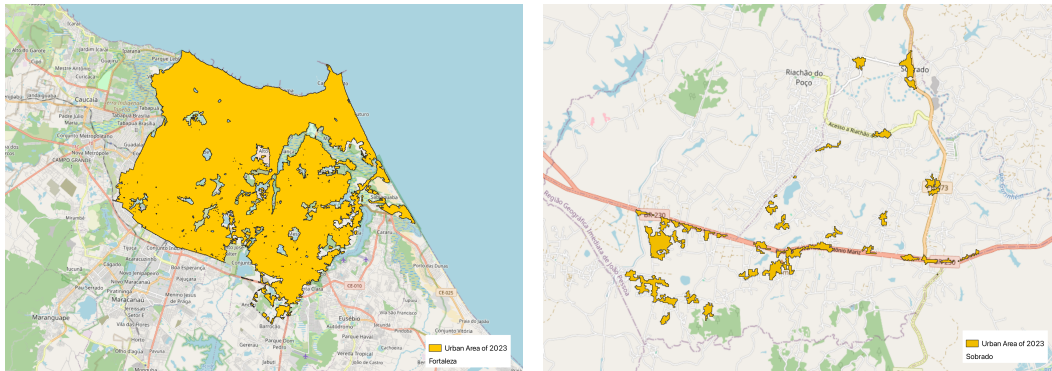


Nova Ramada. Disconnection Value: 0.246.

Curral novo do Piauí. Disconnection Value: 0.989.

Notes: The two municipalities' capitals with the lowest (left) and highest (right) values of [Harari \(2020\)](#)'s disconnection indicator.

FIGURE A.3 Low and high fragmentation in 2023. Urban footprint in the whole territory of the municipality.

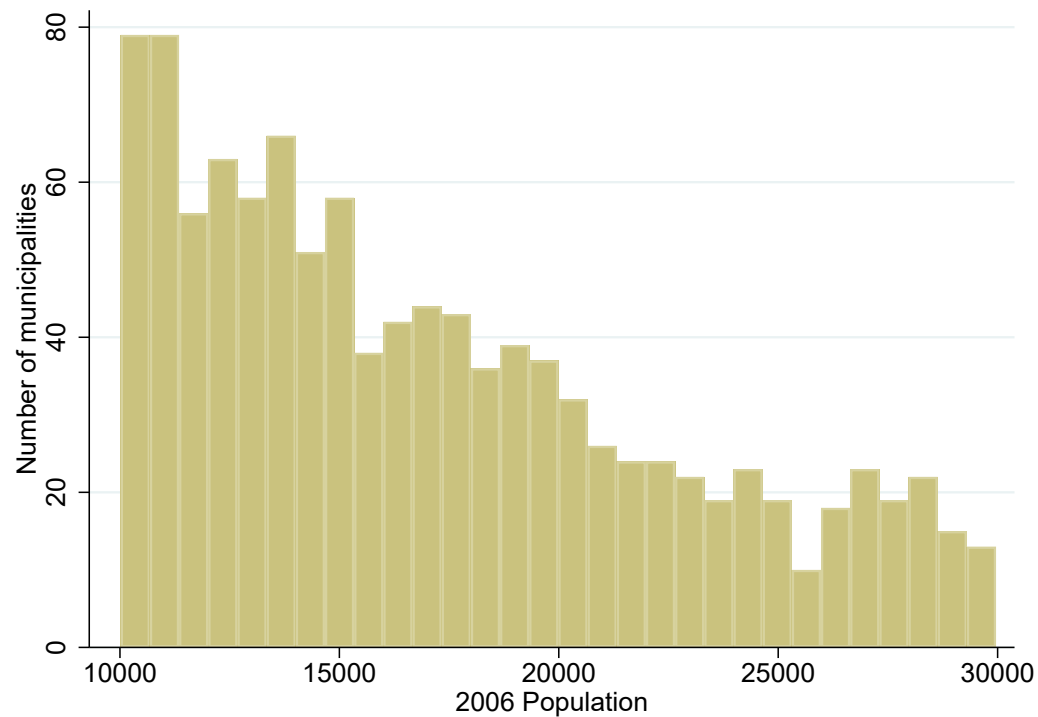


Fortaleza. Index: 0.104

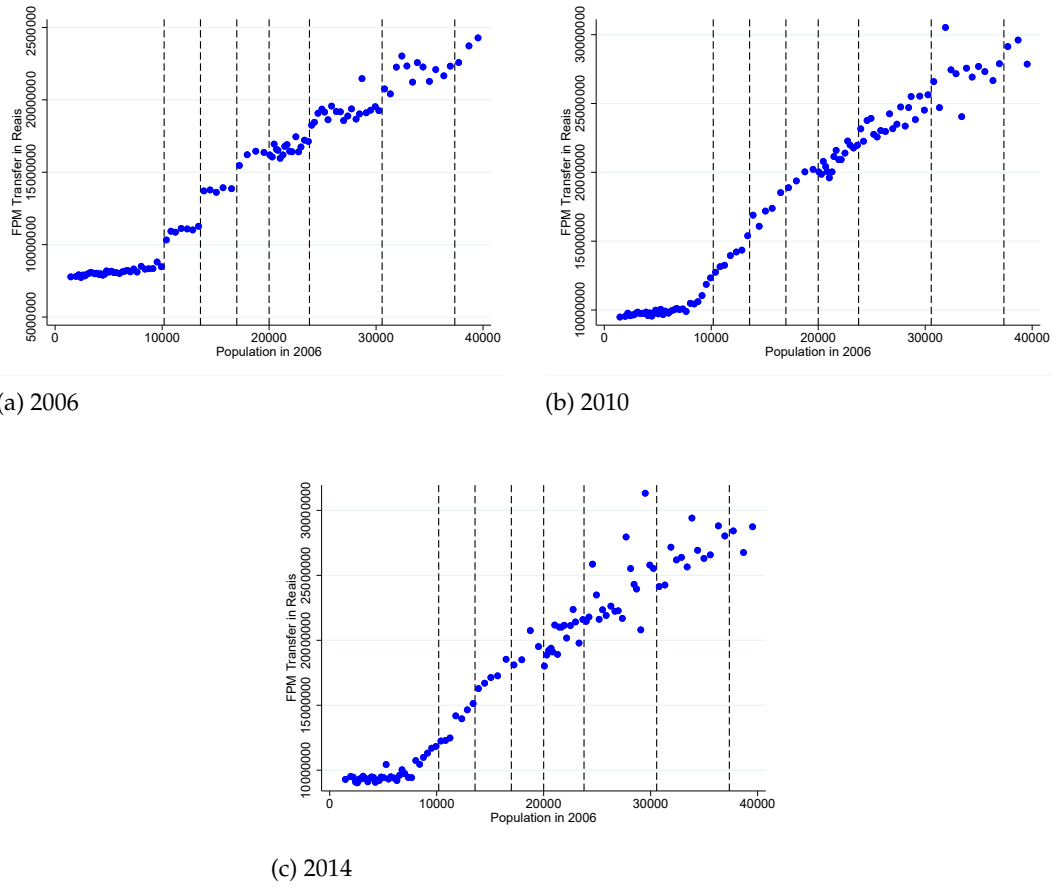
Sobrado. Index: 0.904

Notes: The two municipalities with the lowest (left) and highest (right) [Alves et al. \(2025b\)](#)'s fragmentation indicator.

FIGURE A.4 Number of municipalities near the 20,000 population threshold.

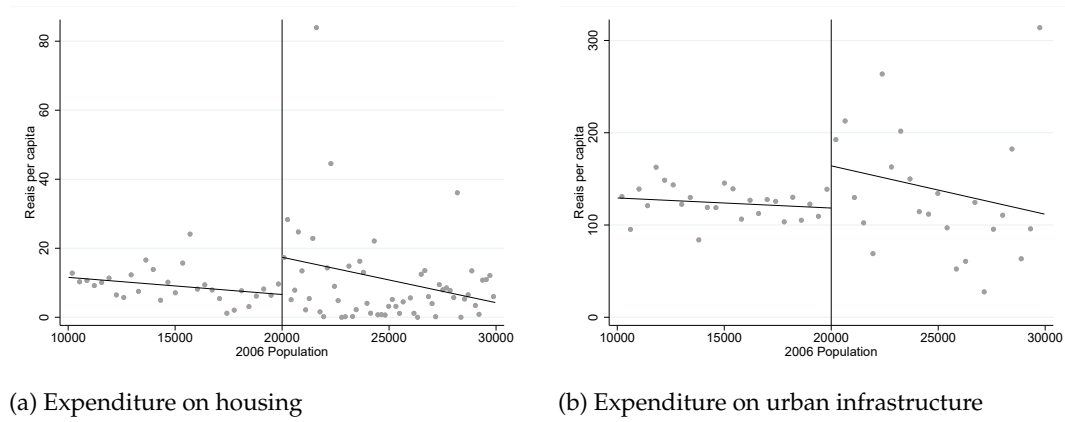


Notes: Population estimated by IBGE. The sample includes municipalities for which the master plan requirement depended only on the population threshold. It therefore excludes municipalities that were part of metropolitan regions or urban agglomerations in 2001 according to IBGE, or of tourist areas or areas influenced by large economic projects according to the 2005 MUNIC survey.

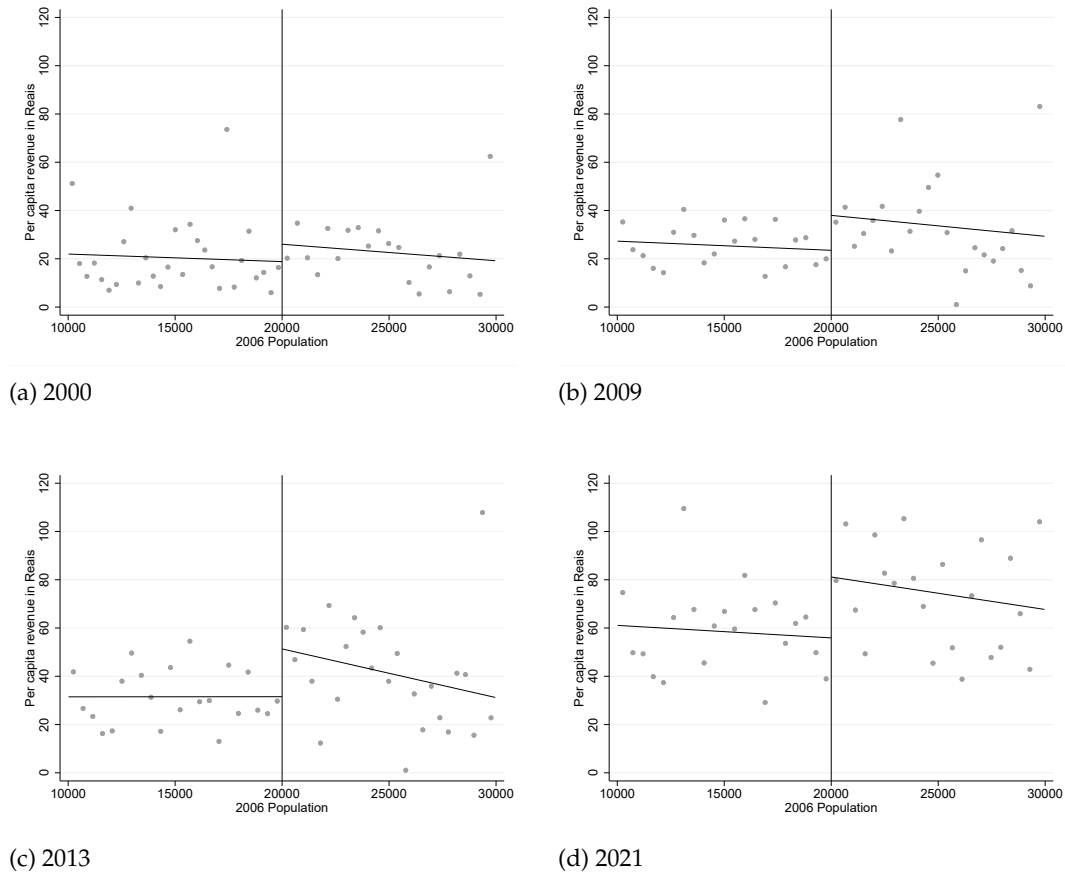
FIGURE A.5 2006 population and FPM transfers across different years.

Notes: The graphs show local averages, in bins of 50 observations, of the 2006 population and the respective year of FPM transfers. Bins are constructed such that observations on each side of the 20,000 2006 population discontinuity belong to different bins. The vertical lines correspond to the FPM thresholds except for the 20,000. Values reflect 2024 reais.

FIGURE A.6 Effect on per capita public expenditure on housing and urban infrastructure: 2010–2012 average.



Notes: The dots are local averages in bins of the same size. This size corresponds to the mimicking-variance evenly-spaced method using spacing estimators provided by the `rdplot` Stata command by [Calonico et al. \(2017\)](#). The lines are first-degree polynomials estimated separately on each side of the threshold. Values reflect 2024 reais.

FIGURE A.7 Effect on per capita revenue from IPTU.

Notes: The dots are local averages in bins of the same size. This size corresponds to the mimicking-variance evenly-spaced method using spacing estimators provided by the `rdplot` Stata command by [Calonico et al. \(2017\)](#). The lines are first-degree polynomials estimated separately on each side of the threshold. Values reflect 2024 reais.

TABLE A.1 Validity of the Regression Discontinuity Design. Effects on Variables Measured Before Treatment

	RD Effect	Std. Err.	p-value	Bandw.	Eff. Obs.	Obs.
<i>Panel A: 2000 Census</i>						
Mean household income	44.45	(36.81)	0.42	14842	1953	3294
Trash: other destination (%)	0.64	(0.98)	0.29	14750	1931	3353
Water: public network (%)	1.20	(3.17)	0.37	13610	1690	3353
Sanitation: sewer network (%)	0.09	(4.61)	0.69	15999	2306	3293
No bathroom (%)	-2.77	(3.17)	0.69	13865	1734	3294
Septic tank (%)	4.34	(2.60)	0.21	15196	2029	3294
Informal occupancy (%)	0.13	(0.11)	0.22	17129	2662	3294
<i>Panel B: Fiscal Variables (2000)</i>						
Total expenditure p.c.	95.50	(115.48)	0.72	4975	458	3193
Housing & urbanism exp. p.c.	-4.94	(20.23)	0.50	6783	655	3193
Total revenue p.c.	102.77	(113.23)	0.66	4874	449	3193
IPTU revenue p.c.	4.46	(7.81)	0.54	13383	1563	3193
ITBI revenue p.c.	1.47	(2.23)	0.84	12185	1349	3193

Notes: Estimates correspond to a sharp regression discontinuity design with the 2006 population as the running variable, 20,000 as the cutoff. The reported coefficients are sharp RD discontinuities in pre-treatment variables. Point estimates are conventional; robust bias-corrected standard errors in parentheses (`rdrobust`, Calonico et al., 2017). Local linear regression with a triangular kernel and MSE-optimal symmetric bandwidth. Fiscal variables are expressed in per capita 2024 Brazilian reais.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.2 First stage without inconsistency adjustment

	(8)	(9)	(12)	(15)	(18)	(21)
RD Effect	0.38*** (0.05)	0.37*** (0.06)	0.36*** (0.06)	0.31*** (0.06)	0.30*** (0.06)	0.32*** (0.06)
Control mean	0.11	0.19	0.26	0.28	0.31	0.32
Bandw.	19976	17923	16276	15642	15355	15034
Eff. Obs.	3084	2943	2425	2196	2094	2003
Obs.	3352	3353	3353	3353	3352	3350

Notes: Replicates the first-stage table using the unadjusted plan-existence series, which keeps the raw MUNIC reports without reconciling reporting inconsistencies. Each column reports the discontinuity at the 20,000 population cutoff (2006 population as the running variable) in the probability of having an adopted master plan in the year shown in the column header. Point estimates are conventional; robust bias-corrected standard errors in parentheses (`rdrobust`, Calonico et al., 2017). Local linear regression, triangular kernel, MSE-optimal bandwidth. Control mean is the average of the dependent variable for municipalities below the cutoff within the respective bandwidth. First stages are attenuated relative to Table 3, consistent with classical misclassification of a binary treatment. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.3 Effects on laws and regulations in 2012 and 2018.

	(1) Urban Council	(2) Urban Perimeter (2012)	(3) Real Right Concession	(4) Impact Study	(5) ZEIS
RD Effect	0.42*** (0.12)	0.29** (0.15)	0.21** (0.09)	0.21*** (0.09)	0.89*** (0.17)
Control mean	0.12	0.66	0.06	0.05	0.39
Bandw.	17,059	16,746	14,583	14,307	14,274
Eff. Obs.	2,666	2,555	1,902	1,831	1,821
Obs.	3,353	3,353	3,353	3,353	3,352
	(6) Special Areas	(7) Urban Perimeter (2018)	(8) Land Subdivision	(9) Zoning	(10) Solo criado
RD Effect	0.67*** (0.17)	0.28** (0.15)	0.43** (0.16)	0.45*** (0.16)	0.37** (0.17)
Control mean	0.28	0.85	0.51	0.45	0.25
Bandw.	15,881	14,156	16,645	16,974	16,828
Eff. Obs.	2,286	1,794	2,527	2,629	2,580
Obs.	3,352	3,352	3,352	3,352	3,352
	(11) Special assessment	(12) Joint development	(13) Impact Study	(14) Building Code	(15) Environmental Zoning
RD Effect	0.42** (0.17)	0.48*** (0.16)	0.66*** (0.17)	0.19 (0.20)	0.40** (0.16)
Control mean	0.49	0.16	0.22	0.62	0.22
Bandw.	16,665	16,215	16,289	13,106	16,899
Eff. Obs.	2,532	2,402	2,433	1,577	2,601
Obs.	3,352	3,352	3,352	3,351	3,352
	(16) Predial servitude	(17) Historic Preservation	(18) Housing Policy	(19) Usucapion	(20) Surface Rights
RD Effect	0.32** (0.15)	0.64*** (0.22)	0.38** (0.18)	0.54*** (0.16)	0.59*** (0.16)
Control mean	0.12	0.23	0.21	0.09	0.11
Bandw.	16,383	13,093	16,358	15,810	17,587
Eff. Obs.	2,458	1,576	2,449	2,262	2,839
Obs.	3,352	3,352	3,351	3,352	3,352

Notes: All dependent variables are binary and capture if the municipality has a certain law at the time of the MUNIC survey. The dependent variables of the first four columns are from the 2012 edition of the survey and the rest are from the 2018 edition. Estimates correspond to a fuzzy regression discontinuity design with the 2006 population as the running variable, 20,000 as the cutoff, and having a master plan in the year preceding the outcome as the treatment. Point estimates are conventional; robust bias-corrected standard errors in parentheses (rdrobust, [Calonico et al. \(2017\)](#)). Control mean is the average of the dependent variable for municipalities below the cutoff within the respective bandwidth. *p < 0.10, **p < 0.05, ***p < 0.01.

TABLE A.4 Effects on Municipal Housing Policy Actions

	(1)	(2)	(3)	(4)
<i>Panel A: Housing construction program (2008 MUNIC)</i>				
RD Effect	0.36*	0.40**	0.32*	0.33**
	(0.19)	(0.19)	(0.21)	(0.19)
Control mean	0.57	0.57	0.55	0.56
Bandw.	15801	12665	3215	5096
Eff. Obs.	2260	1497	318	494
Obs.	3352	3352	3352	3352
<i>Panel B: Municipal housing fund (2009 MUNIC)</i>				
RD Effect	0.27*	0.21	0.36**	0.32**
	(0.15)	(0.15)	(0.18)	(0.17)
Control mean	0.34	0.35	0.35	0.35
Bandw.	16828	13244	2698	4236
Eff. Obs.	2581	1605	265	409
Obs.	3352	3352	3352	3352
<i>Panel C: Housing improvement program (2020 MUNIC)</i>				
RD Effect	0.40*	0.47**	0.25	0.29*
	(0.19)	(0.19)	(0.20)	(0.20)
Control mean	0.24	0.23	0.24	0.24
Bandw.	16863	13354	3039	4359
Eff. Obs.	2527	1584	285	396
Obs.	3281	3281	3281	3281
Pol. Order	1	1	0	0
Kernel	Triang	Unif	Unif	Triang

Notes: Estimates correspond to a fuzzy regression discontinuity design with the 2006 population as the running variable, 20,000 as the cutoff, and having a master plan in the year preceding the outcome as the treatment. Dependent variables are binary indicators from the MUNIC survey. Point estimates are conventional; robust bias-corrected standard errors in parentheses (rdrobust, Calonico et al., 2017). Control mean is the average of the dependent variable for municipalities below the cutoff within the respective bandwidth. *p < 0.10, **p < 0.05, ***p < 0.01.

TABLE A.5 Dynamic effects on IPTU per capita

	Plan in $t - 1$		Years of plan by $t - 1$	
	(1)	(2)	(3)	(4)
	2006 pop.	All years	2006 pop.	All years
Effect	29.7*	37.6**	3.1*	3.5**
	(15.9)	(18.3)	(1.7)	(1.8)
Population cutoffs	2006	2006 to 2022	2006	2006 to 2022
Baseline IPTU (2000)	Yes	Yes	Yes	Yes
Kernel / poly.	Tri / 1	Tri / 1	Tri / 1	Tri / 1
Bandw.	18429	16771	18429	16771
Obs.	45,282	322,262	48,141	360,496

Notes: 2SLS estimates pooling IPTU per capita over outcome years 2007 to 2023, expressed in 2024 reais. Columns 1 and 2 instrument an indicator for an adopted master plan in the year before the outcome. Columns 3 and 4 instrument the cumulative number of years with a plan through the year before the outcome. Columns 1 and 3 use the 2006 population cutoff as the sole instrument. Columns 2 and 4 stack every population year from 2006 to 2022, each threshold crossing serving as a separate instrument, restricted to cells in which the crossing precedes the treatment year. All columns use a triangular kernel, a local linear specification, outcome year fixed effects, running variable slopes specific to the threshold year, and the 2000 value of IPTU per capita as a control. Bandwidth held common within each instrument set. Standard errors clustered by municipality in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.6 Effects on Cadaster

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: computerized land registry. 2015.</i>					
RD Effect	0.24*** (0.10)	0.27*** (0.11)	0.28*** (0.10)	0.28*** (0.11)	0.18** (0.10)
Control mean	0.90	0.88	0.91	0.87	0.90
Bandw.	14851	3599	11938	2407	15027
Eff. Obs.	1846	334	1301	222	1808
Obs.	3154	3154	3154	3154	3030
<i>Panel B: public access cadaster. 2019.</i>					
RD Effect	0.53** (0.27)	0.44** (0.25)	0.47** (0.26)	0.53*** (0.24)	0.61** (0.31)
Control mean	0.36	0.33	0.37	0.31	0.36
Bandw.	11627	3593	9404	2887	11708
Eff. Obs.	1243	322	953	262	1203
Obs.	3103	3103	3103	3103	2984
Pol. Order	1	0	1	0	1
Kernel	Triang	Triang	Unif	Unif	Triang
Controls	No	No	No	No	Yes

Notes: Estimates correspond to a fuzzy regression discontinuity design with the 2006 population as the running variable, 20,000 as the cutoff, and having a master plan in the year preceding the outcome as the treatment. Dependent variables are binary indicators from the 2015 MUNIC survey (computerized cadaster, Panel A) and the 2019 MUNIC survey (publicly accessible cadaster, Panel B). Point estimates are conventional; robust bias-corrected standard errors in parentheses (rdrobust, Calonico et al., 2017). Column 5 controls for the baseline (2000) value of per capita IPTU revenue. Control mean is the average of the dependent variable for municipalities below the cutoff within the respective bandwidth. *p < 0.10, **p < 0.05, ***p < 0.01.

TABLE A.7 Effects on Tax Debt

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Dívida. 2015.</i>					
RD Effect	23.4*	20.5**	26.1*	23.5**	21.2**
	(13.8)	(12.3)	(15.3)	(14.7)	(15.2)
Control mean	10.4	10.9	10.9	10.1	10.9
Bandw.	13488	4472	10647	3584	8000
Eff. Obs.	1620	416	1162	338	776
Obs.	3272	3272	3272	3272	3126
Pol. Order	1	0	1	0	1
Kernel	Triang	Triang	Unif	Unif	Triang
Controls	No	No	No	No	Yes

Notes: Estimates correspond to a fuzzy regression discontinuity design with the 2006 population as the running variable, 20,000 as the cutoff, and having a master plan in the year preceding the outcome as the treatment. The dependent variable is per capita revenue from past-due tax collections (*dívida ativa tributária*) in 2015, measured in 2024 Brazilian reais. Point estimates are conventional; robust bias-corrected standard errors in parentheses (rdrobust, Calonico et al., 2017). Column 5 controls for the baseline (2000) value of total per capita revenue. Control mean is the average of the dependent variable for municipalities below the cutoff within the respective bandwidth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.