Development via Administrative Redistricting*

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Abstract

We study how administrative redistricting promotes local development. Exploiting a large episode of voluntary municipality splits in Brazil and a rich panel of administrative and spatial data, we compare paths of development between areas that split and those that did not. We find that splitting had broadly positive effects, with any potential loss of scale from smaller governments being at least partially compensated by extra federal transfers new municipalities received. We find that splitting led to an expansion of the public sector, improvements in public service delivery, and increases in economic activity in new municipalities. We show that autonomy and reductions to administrative distance help explain results beyond simply gains in revenue. Our findings illustrate that decentralization in form of subsidized voluntary administrative redistricting can improve public service delivery in disadvantaged areas.

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1 Introduction

A classic tension highlighted by the fiscal federalism literature is that large administrative units may not serve all its constituent parts equally well, but the parts worse off may not have the scale or resources to self-finance in case of a secession (Oates, 1972, 1999; Alesina and Spolaore, 1997). A country may decide to subsidize voluntary splits with federal transfers to weaken that trade-off, but doing so introduces a new one: new units receive concentrated benefits at the diffused expense of the rest of the country. To what extent such a policy is beneficial is an open empirical question.

In this paper we exploit a large redistricting episode in Brazil to study one side of this trade-off, namely how subsidized splits impacted local development in the areas requesting to secede. Brazil provides a compelling setting for studying this question for at least three reasons. First, it is a large developing country composed of municipalities holding substantial administrative, fiscal, and political decision making power, and horizontally having the same administrative structure regardless of size. Second, in the interval of eight years between 1988 and 1996 the country went from facilitating voluntary splits (by lowering formal requirements and providing federal subsidies) to, under the widespread political perception that the splits were out of control and generating small inefficient municipalities, passing a Constitutional amendment to effectively block any new and ongoing split requests. With newly collected data on the universe of split requests, we are able to leverage this setting and compare paths of development across areas that split and areas that *almost* split. Third, the availability of high-quality and extensive data allows us to characterize the short- and long-term impacts of splitting on multiple outcomes capturing fiscal performance, economic activity, and public service delivery.

To study the impacts of splits among areas that request to secede, we begin by constructing our estimation sample. We collect and classify historical archives on splitting requests from 11 states initiated between 1988 and 1996, covering 42 percent of all states and 58 percent of the total population. We link these split requests to other sources of spatial and administrative data. Using information from years prior to splitting, we show that requests to split are concentrated in periphery areas displaying worse economic conditions at baseline. Among the many mechanisms through which large administrative units might have originally hurt the poor in the periphery, such as political capture and neglect (Bardhan, 2002) or ethnic fragmentation (Grossman and Lewis, 2014), in the Brazilian context the forces driving requests to split were administrative remoteness and the expected increase in federal transfers (Bremaeker, 1993).

We estimate the causal effects of splitting using a difference-in-differences strategy comparing areas whose requests to split were approved to those whose requests were not. Because they also applied to split, these areas that *almost split* form an arguably valid counterfactual to those that split. Several of them ultimately failed to split due to either political reasons or the 1996 reform that left outstanding requests open. We show that municipalities with approved and unapproved requests to split exhibit similar levels and trends in outcomes prior to their initial year of splitting, corroborating the causal interpretation of the difference-in-differences estimates. We also document that splitting is difficult to predict based on municipality baseline characteristics.

Our analysis yields four main results. First, mechanically, we document that new municipalities receive about 13% extra revenues immediately after the split.[1] They go on to set up new local governments, spending the money on infrastructure and hiring new bureaucrats. Second, we show that splits cause a net improvement in the delivery of public services, particularly those for which municipalities are, by law, responsible for providing. In a cohort-level analysis exploiting variation in splitting across cohorts and municipalities, we find increases in school attendance ranging between 4 to 6 percentage points for cohorts up to age 15 by the time of changes in boundaries. We also document positive effects on public services such as household access to piped water, trash collection, electricity, and sewage, ranging from 2.5 to 7 percent. Third, we find important net effects of splits on the local economy. Our results indicate a degree of structural transformation, with a substitution of formal jobs and establishments from agriculture to retail and services. We find no effects on local tax revenues. Fourth, we take advantage of the granular and spatial structure of the nighttime luminosity data to further investigate which districts within a municipality drive the main findings. We document that districts applying to split entirely explain the net effects on luminosity, whereas other districts in the municipality remain unaffected. Together, our results indicate that splits were generally beneficial to the areas involved in them. Although we cannot rule out negative outcomes such as corruption or patronage due to data limitations, we do not detect any trade-off because of a loss of scale or other mechanisms predicted by standard decentralization theory.

[1] This rule guarantees a floor to revenues from federal transfers for municipalities below a certain population threshold, which implies higher gains per capita the smaller an area is. We provide more details in Section 2.3.

We probe the robustness of our main findings in several ways. First, we show that the conclusions are not sensitive to alternative definitions of outcomes, samples, and specifications. We also report that our estimates are similar when restricting observations to requests submitted right before the 1996 reform halting splits. Second, to address concerns related to unobserved factors affecting our estimates, we conduct a complementary empirical exercise exploiting an important rule to split before 1996: districts requesting to split to become municipalities are required to conduct local referendums and obtain approval from at least half of voters. Exploiting this discontinuity for a representative state, *Minas Gerais*, where information on referendum results are publicly available, we document that the difference-in-discontinuities estimates on luminosity qualitatively follow the preferred difference-in-differences results.

New municipalities are receiving extra transfers and thus not fully ”bearing the cost” of splitting. Guided by a simple model of public goods provision under redistricting in the spirit of Bolton and Roland (1997) and Dur and Staal (2008), we study to what extent our findings are explained by factors beyond revenue, such as the autonomy enjoyed by new municipalities and policy adjustments to local preferences. Mediation analysis, where we control for revenues in our difference-in-differences on luminosity and other outcomes, shows that revenues explains only a small part of the benefits observed. In fact, we find that effects are larger for areas starting off as smaller and more isolated. We can also observe that, comparing applicant and headquarters districts, a substantial share of them elect mayors from different parties after the split.

Ultimately, the lesson from Brazil is that subsidized voluntary splits in settings with physically large administrative units were broadly beneficial to new municipalities. The remaining open question is how expensive the episode was for non-split municipalities, which lost some federal transfers revenue. First, a simple back-of-the-envelope calculation of weighted returns implies the episode was a net positive if returns per dollar on remaining municipalities were below 4.5% on luminosity. Second, in an exercise exploiting state-level variation in the number of splits, we find no correlation between average revenue lost in non-split municipalities and our main development outcomes. In 1996 the Brazilian Congress voted almost unanimously in favor of halting splits on the grounds of local rent-seeking and inefficiency. Our results are not conclusive, but they suggest that this decision could be reconsidered.

This paper contributes to three strands of literature. First, we contribute in several ways to the extensive literature studying the causes and consequences of multi-level government, federalism, and decentralization.[2] As opposed to studies of unit amalgamations (Fox and Gurley, 2006; Barankay and Lockwood, 2007; Reingewertz, 2012; Weese, 2015; Blom-Hansen et al., 2016; Egger et al., 2018), municipal cooperation (Ferraresi et al., 2018; Tricaud, 2022), or splits but with harmonized borders (Lima and Silveira Neto, 2018), the data granularity allows us to separately track the effects of voluntary splits for applicant, headquarters, and remaining areas (Gendźwiłł et al., 2020). In contrast to prior literature that relies on cross-country or cross-section variation, our identification strategy has the advantage of using almost split municipalities as a control group (Treisman, 2002; Grossman and Lewis, 2014).[3] The richness of our data allows us to test for mechanisms and to disentangle the roles of autonomy and reallocation of fiscal resources in explaining our findings. In particular, we also complement Asher et al. (2018) by exploiting new variation to show that administrative remoteness represents an important friction to local development.[4]

Second, this paper speaks to the literature on promoting regional development and decreasing inequality between rich and poor regions within a country, such as increases in public spending (Litschig, 2012; Brollo et al., 2013; Litschig and Morrison, 2013; Gadenne, 2017; Corbi et al., 2019), fiscal decentralization (Martinez-Vazquez et al., 2017), and place-based policies (Kline and Moretti, 2014; Shenoy, 2018). Our main contribution is twofold. First, we exploit administrative redistricting via splitting to understand the mechanisms through which this policy instrument can impact local development. Second, we provide a comprehensive picture of the long-term effects of administrative redistricting on local economy over at least 15 years.

[2]The literature may be grouped by the aspect studied and terms used, such as decentralization (Oates, 1972, 1999; Bardhan, 2002; Hooghe and Marks, 2003; Treisman, 2007; Faguet, 2004, 2014; Gadenne and Singhal, 2014; Mookherjee, 2015; Rodden and Wibbels, eds, 2019); the size of nations (Bolton and Roland, 1997; Alesina and Spolaore, 1997, 2003; Lassen and Serritzlew, 2011); administrative unit proliferation (Green, 2010; Pierskalla, 2016; Grossman and Lewis, 2014; Grossman et al., 2017; Pierskalla, 2019); border reforms (Coate and Knight, 2007; Boffa et al., 2016; Schönholzer, 2018; Gendźwiłł et al., 2020); amalgamations (Fox and Gurley, 2006; Barankay and Lockwood, 2007; Weese, 2015; Egger et al., 2018); and municipal cooperation (Ferraresi et al., 2018; Tricaud, 2022)

[3]Closer to our identification strategy, Lima and Silveira Neto (2018) focus on fiscal outcomes, whereas our work incorporates other dimensions to provide a comprehensive picture of the consequences of splitting.

[4]In the particular case of Brazil, our results also contribute to the policy debate on the optimal number and size of municipalities, often deliberated upon in the Brazilian National Congress (Gomes and MacDowell, 2000; Tomio, 2002, 2005; Cachatori and Cigolini, 2012, 2013; Mattos and Ponczek, 2013; Lipscomb and Mobarak, 2017; Lima and Silveira Neto, 2018).

We also contribute to the broad literature on state capacity (Besley and Persson, 2009; Khemani, 2019) and the personnel economics of the state (Evans and Rauch, 1999; Finan et al., 2017; Pepinsky et al., 2017; Besley et al., 2022). This paper confirms theoretical predictions from the literature, which argues that both the state and a capable bureaucracy matter for local development. We provide suggestive evidence from a developing country that a growing bureaucracy may ultimately result in improvements in public service delivery. Our results also suggest that allowing for new administrative units may serve the dual purpose of raising state capacity in the periphery and freeing these areas from captured former local governments (Bardhan and Mookherjee, 2000; Mansuri and Rao, 2013; Alatas et al., 2019).

The rest of the paper is organized as follows. We describe the institutional background in Section 2. Section 3 delineates our data sources, sample selection, and empirical strategy. Section 4 presents the main results, followed by a discussion of interpretation in Section 5. We discuss how our findings inform a calculation of net effects in Section 6. We offer concluding remarks in Section 7.

2 Institutional Background

2.1 The Role of Municipal Governments

Brazil has three tiers of government with fiscal, administrative, and political responsibilities: federal, state, and municipality governments. In 1988, the country had 4,124 municipalities, which are also the smallest units of government carrying decision-making power.[5] For organizational purposes, municipalities are divided into districts, which have no political or administrative autonomy. One municipality may consist of one or more districts, although no district belongs to two different municipalities.

The enactment of the Federal Constitution in 1988 represents the country's most important step towards federalism and decentralization of political power and financial resources (Arretche, 2000; Favero, 2004), granting additional administrative, fiscal, and political responsibilities to municipalities.[6] Since 1988, municipalities are responsible for

[5]Municipalities in Brazil are *type-1* jurisdictions, which hold a large range of functions, are durable in the sense that adjusting new jurisdictions is costly and rare, and are characterized by non-intersecting boundaries (Hooghe and Marks, 2003).

[6]In general, administrative decentralization implies that different government tiers execute various

providing a wide range of public goods, including primary education, basic health care, water, sanitation, trash collection, and street lighting services.[7] Fiscal autonomy includes the power to collect and manage local taxes, like property and service taxes, and the discretion to administer their own revenues, including inter-governmental transfers and local revenues. In Appendix Figure D.3a, we show that, on average, federal transfers account for between 30 and 60 percent of total municipal revenue, while local taxation and fees account for 5 percent.[8]

Municipal elections are held every four years in October to elect the mayor and municipal councillors. In municipalities with less than 200,000 registered voters, mayors are elected through a single-round system in which the candidate with the majority of votes wins. Larger municipalities have a two-round system: if no candidate gets at least 50 percent of vote share, there is a second round between the two most voted first-round candidates. The candidate who receives most valid votes wins. Municipal councillors are elected through an open list proportional representation system.[9] In January after elections, elected mayors and councillors take office.

2.2 The Creation of New Municipalities

In addition to expanding the role of municipalities, the 1988 Federal Constitution granted to states the authority to establish their own rules and criteria regarding the creation and amalgamation of municipalities (Brandt, 2010). The requirements varied across states and generally involved territorial contiguity, a minimum population, and some level of urban development for new municipalities. The process of creating a new municipality consisted of multiple stages: (1) local leaders or state politicians had to formally request the creation of a new municipality to state assembly; (2) a state legislative committee respon-

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functions and policies. Superior tiers may reverse and overrule decisions made by lower ones. Political decentralization includes *appointment* decentralization, *decision-making* decentralization, or *constitutional* decentralization. Fiscal decentralization involves decision-making decentralization on matters of taxation and expenditure (Ebel and Yilmaz, 2002; Treisman, 2007).

[7]Municipal, state, and federal governments coordinate to provide certain public goods, such as sanitation and health care. Yet, municipalities are exclusively responsible for providing other public goods such as primary education.

[8]While no reliable data exists about local tax rates and how often they change in the 1990s, anecdotal evidence suggests that changes in local tax rates are rare and not large.

[9]In this system, voters can vote either on individual candidates or party coalitions. Then, within coalition, candidates are ordered by vote share and receive seats up to the number of seats the coalition gets. Seats are allocated to coalitions following the D'Hondt method.

sible for evaluating this request approved it; (3) the state legislature authorized a local referendum in the applicant area, though the state governor could veto it; (4) if the majority of voters in the referendum voted for splitting, the request was put forward for voting in the state legislature; (5) thereafter, the state and federal governments had to approve it, although both of them had the discretion to veto it (Tomio, 2002). In practice, these vetoes were rare. The country experienced an unprecedented increase in the number of districts requesting to split and breaking off to become municipalities in the first half of the 1990s.

In 1996, the National Congress voted almost unanimously to curb the creation of new municipalities and enacted the Constitutional Amendment 15/1996 (henceforth "1996 CA"). It removed from states and assigned to the federal government the authority to regulate splits and amalgamations. The process to create a new municipality returned to the pre-1988 system except for three major changes. First, districts requesting to split have to conduct a referendum involving the entire municipality, not only the applicant district (or districts), and obtain approval from the majority of voters. Second, the federal government requires evidence that the applicant district (or districts) would be financially viable as a municipality in terms of fiscal sustainability (Klering et al., 2012). Third, splitting would depend on further federal legislation, which was never enacted since then. The 1996 CA, therefore, *de facto* induced a halt in the creation of new municipalities and led to various open and unapproved districts' requests.

Once the splitting request is approved, the applicant district (or group of applicant districts) is established as a new municipality after municipal elections, when the elected mayor and municipal councillors take office. Figure 1 displays a 34 percent increase in the number of municipalities between 1989 and 1997, jumping from 4,124 to 5,507. We observe two main waves of splitting approved before the 1996 CA, concentrated in 1993 and 1997, the years immediately after municipal elections. The splitting process leads to an extensive devolution of administrative, fiscal, and political power to new municipalities, also viewed as a form of decentralization.[10]

2.3 The Reasons for Splitting

Among the most common factors contributing to boundary changes in Brazil, we highlight two of them: neglect from the headquarters and fiscal incentives. Several studies

[10]This is an example of *horizontal* decentralization process. It differs from *vertical* decentralization, which refers to the creation of new tiers of government or to transfer of functions from a higher tier to a lower one.

suggest that large disparities in the provision of public goods across districts within a municipality play an important role in the decision to request to split (de Mello, 1992; Cachatori and Cigolini, 2012). In a survey carried out with a representative sample of mayors in 1992, Bremaeker (1993) shows that the majority of respondents reported neglect by local governments (63 percent) and physical distance to administrative headquarters (24 percent) as the main reasons for splitting.

Because splitting affects the distribution of federal transfers, particularly the *Fundo de Participação dos Municípios* (henceforth "FPM"), fiscal incentives are also relevant in this context. FPM is the main source through which the federal government provides monetary transfers to municipalities, accounting for around 80 percent of all federal transfers and 31 percent of municipal revenues (Corbi et al., 2019). Municipalities must spend 15 percent of FPM transfers on education and health separately, and there is no restriction for the remainder (Brollo et al., 2013).

The 1988 Federal Constitution established the current allocation mechanism of the funds. Every year, 22.5 percent of total revenues from federal income and industrial product taxes are reserved for FPM. Each state receives a block grant to be shared among its municipalities so that transfers are zero-sum within state. Each municipality's share is determined by a concave step-wise population-based formula. This formula assigns each population bracket a coefficient and disproportionately benefits small municipalities: revenues per capita are larger in lower population brackets.[11] Municipalities in the same state and population bracket obtain the same amount of transfers. In sum, as illustrated in Appendix Figure D.3a, the FPM share in a municipality's total revenue shrinks non-linearly with population size.[12]

When splits occur, new municipalities start receiving FPM transfers. Most splits are concentrated in small municipalities, suggesting that gains in FPM transfers may constitute an important driver for splitting requests. Yet, the direction of change in FPM transfers for headquarters and remaining areas is unclear, depending on a combination of factors: the allocation of funds within municipality prior to splitting, the curvature of

[11] Litschig (2012); Brollo et al. (2013); Litschig and Morrison (2013); Gadenne (2017) and Corbi et al. (2019) exploit discontinuities in population brackets to estimate the effects of federal transfers on multiple economic outcomes.

[12] In the aggregate, however, smaller municipalities account for small share of total transfers. For instance, Appendix Figure D.3b indicates that the bottom half of municipalities receive only 26 percent of all FPM transfers.

the FPM curve and the quantity of splits within the state.[13]

To evaluate the different forces at play and provide empirical predictions, in Appendix Section A, we outline a simple conceptual framework in which the municipality headquarters chooses the allocation of public goods to districts forming the municipality. The model illustrates two key predictions to motivate our empirical analysis. First, districts who apply to split are more likely to benefit from splitting if they are neglected by the headquarters or experience larger fiscal gains through transfers. Second, the consequences of splitting for headquarter districts and the rest of the country may be negligible.

3 Data and Empirical Strategy

3.1 Data

3.1.1 Splitting Requests

We catalog all splitting requests from two sources. First, we gather information from the Brazilian Institute of Geography and Statistics (IBGE) on the dates municipalities were officially created, and who their parent municipalities were before the split. Second, we manually collect and classify historical archives on splitting requests. Prior to the 1996 CA, state assemblies set their own rules for evaluating splitting requests, implying that the availability, level of detail, and quality of these archives vary across states. The final data contain splitting requests initiated by districts from 11 states (42 percent of all states, covering 58 percent of the country's population in 1991) between 1989 and 1996, regardless of whether requests were approved. Information on splitting requests are avail-

[13] To illustrate this point, consider the following framework. Suppose a municipality is composed of two districts, A and B, with population of sizes α_A and α_B, respectively. District B considers splitting into a new municipality. Before the split, FPM revenues are $R_A = w_A T(\alpha_A + \alpha_B)$ and $R_B = w_B T(\alpha_A + \alpha_B)$, where $w_A + w_B = 1$. Weights w_A and w_B are shares of total revenue received by each district, which may be proportional to population (i.e. $w_A = \frac{\alpha_A}{\alpha_A + \alpha_B}$) or not. Splitting generates higher revenues for A if $w_A > \frac{T(\alpha_A)}{T(\alpha_A+\alpha_B)}$, and for B if $w_B > \frac{T(\alpha_B)}{T(\alpha_A+\alpha_B)}$. We first conclude that total revenues likely increase (because $T(\alpha_A) + T(\alpha_B) > T(\alpha_A + \alpha_B)$), and that revenues for B also increase (because w_B is small and T is increasing and concave around small α_B). Revenues for A may increase if w_A is sufficiently low and if $\frac{T(\alpha_A)}{T(\alpha_A+\alpha_B)}$ is sufficiently high; and vice-versa. Since municipalities receive constant amounts within state and population bracket, the latter comparison depends on whether district A moves to a lower bracket after the split. Lastly, changes in T also depend on the total number of splits within state because one split lowers the total amount available to every other municipality.

able for the following states: *Amapá*, *Espírito Santo*, *Goiás*, *Mato Grosso*, *Minas Gerais*, *Pará*, *Paraná*, *Rio Grande do Sul*, *Rondônia*, *Santa Catarina*, and *São Paulo*. The remaining states do not provide public records on requests. Section C of the Appendix describes the final data in detail.

We manually scrape legislative reports about referendum results for the state of *Minas Gerais*, one of the few for which reliable records are available, including information on turnout and percentage of valid votes and voters in favor of splitting. We validate information from reports by cross-checking them with our data on split requests.

3.1.2 Outcome Data

We combine different sources of spatial and administrative data to examine whether and how splitting affects public revenues and expenses, local development, and public service delivery. We describe these sources in detail below.[14]

Public Finance. We collect information on revenues and expenditures at the municipality level from the Brazilian National Treasury (Finbra). Available since 1989, the data contain details on revenue sources (e.g., local taxation and intergovernmental transfers) and expenditure categories (e.g., capital and current expenses).

Demographic Census. We use decennial census data in 1991, 2000, and 2010 sourced from IBGE and the Atlas of Human Development in Brazil (United Nations Development Programme, 2013) to recover demographic and socioeconomic characteristics, including population size, urbanization rate, education, health, household access to public services such as trash collection or sewage, and income. For our baseline specification, we aggregate data at the municipality level because district identifiers are not available. We also use individual-level microdata on literacy and school attendance when exploiting variation across birth cohorts.

Formal Labor Market. We draw labor market information from the annual matched employer-employee data, the *Relação Anual de Informações Sociais* (RAIS), carried out by the Brazilian Ministry of Economy. The data cover the formal sector (Dix-Carneiro, 2014; Alvarez et al., 2018) between 1995 and 2018 and include a rich set of worker, job, and establishment characteristics. We use worker-level data to generate information on the total number of employees and establishments in the public and private sectors at the munic-

[14]Unfortunately, we are not aware of data collecting information on housing prices.

ipality level. We also generate these variables by economic sector (agriculture, mining, manufacturing, construction, retail, and services) and areas (e.g., education and health).

Night Lights. We use satellite imagery of night-time lights organized by the U.S. National Oceanographic and Atmospheric Administration (NOAA) and the National Geophysical Data Center (NGDC).[15] The data consist of grids with integer values spanning from 0 (no light) to 63 that record the intensity of lights for every year between 1992 and 2013.[16] We construct district-level data containing annual information on both the intensive and extensive margins of luminosity, measured by the weighted average of lights across grids within a district and whether this average is above zero. To capture regional inequality, we calculate a luminosity Gini index from variation across pixels within each district.[17]

Elections. We collect data on municipalities' electoral outcomes from the Superior Electoral Court (TSE). Available every four years since 1988, the information richness grows over time. Between 1988 and 1996, we observe only the elected mayor's name and party. The list of candidates and vote shares of municipal elections, along with other information, are reported since 2000.

Our analysis also includes minor data sources capturing geographic characteristics, such as soil suitability from FAO-GAEZ and terrain ruggedness from Carter (2018).

3.1.3 Sample Selection

We take several steps to build our estimation sample. Starting from a universe of 4,298 municipalities from the 1991 census,[18] we keep municipalities meeting the following cri-

[15] Intensity of night lights measures both outdoor and some indoor use of lights. Henderson et al. (2012) and Henderson et al. (2018) show that night lights are a good proxy for long-term GDP growth. This is useful in our context because information on economic activity is not collected at the district level and data on electricity consumption are only available for more recent years.

[16] A grid cell captures a 30 arc-second output pixel, equivalent to about 0.86 square kilometers at the Equator.

[17] We caution against a strong interpretation of results on luminosity Gini index due to potential measurement error caused by pixel sizes being large (about a 0.86 squared kilometer at the Equator). For instance, if rich and poor households are uniformly distributed across the pixel space, then luminosity Gini index would be zero.

[18] Changes in municipality boundaries are generally not nested. To account for them, we adopt the standard procedure of harmonizing boundaries between 1991 and 2010 into *minimum comparable areas*, as done in Lipscomb and Mobarak (2017) and Lima and Silveira Neto (2018). This approach yields a sample of 4,298 minimum comparable areas, which we refer to as municipalities throughout this paper, instead of using the list of 5,565 original municipalities in 2010, keeping the spatial units constant over time. We use the material publicly provided by Ehrl (2017).

teria: (1) municipalities that belong to one of eleven states with records on split requests; (2) municipalities with a single split event or with districts having split requests between 1989 and 1996; and (3) municipalities that are not state capitals since they are the headquarters of state governments. The restrictions yield a final sample of 448 municipalities.

While we mostly perform our analysis at the municipality level due to data availability, several data are also available at the district level, allowing us to gain insights on differences within and across municipalities. Therefore, we apply similar restrictions to districts to construct a secondary sample. Starting from a sample of 8,855 districts from the 1991 census, we first restrict it to districts meeting the following criteria: (1) districts from 11 states with records on split requests; (2) districts that do not belong to state capitals; (3) districts in municipalities where split requests are started by districts;[19] and (4) districts in municipalities with a single split event or split requests between 1989 and 1996 to avoid multiple events. These restrictions combined leave us with a sample of 1,259 districts. They can be classified into three groups: (1) *applicants*, corresponding to periphery districts that requested to split; (2) *remaining*, containing periphery districts that did not request to split themselves, but were located in municipalities where some district did so; and (3) *headquarters*, including headquarter districts in municipalities with a district requesting to split. This division leads to 552 applicants, 325 remaining, and 382 headquarter districts.

3.2 Who Applies to Split?

Before outlining our empirical strategy, we discuss how municipalities select into splitting and how districts select into applying to split. Table 1 presents summary statistics for baseline municipality characteristics in 1991 levels. Overall, municipalities in our final estimation sample containing an applicant district (Column 1) are comparable to municipalities without split requests (Column 3) in various dimensions, except for population size, area, and amount of federal transfers as share of total revenues, consistent with fiscal incentives from splitting. Municipalities in both groups display similar population composition and measures of education, health, public services, and income prior to splitting. In addition, unlike other countries, such as Indonesia (Pierskalla, 2016; Bazzi and Gudgeon, 2021) or India (Dunning, 2019), differences in racial and religious composition are

[19] Several split requests are initiated by areas smaller than districts, such as neighborhoods or parks, so we exclude these cases from the estimation sample.

small in magnitude across both groups. For this reason, unlike the literature studying drivers of splits, we do not incorporate social fragmentation into our analysis.

Our institutional context suggests that districts requesting to split are more likely to be less developed than other parts of the country. Our data generally corroborate this prediction. In Table 2, we compare the average district characteristics in the baseline period, before the waves of splitting, between applicants (Column 1) and headquarters (Column 5). On average, applicant districts display worse economic and demographic conditions. They are smaller in total population and area, are less urban, and exhibit lower levels of public service delivery. As expected, they are also located farther from their parent town halls. Yet, applicant districts are larger and more developed than remaining districts (Column 3).[20]

3.3 Empirical Strategy

3.3.1 Identification

In order to identify the causal effects of splitting, we classify our final sample of 448 municipalities into two groups: *split* (324) and *almost split* (124). As the denomination suggests, almost split municipalities contain a district that applied and failed to split into a new municipality. They form a credible counterfactual to split municipalities because they attempted to split and had a good chance of having their requests accepted, mitigating concerns related to selection into application that could affect our estimates. Requests could ultimately fail due to a variety of reasons. For example, they may be left open after the enactment of the 1996 CA, which left those initiated by 1994 or 1995 with not enough time to go through all the required steps described in Section 2.2. Alternatively, legislative commissions may refuse it, the referendum may not return a majority, or the state governor may veto the request.[21] Similar reasoning applies to districts. The new division leads yields samples of 552 applicants (441 split and 111 almost split); 325 remaining (261

[20]For completeness, Columns (7)–(10) display summary statistics for districts outside our estimation sample. We see that districts not involved in splits are similar to those that are across most dimensions besides population, area, urbanization and terrain ruggedness.

[21]Tomio (2005) provides a detailed account of municipality splits in the state of *Rio Grande do Sul* and statistics on at what point of the legislative process were requests denied (Table 5). By 2002, out of 398 requests, a total of 64 percent had ultimately passed. 10 percent were still left open, 13 percent were rejected in legislature commissions, 5 percent were rejected by the legislature's plenary, 6 percent had a governor's veto stand, and 2 percent had a referendum majority vote against the split.

split and 64 almost split); and 382 headquarter districts (292 split and 90 almost split).

Based on this classifications, Figure 2 plots split and almost split municipalities on a map of Brazil.[22] Two relevant patterns emerge: split requests are geographically scattered and, despite some clustering due to the state rules for splitting, there is a large geographical variation in split and almost split events.

3.3.2 Regression Specifications

To understand how the impact of municipality splits, we estimate the following difference-in-differences model:

$$y_{mt} = \alpha_m + \alpha_{s(m)t} + X_m^{1991}\alpha_t + \sum_{\tau=-\underline{\tau}}^{\overline{\tau}} \beta_\tau Split_m \mathbf{1}[t - W_m = \tau] + \gamma Post_{mt} + \varepsilon_{mt} \qquad (1)$$

in which y_{mt} represents outcomes for municipality m in year t; α_m are municipality fixed effects; $\alpha_{s(m)t}$ controls for any state-by-time fixed effects; and X_m^{1991} are baseline variables extracted from Table 1 interacted with time fixed effects. $Split_m$ is an indicator variable for whether the municipality m ever split; and $\mathbf{1}[t - W_m = \tau]$ are dummies indicating year relative to the wave-year W_m when municipality m split (either 1993 or 1997). The choices of start time $\underline{\tau}$ and end time $\overline{\tau}$ are a function of the data and depend on the outcome of interest y_{mt}. The variable $Post_{mt} \equiv \mathbf{1}[t \geq W_m]$ indicates time periods after the municipality's wave-year. Standard errors are clustered at the state-split wave level.

In Equation (1), we normalize $\beta_{-1} = 0$ so that our estimates are relative to the year before splitting, 1992 or 1996. The post-event coefficients of interest, β_τ, capture the dynamics effects of splitting relative to that year. The main identification assumption relies on the timing of splitting being uncorrelated with the outcomes of interest, conditional on our set of fixed effects and controls. In particular, outcomes for treated and control municipalities would have followed parallel trends in $\tau \geq 0$ if no splitting had occurred. We test this assumption by assessing whether the pre-event coefficients of interest are statistically indistinguishable from zero. We find no evidence of pre-trends in Section 4.[23]

[22]Figure 2 also presents a diagram illustrating the simple comparison between split (in blue areas) and almost split (in orange) municipalities. Each municipality is divided into districts classified as applicant, remaining, and headquarter districts.

[23]To further validate our findings, we complement our difference-in-differences results with an RD approach embedded in local referendums — districts with at least half of voters in favor of splitting are more likely to split — and find that the RD estimates qualitatively follow the difference-in-differences results. We

Our empirical strategy contrasts municipalities that split to those that applied for splitting but did not receive an approval. Having some sense of what predicts approval is an useful exercise. In Appendix Table D.1, we display least squares estimates of the probability of splitting as a function of baseline characteristics at the municipality level in 1991. Columns (1) and (2) show estimates without and with state fixed effects to account for state-specific splitting policies and geographic clustering of splitting decisions. In both specifications, we document that most baseline covariates do not predict splitting in standard levels of statistical significance. The exceptions are log distance to state capital, years of education, share of preschool attendance, and shares of households with access to piped water and trash collection. We notice that the point estimates are predominantly small in magnitude and the statistical significance is sensitive to the inclusion of state fixed effects. To be conservative, we saturate all our estimates of Equation (1) with interactions of these baseline characteristics with time dummies to capture differential trends in these dimensions. We also emphasize that our identification strategy does not require balance on pre-splitting covariates in levels. It relies only on parallel trends between split and almost split municipalities before splitting.

4 Main Results

This Section presents the main results in four parts. First, we show that splits cause a mechanical gain in federal transfers and that new municipalities go on to set up new governments. Second, we show that splits cause a net improvement in the delivery of public services. Third, we find important net effects of splits on the local economy. Fourth, we document that districts applying to split entirely explain the net effects on luminosity, whereas other districts in the municipality remain unaffected. Finally, we also probe the robustness of our results.

4.1 Revenue and Setup of New Governments

Our institutional context, described in Section 2, suggests that gains in federal transfers are larger for municipalities with smaller populations, constituting relevant incentives for districts to request to split. Using public finance data, we estimate Equation (1) for log

describe this strategy in Section 4.5.

revenues. Figure 3 presents the annual point estimates along with 95 percent confidence intervals. Pre-event coefficients are statistically close to zero, lending support to the parallel trends assumption from our identification strategy. Immediately after splitting, on average, municipalities that split experience about a 12 percent increase in federal transfers per capita relative to almost split municipalities. Considering federal transfers per capita as the outcome variable, Column (3) of Appendix Table D.2 indicates an 31 percent increase. These coefficients suggest that splitting generates substantial reallocation of local revenues.

We next examine how new municipalities go on to spend this extra revenue and set up new governments. Based on Equation (1), Panel (a) of Figure 4 shows annual coefficients for capital expenditures per capita, which refer to purchases of machinery, vehicles, buildings, and the like (Lima and Silveira Neto, 2018), and account for 16 percent of municipal expenditures. There is a spike at around 50 percent in the year of splitting, followed by an increase of 22 percent over the next 15 years (Appendix Table D.2). Panel (b) of Figure 4 reports the results for current expenditures, equivalent to maintenance and operation costs of public services, such as payrolls and administrative costs. These costs represent 84 percent of municipal expenditures. We find that, after splitting, municipalities experience an increase of about 12 percent in current expenditures. Lima and Silveira Neto (2018) argue that capital expenditure tends to be initially higher than current expenditures due to installation and entrance costs. Current expenditures, however, are mostly payroll with fixed contracts. Regulation also inhibits indiscriminate hiring in the public sector, explaining the stable trends one year after splitting.

We also validate the above results using the matched employer-employee RAIS data. The goal is to gauge the effects on the size of the public sector considering the number of municipal public employees and total payroll as outcome variables. Panels (c) and (d) of Figure 4 and Appendix Table D.2 document that splitting is associated with an average increase of around 18 percent in both outcomes. We do not find impacts on federal or state public employment, reassuring that the growth in bureaucracy exclusively comes from municipal governments.

4.2 Impacts on Public Service Delivery

We investigate whether and how splitting affects public service delivery using two complementary approaches. First, we leverage individual-level information from three waves of Census data to conduct a cohort-level analysis exploiting differential exposure to splitting across birth cohorts. Second, we take advantage of the richness of the municipality-level Census data to provide a comprehensive picture of the consequences of splitting on public infrastructure and poverty.[24]

4.2.1 Effects on Education by Age

We ideally would like to have data on the total amount of public goods, such as public schools and hospitals built, to capture responses along this margin. Such data from the years before splitting, the early 1990s, are unavailable in Brazil. To overcome this limitation, we propose an indirect test in the spirit of Duflo (2001), exploiting variation in splitting across birth cohorts and municipalities. Intuitively, if additional schools generate an increase in educational attainment, younger cohorts more exposed to splitting should experience higher levels of schooling than older and less affected cohorts.

Exploiting Census data, we estimate the impact of municipality splits on individuals with a version of Equation (1) with heterogeneity by age:

$$y_{imt} = \alpha_{s(m)t} + \alpha_{k(i)m} + \alpha_{k(i)t} + \sum_{\tau=8}^{30} \beta_\tau Split_{mt} \mathbf{1}[k(i) = \tau] + X_i\lambda + \varepsilon_{imt}, \qquad (2)$$

in which y_{imt} represents outcomes for person i in municipality m and at year t; $\alpha_{s(m)t}$, $\alpha_{k(i)m}$, $\alpha_{k(i)t}$ are state-time, age-municipality, and age-time fixed effects, respectively; $Split_{mt}$ is an indicator variable for whether the municipality m split, and takes values equal to zero for $t = 1991$ and equal to one for years $t \in \{2000, 2010\}$ in municipalities that split; and $\mathbf{1}[k(i) = \tau]$ are dummies for each age. The term X_i refers to a vector of individual controls, such as gender, race, religion, and nationality. We cluster standard errors ε_i at the state-split wave level. The identification assumption for this exercise is that, absent splitting, birth cohorts would have followed the same educational trends in split and almost

[24] We also complement the literature studying impacts of decentralization on public service delivery. For instance, Bianchi et al. (2022) show that fiscal decentralization led to the expansion of municipal services, such as nursery schools, in Italy.

split municipalities.

Figure 5 presents our results for school attendance and literacy rates. Gray areas represent 95 percent confidence intervals. Panel (a) documents increases ranging between 2 and 5 percentage points in school attendance for people below 16 years old after splitting occurred. Panel (b) shows an increase of up to 5 percentage points in literacy rates for people below 15 years old. The gains in education concentrated among younger people are consistent with the government division of roles in education: municipalities are responsible for providing preschool and primary education, whereas state governments are in charge of high schools.[25] In Appendix Figure D.5, we corroborate an increased local public provision of education through higher public investments: after splitting, there is a crowd-out of employment from non-profits to government organizations in the education sector.

4.2.2 Public Services and Poverty

We further shed light on how splitting affects public service delivery using additional measures available at the municipality level from the Census data. Although the data contain various outcomes to study additional margins of response, it does not allow us to report pre-event coefficients because 1991 is the first and only wave before the 1996 CA we can use. Figure 6 and Appendix Table D.3 report coefficients after estimating Equation (1) for main outcomes related to access to public services and poverty.

We first show estimates for public goods provision. We document positive impacts on household access to piped water, trash collection, electricity, and sewage, ranging from 2.5 to 7 percent. Only the coefficients associated with trash collection and sewage are statistically significant at the 10 percent level. Interestingly, the effects are weaker for services for which other levels of government share responsibility, creating uncertainty about the role attributed to each government level, such as water and sanitation (Kresch, 2017). We also find negative but imprecise estimates for poverty and extreme poverty rates, which fall by about 2.1 and 1.7 percentage points respectively.

[25] The effects are likely not bounded by top censoring of attendance or literacy rates. To put the numbers into perspective, we plot the average school attendance and literacy rates for different years and ages in Appendix Figure D.4. In 1991 numbers are far from 100 percent: school attendance peaks at 82 percent for age 11 while literacy only reaches 87 percent for people age 15.

4.3 Impacts on the Local Economy

We are further interested at how much municipality splits impacts the local economy and aggregate income beyond the local public sector and service provision. To explore this question, we leverage data on formal employment in the private sector and municipality revenues from local taxes.

Based on Equation (1), Figure 7 and Appendix Table D.2 indicate that municipalities experience some economic growth and structural transformation after a split. Panels (a) and (c) show that the number of establishments increases by about 7 percent while the average number of private sector jobs does not change. Both results mask heterogeneity in Panels (b) and (d): results are more strongly positive and significant for the retail and services sector. Panel (e) however finds no discernible impact on municipality revenue from local taxation.[26] Because changes in tax rates are unusual and there are little differences in local taxes across municipalities (de Carvalho, 2008), the lack of effects on tax revenues indirectly suggests that tax base does not change after splitting. Finally, due to data limitations, our estimates do not rule out spillover effects of splitting on the informal economy.[27]

4.4 Nighttime Luminosity Across Districts

The evidence reported so far is consistent with both symmetric and asymmetric mechanisms of development across a municipality's area. In principle it could be that there were uniform gains (or losses) of revenue and state capacity between districts, generating uniform improvements in public services and economic activity. Or it could be that new municipalities drive all results given their new autonomy and resources, with other districts being largely unaffected. In this Section we leverage the spatial granularity in nighttime luminosity data to test for what group of districts drives our results. We extrapolate luminosity as proxy for all other outcomes given that it has been shown to strongly correlate with local development (Henderson et al., 2012).

To that end, we use annual night-time lights data from 1992 to 2013 and run the follow-

[26] Local revenues from local taxes include ISS (tax on services); IPTU (property tax); IBTI (property transfer tax); and fees, like public lighting fees.

[27] In 1992, the Brazilian informal labor market was estimated to account for 56 percent of total employment (Ipeadata, *Grau de informalidade - definição I*).

ing difference-in-differences regression separately for each of the three district samples:

$$y_{dt} = \alpha_d + \alpha_{s(d)t} + X_d^{1991}\alpha_t + \sum_{\tau=-\underline{\tau}}^{\overline{\tau}} \beta_\tau Split_{m(d)} \mathbf{1}[t - W_d = \tau] + \gamma Post_{dt} + \varepsilon_{dt}, \tag{3}$$

in which subscripts d, m, s, and t stand for district, municipality, state, and year; α_d are district fixed effects; $\alpha_{s(d)t}$ control for state-by-time fixed effects. We add a vector of baseline controls X_d^{1991} consisting of all baseline variables from Table 2 interacted with time fixed effects. The remaining variables are similar to Equation (1) except that subscripts represent districts. Standard errors ε_{dt} are clustered at the state-split wave level.

For the ease of comparison, we start by displaying the municipality-level estimates from Equation (1) in Panel (a) of Figure 8. We find that luminosity grows in the first 5 years after splitting and stabilizes at around 10 percent afterwards. Turning to the district-level estimates from Equation (3), Panel (b) of Figure 8 shows the estimates for applicant (blue), headquarters (green), and remaining (red) districts. We notice three striking patterns. First, all pre-event coefficients are statistically close to zero. Second, for applicant districts, there is rapid growth immediately after splitting, peaking 5–8 years later at about 40 log points. The coefficients remain stable, suggesting a 33 log points, or 39 percent, increase 15 years after splitting. Third, for remaining and headquarter districts, both involved in non-voluntary splits, we observe no statistically significant effect on luminosity 15 years after a split.[28]

We are also interested in understanding to what extent the impacts on luminosity are driven by extensive or intensive margins, and whether spatial inequality within municipality declines. We use two measures described in Section 3.1 as outcomes: the share of lit pixels and luminosity Gini index. Panels (c) and (d) of Figure 8 plot the coefficients. We find a 7 percent increase in lit pixels, which captures pixels with any luminosity above zero, and a 4 percent decline in luminosity Gini index for applicant districts. While spatial inequality decreases slightly in these areas, consistent with our previous results, we do not find any impact for remaining districts and only smaller effects for headquarters districts.

[28]Appendix Table D.4 reports the aggregate estimates for log of average luminosity considering different specifications. In addition, we follow Oster (2019) to assess potential bias due to unobservable factors using the sensitivity of the treatment to additional controls. Under the assumption of proportional selection, Appendix Table D.5 reports that selection in unobservables would need to be about 12 percent of selection in observables to explain our baseline estimate for applicant districts.

4.5 Robustness Checks

We probe the robustness of our results at the district level to alternative definitions of outcomes, samples, and specifications in Table 5. For brevity, we report the results of Equation (3) only for applicant districts.[29] In Panel A Column (1) reproduces our benchmark result. Column (2) does not add 0.1 to the average luminosity so that its log is not defined for all districts. Column (3) instead uses the inverted hyperbolic sign transformation as an alternative way to deal with zeros in luminosity data. To investigate if the results differ when we restrict attention to the wave of splits affected by the 1996 CA, we present estimates only for the 1997 wave in Column (4). Because the process to split is lengthy, sometimes taking years, the timing of the 1996 CA is likely to be exogenous to our outcomes for the 1997 wave, whose sample mostly consists of requests initiated between 1994 and 1996. We find that the coefficient is similar in magnitude to Column (1). To control for geographic shocks to economic activity in microregions,[30] Column (5) includes microregion-by-year fixed effects, yielding similar conclusions. In Panel B we estimate standard errors with different choices of clustering, none of which changes the benchmark significance levels.

Two additional issues could bias our estimates. First, shocks may differentially hit split and almost split municipalities in timing coinciding with event years (1993 or 1997). For example, split municipalities could be affected by specific government programs starting after the split, affecting our outcomes of interest. To the best of our knowledge, we are not aware of such shocks in our context. Second, our results could be positively biased if there is a positive district selection into splitting along unobservable characteristics, such as economic growth potential, better organizational capacity, or connections with the state legislature. To alleviate this concern, we develop a complementary empirical strategy exploiting variation in local referendum vote shares and focusing on luminosity as the main outcome. We describe it below.

Prior to 1996, to become a municipality, the district applying to break off needed to conduct a local referendum and obtain approval from at least half of voters. We exploit the 50 percent cutoff to perform a difference-in-discontinuities (RD-DD) exercise (Grembi

[29]Results for remaining and headquarter districts as well as other outcomes are available upon request.

[30]As of 2017, the Census Bureau divides the country into 558 microregions, equivalent to commuting zones in the U.S. Each microregion contains a cluster of municipalities with similarities in socioeconomic and historical characteristics. For more details, see https://www.ibge.gov.br/en/geosciences/territorial-organization/regional-division/21536-regional-divisions-of-brazil.html.

et al., 2016). We restrict our analysis to a large and representative state, *Minas Gerais*,[31] for which data on referendum results are available, and estimate the following RD-DD specification in two stages:

$$Split_{m(d)} = \psi + \phi \mathbf{1}[V_d \geq 50\%] + \kappa g(V_d) + X_d \omega + \eta_d \tag{4}$$

$$y_{dt} = \alpha_d + \alpha_t + \beta Split_d Post_{w(d)} + \gamma g(V_d) Post_{w(d)} + X_{dt}\lambda + \varepsilon_{dt} \tag{5}$$

in which, from the first-stage Equation (4), $Split_{m(d)}$ is an indicator variable for whether the municipality m with district d split after the referendum; V_d represents the referendum vote share in favor of splitting in district d; and $\mathbf{1}[V_d \geq 50\%]$ is an indicator for whether district d had at least half of votes. Following Gelman and Imbens (2019), we define $g(V_d)$ as a linear distance from the cutoff. In our second-stage Equation (5), we include district and year fixed effects, α_d and α_t. We control for $Post_{w(d)}$, which is an indicator variable for the years after the wave-year w of splitting request for municipality $m(d)$, and the vector of time-varying controls X_{dt} is the same as in Equation (3). Our coefficient of interest β captures the effect of splitting. To keep our estimation comparable and account for fewer observations on the left side of the cutoff, our preferred specification considers a 15 percent bandwidth.

We validate our strategy by showing no significant discontinuities in pre-referendum observable district characteristics around the cutoff in Appendix Table F.7.[32] Turning to the main results, Appendix Figure F.7a provides a visual evidence of the first-stage, from which we notice that splitting is an almost deterministic function of reaching a majority vote. Appendix Figure F.8 displays the reduced-form estimates for log luminosity among applicant districts and documents a clear jump around the cutoff. We report all point estimates for applicant districts in Appendix Table F.8. In Column (2), we find that districts above the cutoff experience an increase of 50 percent in luminosity relative to those below it. Column (3) refers to the second-stage estimate. For a more meaningful interpretation, the Wald estimate points to a coefficient of log luminosity of 0.23 (= 0.22/0.96). It is slightly larger than our benchmark difference-in-differences estimate in Column (4) for a sample restricted to *Minas Gerais*. While any comparison in magnitude between these

[31] With an area larger than France, *Minas Gerais* is the second most populous and third richest state in Brazil. The state's ethnic composition and geography closely resemble the country's averages.

[32] We measure baseline covariates in 1991. Appendix Figure F.7b depicts the distribution of vote shares around the 50 percent cutoff and confirms that very few districts had less than half of voters agreeing to split.

coefficients should be taken with a grain of salt due to differences in sample size and composition, we interpret them as qualitatively similar results.

5 Interpretation

Together, our main results suggest that municipality splits have broadly positive net effects on public service delivery and the local economy, and that such changes are mainly driven by applicant districts who seceded into new municipalities. In this Section we investigate to what extent our findings can be attributed to mechanisms beyond the gains in total revenue, such as reductions of administrative distance and political representation. We interpret the results from Section 4 guided by our framework of public goods provision under redistricting in the spirit of Bolton and Roland (1997) and Dur and Staal (2008) developed in Appendix Section A and to the extent that the data allow.

5.1 Revenues and Beyond

An important question in our context is to what extent our findings are mechanically driven by the growth in government revenue through federal transfers as shown in Section 4.1. To identify the first-order effects of transfers on our outcomes of interest, ideally there would be enough variation in transfers among municipalities that split or almost split. This would allow for a difference-in-differences comparison among the two groups. However, as shown in Appendix Figure D.3d, there is little variation in transfers among municipalities that almost or did not split. Instead, we propose two alternative empirical exercises to test this relationship.

First, to assess the extent to which federal transfers explain our main results, we control for total expenditures in Equation (1) both at the municipality and the district level. This "horse-race" approach holds federal transfers constant when comparing split and almost split municipalities, and the coefficient associated with $Post_{mt} \times Split_m$ would approach zero if federal transfers entirely explain our outcomes. Table 3 reports coefficients for selected outcomes, each with the number of observations corresponding to what data are available. Similar to Figure 6, Panel A shows that splits improved public service delivery, however with low statistical significance in this case, and that expenditures partially offsets the total effect. Panel B shows a similar pattern, with effects on the local economy being only partially offset by expenditures.

Our analogous exercise at the district level requires an assumption on public finance sharing sharing pre-split. Because data are only available at the municipality level, we construct district-level pre-split series assuming revenues and expenditures were shared proportional to each district's population in 1991. Table 4 reports clearer patterns at the district level and exploiting luminosity as the outcome of interest. We find in Column (2) that controlling for expenditures reduces the difference-in-differences coefficient from 0.25 to 0.2.

Moreover, we test for the roles of administrative remoteness and scale in driving the impacts of municipality splits. The physical distance between a municipality's headquarters and its other districts, or *administrative remoteness*, may constitute an important friction for the provision of public services and for development more broadly (Bardhan, 2002; Mansuri and Rao, 2013; Krishna and Schober, 2014; Asher et al., 2018; Brinkerhoff et al., 2018). For instance, it may reduce the amount and quality of information that headquarters has on local needs, leading to fewer public investments (Oates, 1999). High transportation costs may induce bureaucrats to travel less often to remote areas or create additional barriers for citizens to manifest their preferences to politicians located in the town hall. Headquarters may thus be more likely to neglect remote districts when choosing levels of public goods to provide, as discussed in Section 2 and Appendix Section A. In Columns (3) and (4) we find that, controlling for expenditures, the effects of municipality splits are stronger in smaller, rural, and more isolated districts.

5.2 Political Representation

We provide suggestive evidence for the role of politics in explaining our results. Administrative redistricting may spur local development by allowing public policies to be better tailored to local needs (Oates, 1972). Districts forming new municipalities may elect new leaders and convey their preferences to the new administration, which may not be politically aligned with its former headquarters'.

We leverage the limited data on elections available for the 1990s to show that applicant and headquarter districts elect mayors from different parties most of the time after splitting. The pattern documented in Figure 9 starts at about 75 percent immediately after a split, and grows to about 85 percent over two decades. These findings relate to the strand of literature examining how decentralization is influenced and shapes political processes.[33] Our results are consistent with basic models of representative politics where elected officials reflect local preferences for policy (Persson and Tabellini, 2000). Unlike our context, in which districts could unilaterally request to split, Hassan (2016) and Gottlieb et al. (2019) model splitting as an endogenous distributive policy chosen by the incumbent. Since local elections are single-district, incumbents may benefit electorally from splits only to the extent that voters within the applicant district were in the opposition. Due to data limitations, we do not observe vote shares (either at municipality or district levels) or how competitive elections were at the time, implying that we cannot directly test their theory.

6 Net Effects

Our discussion so far highlighted the non-existence of trade-offs for subsidized voluntary splits: they had broadly positive effects for new municipalities and no clear negative consequences to headquarters and remaining districts. In Sections 2 and 5 we argued that this is at least partially explained by the extra federal transfers new municipalities received, which originate as diffused losses to all other non-split municipalities in Brazil. In this Section we provide back-of-the-envelope exercises to shed light on whether this second trade-off was worth it.

Our first exercise exploits within-state variation in the number of municipality splits, and thus in the amount of federal transfers reallocated, to test if revenue loss correlates with worsening of development outcomes. We correlate the pre-post average loss in revenue for non-split municipalities with their changes in public and private jobs, number of establishments, and average luminosity 15 years after the 1997 wave of splitting.[34] We report the coefficients in Table 6 and scatter plots in Appendix Figure D.6. Overall, except for a small decline in the number of establishments, we find no evidence that a decrease in transfers is associated with significant impacts on labor market outcomes and lumi-

[33]For instance, Myerson (2006) argues that decentralization may increase yardstick competition between jurisdictions, thus increasing the talent pool for national leadership and improving the chances of selecting capable administrators. Boffa et al. (2016) argue that centralization has an important advantage: by combining regions with diverse numbers of informed voters, the average level of information increases, which limits rent-seeking. For a review of the literature, see Grossman et al. (2017) and Pierskalla (2019).

[34]As shown in Appendix Figure D.3, a one percentage point in population residing in new municipalities implies a loss to non-split municipalities of approximately 2.1 percentage points in federal transfers.

nosity. We interpret the lack of spillover effects as consistent with decreasing returns to spending. When the expected revenues decrease, municipalities may be less likely to engage in wasteful spending, in which the marginal value is below the social costs of funds (Liebman and Mahoney, 2017), without significantly affecting economic outcomes.

Our second exercise is a back-of-the-envelope weighted accounting of returns per dollar guided by Proposition 2 in Appendix Section A. We ask what would the return-on-a-dollar have to be in non-split municipalities for the aggregate effect of reallocation of transfers, denoted as ΔV, to be positive. We add changes in returns for the three groups of areas in Brazil: headquarters and remaining districts (denoted as A), new municipalities (denoted as B), and non-split municipalities (denoted as 2). We observe population shares (α) and the percentage point reallocation of federal transfers (ΔT) at the time of splits. We estimate returns-on-a-dollar R with data from gains in federal transfers for new municipalities in Section 4.1 and from our luminosity results in Section 4.4.

$$\begin{aligned}\Delta V &= \Delta V_A + \Delta V_B + \Delta V_2 \\ &= \Delta T_A \times \underbrace{R_A}_{\approx 0} \times \underbrace{\alpha_A}_{\approx 22.2\%} + \underbrace{\Delta T}_{\approx 5.5 p.p.} \Big(\underbrace{R_B}_{\approx 77\% = 24\%/35\%} \times \underbrace{\alpha_B}_{\approx 4.3\%} - R_2 \times \underbrace{\alpha_2}_{\approx 73.4\%} \Big)\end{aligned}$$

Given the numbers above, ΔV is positive if the luminosity returns-on-a-dollar for non-split municipalities R_2 is lower than or equal to 4.5%. To recover an estimate of R_2, we implement a Regression-Discontinuituy (RD) design exercise in the spirit of Litschig (2012), Brollo et al. (2013), and Corbi et al. (2019), which we detail in Appendix Section E. Exploiting population cutoffs in the formula with which revenue sharing is calculated within states, we report in Table E.6 that although municipalities to the right of the first cutoff receive 13 percent more in federal transfers, its impact on public and private jobs, number of establishments, and average luminosity is statistically zero. In sum, we find suggestive evidence that the net returns of municipality splits, ΔV, may be positive, and that municipality splits in Brazil may have been a net positive for the country.

7 Conclusion

This paper provides an empirical account of the long-term economic effects of a large administrative redistricting event in Brazil. Exploiting a window of time of exceptionally low requirements for municipality splits, during which Brazil experienced a 23 percent growth in number of units, we estimate the impacts of voluntary municipality splits. We manually collect data on the universe of split requests and assemble a rich panel of municipalities and districts over time. New municipalities gain new autonomy and substantial inflows of federal transfers. Our estimates paint a broadly positive picture: splits municipalities are able to set new governments, to improve the delivery of public services, and to grow their local economic activity. These effects are likely all driven by applicant districts who secede into a new municipality, and are subsidized by gains in federal transfers.

Our findings have relevant policy implications for countries weighing the equity-efficiency trade-off when choosing how much autonomy and fiscal resources to decentralize to poor and remote areas. While administrative redistricting could in principle come at a high price to the rest of the country due to a redistribution of revenues, we find no clear evidence of that, given the Brazilian case and its particularities. Our findings shine a positive light on decentralization reforms, already widely studied but often classified as "cautionary tales" in recent decades (Kremer et al., 2003).

This paper leaves various interesting paths open for future research. A next natural step is to formally study distributional impacts and address the equity-efficiency trade-off in a general equilibrium framework (Kline and Moretti, 2014; Fajgelbaum and Gaubert, 2020; Gaubert et al., 2020). Relating our estimates to structural parameters would allow for welfare calculations and comparisons to counterfactual redistricting policies. Second, if higher quality political data become available covering the 1990s, future research could study whether a political resource curse impacts our estimates (Brollo et al., 2013). The literature has shown that windfalls of government revenue exacerbate the political monitoring problem and deteriorate the quality of political candidates. Understanding whether this also happens in our context of administrative splits would shed light on the efficiency of the observed changes in public policies. Lastly, fleshing out exactly how governments are formed in new municipalities, what specific promises and investments they make, and how splits affect political yardstick competition and representation (Grossman et al., 2017) would provide this story a new level of detail.

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8 Tables

Table 1: Baseline Descriptive Statistics in Levels - Municipalities

	Contains Applicant		Rest		*Differences*	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Diff. (5)	p-value (6)
Number of Districts	3	1.8	1.6	1	1.4	<0.01
Population (000's)	40.5	71.8	21.7	75.3	18.8	<0.01
Area (000's km2)	2.5	10.3	.9	2.7	1.6	<0.01
% Urban Population	58.5	23.8	59.2	22.8	-.7	0.54
% Population 14-	22.9	3.1	22.2	2.9	.7	<0.01
% Population 15-24	19.4	1.4	19.3	1.4	.1	0.34
% Population 25-34	15.8	1.9	15.9	1.8	-.1	0.55
% Population 65+	4.9	1.4	5.4	1.5	-.5	<0.01
Years of Education	8.8	1.4	8.8	1.4	.1	0.41
% Literacy 11-14	91.6	8.9	92.3	8	-.7	0.12
% Literacy 25+	74.7	12.9	74.2	10.6	.5	0.43
Preschool Attnd.	13.1	9.7	17.4	14	-4.3	<0.01
Middleschool Attnd.	88.1	10.7	89.7	11.5	-1.7	<0.01
High School Attnd.	28.1	14.4	28.3	14.1	-.1	0.87
Life Expectancy	66.8	2.7	66.8	2.6	0	0.83
Child Mortality 1-	32.3	9.7	32.3	9	0	0.92
Child Mortality 5-	38.6	12.8	39	11.8	-.4	0.55
% Piped Water	71.2	24.2	74.9	21.8	-3.7	<0.01
% Trash Collection	63.5	27.3	67.3	29.4	-3.8	0.01
% Electricity	81.3	20	83.8	18.9	-2.5	0.01
% Sewage	96.1	7.7	96.8	8	-.7	0.12
HHI Race	64.3	13.9	62.2	14.9	2	<0.01
HHI Religion	75.8	12.2	79.3	12	-3.5	<0.01
ln(Dist. State Capital)	5.4	.8	5.3	.8	.1	0.09
ln(Income p.c.)	5.7	.5	5.6	.4	0	0.23
% Extreme Poverty	19.6	14.9	17.6	13.6	2.1	<0.01
% Poverty	42.8	20.6	42.3	19.2	.6	0.59
% Federal Transfers	37.2	17	43.6	18.5	-6.4	<0.01
	N = 448		N = 1925			

Notes: Table reports characteristics from various data sources at municipality level (1991 Demographic Census, and *Finanças Brasileiras*). Samples are defined in Sections 2 and 3.1.

Table 2: Baseline Descriptive Statistics in Levels - Districts

	In Sample						Rest				*Differences*			
	Applicant		Remaining		Headquarters		Periphery		Headquarters		(1)-(3)		(1)-(5)	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	Mean (7)	SD (8)	Mean (9)	SD (10)	Diff. (11)	p-value (12)	Diff. (13)	p-value (14)
Population (000's)	5.4	12.4	3.2	5.5	31.7	64	3.6	13.1	17.9	48.9	2.6	$<$0.01	-25.8	$<$0.01
% Urban Population	.4	.3	.3	.2	.7	.2	.3	.2	.6	.2	.1	$<$0.01	-.3	$<$0.01
% Male	.5	0	.5	0	.5	0	.5	0	.5	0	0	$<$0.01	0	$<$0.01
% Literacy	.7	.1	.7	.1	.7	.1	.6	.1	.7	.1	0	.3	0	$<$0.01
% Piped Water	.5	.3	.5	.3	.5	.4	.5	.3	.5	.4	0	.53	-.1	$<$0.01
% Sanitation	.6	.4	.7	.3	.6	.4	.6	.3	.6	.4	0	.05	0	.45
% Trash Removal	.1	.2	.1	.2	.3	.3	.1	.2	.3	.3	0	.03	-.2	$<$0.01
Avg. Luminosity	1.8	5.8	1.4	5.2	3.1	6.5	1.9	8.1	2.5	7.3	.5	.19	-1.2	$<$0.01
Area (000's km2)	.5	1.5	.3	.5	.9	2.5	.3	.9	.6	1.5	.3	$<$0.01	-.4	$<$0.01
ln(Distance to Parent Townhall)	3	.6	2.8	.6	1.5	1	2.7	.6	1.4	.9	.1	$<$0.01	1.4	$<$0.01
ln(Distance to State Capital)	5.5	.8	5.4	.7	5.4	.8	5.2	.9	5.3	.8	.1	.09	0	.35
ln(Maize Suitability)	8.7	.3	8.7	.3	8.6	.3	8.5	.3	8.5	.2	0	.98	0	.06
ln(Wet Rice Suitability)	8.6	.8	8.6	.5	8.7	.5	8.6	.9	8.6	.8	0	.58	0	.42
ln(Soybean Suitability)	7.7	.4	7.7	.2	7.7	.2	7.6	.8	7.7	.7	0	.47	0	.67
ln(Wheat Suitability)	6.5	2.9	6.8	2.7	6.6	2.8	6.5	3	6.5	2.9	-.2	.35	0	.85
Terrain Ruggedness	83.2	78.2	72.8	68.5	76.2	72.7	68.6	71.7	68.7	71.4	9.7	.06	6.9	.17
	N = 552		N = 325		N = 382		N = 916		N = 1772					

Notes: This table reports descriptive statistics for districts using information from 1991 Demographic Census, 1992 night lights, MapBiomas, FAO-GAEZ soil suitability, and Carter (2018)'s terrain ruggedness data. The variables are: total population (in thousands), shares of urban and male population, literacy rate, share of households with access to piped water, sanitation and trash removal, average luminosity, total area (in thousands km^2), log distance to town hall, log distance to state capital, log soil suitability for different crops (maize, wet rice, soybean and wheat), and log terrain ruggedness. See Sections 2 and 3.1 for further details on sample construction.

Table 3: Heterogeneity - Municipalities

Panel A: Public Services	Child Mortality 5-		Literacy 15-17		% Trash Removal	
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Split	-0.83	-0.79	1.35	1.10	4.50*	3.39
	(0.54)	(0.54)	(1.09)	(0.97)	(2.33)	(2.09)
ln(Expenditures)		-0.20		1.33*		5.76**
		(0.25)		(0.73)		(2.25)
Observations	1,324	1,324	1,324	1,324	1,324	1,324
R-squared	0.96	0.96	0.82	0.83	0.87	0.87
State-Year FE	✓	✓	✓	✓	✓	✓
Controls-Time FE	✓	✓	✓	✓	✓	✓
Mean Pre-Split	38.69	38.69	92.70	92.70	63.73	63.73
SD Pre-Split	1.590	1.590	1.490	1.490	2.020	2.020
Panel B: Local Economy	ln(Luminosity)		ln(Establishments)		ln(Private Jobs)	
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Split	0.12**	0.11**	0.06	0.06	0.07	0.06
	(0.04)	(0.04)	(0.07)	(0.07)	(0.06)	(0.06)
ln(Expenditures)		0.07***		0.04		0.04
		(0.01)		(0.03)		(0.04)
Observations	8,106	8,106	7,229	7,229	7,229	7,229
R-squared	0.98	0.98	0.99	0.99	0.97	0.97
State-Year FE	✓	✓	✓	✓	✓	✓
Controls-Time FE	✓	✓	✓	✓	✓	✓
Mean Pre-Split	-0.0500	-0.0500	5.390	5.390	7.160	7.160
SD Pre-Split	1.590	1.590	1.490	1.490	2.020	2.020

Notes: Results from estimation of Equation (1) in Section 5. Controls interacted with time fixed effects include number of districts in 1991, ln(population) in 1991, % urban in 1991, ln(area), ln(distance to state capital), ln(income p.c.) in 1991, and HHI for race and religion in 1991. Standard errors clustered at the microregion level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Heterogeneity - Districts

	ln(0.1 + Luminosity)			
	(1)	(2)	(3)	(4)
Post x Split	0.25***	0.20***	1.03***	-0.23
	(0.02)	(0.03)	(0.34)	(0.47)
ln(Expenditures)		0.08**	0.07**	0.08**
		(0.03)	(0.03)	(0.03)
Post x Split x ln(Pop. 1991)			-0.01	0.06
			(0.06)	(0.05)
Post x Split x ln(Area)			-0.09***	-0.23***
			(0.03)	(0.06)
Post x Split x % Urban 1991			-0.48**	-0.47**
			(0.22)	(0.20)
Post x Split x ln(Dist. Parent Townhall)				0.24
				(0.18)
Post x Split x ln(Dist. State Capital)				0.12**
				(0.05)
Observations	9,821	9,821	9,821	9,821
R-squared	0.97	0.97	0.97	0.97
District FE	✓	✓	✓	✓
State-Year FE	✓	✓	✓	✓
Controls-Time FE	✓	✓	✓	✓
Mean Pre-Split	-0.769	-0.769	-0.769	-0.769
SD Pre-Split	1.521	1.521	1.521	1.521

Notes: Table reports point estimate results from Equation (3). All Columns include district and state-year fixed effects, and restrict observations to applicant districts as defined in Sections 2 and 3.1. Controls interacted with time fixed effects include ln(area), ln(distance to parent town hall), ln(distance to state capital), soil suitability for maize, wet rice, soybean, and wheat, terrain ruggedness, ln(luminosity) in 1992, land use shares in 1991 for urban, agriculture, pasture and forest. Standard errors clustered at the microregion level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Robustness for Applicant Districts

	(1)	(2)	(3)	(4)	(5)
Panel A: Specifications					
Post x Split	0.25***	0.27***	0.13***	0.25***	0.19***
	(0.02)	(0.06)	(0.02)	(0.03)	(0.06)
Observations	10,122	9,616	10,122	4,920	10,122
R-squared	0.97	0.96	0.98	0.97	0.98
Choice	Benchmark	ln(Luminosity)	asinh(Luminosity)	1997 Wave	Microregion FE
Panel B: Standard errors					
Post x Split	0.25***	0.25***	0.25***	0.25***	0.25***
	(0.03)	(0.05)	(0.05)	(0.02)	(0.02)
Observations	10,122	10,122	10,122	10,122	10,122
R-squared	0.97	0.97	0.97	0.97	0.97
Std Error Clustering	-	Municipality	Microregion	State-Split Wave	State
Number of Clusters		422	194	20	11

Notes: Table reports point estimate results from Equation (3). All Columns include district fixed effects. Controls interacted with time fixed effects include ln(area), ln(distance to parent town hall), ln(distance to state capital), soil suitability for maize, wet rice, soybean, and wheat, terrain ruggedness, and ln(luminosity) in 1992. Standard errors in Panel A are clustered at the state x split wave level in parentheses, except when stated otherwise. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

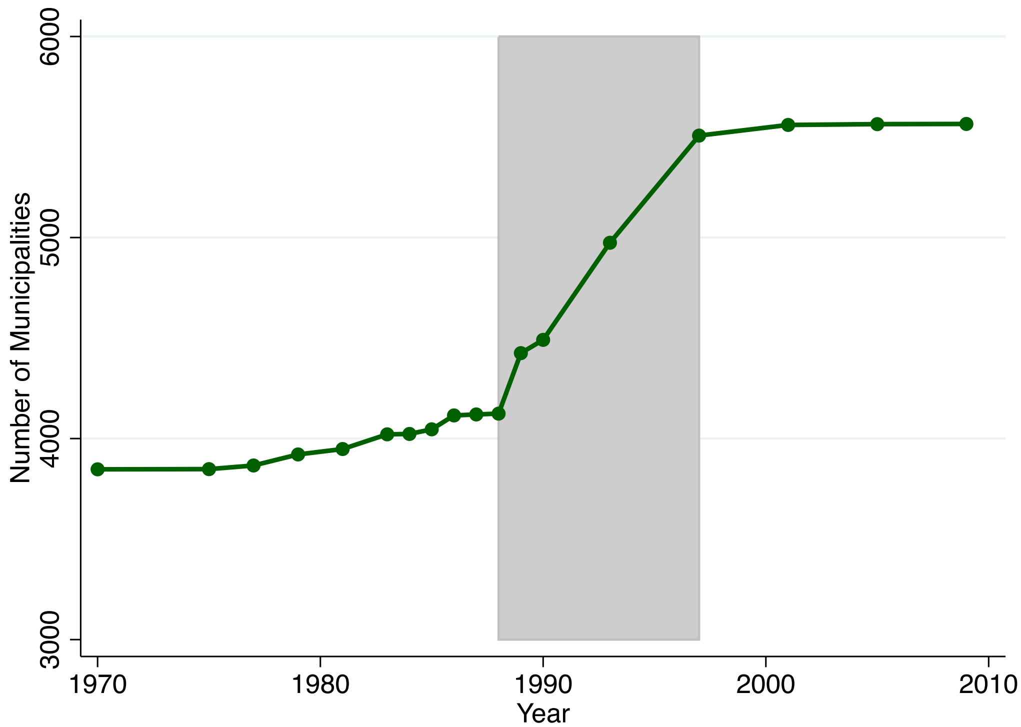
Table 6: Long-Term Spillovers on Non-Split Municipalities

	Public Jobs (1)	Private Jobs (2)	Establishments (3)	Luminosity (4)
Federal Transfers	-5.86	-9.73	-4.00**	1.23
	(5.83)	(6.08)	(1.66)	(1.00)
Observations	25	25	25	25
R-squared	0.38	0.48	0.71	0.54
Region FE	✓	✓	✓	✓
Split Wave	1997	1997	1997	1997
Mean	226.7	366.4	189.8	117.4
SD	403.9	442.5	133.7	59.62

Notes: Table reports OLS correlations between percentage point changes in federal transfers and percentage point changes in outcomes for non-split municipalities at the state level. Outcomes are measured 15 years after the split wave. Samples exclude the states *Distrito Federal* and *Roraima*.

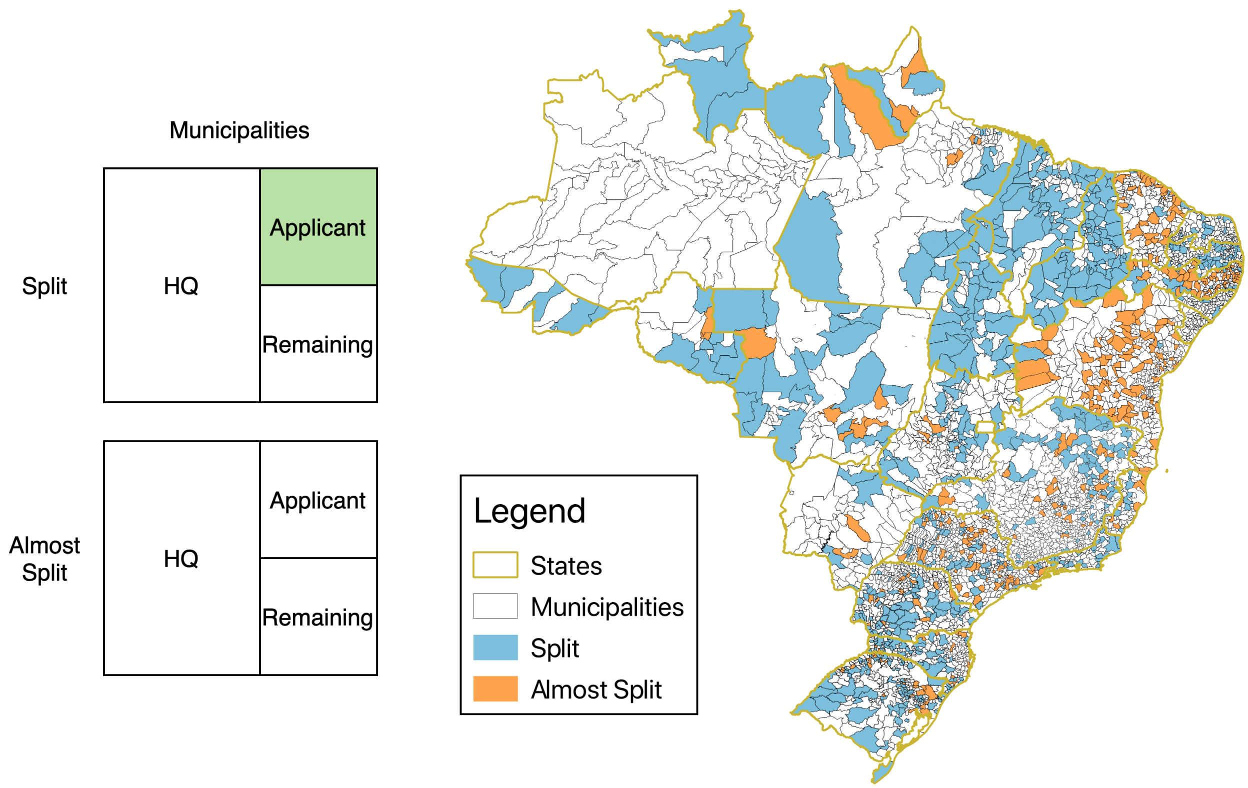
9 Figures

Figure 1: Evolution of Total Number of Municipalities



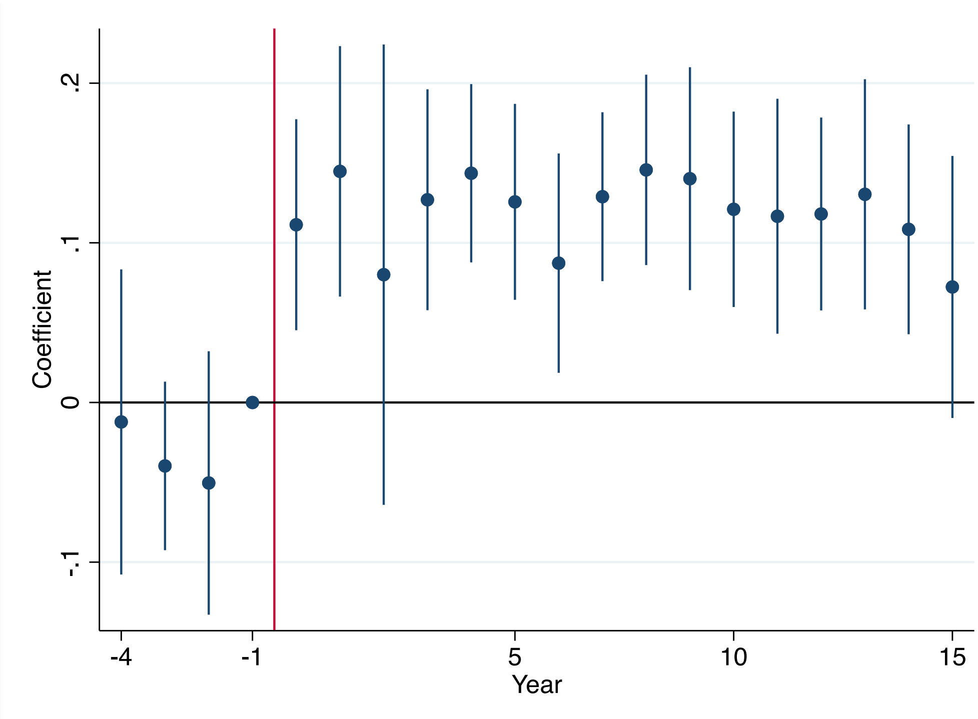
Notes: Graph shows how the total number of municipalities evolved in Brazil between 1970 and 2010. New municipalities are established in the beginning of election terms after obtaining approval to split. The gray area highlights our period of study, between the enactments of the 1988 Federal Constitution and the 1996 Constitutional Amendment. Information is obtained from the Brazilian Institute of Geography and Statistics (IBGE).

Figure 2: Split Diagram and Map of Brazil



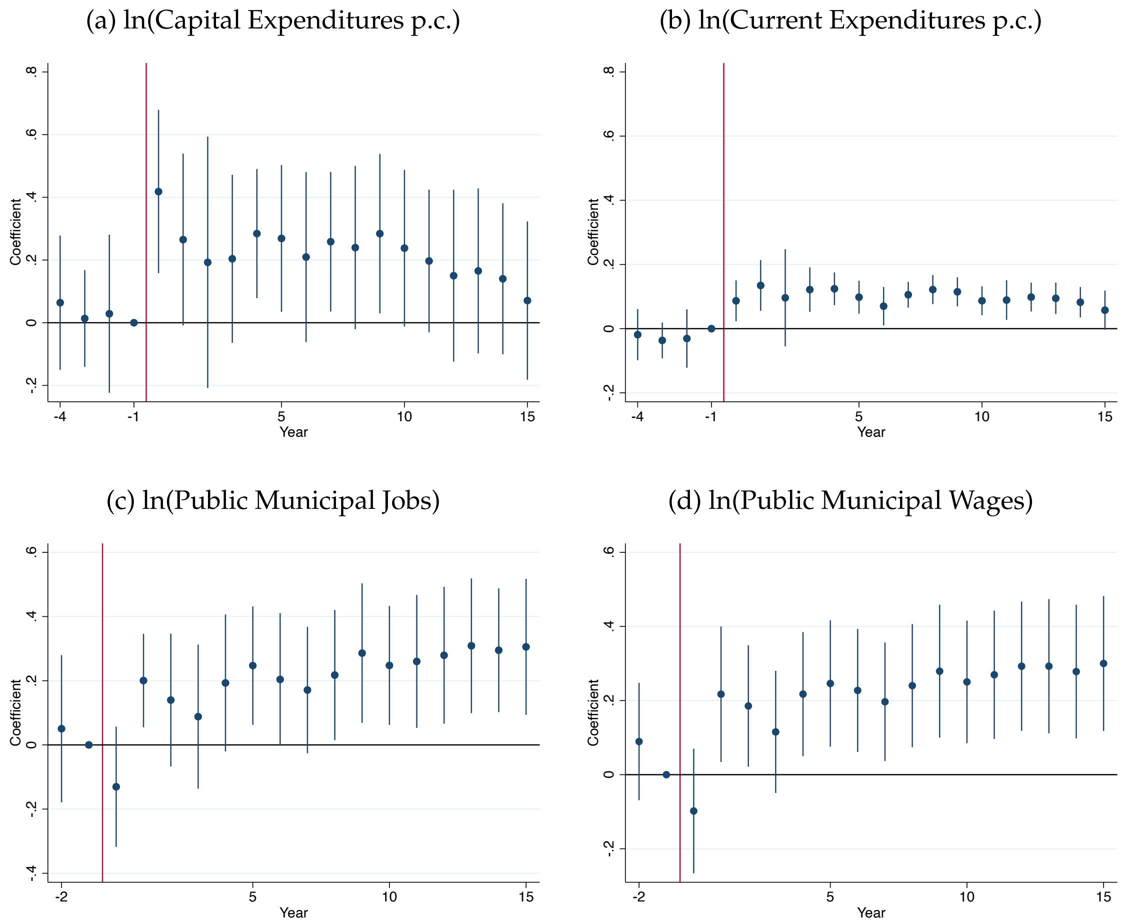
Notes: The diagram on the left illustrates the structure of split requests in our sample. Municipalities are divided into applicant, remaining and headquarters district. We color green applicant districts that succeed at splitting. More details can be found in Section 3.2. The map on the right represents Brazil in 1991. Municipalities that split are colored blue, while municipalities that almost split are colored orange. Our samples are defined in Section 3.1.3.

Figure 3: Increases in Local Revenues



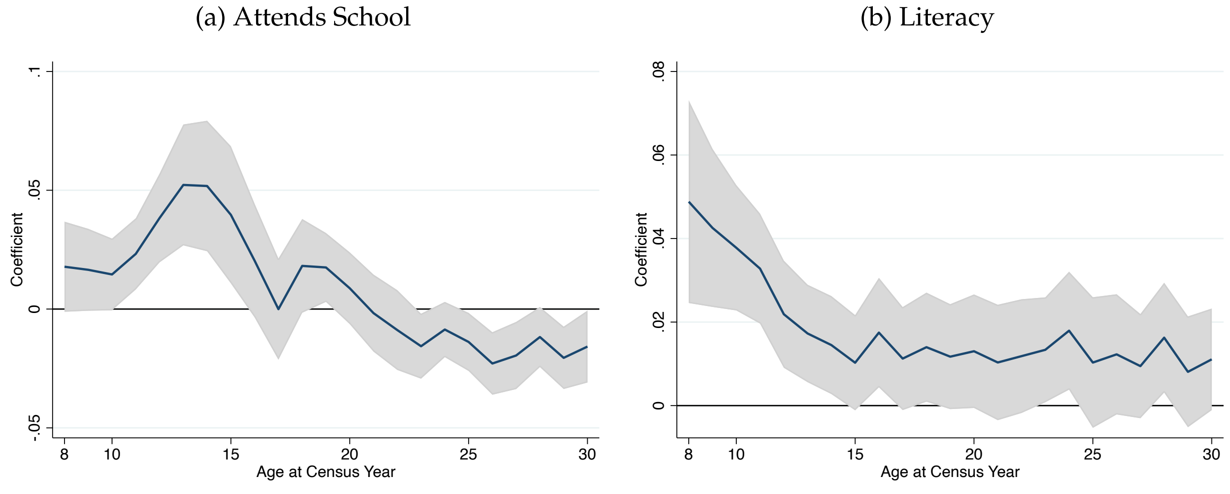
Notes: This figure reports point estimates of the annual effects of splitting on ln(Revenues) after estimating Equation (1). We use information from the *Finanças Brasileiras* (Finbra) data between 1989 and 2018. The omitted category is the year before splitting. Bars represent the 95%-confidence intervals.

Figure 4: Public Expenditures and Personnel



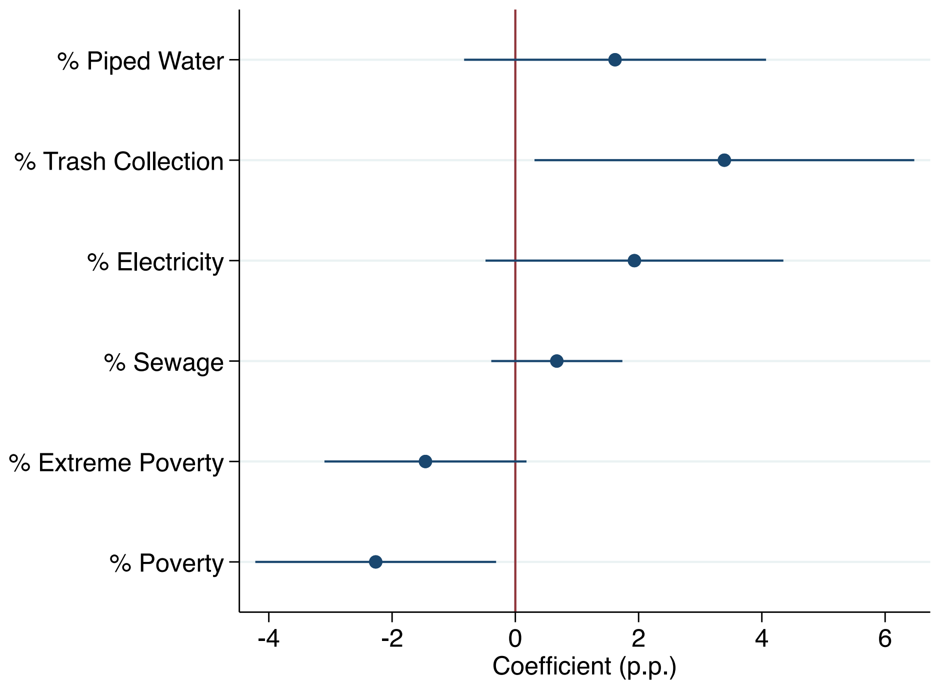
Notes: This figure reports point estimates of the annual effects of splitting on log capital expenditures per capita, log current expenditures per capita, log total number of public municipal jobs, and log public municipal wages after estimating Equation (1). We use information from Finbra (1989 – 2018) and RAIS (1995 – 2018) data. The omitted category is the year before splitting. Bars represent the 95%-confidence intervals.

Figure 5: Effects of Municipality Splits by Age



Notes: This figure reports results after estimating the difference-in-differences specification with interactions for each age dummy as in Equation (2). We use information from the 1991, 2000, and 2010 Demographic Census data. Gray areas represent the 95%-confidence intervals. We display raw data in Appendix Figure D.4.

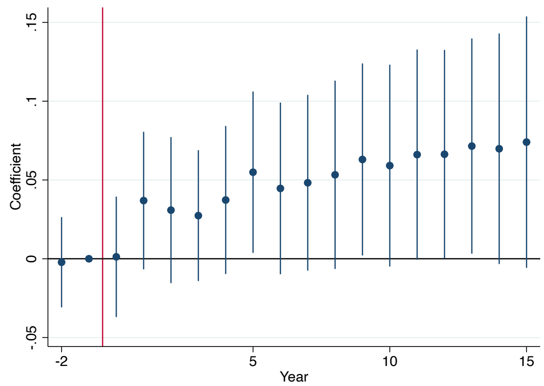
Figure 6: Access to Infrastructure and Poverty



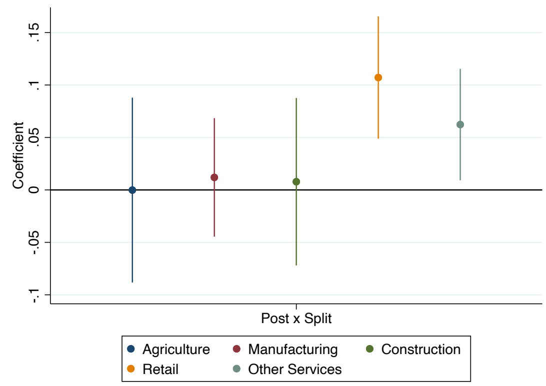
Notes: This figure reports the aggregate effects of splitting on public service delivery using different infrastructure and poverty measures after estimating a modified version of Equation (1). The infrastructure variables are shares of households with piped water, trash collection services, electricity, and sewage. Poverty outcomes are shares of household living in extreme poverty and poverty conditions. We use information from the 1991, 2000, and 2010 Demographic Census data. Bars represent the 95%-confidence intervals. Coefficients and further details can be found in Appendix Table D.3.

Figure 7: The Local Formal Economy

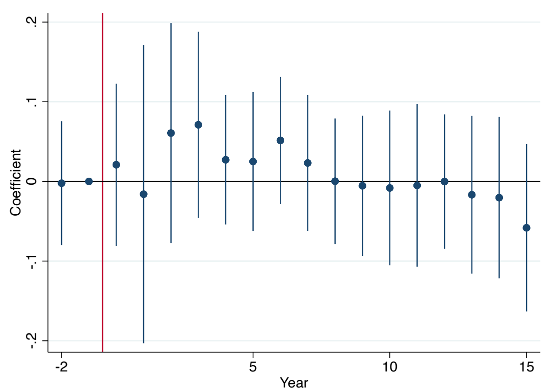
(a) ln(Establishments)



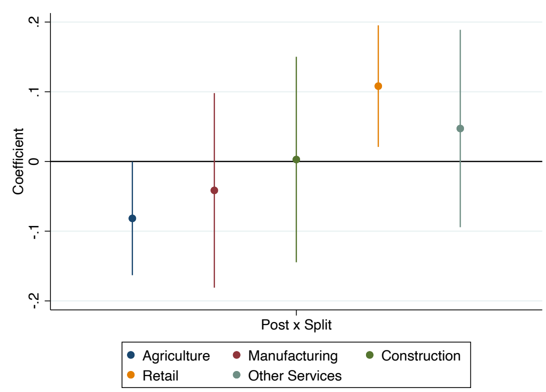
(b) ln(Establishments)



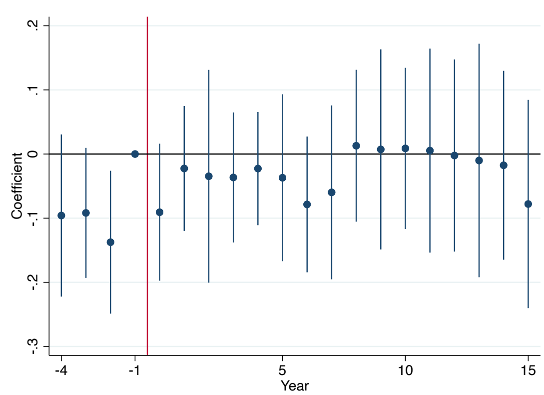
(c) ln(Private Sector Jobs)



(d) ln(Private Sector Jobs)



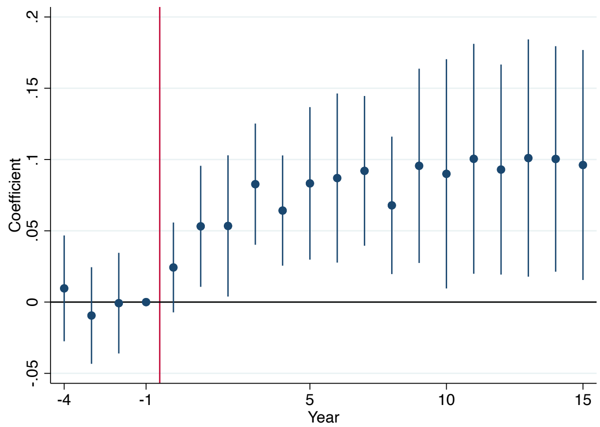
(e) ln(Local Taxation p.c.)



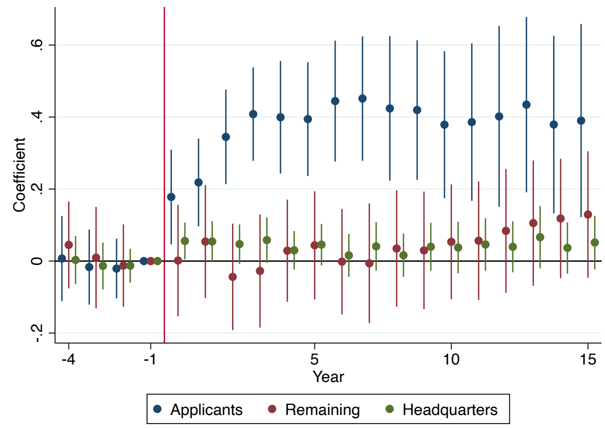
Notes: This figure reports point estimates of the annual effects of splitting on log local tax revenue per capita, log total number of establishments, log total number of private jobs, and log private wages after estimating Equation (1). We use information from Finbra (1989 – 2018) and RAIS (1995 – 2018) data. The omitted category is the year before splitting. Bars represent the 95%-confidence intervals.

Figure 8: ln(0.1 + Luminosity)

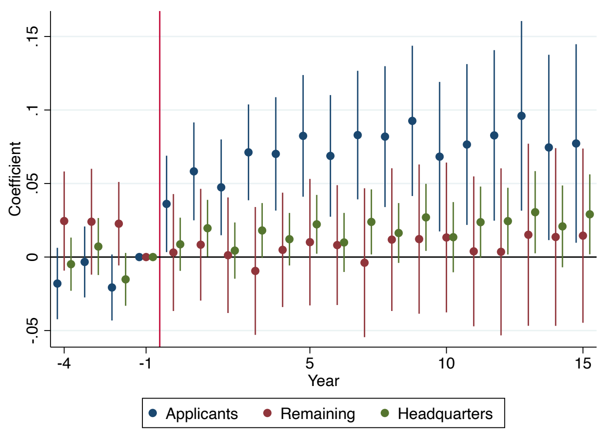
(a) Municipality level



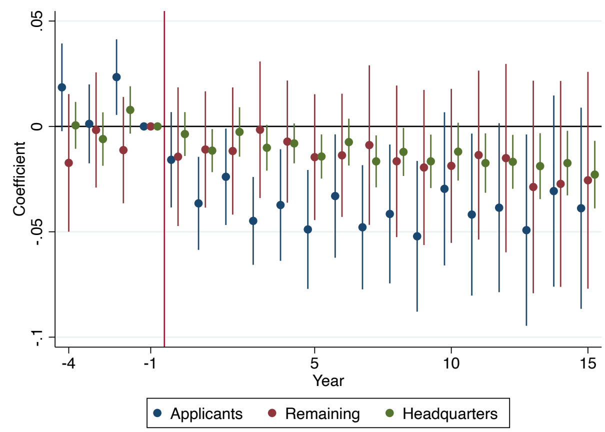
(b) District level



(c) % Pixels Lit - District level

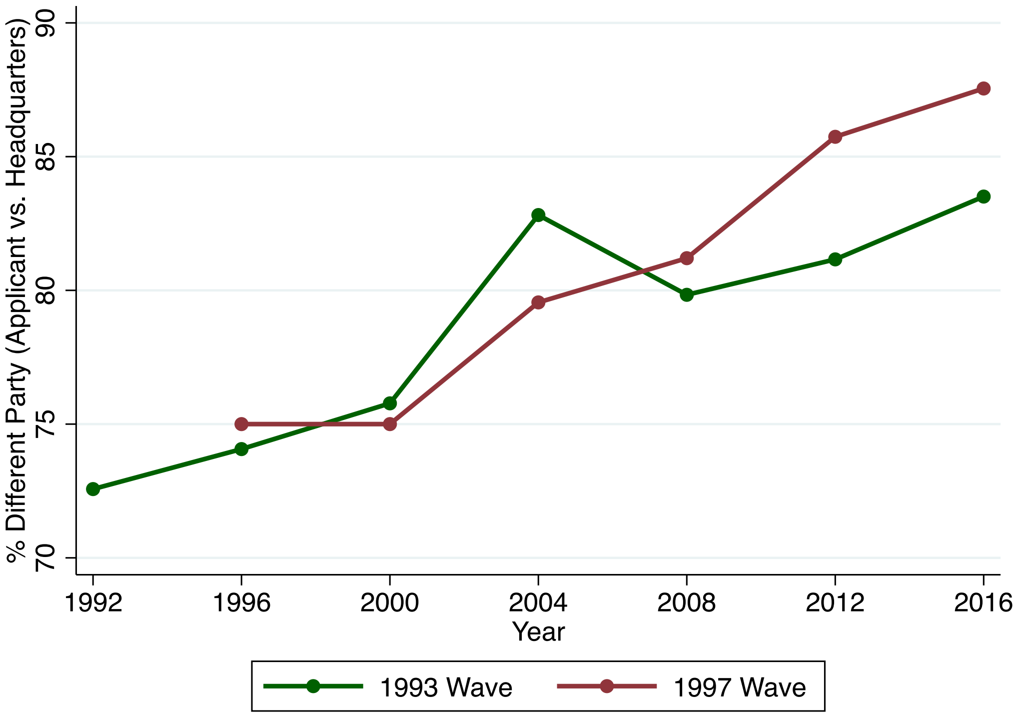


(d) Luminosity Gini - District level



Notes: This figure reports point estimates of the annual effects of splitting on luminosity. We use information from the night lights data (1992–2013). Figure 8a reports results for ln(0.1 + luminosity) at the municipality level after estimating Equation (1). Figure 8b reports results for ln(0.1 + average luminosity) at the district level after estimating Equation (3). We display coefficients separately for three sets of groups: applicant (blue), remaining (red), and headquarters (green) districts. Bars represent the 95%-confidence intervals. Estimates and further details can be found in Appendix Table D.4.

Figure 9: Divergent Political Preferences After Splits



Notes: Figure plots the percentage of municipalities where applicant and headquarters districts elected mayors from different parties after the split. Data on elections is only available at the municipality level. Therefore, we only plot trends for municipalities that split.

Appendices

A Conceptual Framework

This Section sketches a simple model to illustrate how administrative unit splits affect the provision of public services and to guide our empirical analysis. Our model incorporates several features from our context and highlights the mechanisms, including neglect from the headquarters and federal transfers (Bolton and Roland, 1997; Dur and Staal, 2008). We present further details of the model and formal proofs in Appendix Section B.

We work with a one-period model. We assume there is a municipality composed of two districts, A and B. The municipal population is immobile and divided into districts A and B, each having population α_A and α_B, respectively. There is no income heterogeneity within the district so that all residents have *per capita* income y. Two sources of municipal revenues finance public goods g: income taxes τ and federal transfers $T(\cdot)$. To map our model on to the Brazilian context as described in Section 2, $T(\cdot)$ depends on population size. In addition, we assume that federal transfers $T(\cdot)$ are weakly increasing and concave, while *per capita* federal transfers are weakly decreasing and convex in population size. The utility takes a quasi-linear form, $U_i = \theta_i \ln(g_i) + (1-\tau)y_i$, where θ_i captures local preferences for public goods in district i. We normalize the price of public goods to one.

District A contains the municipality headquarters and, for this reason, holds decision-making power, including the responsibility of choosing the allocation of public goods in both districts. When districts A and B are united and form a municipality, district A chooses the levels of public goods in districts A and B, g_A^U and g_B^U, that maximizes a Pareto weighted sum of utilities subject to a budget constraint. Put differently, district A solves the following maximization problem:

$$\max_{g_A, g_B, \tau} \quad (1-\lambda)\alpha_A U_A + \lambda \alpha_B U_B \quad \text{subject to} \quad g_A + g_B \leq \tau y + T(\alpha_A + \alpha_B), \tag{6}$$

where $y \equiv \alpha_A y_A + \alpha_B y_B$ and λ is the intra-municipality Pareto weight capturing the relative welfare strength of the two districts in deciding over the amount of public goods.

In the case of splitting, district B becomes a municipality and is granted decision-making power to decide the level of public goods g_B^S. We characterize the maximization problem as follows:

$$\max_{g_B,\tau} \quad \alpha_B U_B \quad \text{subject to} \quad g_B \leq \tau \alpha_B y_B + T(\alpha_B), \tag{7}$$

where $T(\alpha_B)$ represents the amount of federal transfers that the new municipality receives. The parent municipality, now comprising only district A, solves for g_A and τ through an analogous maximization problem.

Solving the above maximization problems and comparing the solutions, we can state the following proposition:

Proposition 1. *The benefits of splitting for district B are larger if:*

1. ***(Neglect)*** *Its welfare was neglected by the headquarters (lower λ);*
2. ***(Fiscal Incentives)*** *It is small in population size (lower α_B) and there are*
   - *(A1) a high comparative gain in transfers if split* $\left(\frac{T(\alpha_A+\alpha_B)}{y} \leq \frac{T(\alpha_B)}{\alpha_B y_B}\right)$*; and*
   - *(A2) a high comparative tax base* $\left(\frac{\theta_B}{\theta_A} \leq \frac{y_B}{y_A}\right)$.

Proof. See Appendix Section B.1. □

To understand the distributional effects of splitting, we extend our framework to introduce a second municipality with population α_2.[35] To illustrate the redistribution of federal transfers after a split, define T_i^U as the amount in transfers area i receives when municipality 1 is united, and let T_i^S be the transfers when municipality 1 splits.

To match the Brazilian context, we assume that transfers are zero-sum, always summing to a constant $\overline{T}$. We also assume that $T_A^S + T_B^S \geq T_{A+B}^U$ and $T_2^U \geq T_2^S$. We define the indirect utility of transfers for each area i when united as V_i^U and when split as V_i^S. We can express the changes in indirect utility for area i after a split as $\Delta V_i \equiv V_i^S - V_i^U$. Our next proposition details what determines changes in total welfare after a split.

Proposition 2. *If district B is relatively small* $\left(\frac{\alpha_B}{\alpha_A} \to 0\right)$ *and neglected by its parent district* $(\lambda \to 0)$*, and if municipality 2 is relatively large* $\left(\frac{\alpha_2}{\alpha_A+\alpha_B} \to \infty\right)$*, then (i) ΔV_A is small, (ii) ΔV_B is positive and large, and (iii) ΔV_2 is negative and small.*

[35] For the sake of simplicity we introduce a single new municipality to represent the rest of the state. Introducing a *set* of municipalities would be appropriate if the model was to fit the data directly.

Proof. See Appendix Section B.2. □

The intuition behind Proposition 2 is straightforward. Because of decreasing returns to spending, for a given configuration of population sizes and neglect by the headquarters district, the transfers moved from municipality 2 to district B may do little harm to the former and substantially benefit the latter. District A's welfare changes little, either positively or negatively, depending on whether its transfers change or not.[36] We directly test these predictions in Section 4 by separately evaluating the consequences of splitting for headquarter and non-headquarter districts.

We highlight that, despite being outside the scope of this paper, the model can be extended to incorporate specific features from other contexts. For example, it is possible to allow for individuals "voting with their feet" (Tiebout, 1956) with adjustments in population shares after policy choices. Ethnic divisions between areas (Alesina et al., 2004; Pierskalla, 2016; Bazzi and Gudgeon, 2021) or municipality mergers (Weese, 2015; Blom-Hansen et al., 2016) are also potential extensions of the model.

B Proofs of Propositions

B.1 Proof of Proposition 1

Proof. In order to approximate the Brazilian context, we assume throughout that $\lambda \leq 0.5$, $\alpha_B < \alpha_A$, and $y_B < y_A$. We also highlight two conditions which come up in the proofs below:

(A1) a high comparative gain in transfers if split $\left(\frac{T(\alpha_A+\alpha_B)}{y} \leq \frac{T(\alpha_B)}{\alpha_B y_B}\right)$; and

(A2) a high comparative tax base $\left(\frac{\theta_B}{\theta_A} \leq \frac{y_B}{y_A}\right)$.

From the unified policy choice problem (6), assuming there exists an interior optimum, we can solve the First Order Condition (FOC) for

$$\frac{g_B^U}{g_A^U} = \frac{\lambda}{1-\lambda}\frac{\alpha_B}{\alpha_A}\frac{\theta_B}{\theta_A} \tag{8}$$

[36] In a setting allowing for agglomeration effects, this result could be further exacerbated (Kline and Moretti, 2014).

The agent's private spending is $c_i = (1-\tau)y_i$. We can solve for a closed-form levels of public good provision and taxation under unification:

$$g_A^U = (1-\lambda)\alpha_A\theta_A\frac{y}{\bar{y}} \qquad g_B^U = \lambda\alpha_B\theta_B\frac{y}{\bar{y}} \qquad \tau^U = \frac{\bar{\theta}}{\bar{y}} - \frac{T(\alpha_A+\alpha_B)}{y} \tag{9}$$

where $\bar{y} \equiv (1-\lambda)\alpha_A y_A + \lambda\alpha_B y_B$, $y \equiv \alpha_A y_A + \alpha_B y_B$, $\bar{\theta} \equiv (1-\lambda)\alpha_A\theta_A + \lambda\alpha_B\theta_B$, and $\theta \equiv \alpha_A\theta_A + \alpha_B\theta_B$.

Under the same functional-form assumptions, from problem (7), it is straightforward to show that

$$g_A^S = \alpha_A\theta_A \qquad g_B^S = \alpha_B\theta_B \qquad \tau_A^S = \frac{\theta_A}{y_A} - \frac{T(\alpha_A)}{\alpha_A y_A} \qquad \tau_B^S = \frac{\theta_B}{y_B} - \frac{T(\alpha_B)}{\alpha_B y_B} \tag{10}$$

Thus, district B unilaterally chooses to split if $U_B^S \geq U_B^U$. Substituting in Equations (9) and (10), we express the surplus condition as:

$$\begin{aligned} G(\lambda,\alpha_A,\alpha_B,\theta_A,\theta_B,y_A,y_B,T) &\equiv U_B^S - U_B^U \\ &= \theta_B[\ln(g_B^S) - \ln(g_B^U)] + (\tau^U - \tau_B^S)y_B \\ &= \theta_B \ln\left(\frac{\bar{y}}{\lambda y}\right) + \left(\frac{\bar{\theta}}{\bar{y}} - \frac{\theta_B}{y_B} + \frac{T(\alpha_B)}{\alpha_B y_B} - \frac{T(\alpha_A+\alpha_B)}{y}\right) y_B \\ &\geq 0 \end{aligned} \tag{11}$$

With simple algebra we can show that

1. $\frac{\partial G}{\partial \lambda} = -\frac{\alpha_A}{\lambda \bar{y}^2}[(1-\lambda)\alpha_A\theta_B y_A^2 + \lambda\alpha_B\theta_A y_B^2] \leq 0.$
2. $\frac{\partial G}{\partial \alpha_B} = -y_B\left[\frac{(1-2\lambda)\alpha_A\theta_B y_A}{\lambda y\bar{y}} + \frac{(1-\lambda)\lambda\alpha_A(\theta_A y_B - \theta_B y_A)}{\bar{y}^2} + \frac{T'(\alpha_A+\alpha_B)y - T(\alpha_A+\alpha_B)y_B}{y^2}\right] + \frac{\alpha_B T'(\alpha_B) - T(\alpha_B)}{\alpha_B}$

   After more algebra we conclude that $\frac{\partial G}{\partial \alpha_B} \leq 0$ if conditions (A1) and (A2) hold.
3. $\frac{\partial G}{\partial \theta_A} = \frac{(1-\lambda)\alpha_A y_A}{\bar{y}} \geq 0$
4. $\frac{\partial G}{\partial \theta_B} = \ln\left(\frac{\bar{y}}{\lambda y}\right) - \frac{(1-\lambda)\alpha_A y_A}{\bar{y}} \lesseqgtr 0.$
5. $\frac{\partial G}{\partial y_A} = -\frac{\alpha_A y_B}{y^2\bar{y}^2}[\bar{\theta}y[(1-\lambda)y - (1-2\lambda)\alpha_B\theta_B] - T(\alpha_A+\alpha_B)\bar{y}^2] \lesseqgtr 0$

6. $\frac{\partial G}{\partial y_B} = \frac{\alpha_A y_A}{y^2 \overline{y}^2}[y((1-\lambda)\overline{\theta}y + (1-2\lambda)\alpha_B\theta_B) - T(\alpha_A+\alpha_B)\overline{y}^2] \lesseqgtr 0$

To further understand how choices of public goods provision and local taxation change with a split, we derive similar calculations for g_B and τ_B. If district B splits, it increases its provision of public goods ($g_B^S \geq g_B^U$) if, and only if

$$\begin{aligned} H(\lambda, \alpha_A, \alpha_B, \theta_A, \theta_B, y_A, y_B) &\equiv g_B^S - g_B^U \\ &= \alpha_B\theta_B - \frac{\lambda\alpha_B\theta_B y}{\overline{y}} \\ &= \frac{(1-2\lambda)\alpha_A\alpha_B\theta_B y_A}{\overline{y}} \geq 0 \end{aligned} \tag{12}$$

With simple algebra we can show that

1. $\frac{\partial H}{\partial \lambda} = -\frac{\alpha_A\alpha_B\theta_B y_A y}{\overline{y}^2} \leq 0$

2. $\frac{\partial H}{\partial \alpha_B} = -\frac{(1-2\lambda)\theta_B y_A[\lambda\alpha_B^2 y_B - (1-\lambda)\alpha_A^2 y_A]}{\overline{y}^2} \geq 0.$

3. $\frac{\partial H}{\partial \theta_A} = 0$

4. $\frac{\partial H}{\partial \theta_B} = \frac{(1-2\lambda)\alpha_A\alpha_B y_A}{\overline{y}} \geq 0.$

5. $\frac{\partial H}{\partial y_A} = \frac{(1-2\lambda)\lambda\alpha_A\alpha_B^2\theta_B y_B}{\overline{y}^2} \geq 0.$

6. $\frac{\partial H}{\partial y_B} = -\frac{(1-2\lambda)\lambda\alpha_A\alpha_B^2\theta_B y_A}{\overline{y}^2} \leq 0.$

Moreover, district B changes local tax rates from τ^U to τ_B^S after a split. Substituting in all terms and rearranging, this is equivalent to

$$\begin{aligned} \tau_B^S - \tau^U &= \frac{\theta_B}{y_B} - \frac{\overline{\theta}}{\overline{y}} + \frac{T(\alpha_A+\alpha_B)}{y} - \frac{T(\alpha_B)}{\alpha_B y_B} \\ &= \frac{(1-\alpha)\alpha_A\alpha_B y[\theta_B y_A - \theta_A y_B] + \overline{y}[\alpha_B y_B T(\alpha_A+\alpha_B) - yT(\alpha_B)]}{\alpha_B y_B y \overline{y}} \end{aligned} \tag{13}$$

We conclude that local tax rates after a split are lower than when districts are united, i.e. $\tau_B^S \leq \tau^U$, if conditions (A1) and (A2) hold.

□

B.2 Proof of Proposition 2

Proof. Assume that district B is relatively small $\left(\frac{\alpha_B}{\alpha_A} \to 0\right)$ and neglected by its parent district ($\lambda \to 0$), and if municipality 2 is relatively large $\left(\frac{\alpha_2}{\alpha_A+\alpha_B} \to \infty\right)$. Moreover, simplifying notation from Section B.1 gives us

$$\Delta V_A = \theta_A \ln\left(\frac{\overline{y}}{(1-\lambda)y}\right) + \left(\frac{\overline{\theta}}{\overline{\overline{y}}} - \frac{\theta_A}{y_A} + \frac{T(\alpha_A)}{\alpha_A y_A} - \frac{T(\alpha_A+\alpha_B)}{y}\right) y_A \tag{14}$$

$$\Delta V_B = \theta_B \ln\left(\frac{\overline{y}}{\lambda y}\right) + \left(\frac{\overline{\theta}}{\overline{\overline{y}}} - \frac{\theta_B}{y_B} + \frac{T(\alpha_B)}{\alpha_B y_B} - \frac{T(\alpha_A+\alpha_B)}{y}\right) y_B \tag{15}$$

$$\Delta V_2 = \frac{T^S(\alpha_2) - T^U(\alpha_2)}{\alpha_2} \tag{16}$$

Given our assumptions, it is straightforward to show that $\Delta V_A \to 0$, $\Delta V_B \to \infty$, $\Delta V_2 \to 0$.

$\square$

C Data Construction

C.1 Splitting Requests

This appendix contains a detailed description of the data on split requests used in this paper. As previously explained, we construct a novel data set containing all requests to split made by districts during the years between 1989 and 1996 from historical archives. Prior to the enactment of the 1996 Constitutional Amendment (CA), each state assembly had discretion to set its own rules to regulate over splitting, leading to substantial variation in local legislation and records on split requests.

Brazil has 26 state legislative assemblies.[37]. For each state assembly, we search for digitized historical records on split requests from the first half of the 1990s. We find records for twelve states: Amapá, Amazonas, Espírito Santo, Goiás, Mato Grosso, Minas Gerais, Pará, Paraná, Rio Grande do Sul, Rondônia, Santa Catarina, and São Paulo. The availability and quality of the data widely vary across states. We exemplify the online material we have access to in Figure C.1 below.

In what follows, we list the variables we construct from the records for each state:

Amapá. indicator for whether district has requested to split; indicator for whether district has the request approved; id of the split process; date when the process began; date when the referendum was approved; and result of the referendum.

Amazonas. indicator for whether district has requested to split; indicator for whether district has the request approved; and result of the referendum.

Espírito Santo. indicator for whether district has requested to split; indicator for whether district has the request approved; date when the process began; date when the referendum was approved; and result of the referendum.

Goiás. indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; id of the split process; date when the referendum was approved; and result of the referendum.

Mato Grosso. indicator for whether district has requested to split; indicator for whether district has the request approved; id of the split process; date when the process began;

[37] The country has 27 federal units, encompassing 26 states and the Federal District. The Federal District does not have a state assembly. Instead, it has a legislative chamber.

and result of the referendum.

Minas Gerais. indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; date when the request was archived; id of the split process; date when the process began; date when the referendum was approved; and result of the referendum.

Pará. indicator for whether district has requested to split; indicator for whether district has the request approved; id of the split process; date when the process began; date when the referendum was approved; and result of the referendum.

Paraná. indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; id of the split process; date when the process began; and result of the referendum.

Rio Grande do Sul. indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; id of the split process; date when the process began; date when the referendum was approved; and result of the referendum.

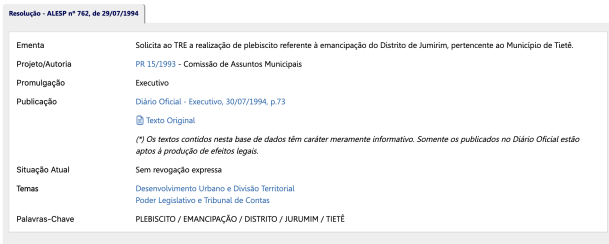
Rondônia. indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; date when the request was archived; id of the split process; date when the referendum was approved; and result of the referendum.

Santa Catarina. indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; date when the request was archived; id of the split process; date when the process began; date when the referendum was approved; and result of the referendum.

São Paulo. indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; id of the split process; date when the process began; date when the referendum was approved; and result of the referendum.

Figure C.1: Examples of Raw Material for Split Request Data Collection

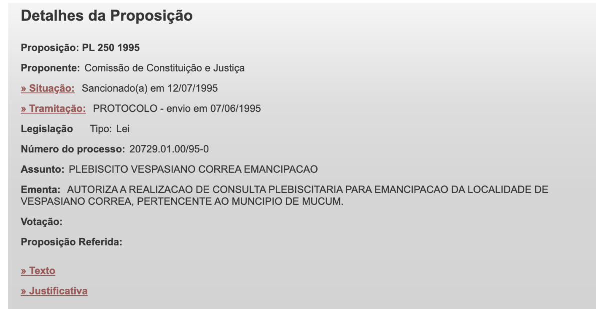
(a) São Paulo



Resolução - ALESP nº 762, de 29/07/1994

Ementa	Solicita ao TRE a realização de plebiscito referente à emancipação do Distrito de Jumirim, pertencente ao Município de Tietê.
Projeto/Autoria	PR 15/1993 - Comissão de Assuntos Municipais
Promulgação	Executivo
Publicação	Diário Oficial - Executivo, 30/07/1994, p.73
	Texto Original
	(*) Os textos contidos nesta base de dados têm caráter meramente informativo. Somente os publicados no Diário Oficial estão aptos à produção de efeitos legais.
Situação Atual	Sem revogação expressa
Temas	Desenvolvimento Urbano e Divisão Territorial Poder Legislativo e Tribunal de Contas
Palavras-Chave	PLEBISCITO / EMANCIPAÇÃO / DISTRITO / JURUMIM / TIETÊ

(b) Rio Grande do Sul



Detalhes da Proposição

Proposição: PL 250 1995

Proponente: Comissão de Constituição e Justiça

» Situação: Sancionado(a) em 12/07/1995

» Tramitação: PROTOCOLO - envio em 07/06/1995

Legislação Tipo: Lei

Número do processo: 20729.01.00/95-0

Assunto: PLEBISCITO VESPASIANO CORREA EMANCIPACAO

Ementa: AUTORIZA A REALIZACAO DE CONSULTA PLEBISCITARIA PARA EMANCIPACAO DA LOCALIDADE DE VESPASIANO CORREA, PERTENCENTE AO MUNCIPIO DE MUCUM.

Votação:

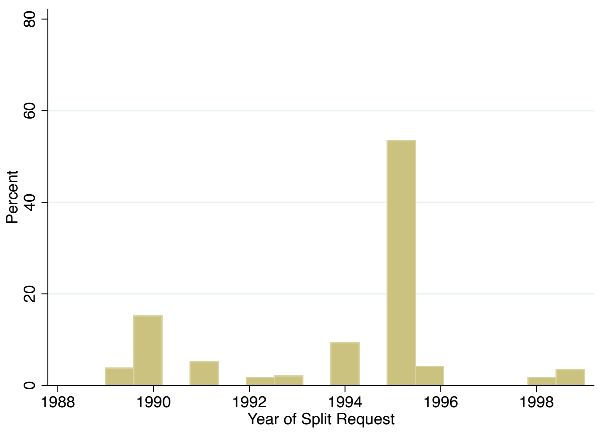
Proposição Referida:

» Texto

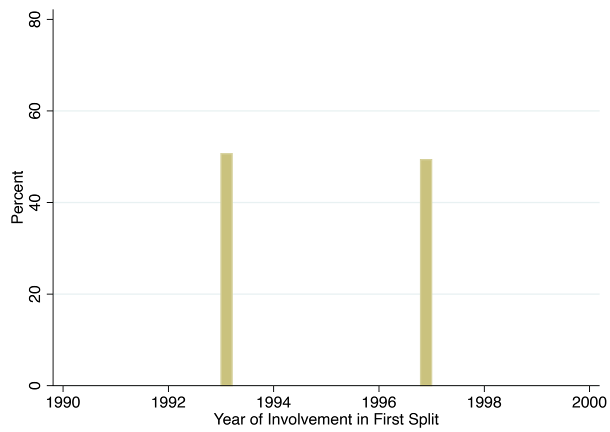
» Justificativa

Figure C.2: Histograms of Event Years

(a) Year of *Request* for Almost Split-Applicants



(b) Year of *Split* for Split-Applicants



D Additional Results

Table D.1: Descriptive Statistics in Levels by Split Wave - Districts

	Applicants		New		(1)-(2)		(3)-(4)	
	1993	1997	1993	1997	Diff.	p	Diff.	p
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population (000's)	5.73	4.95	4.94	4.32	-.78	.46	-.62	.27
% Urban Population	.42	.34	.4	.33	-.08	0	-.07	0
% Male	.52	.52	.52	.52	0	.73	0	.77
% Literacy	.69	.62	.68	.61	-.06	0	-.07	0
% Piped Water	.45	.45	.5	.43	0	.92	-.07	.02
% Sanitation	.58	.63	.65	.63	.06	.06	-.02	.56
% Trash Removal	.1	.1	.1	.09	0	.76	-.02	.38
Avg. Luminosity	1.97	1.51	1.23	.73	-.45	.36	-.49	.17
Area (000's km2)	.5	.61	.57	.69	.11	.4	.12	.43
ln(Distance to Parent Townhall)	2.97	2.96	3.05	3.05	-.01	.92	.01	.93
ln(Distance to State Capital)	5.49	5.45	5.5	5.6	-.04	.59	.09	.17
ln(Maize Suitability)	8.64	8.69	8.68	8.69	.04	.05	.01	.58
ln(Wet Rice Suitability)	8.57	8.68	8.56	8.68	.11	.08	.12	.14
ln(Soybean Suitability)	7.7	7.74	7.7	7.73	.04	.27	.03	.46
ln(Wheat Suitability)	6.56	6.52	6.45	6.66	-.04	.88	.2	.48
Terrain Ruggedness	86.16	79.44	95.21	83.22	-6.72	.32	-11.99	.12
	N = 306	N = 246	N = 256	N = 185				

Notes: This table reports results from an OLS regression at the municipality level estimating the relationship between there being a split in the municipality and observable characteristics from the baseline period. Standard errors clustered at the state-split wave level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table D.2: Difference-in-Differences Results for Public Finance and the Local Economy

	ln(Rev. p.c.)	ln(Exp. p.c.)	ln(Federal Transfers p.c.)	ln(Capital Exp. p.c.)	ln(Current Exp. p.c.)	ln(Public Municipal Jobs)
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Split	0.11***	0.13***	0.31***	0.20***	0.12***	0.18**
	(0.02)	(0.03)	(0.03)	(0.07)	(0.02)	(0.08)
Observations	7,819	7,819	7,795	7,814	7,818	7,010
R-squared	0.94	0.94	0.90	0.75	0.94	0.87
State-Year FE	✓	✓	✓	✓	✓	✓
Controls-Time FE	✓	✓	✓	✓	✓	✓

	ln(Public Municipal Wages)	ln(Local Taxation Revenues p.c.)	ln(Establishments)	ln(Private Jobs)	ln(Private Wages)
	(7)	(8)	(9)	(10)	(11)
Post x Split	0.17***	0.05	0.05*	0.01	0.06
	(0.06)	(0.04)	(0.03)	(0.07)	(0.07)
Observations	7,010	7,818	7,086	7,086	7,086
R-squared	0.91	0.92	0.99	0.98	0.98
State-Year FE	✓	✓	✓	✓	✓
Controls-Time FE	✓	✓	✓	✓	✓

Notes: Point estimates for Figure 7. Results from estimation of Equation (1) in Section 4. Main data sources are the *Finanças Brasileiras* (Finbra) data set for 1989 to 2018 and RAIS for 1995 to 2018. We classify jobs and establishments according to sector with information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO). Controls interacted with time fixed effects include number of districts in 1991, ln(Population) in 1991, % Urban in 1991, ln(area), ln(distance to state capital), ln(Income p.c.) in 1991, and HHI for race and religion in 1991. Standard errors clustered at the micro-region level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table D.3: Difference-in-Differences Results for Education and Public Services Delivery

	Attnd. Preschool (1)	Attnd. Middle School (2)	Attnd. High School (3)	% Literacy 11-14 yr. (4)	% Literacy 25+ yr. (5)	Years of Educ. (6)
Post x Split	3.92***	2.25**	-0.23	1.68***	0.95**	0.12*
	(1.47)	(1.10)	(1.12)	(0.58)	(0.43)	(0.07)
Observations	1,344	1,344	1,344	1,344	1,344	1,344
R-squared	0.91	0.80	0.91	0.85	0.97	0.89
Mean	13.11	88.07	28.13	91.64	74.65	8.83
SD	9.68	10.73	14.44	8.94	12.86	1.44
	% Piped Water (7)	% Trash Collection (8)	% Electricity (9)	% Sewage (10)	% Extreme Poverty (11)	% Poverty (12)
Post x Split	1.77	4.42***	2.50	1.00*	-1.33	-1.77
	(1.24)	(1.59)	(1.60)	(0.60)	(1.08)	(1.24)
Observations	1,344	1,344	1,344	1,344	1,344	1,344
R-squared	0.89	0.87	0.83	0.89	0.89	0.94
Mean	71.18	63.51	81.33	96.10	19.62	42.81
SD	24.17	27.35	20.03	7.66	14.86	20.6

Notes: Point estimates for Figure 6. Results from estimation of a simplified version of Equation (1) in Section 4. Each coefficient is calculated from a regression with 1344 observations on the given standardized outcome. Main data sources are the Demographic Census of 1991, 2000 and 2010. Standard errors clustered at the state-split wave level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table D.4: Difference-in-Differences Estimates for Districts

	ln(0.1 + Luminosity)		
	Applicants (1)	Remaining (2)	Headquarters (3)
Post x Split	0.33***	0.03	0.05
	(0.07)	(0.07)	(0.04)
Observations	10,122	5,893	6,947
R-squared	0.97	0.97	0.99
Mean Pre-Split	-0.761	-0.856	0.183
SD Pre-Split	1.525	1.433	1.432

	% Pixels Lit		
	Applicants (1)	Remaining (2)	Headquarters (3)
Post x Split	0.07***	-0.01	0.02**
	(0.02)	(0.02)	(0.01)
Observations	10,122	5,893	6,947
R-squared	0.97	0.95	0.98
Mean Pre-Split	0.176	0.158	0.244
SD Pre-Split	0.289	0.260	0.290

	Luminosity Gini		
	Applicants (1)	Remaining (2)	Headquarters (3)
Post x Split	-0.04***	-0.01	-0.01**
	(0.01)	(0.02)	(0.01)
Observations	9,616	4,976	6,918
R-squared	0.97	0.95	0.98
Mean Pre-Split	0.835	0.838	0.842
SD Pre-Split	0.236	0.208	0.183

Notes: Table reports point estimate results from Equation (3) in Section 4 plotted in Figure 8. All Columns include district and state-time fixed effects. Controls interacted with time fixed effects include ln(area), ln(distance to parent town hall), ln(distance to state capital), soil suitability for maize, wet rice, soybean, and wheat, terrain ruggedness, and ln(Luminosity) in 1992. Standard errors clustered at the state-split wave level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

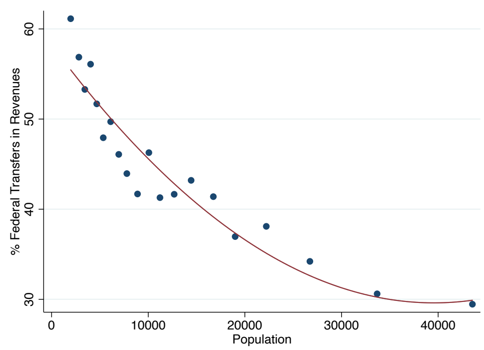
Table D.5: Oster (2019) Correction

	Baseline		β		δ
Applicant: Post $\times$ Split	.33	.17	.17	.18	.12
Remaining: Post $\times$ Split	.007	-4.63	-.99	-.54	-.03
Headquarters: Post $\times$ Split	.014	-.03	-.08	1.06	.38
δ		1	2	5	
β					0
R_{max}		1	1	1	1

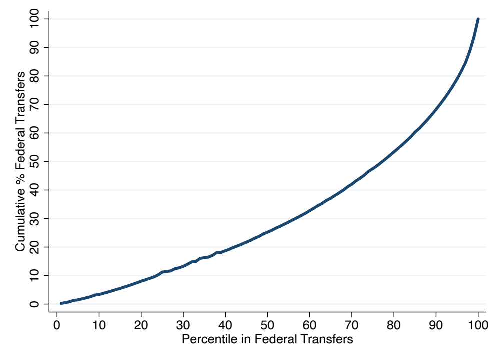
Notes: Results from the procedure proposed by Oster (2019) for estimates from Equation (3).

Figure D.3: Distribution of Federal Transfers

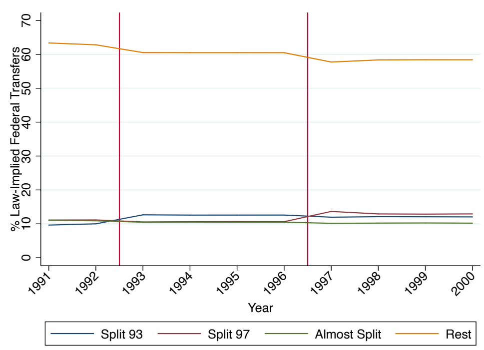
(a) % Transfers from Municipality Revenues 1991



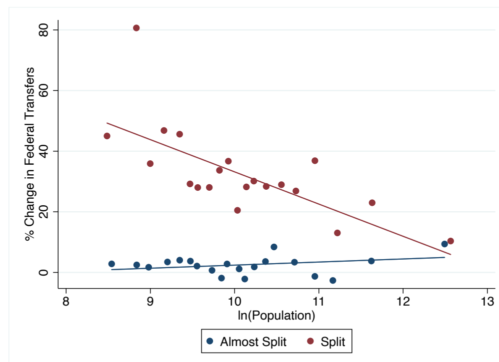
(b) Transfers Gini 1991 (bottom 50% get ≈ 26%)



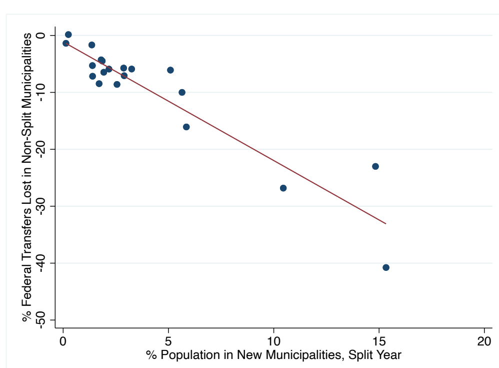
(c) Group Shares After Split Waves



(d) Federal Transfers Change by Group



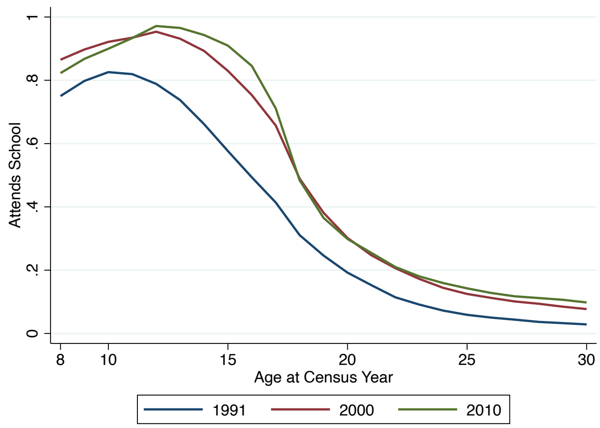
(e) Federal Transfers Change for Non-Split Municipalities, 1997 Wave Split, State Level



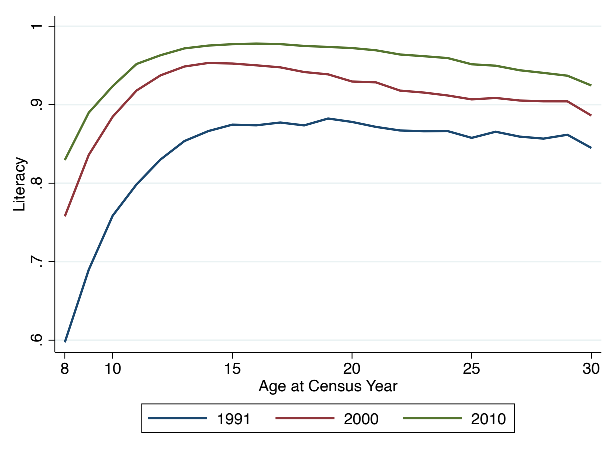
Notes: Figures describing patterns of revenues from federal transfers (*Fundo de Participação dos Municípios*) over time, as described in Section 2. Panel (a) describes the disproportionate share of municipal revenues from federal transfers for small municipalities in 1991. Panel (b) plots the distribution of federal transfers in 1991. Panel (c) plots the reallocation of law-implied federal transfers after the 1993 and 1997 split waves. Panel (d) shows how the gains in revenues from federal transfers accrue particularly to small new municipalities. Panel (e) shows the within-state reallocation of federal transfers from non-split to new municipalities after the 1997 split wave.

Figure D.4: Education Outcomes - Raw Data

(a) Attends School



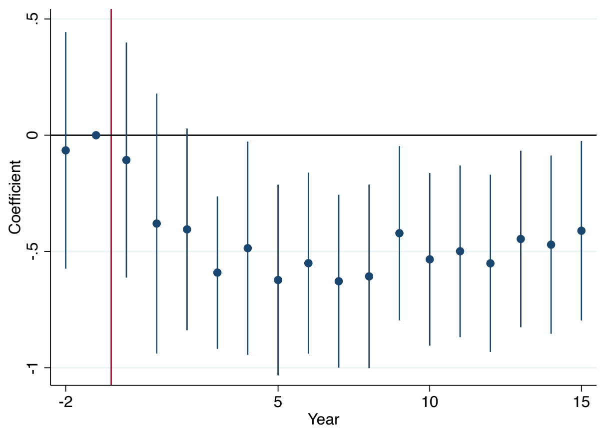
(b) Literacy



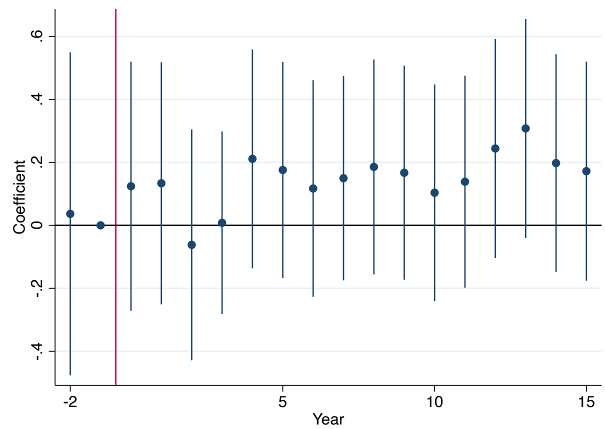
Notes: Panels (a) and (b) display the raw data, relative to Figure 5, by year and age. Main data sources are the Demographic Census microdata from of 1991, 2000 and 2010. Questions about educational attainment are only asked for individuals age 5 or older.

Figure D.5: Crowd-out of Nonprofit Jobs in Education

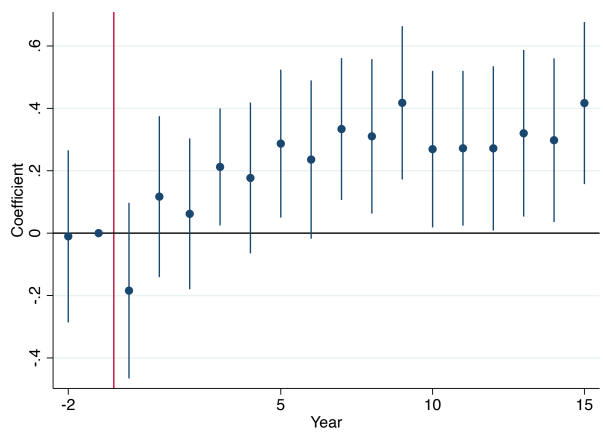
(a) ln(Nonprofit Jobs)



(b) ln(Private Sector Jobs)



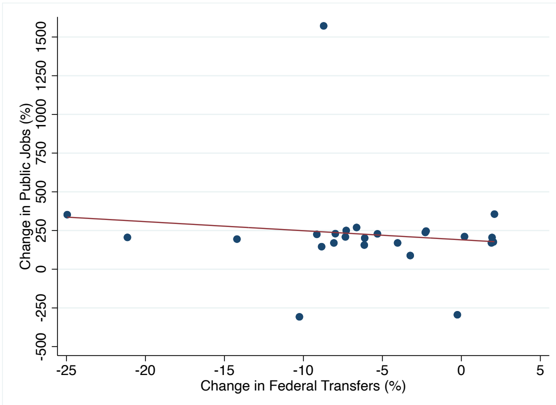
(c) ln(Public Jobs)



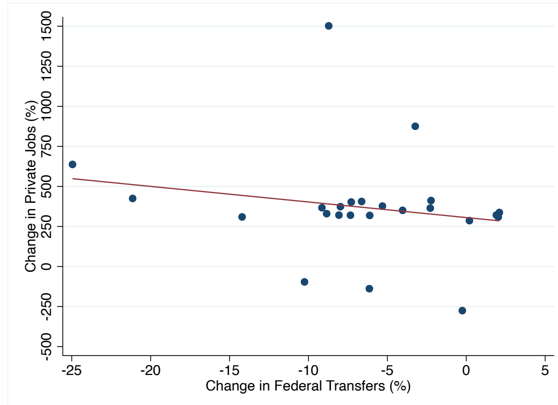
Notes: Results from estimation of Equation (1) in Section 4. Main data source is the *Relação Anual de Informações Sociais* (RAIS) data set for 1995 to 2018. We classify jobs and establishments according to sector with information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO). Controls interacted with time fixed effects include number of districts in 1991, ln(population) in 1991, % urban in 1991, ln(area), ln(distance to state capital), ln(income p.c.) in 1991, and HHI for race and religion in 1991. Bars represent the 95%-confidence intervals.

Figure D.6: Long-Term Within-State Spillovers

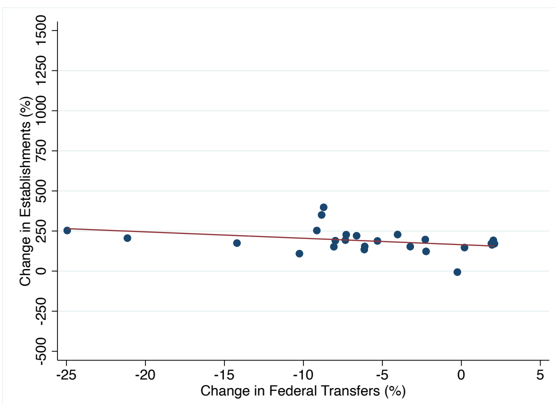
(a) Public Jobs



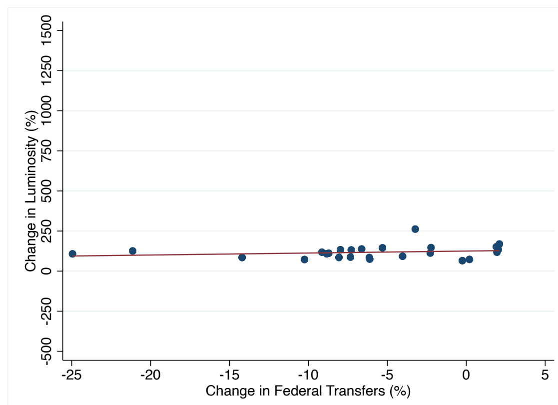
(b) Private Jobs



(c) Establishments



(d) Luminosity



Notes: Figure reports correlations between changes in federal transfers and outcomes for non-split municipalities at the state level after residualizing for region dummies. Outcomes are percentage changes 15 years after the split wave. Samples exclude the states *Distrito Federal* and *Roraima*.

E Details for Regression-Discontinuity in Federal Transfers

As we described in Section 2, the FPM revenues are first given in fixed blocks to states and, second, allocated to municipalities within state through a coefficient rule based on population size. More precisely, define FPM_m^s as the amount of federal transfers received by municipality m in state s in a given year. The allocation mechanism formula is the following:

$$FPM_m^s = FPM^s \frac{\lambda_m}{\sum_{m \in s} \lambda_m},$$

in which FPM^s is the amount of federal transfers allocated to state s. λ_m is the FPM coefficient of municipality m based on its population. The fraction $\frac{\lambda_m}{\sum_{m \in s} \lambda_m}$ is the share of state FPM transfers (FPM^s) allocated to municipality m in state s in a given year.

The coefficients λ_m mark a series of population cutoffs. For simplicity, we restrict our attention the first discontinuity (10,189 inhabitants) since it closely approximates the bracket in which the majority of our sample of new municipalities are located. Using a sample of municipalities described in Section 3.1.3, we estimate a regression discontinuity for selected outcomes expressed in log: total number of public jobs, establishments, total number of private jobs, and average luminosity. We compare municipalities barely located to the left (receive less federal transfers) and barely to the right (more transfers) of the population threshold. In particular, we consider the following specification:

$$y_{mt} = \alpha_m + \alpha_t + g(P_{m,t-1}) + \beta T_{mt} + \varepsilon_{mt} \tag{17}$$

in which y_{mt} represents outcomes for municipality m in year t. We include municipality and year fixed effects α_m and α_t. The function $g(\cdot)$ controls for linear polynomials of lagged population $P_{m,t-1}$, and T_{mt} indicates whether a municipality is treated for being located to the right of the population cutoff.

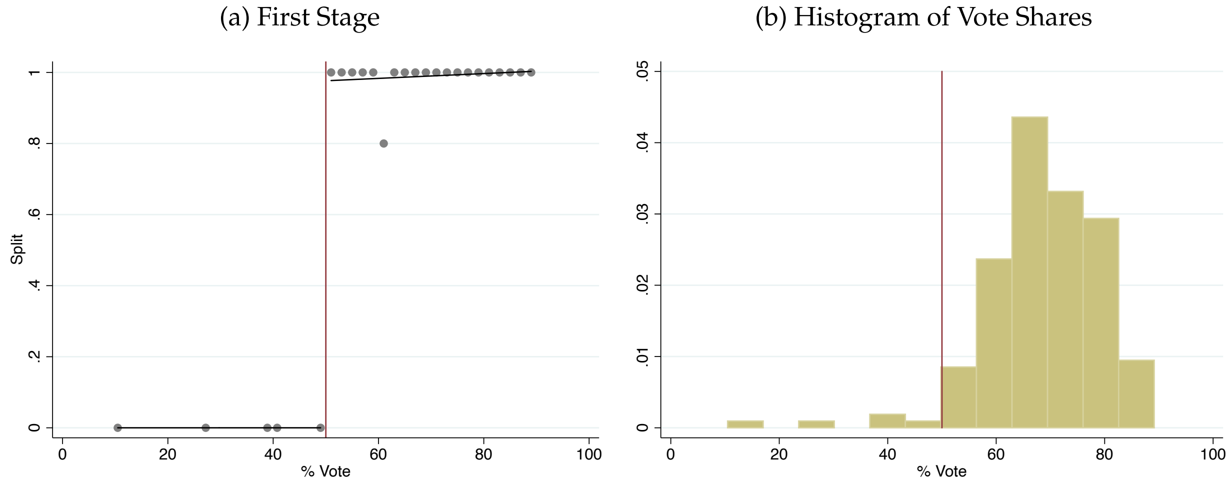
Table E.6: Effects of Federal Transfers - Regression Discontinuity

	ln(Transfers) (millions) (1)	ln(Public Jobs) (2)	ln(Estab.) (3)	ln(Private Jobs) (4)	ln(Luminosity) (5)
RD Estimate	0.13***	0.06	-0.04	-0.15	-0.00
	(0.03)	(0.10)	(0.11)	(0.15)	(0.16)
Observations	1,741	1,964	2,044	2,042	2,357
State-Year FE	✓	✓	✓	✓	✓
Optimal Bandwidth (%)	4	4.9	6.2	3.5	3.8

Notes: Results from estimation of Equation (17). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

F Details for Difference-in-Discontinuities in Luminosity

Figure F.7: Referendums in *Minas Gerais*



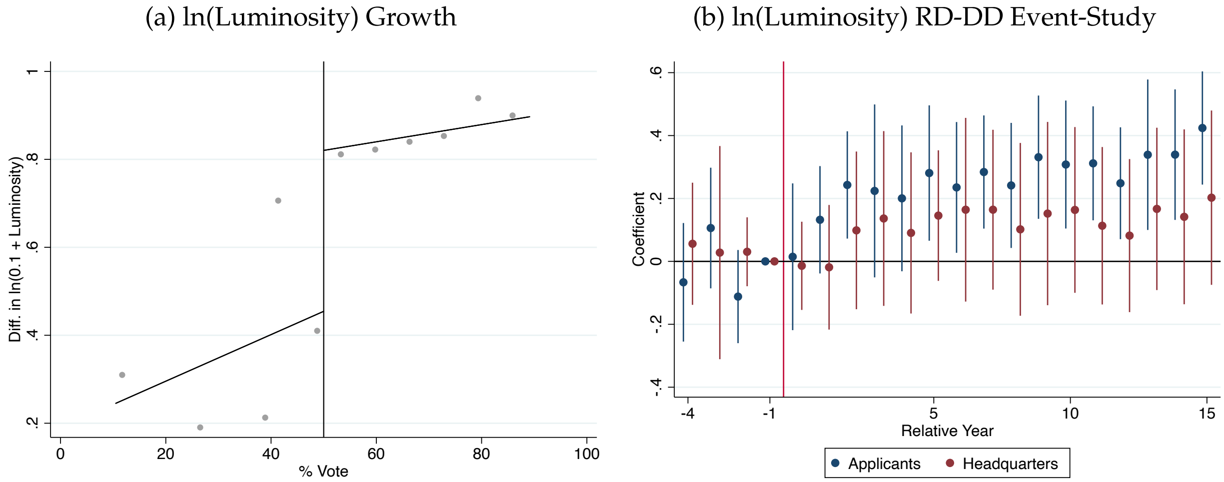
Notes: This figure plots data at the district level with the sample restricted to applicants. Panel (a) plots the binned first stage of referendum votes on likelihood of splitting. Panel (b) plots the frequency of referendum vote shares. As described in Section 2, districts required a unilateral referendum with at least 50% turnout and votes in favor as one of necessary steps for splitting.

Table F.7: Discontinuity Test on Covariates

VARIABLES	(1) ln(Population)	(2) ln(Area)	(3) ln(Luminosity)	(4) ln(Dist. Parent TH)
Referendum Vote $\geq$ 50%	0.57	0.18	1.87	-0.05
	(0.44)	(0.75)	(2.57)	(0.53)
Observations	50	50	50	50
R-squared	0.48	0.23	0.26	0.13
Mean	3.120	5.706	-4.102	3.120
SD	0.631	0.947	3.311	0.631

Notes: Estimates from Equation (4) in Section 4.5. *** p<0.01, ** p<0.05, * p<0.1.

Figure F.8: Difference-in-Discontinuities



Notes: Results from estimation described in Section 4.5. Panel (a) plots the growth in ln(0.1 + Luminosity) for applicant districts below and above the plebiscite approval cutoff of 50%. Panel (b) plots the Differences-in-Discontinuities results. Each set of coefficients is from one differences-in-differences equation, one for applicant and another for remaining districts. Controls interacted with time fixed effects include ln(area), ln(distance to parent town hall), ln(distance to state capital), soil suitability for maize, wet rice, soybean, and wheat, terrain ruggedness, and ln(Luminosity) in 1992. Bars represent the 95%-confidence intervals.

Table F.8: Fuzzy Difference-in-Discontinuities on ln(0.1 + Luminosity)

	First Stage (1)	Reduced Form (2)	Second Stage (3)	DD (4)
Referendum Vote $\geq$ 50%	0.96***			
	(0.18)			
Post x Referendum Vote $\geq$ 50%		0.16***		
		(0.05)		
Post x Split			0.22***	0.17***
			(0.05)	(0.02)
Observations	50	985	985	2,422
R-squared	0.64	0.98	0.98	0.98
District FE	-	✓	✓	✓
Controls-Year FE	-	✓	✓	✓
Mean	0.88	-0.82	-0.82	-0.6
SD	0.39	1.83	1.83	1.94

Notes: Estimates from Equations (4) and (5) in Section 4.5. *** p<0.01, ** p<0.05, * p<0.1.