Decentralizing Development: The Economic Impacts of Government Splits*

Ricardo Dahis† Christiane Szerman‡

May 11, 2025

Abstract

Changes in political boundaries aimed at devolving power to local governments are common in many countries. We examine the economic impacts of the creation of smaller government units through splitting. Exploiting reforms that led to sharp increases in the number of government units in Brazil, we show that voluntary splitting enlarges the public sector, enhances public service delivery, and stimulates economic activity in new local governments over the long term. These gains in economic activity are not offset by visible losses elsewhere and are stronger in peripheral, remote, and underdeveloped areas neglected by their parent governments. Increases in fiscal revenues and decentralization of decision-making power contribute to the positive effects on local economic activity.

*We thank Seema Jayachandran, Nancy Qian, and Chris Udry for their invaluable advice. We also thank Lori Beaman, Nicola Bianchi, Prashant Bharadwaj, Leah Boustan, Richard Bluhm, Mariana Carvalho, Alexandra Cirone, Henry Farber, Thomas Fujiwara, David Lee, Riccardo Marchingiglio, Alexandre Mas, Stelios Michalopoulos, Ameet Morjaria, Joris Mueller, Christopher Neilson, Paul Niehaus, Jacopo Ponticelli, Enrico Spolaore, Miguel Talamas, Edoardo Teso, Owen Zidar, anonymous referees, and participants at the NU Development Lunch, Princeton University, UC San Diego (OMG, Joint Lunch, and GSRS), UCB, PUC-Rio, New Economic School, Universidad del Rosario, University of Mannheim, EESP, National University of Singapore, Singapore Management University, Brown University, OSUS, ASSA 2023, and at many conferences for comments and suggestions. We thank Renan Holanda and Cláudio Júnior for excellent research assistance, and Ana Barufi and Rudi Rocha for kindly sharing data. Christiane gratefully acknowledges financial support from Princeton University and the Development Bank of Latin America (CAF).

†Department of Economics, Monash University. E-mail: ricardo.dahis@monash.edu.

‡Department of Economics, London School of Economics. E-mail: c.szerman@lse.ac.uk.

1 Introduction

Many countries have changed their political boundaries over the last 30 years to devolve power to local governments, and more countries are likely to follow suit (Grossman and Lewis, 2014). A large theoretical literature has explored various implications of redistricting for economic development. Proponents argue that the creation of smaller government units encourages competition in the provision of public goods (Tiebout, 1956), policies tailored to local preferences (Oates, 1972), and better monitoring of local governments (Besley and Case, 1995). Critics posit that this process leads to the proliferation of new government units that are susceptible to capture by special interests and may not be able to fund their operations (Boffa et al., 2016). This trade-off could be exacerbated if these new government units are too small to self-finance and require subsidies from the rest of the country to cover their expenses (Alesina and Spolaore, 1997). This paper examines this issue by assessing the long-run impacts of a large redistricting episode through the creation of new local governments (henceforth "splitting").

Brazil provides an interesting setting for studying the economic consequences of splitting. First, the country is divided into municipalities that hold substantial administrative, fiscal, and political decision-making power. These municipalities consist of one or more districts that lack decision-making power. Second, Brazil, which had relatively few municipalities by 1988, experienced one of the largest within-country splitting episodes worldwide.[1] Due to generous federal subsidies and lenient redistricting regulations, the number of municipalities increased by 34 percent from 4,124 to 5,507 between 1988 and 1996. These newly formed municipalities, previously districts, gained power and took on responsibilities assigned to local governments, including overseeing the provision of various public services, collecting local taxes, and managing fiscal revenues. In response to concerns about the rapid increase in the number of new municipalities, a reform in 1996 was implemented to curb their proliferation. The features of this institutional context create useful quasi-experimental variations for identification. Third, the availability of comprehensive data on public service delivery, labor market, economic activity, and fiscal performance offers an opportunity to evaluate both short- and long-term impacts.

[1]With a total area of about 8.5 million km^2, the average size of a Brazilian municipality was 2,017 km^2 in 1988. For comparison, the average municipality size in Italy is about 38 km^2. In the United States, the average county size is 2,577 km^2, but the counties are further divided into nearly 19.5 thousand incorporated cities with decision-making power. In India, the average district covers approximately 4,078 km^2 and is further subdivided into about 664 thousand villages, each with some decision-making power.

Quantifying the economic impacts of splitting is challenging. Splits across the world are typically voluntary and reflect the choices of the regions involved. These choices may be influenced by the underlying characteristics that can also affect local development. In our context, areas requesting to split tend to be less developed and often feel neglected by their parent municipalities. These areas are also eligible for larger federal transfers once they become municipalities, due to a non-linear transfer allocation mechanism that disproportionately benefits less populated municipalities (Tomio, 2002). Therefore, comparing municipalities that have undergone a split with those that have not is unlikely to identify the causal impacts of splitting.

To overcome this challenge and to document and characterize the selection into splitting, we construct a new dataset containing areas that unilaterally requested to split. We collect and classify digitized historical archives of these requests, covering the period between 1988 and 1996. Our difference-in-differences design compares areas with ratified requests to untreated areas whose requests to split were not approved, either due to political reasons or the 1996 reform. Because the *almost split* areas applied and failed to split for reasons unrelated to specific factors affecting local development, they serve as a counterfactual to the areas that ultimately split. We document that both treated and control areas exhibit similar levels and trends in various economic outcomes prior to splitting, supporting the causal interpretation of our difference-in-differences estimates.

The first result indicates that splitting improves public service delivery. Compared to their counterfactual, municipalities that split experience higher capital expenses (e.g., machinery and buildings) and current expenses (e.g., payroll and administrative costs), increasing by 27 and 17 percent, to establish new local governments. This expansion of the public sector leads to improvements in public services, including a 1 percentage point increase in household access to sewage and a 4.4 percentage point increase in household access to trash collection. Consistent with the expansion of educational infrastructure, analysis of individual-level Census data also reveals that younger, and therefore more exposed, cohorts show the greatest improvements in school attendance and literacy rates.

We also investigate the effects on economic activity beyond the public sector. Using matched employer-employee records, we find no evidence of a similar corresponding expansion in the private sector. However, these aggregate impacts mask substantial heterogeneity between economic sectors. We observe an increase in new establishments in the service sector. In addition, satellite records of nighttime light density confirm that municipalities that split experience an increase in economic activity. Using finer data, we disaggregate the effects at the municipality level by estimating the impacts separately for districts that did and did not request to split. We uncover significant distributional impacts: The gains from splitting are concentrated in districts that applied to split, while the remaining districts are little affected. Within the districts that applied to split, the increase in luminosity is not exclusively concentrated around the town hall.

Our main findings are robust across various checks, including alternative outcomes, samples, and specifications with flexible controls to account for observable baseline differences between the treated and control areas. We observe consistent patterns when disaggregating our results by waves of splitting, further strengthening their internal validity. Despite the robustness of our difference-in-differences findings, there remains a concern that unobserved factors could influence our estimates. To address this concern, we propose a novel research design. Before 1996, areas requesting to split had to conduct local referendums and secure approval by a simple majority. We leverage this rule in a difference-in-discontinuities design applied to *Minas Gerais*, a representative state with available referendum results. By comparing areas that narrowly secured the majority of votes needed to split with those that did not, we find qualitatively similar results.

What can explain the gains in public service delivery and economic activity? One explanation is that splitting results in higher fiscal revenues. Municipalities that split experience an average increase of 15 percent in revenues after splitting, primarily due to higher transfers from the federal government to the new local governments. These additional revenues are spent on bureaucracy and infrastructure, leading to mechanical increases in public services and economic activity.

Another non-mutually exclusive explanation is that splitting leads to the decentralization of decision-making power to new local governments. With administrative, fiscal, and political autonomy, they might be better able to address local needs. Several pieces of evidence also support this explanation. First, consistent with higher decision-making autonomy, improvements in public services after splitting are concentrated in activities exclusively controlled by local governments. We find no such gains for activities shared with the federal and state governments, despite the extra revenues. Second, the economic impacts are largest for small, rural, and remote areas that tend to be more captured and neglected by their former headquarters. This result indicates that autonomy translates into more substantial improvements in bureaucracy, public services, and economic activity for peripheral, remote, and underveloped areas with more constraints on state capacity. This is one of the main goals of decentralization (Bardhan, 2002). Third, an analysis of historical electoral data reveals that, after splitting, new municipalities often elect politicians affiliated with different political parties than those from their parent municipalities. This suggests that new municipalities are better able to implement policies that reflect local preferences, another premise of decentralization (Oates, 1972). Fourth, our mediation analysis shows that extra revenues account for part, but not all, of the economic impacts.

We next investigate whether the policy burdens the rest of the country due to resource losses. Using state-level variation in the loss of revenues from federal transfers caused by splitting, we examine the economic impacts on municipalities that did not change political boundaries. We cannot reject the null hypothesis of no spillovers. Although the small sample size prevents conclusive evidence, a lack of visible burden would be consistent with the idea that lost revenues may have previously subsidized wasteful expenses with a marginal value below the social costs of funds (Liebman and Mahoney, 2017). The reallocation of resources may decrease low-value spending in areas that did not voluntarily split and raise aggregate welfare.

A final analysis discusses our findings' implications for the local economy. We estimate a cost per public job of US$ 3,635 per year, significantly lower than the estimated cost per job of US$ 8,000 from the impact of increases in federal transfers to municipalities in Brazil (Corbi et al., 2019). We also find that splitting generates an output multiplier of 2.06–4.34. These numbers are aligned with, but slightly larger than, the median output multiplier of 1.9 from the literature on fiscal spending (Chodorow-Reich, 2019). We interpret our findings as consistent with both higher revenues from federal transfers and the decentralization of decision-making power contributing to boosting the local economy.

Our findings also have policy implications. First, although this paper does not quantify the optimal size of local governments, our results suggest that settings with large government units, such as in the Brazilian case, can benefit from subsidized and voluntary splits. In particular, new municipalities drive the gains in public services and economic activity, while we find no conclusive evidence for the rest of the country. This does not, however, imply that more splits are always advantageous. Second, we show that the benefits are greatest for peripheral, remote, and underdeveloped areas, providing potential lessons for other similar settings. Our findings suggest that splitting can achieve one of the main goals of decentralization: Making local governments more responsive and efficient in promoting long-run development (Bardhan, 2002).

This paper contributes to several lines of research. It broadly speaks to an extensive literature studying the causes and consequences of decentralization through splitting.[2] Building on the seminal works of Tiebout (1956) and Oates (1972), Alesina and Spolaore (2005) summarize the main trade-offs of smaller government units: The costs of losing scale and resources to provide public goods may be counterbalanced by the benefits of more homogeneous units and local power. Recent papers have empirically examined the trade-offs of splitting (Grossman et al., 2017; Lima and Silveira Neto, 2018; Cassidy and Velayudhan, 2022; Cohen, 2022). Narasimhan and Weaver (2024) document that smaller and less populated government units led to better access to public goods due to stronger civic engagement and political incentives in the Indian state of Uttar Pradesh. In Brazil, Lima and Silveira Neto (2018) find that splitting is associated with higher public expenses and has limited impacts on public goods. This paper expands on fiscal outcomes and uncovers the distributional effects of the policy.[3] We show that the gains from splitting are concentrated in new local governments and are stronger in peripheral and remote, backward areas neglected by their parent governments. We also quantify the impacts beyond the programs administrated by local governments, including whether higher public expenses crowd in the private sector, and adjudicate between mechanisms hypothesized by theories of decentralization. We find evidence of reduced capture and neglect, as well as increased political incentives, as important mechanisms.

The finding that peripheral, remote backward areas benefit the most from splitting contributes to a large literature on the effects of policies aimed at reducing regional inequality. Examples of policies include public investments and tax incentives towards disadvantaged geographic areas (Busso et al., 2013; Kline and Moretti, 2014; Shenoy, 2018; Slattery and Zidar, 2020), extra grant revenues (Caselli and Michaels, 2013; Litschig and Morrison, 2013; Gadenne, 2017; Corbi et al., 2019), and fiscal decentralization (Martínez-Vázquez et al., 2017; Bianchi et al., 2023). In our context, administrative remoteness and

[2]We note that other terms have also been used to study decentralization (Oates, 1972, 1999; Bardhan, 2002; Faguet, 2004, 2014; Barankay and Lockwood, 2007; Treisman, 2007; Gadenne and Singhal, 2014; Mookherjee, 2015), such as the size of nations (Bolton and Roland, 1997; Alesina and Spolaore, 1997, 2005; Lassen and Serritzlew, 2011), local government proliferation or fragmentation (Grossman and Lewis, 2014; Pierskalla, 2016; Grossman et al., 2017), border or territorial reforms (Coate and Knight, 2007; Boffa et al., 2016; Gendźwiłł et al., 2020; Bazzi and Gudgeon, 2021), amalgamations (Weese, 2015), and municipal cooperation and annexation (Schönholzer and Zhang, 2017; Ferraresi et al., 2018; Tricaud, 2022)

[3]Our results also add to the policy debate on the creation of new municipalities, often deliberated upon in the Brazilian National Congress (Tomio, 2002; Mattos and Ponczek, 2013; Lipscomb and Mobarak, 2017).

neglect from the headquarters regions are strongly associated with regional inequality. We find that splitting mitigates these frictions and generates persistent economic gains to peripheral regions. These findings are also related to the literature studying how geographical isolation and size affect state capacity and development (Ashraf et al., 2010; Stasavage, 2010; Nunn and Puga, 2012; Michalopoulos and Papaioannou, 2014; Campante et al., 2019; Bai and Jia, 2023; Bluhm et al., 2023; Chambru et al., 2024).

Our empirical findings also contribute to the literature investigating the determinants of state capacity (Besley and Persson, 2009, 2010; Acemoglu et al., 2015; Gennaioli and Voth, 2015; Mookherjee, 2015; Johnson and Koyama, 2017). Our policy experiment allows us to assess how the size of local government influences economic outcomes. The evidence that the creation of new local governments can both expand state capacity —by growing bureaucracy and implementing policies tailored to local conditions—and free peripheral regions from the capture of former governments is a novel contribution. These benefits do not require increased expenditures from higher levels of government, such as federal or state governments, nor do they result in shifts in economic activity from one area to another. This is relevant from a policy perspective, as countries often need to enhance state capacity under severe budget constraints.

The rest of the paper is organized as follows. Sections 2 and 3 describe the background and the data. Sections 4 and 5 present the empirical strategy and the main results. In Sections 6 and 7, we analyze the mechanisms, the spillover effects, and the implications of our findings for the local economy. Section 8 concludes.

2 Institutional Background

2.1 The Role of Municipal Governments

Brazil has three tiers of government that hold administrative, fiscal, and political power: federal, state, and municipal. Municipalities are the smallest governmental units with decision-making authority. Each municipality is divided into one or more districts, which are administrative subdivisions without any political autonomy. No district belongs to more than one municipality

The enactment of the Federal Constitution in 1988 represents the most important step towards fiscal federalism and *vertical* decentralization of administrative, fiscal, and political power (Arretche, 2000; Favero, 2004). Since 1988, municipalities have been responsible for overseeing the provision of several public services, including primary education, basic health care, sanitation, trash collection, and street lighting. Municipalities share responsibility for providing certain public services, such as sanitation and health care. For other services, such as primary education, they are the sole providers. Municipalities have fiscal autonomy to collect and manage local taxes (e.g., property and service taxes) and administer their own fiscal revenues (e.g., inter-governmental transfers and local revenues).

Every four years, there are municipal elections in October to elect mayors and municipal councilors.[4] In January after the elections, the elected officials take office.

2.2 The Creation of New Municipalities

The 1988 Federal Constitution also granted states the authority to establish their criteria regarding the creation and amalgamation of municipalities. The requirements, which varied across states, generally involved territorial contiguity, a minimum population, and some level of urban development for new municipalities.

The creation of a new municipality required a multi-stage process: (1) local leaders or state politicians representing an applicant area had to request the state assembly to create a new municipality; (2) the state legislative committee responsible for the request evaluated and approved it; (3) the state legislature authorized a referendum in the applicant area, although the state governor could veto it; (4) if the majority of voters in the local referendum voted in favor of splitting, the request was forwarded to the state legislature for a vote; (5) the state and federal governments had to approve or veto the request (Tomio, 2002). In practice, such vetoes were rare. These flexible rules led to a unique episode of *horizontal decentralization* in the first half of the 1990s, with an unprecedented number of districts initiating requests to split and become municipalities. Our data indicate that 39.3 percent of eligible districts applied to split between 1989 and 1996.[5]

In response to the rapid increase in new municipalities and concerns that these splits

[4]In municipalities with fewer than 200,000 voters, there is a single-round system. The candidate for mayor receiving the majority of votes is elected. Larger municipalities have a two-round system. In case no mayoral candidate gets at least 50 percent of votes, there is a second round with the two most-voted first-round candidates. The candidate receiving the most votes wins. Municipal councilors are elected through an open-list proportional representation system.

[5]Eligible districts are defined as non-headquarters districts with a population exceeding 5,000 according to the 1991 Census.

were inefficient and driven by local patronage, the Brazilian Congress enacted the Constitutional Amendment 15/1996 (henceforth "1996 CA"). The 1996 CA transferred the authority to regulate the creation of municipalities to the federal government. Three major changes stand out. First, districts requesting to split must conduct a referendum with the entire municipality, not just the applicant districts, and secure approval from the majority of voters. Second, the federal government requires evidence of fiscal sustainability from the applicant districts. Third, redistricting would depend on further, albeit never enacted, legislation by the federal government. Consequently, the 1996 CA effectively halted the creation of new municipalities and left various split requests pending and unapproved.

Once a request to split is approved, the applicant district (or group of districts) becomes a new municipality following the subsequent municipal elections, when the newly elected mayor and municipal councilors take office. In line with flexible redistricting regulations, Appendix Figure C.1 shows a 34 percent increase in the number of municipalities from 1989 to 1997, jumping from 4,124 to 5,507. Due to data availability, this paper focuses on the two main waves of splitting before the 1996 CA, both in 1993 and 1997, immediately following municipal elections.

2.3 The Reasons for Splitting

Several factors have influenced the splitting process in Brazil. We highlight two factors: neglect from headquarters and fiscal incentives. Previous studies have identified significant disparities in public service provision across districts within a municipality as a major driver of split requests (Cachatori and Cigolini, 2013; Klering et al., 2012). In a survey with mayors in 1992, Bremaeker (1993) confirms that the majority of respondents cited neglect by local governments (63 percent) and the large territorial size of local governments (24 percent) as the main reasons for seeking splits.

Fiscal incentives are also relevant. The creation of new municipalities affects the distribution of the *Fundo de Participação dos Municípios* (henceforth "FPM"), the main fund through which the federal government provides transfers to municipalities. The fund operates as follows. Each year, 22.5 percent of total revenues from federal income and industrial product taxes are allocated to the FPM. Each state receives a block grant to distribute among their municipalities, implying that transfers are zero sum within each state. Each municipality then receives a share based on a convex step-wise population-based formula that assigns coefficients to various population brackets. This formula includes a floor that benefits smaller municipalities. Although this floor is intended to offset fixed government setup costs, in practice, transfers per capita grew disproportionately for municipalities below 10,188 people. Municipalities within the same state and bracket receive equal amounts of transfers.[6] In addition, 15 percent of FPM transfers are earmarked for education and 15 percent for health, while the remaining funds are unearmarked (Brollo et al., 2013). On average, federal transfers make up between 30 and 60 percent of municipal revenues, whereas local taxation and fees represent about 5 percent.

When splits occur and new municipalities are established, these new municipalities begin receiving FPM transfers. Most of these splits are concentrated in smaller municipalities. Consequently, after each split, all other non-split municipalities within the state also experience a reduction in their revenue, which is reallocated to the newly split municipalities.[7] However, we note that the net change in FPM transfers for the headquarters and remaining municipalities is not straightforward, as it depends on several factors: the distribution of funds within the municipality prior to splitting, the curvature of the FPM population function, and the number of splits occurring within the state.

To evaluate the various forces at play and their empirical predictions, Appendix A presents a simple conceptual framework in which the municipal headquarters determine the allocation of public goods across districts within the municipality. The model highlights two key predictions to guide our empirical analysis. First, districts applying to split may benefit more from it if they are neglected by local governments or receive substantial fiscal incentives. Second, the impact of splitting on the headquarters districts and the rest of the country may be negligible.

3 Data

This paper uses newly collected data on split requests, along with various sources of spatial and administrative data, to examine how splitting impacts local development through public service delivery, formal labor market, economic activity, and fiscal performance.

[6] 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 transfers on economic outcomes.

[7] In our data, municipalities that split increased their share of federal transfers by about 20.3 percent, while those that did not experience a split saw a reduction of approximately 13.7 percent in their share.

Split Requests. We obtain information on the official creation dates of municipalities and their parent municipalities before the split from the Brazilian Institute of Geography and Statistics (IBGE). To catalog split requests, we collect and classify historical archives of such split requests, which vary significantly in availability, detail, and quality across states due to distinct redistricting requirements set by state assemblies before the 1996 CA. This final final dataset includes split requests—regardless of their approval status—from districts in 11 states (Amapá, Espírito Santo, Goiás, Mato Grosso, Minas Gerais, Pará, Paraná, Rio Grande do Sul, Rondônia, Santa Catarina, and São Paulo). This sample represents 41 percent of all states and covers 58 percent of the population and 63 percent of splits occurring between 1989 and 1996. The remaining states do not provide public records on split requests. Appendix C details the data collection. The dataset is available in the data supplement (Dahis and Szerman, 2025).

We also scrape legislative reports on referendum results for the state of Minas Gerais. To our knowledge, this is the only state with publicly available records, including information on turnout and the percentage of valid votes in favor of splitting. We validate these reports by cross-checking them with our data on split requests. The dataset is available in the data supplement (Dahis and Szerman, 2025).

Public Service Delivery. Information on public service delivery (e.g., household access to trash collection and sewage), along with demographic and socioeconomic characteristics (e.g., population size, urbanization rate, education, health, and income), come from the decennial Brazilian Demographic Census microdata, and are only available at a decadal frequency (IBGE, 2010a). For the municipality-level analysis, we use data from the 1991, 2000, and 2010 versions of the Atlas of Human Development (PNUD, 2013). For the district-level analysis, we group the Census tract-level data from IBGE, which contains a much more limited set of outcomes, into districts. For the individual-level analysis, we exploit variation across birth cohorts using individual-level microdata on literacy and school attendance, also from IBGE.

Formal Labor Market. We draw labor market information from the annual matched employer-employee data, the *Relação Anual de Informações Sociais* (RAIS).[8] The data cover

[8] The dataset was processed with code from Dahis (2024), which employs auxiliary files on inflation from IBGE (2025a), the monthly minimum wage from Ipeadata (2025b), conversion tables between CBO 1994 and ISCO 1988 codes from Muendler (2025), and the hierarchical structure of CBO 2002 categories from IBGE

the entire formal sector between 1985 and 2018, offering a comprehensive set of worker, job, and establishment characteristics. The total number of jobs and establishments in the public and private sectors at the municipality level can be calculated between 1995 and 2018. We also break down these variables by economic sectors (e.g., agriculture, mining, manufacturing, construction, retail, and services) and areas (e.g., education and health). A key limitation is that we cannot examine the impacts on the informal economy, due to the lack of data on the informal sector before 2000.[9]

Economic Activity. To measure economic activity, we use satellite imagery of night-time lights provided by the U.S. National Oceanic and Atmospheric Administration (NOAA) and the National Geophysical Data Center (NGDC), harmonized over time by Li et al. (2020).[10] The annual data consists of grids with integer values ranging from 0 (no light) to 63, representing the intensity of lights from 1992 to 2013.[11] Utilizing its granularity, we construct district- and municipality-level information on the intensive and extensive margins of luminosity, calculated as the weighted average of lights across grids within a district or a municipality and whether this average exceeds zero.

Fiscal Performance. Information on expenditures and revenues since 1989 comes from the National Treasury and are made available by Data Basis (2022e). The municipality-level data details revenue sources (e.g., local taxation and transfers) and expenditure categories (e.g., capital and current expenses).[12] We obtain information on federal transfers from Tesouro Nacional (2025).

Other data. Our analysis rely on other minor sources of data. Municipal and district directories with essential identifiers and administrative information are sourced from the Data Basis project (Data Basis, 2022a). We follow the methodology and code developed

(2025b).

[9] According to Ipeadata, the informal sector accounted for 56 percent of total employment in 1992.

[10] The intensity of night lights captures both outdoor and some indoor light use. Henderson et al. (2012) and Henderson et al. (2018) demonstrate that night lights are a reliable proxy for long-term GDP growth. This is particularly useful in our context, as there is no district-level data on economic activity, and data on electricity consumption are only available for more recent years.

[11] Each grid cell is a 30 arc-second output pixel or 0.86 square kilometers at the Equator.

[12] To our knowledge, there is no data on local tax rates during the 1990s. However, anecdotal evidence suggests that changes in local tax rates were uncommon and negligible.

by Ehrl (2017a,b) to map municipalities to minimum comparable areas (AMCs). For complete municipal genealogy—including parent-child relationships and creation dates—we utilize data from IBGE (2016). Geographical boundaries of Brazilian municipalities and districts as of 2010 are obtained from IBGE (2010b). Information on municipality physical area come from Ipeadata (2025c). Demographic and economic data include municipal population statistics from Data Basis (2022d) and municipal GDP from Data Basis (2022c), supplemented by state-level GDP figures from Ipeadata (2025a). We obtain price index data for deflating financial series from Banco Central do Brasil (2025). Geographic characteristics, such as soil suitability, are sourced from FAO-GAEZ (2021), while terrain ruggedness data come from Carter (2018). To explore some mechanisms underlying our results, we also use municipal-level electoral data from the Superior Electoral Court (TSE), made available by Data Basis (2022b). For the period between 1988 and 1996, data are made available by Dahis and Szerman (2025) and include only the names and party affiliations of elected mayors. More detailed information, such as the list of mayoral candidates and their vote shares, started to be reported in 1998.

Municipality-Level Sample. To address the limitation that changes in municipal boundaries may not always be nested, we employ a standard procedure to harmonize boundaries from 1991 to 2010 into minimum comparable areas (Lipscomb and Mobarak, 2017; Lima and Silveira Neto, 2018). This process results in a sample of 4,298 minimum comparable areas, which we refer to as municipalities. We use this approach, rather than the list of 5,565 municipalities from 2010, to keep the spatial units constant over time. For our main estimation sample, we start with the 4,298 municipalities from the 1991 Demographic Census and keep those that meet three criteria: (1) municipalities located in one of the 11 states with documented split requests; (2) municipalities with either a single split event or districts with split requests between 1989 and 1996, to avoid multiple events; and (3) municipalities that are not state capitals, as they also serve as headquarters of state governments. These restrictions result in a final sample of 448 municipalities.

District-Level Sample. Due to data availability, most of our analysis is conducted at the municipality level. However, data is also available at the district level for certain outcomes, such as luminosity and selected public services, allowing us to provide additional insights into differences within and across municipalities. We start with 8,855 districts from the 1991 Demographic Census and apply similar criteria as before to construct a district-level sample.[13] These criteria result in a final sample of 1,259 districts. We then classify these districts into three categories: (1) *applicant* districts, which are peripheral districts that requested to split; (2) *remaining* districts, which are peripheral districts that did not request a split but are in municipalities where some district did; and (3) *headquarters* districts, which are districts serving as headquarters in municipalities with at least one district requesting a split. This classification yields a final district-level sample of 552 applicant districts, 325 remaining districts, and 382 headquarters districts, which we use to assess the distributional impacts of splitting.

4 Empirical Strategy

4.1 Who Applies to Split?

To examine how municipalities select into splitting, Table 1 reports summary statistics of baseline characteristics for municipalities in 1991. The data show that, before splitting, municipalities with at least one applicant district (Column (1)) are comparable to those without any split requests (Column (3)) in various dimensions, including population composition and income. However, municipalities with applicant districts tend to be larger in population and area, have lower levels of public services, and receive a smaller share of federal transfers relative to total revenues. Unlike countries such as Indonesia or India (Pierskalla, 2016; Bazzi and Gudgeon, 2021), differences in racial and religious composition are minimal, ruling out social fragmentation as a key driver of splitting.

We also examine how districts select to apply to split. Our district-level analysis confirms that districts requesting a split are generally less developed compared to other districts. Appendix Table D.1 compares baseline characteristics between applicant (Column (1)) and headquarters districts (Column (5)). On average, the applicant districts have worse economic and demographic conditions before splitting. They tend to be smaller in population and area, less urbanized, and located farther from their parent town halls.[14]

[13] We include districts that meet four criteria: (1) districts within one of the 11 states with documented split requests; (2) districts not located in state capitals; (3) municipalities where split requests were initiated by districts, rather than by smaller areas such as neighborhoods or parks; and (4) districts in municipalities with a single split event or with split requests between 1989 and 1996, to avoid multiple events.

[14] Interestingly, applicant districts are larger and more developed than the remaining districts (Column 3). Columns (7)–(10) also display summary statistics for districts not included in the estimation sample.

4.2 Identification

Our goal is to examine how splitting affects economic performance. To mitigate concerns related to selection into splitting, our estimation sample includes municipalities with an application to split. We define municipalities containing a district that applied but failed to split as the control group. These *almost split* municipalities serve as a credible counterfactual to those that ultimately split.[15] With initial favorable chances of approval, their requests were denied for reasons arguably unrelated to economic performance, including vetoes by state legislative committees or governors, referenda lacking majority support, and the 1996 CA, which left requests initiated in 1994 and 1995 unresolved due to the insufficient time to conclude the muti-stage process outlined in Section 2.2.[16]

The treatment group consists of municipalities that split. The control group, comprising almost split municipalities, includes never-treated units (i.e., those that applied to split but did not succeed) and excludes not-yet-treated units (i.e., those that applied to split and did so after the waves of 1993 and 1997). This division results in a sample of 448 municipalities, which contains 324 split and 124 almost split municipalities. Figure 1 plots them. Two key patterns arise: First, requests to split are geographically scattered. Second, while there is some clustering due to state-level redistricting regulations, there is significant geographical variation in the distribution of split and almost split events.

We apply similar classifications to our sample of districts: 552 applicant districts (divided into 441 split and 111 almost split units), 325 remaining districts (261 split and 64 almost split units), and 382 headquarters districts (292 split and 90 almost split units). Appendix Table D.2 presents the means of baseline characteristics for districts in 1991, categorized by treatment status and split waves. Compared to their almost split counterparts, districts that split have smaller populations, larger areas, and are located farther from their parent town halls. We also observe some negative selection into splitting over time, as districts involved in the later wave exhibit worse economic conditions.

[15] Figure 1 presents a simple diagram comparing split (blue) and almost split (orange) municipalities. It also illustrates how municipalities are divided into the applicant, remaining, and headquarters districts.

[16] Table 5 of Tomio (2005) provides statistics on the outcomes of split requests in the state of Rio Grande do Sul. Out of 398 requests, 64 percent were ultimately approved; 10 percent remained open; 13 percent were rejected by legislative committees; 5 percent were rejected by the plenary of the legislature; 6 percent were vetoed by the state governor; and 2 percent were rejected in local referendums.

4.3 Main Econometric Specification

To estimate the impacts of splitting on municipal outcomes, we apply the following difference-in-differences specification to the municipality-level sample:

$$y_{mst} = \alpha_m + \alpha_{st} + \sum_{\tau=-\underline{\tau}}^{\overline{\tau}} \beta_\tau Split_m \mathbf{1}[t - W_m = \tau] + \varepsilon_{mst}, \quad (1)$$

in which y_{mt} stands for outcomes for municipality m and state s in time t; α_m represents municipality fixed effects; α_{st} controls for state-by-time fixed effects; $Split_m$ is an indicator variable for whether municipality m split; and $\mathbf{1}[t - W_m = \tau]$ are dummies indicating time relative to the wave-year W_m when municipality m split (either 1993 or 1997).[17] Both the start time $\underline{\tau}$ and end time $\overline{\tau}$ depend on the data availability for the outcome of interest y_{mt}. 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 dynamic effects of splitting relative to that year. Standard errors are two-way clustered at the state and split wave levels.

The impacts of splitting come from comparing treated municipalities to counterfactual municipalities that almost split and, therefore, are never treated. Including almost split municipalities alleviates concerns related to event-study specifications that rely only on variation in the timing of treatment (Goodman-Bacon, 2021; Borusyak et al., 2022). Because our data contain only two waves of splits, it is unlikely that our results will be affected by issues related to the variation in the timing of treatment raised by the recent literature on difference-in-differences (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021). In fact, our robustness checks show similar patterns when we break down our results by splitting waves.

The identification assumptions rely on the timing of splitting being uncorrelated with the outcomes of interest, *conditional* on the set of controls. The key identifying assumption is that outcomes for treated and control municipalities would have followed parallel trends in $\tau \geq 0$ if no splitting had occurred. We show that most pre-event coefficients of interest are statistically indistinguishable from zero.

Even restricting the sample to municipalities that applied to split and attesting parallel

[17] Although Appendix Figure C.1 shows that several splits occurred prior to 1993, the coverage of several data sources, such as night lights and matched employer-employee records, starts during the early 1990s. Our empirical analysis thus is restricted to the 1993 and 1997 waves.

pre-trends, one might still be concerned that split and almost split municipalities differ in various dimensions. We address these issues in several ways. First, including municipality fixed effects α_m mitigates concerns related to time-invariant characteristics of the municipalities that could be correlated with both the splitting event and the outcomes of interest. Second, by adding state-by-time fixed effects α_{st} to Equation (1), we further narrow our comparison to municipalities within the same state. Third, we present year-by-year estimates of outcomes with annual data. Stable pre-trends and sharp effects around the exact time of splitting reassure that we estimate the impacts of splitting rather than the impacts of unobservable municipality-specific factors. Fourth, one of our robustness checks accounts for heterogeneous initial characteristics that can also influence economic performance. We further control for baseline characteristics from Table 1 interacted with time fixed effects, allowing for differential trends across municipalities with different initial characteristics. A remaining concern is that our estimates might be biased upwards due to the rules for federal transfers explained in Section 2. The splitting process induces a mechanical reallocation of federal transfers from the control to the treatment group. Section 7 shows that, if anything, the spillover effects are likely small.

To further rule out unobservable factors influencing our estimates and validate our findings, we leverage an additional feature from the institutional context. Before 1996, districts requesting to split had to conduct local referenda and obtain approval by a simple majority. As a robustness check, we complement our difference-in-differences approach with a difference-in-discontinuities design, exploiting the final results from local referenda. Section 5.4 shows that both approaches generate qualitatively similar estimates.

5 Main Results

We start by examining how new municipalities establish local governments and whether splitting improves public service delivery. We then demonstrate that splitting has positive economic impacts beyond the public sector. Additionally, we find relevant distributional consequences, with applicant districts driving the economic gains.

5.1 Setting up New Local Governments: Impacts on Public Services

Bureaucracy in the Public Sector. We examine how new municipalities establish local governments. Figures 2a and 2b show $\hat{\beta}_\tau$, along with 95 percent confidence intervals, after estimating Equation (1) for selected variables capturing public expenses. Appendix Table D.3 shows the aggregate impacts. The pre-event coefficients are statistically equal to zero, supporting the assumption that both split and almost split municipalities have similar pre-split trends. Following splitting, municipalities that undergo the split experience a sharp increase in public expenses.

Figure 2a displays results for capital expenditures per capita. These expenditures, which account for 16 percent of total municipal expenditures, refer to purchases of machinery, vehicles, buildings, and similar items. We observe a spike of around 40 percent in the year of splitting, followed by a stable increase of approximately 27 percent over the subsequent 15 years. Figure 2b reports the results for current expenditures, which constitute 84 percent of municipal expenditures and cover maintenance and operational costs for providing public services (e.g., payroll and administrative costs). Following the split, current expenditures in treated municipalities increase by about 17 percent, a pattern that becomes stable and persistent over time. Lima and Silveira Neto (2018) argue that capital expenditures tend to be initially higher than current expenditures due to initial setup costs. Rules prohibiting indiscriminate hiring in the public sector also contribute to the stable trends in current expenditures after splitting.

We also utilize the detailed matched employer-employee RAIS data to validate the previous findings and to quantify the impacts on the size of the public sector. Figures 2c and 2d report that splitting is associated with an increase of around 16 percent in the number of municipal public jobs, while the average municipal wages remain unchanged. We observe no changes in public employment at the state and federal levels, confirming that the growth of the public sector is exclusively driven by the new municipalities.

Public Services Delivery. Next, we investigate the extent to which the growth of the public sector influences public service delivery. Although decennial Census data allow for a comprehensive analysis of how splitting affects public service delivery, an important caveat is that we cannot directly assess pre-trends due to having only one pre-split data point, the 1991 Demographic Census. However, the absence of pre-trends in more frequent alternative data sources, such as RAIS data, helps alleviate this concern

Figure 3 and Appendix Table D.4 report coefficients from estimating Equation (1). We find that household access to trash collection and sewage increases by 4.4 and 1 percentage points (with the former being significant at the 10 percent level). No significant impacts are observed for piped water and electricity. Interestingly, the impacts are weaker for public services whose provision mandate is shared with state and federal governments, such as in the water and sanitation sectors. These results are consistent with shared mandates leading to lower investments in these services due to uncertainty about which level of government is ultimately responsible for their provision (Kresch, 2020).[18]

We also employ a complementary empirical approach to quantify other margins of response. Due to the lack of pre-split data on public goods from the early 1990s, such as education and health infrastructure, we propose a test that exploits variation in exposure to splitting across municipalities and birth cohorts. If splitting leads to an increase in the stock of schools or education inputs, and both the year of birth and municipality of residence influence exposure to this increase, then younger individuals who are more exposed to splitting would presumably experience higher levels of schooling compared to older, less exposed individuals (Duflo, 2001).

To investigate the impacts of splitting across different age groups, we use individual-level Census data and estimate a modified version of Equation (1) with an extra dimension of heterogeneity by age. We compare educational outcomes across different age groups in split versus almost-split municipalities, before and after the split, through the following difference-in-differences model:

$$y_{ikmst} = \alpha_{st} + \alpha_{km} + \alpha_{kt} + \sum_{\tau=8}^{30} \beta_\tau Split_{mt} \mathbf{1}[k = \tau] + \mathbf{X}_i \lambda + \varepsilon_{ikmst}, \quad (2)$$

in which y_{imst} represents outcomes for person i with age k in municipality m, state s, and year t; α_{st}, α_{km}, α_{kt} 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 all municipalities in $t = 1991$ and equal to one in years $t \in \{2000, 2010\}$ for municipalities that split; and $\mathbf{1}[k = \tau]$ are dummies for each age. The term $\mathbf{X}_i$ refers to a vector of individual-level controls, such as gender, race, religion, and

[18] Using municipality-level Census data, we find that municipalities that split experience higher literacy rates and years of education. We also observe increases in preschool and middle school attendance, but no similar pattern for high school attendance. We interpret these results as consistent with the division of roles between governments. Municipalities are responsible for providing preschool and primary education, while state governments oversee high schools.

nationality. The post-event coefficients of interest, β_τ, capture the aggregate effects of splitting for each age group, which ranges from 8 to 30 years, in the Census year. Standard errors are two-way clustered at the state and split wave levels. Intuitively, this approach preserves the core variation in comparing educational outcomes between split and almost-split municipalities, before and after the split, as in Section 4.2. The key difference is that the individual-level Census data allow us to decompose the average effect into age-specific effects and to further control for age-municipality and age-time fixed effects.

In line with higher investments in educational infrastructure, raw trends from Appendix Figure D.1 corroborate that younger age groups from municipalities that split experience greater gains in school attendance and literacy rates between 1991 and 2010. Figure 4a shows that splitting is associated with increases in school attendance ranging between 2 and 5 percentage points, while Figure 4b indicates increases of up to 4 percentage points in literacy rates for people under 15 years of age. Using RAIS data, Appendix Figure D.2 further reveals a crowd-out effect of employment from non-profit to government organizations in the educational sector, pointing that higher levels of education result from increased public investments following the split.

5.2 Beyond the Public Sector: Impacts on Economic Activity

Private Sector. We now turn to the economic impacts *beyond* the public sector. Using the near-universe of the private sector from RAIS, we estimate Equation (1), which compares the number of private establishments and jobs in the formal sector in treated and control municipalities, before and after the splitting shocks. Figures 5a and 5b and Appendix Table D.5 illustrate the dynamic and aggregate impacts around splitting. While the point estimates are positive, we cannot reject the null hypothesis of zero effect, suggesting that the private sector does not expand to the same extent as the public sector. The aggregate results, however, mask substantial heterogeneity across economic sectors. Appendix Figure D.3 indicates some degree of structural transformation towards services, as the majority of new private establishments come from the retail sector.

Economic Activity. Thus far, the empirical results indicate positive and persistent economic impacts of splitting. To quantify the impacts on economic activity, which captures the public, private, and informal sectors, we estimate Equation (1) with spatial data from nighttime lights (Chen and Nordhaus, 2011; Henderson et al., 2012; Pinkovskiy and Sala-i Martin, 2016; Henderson et al., 2018). The lack of improvements in household access to electricity from Figure 2 suggests that nighttime lights are unlikely to be driven by street lighting. Figure 5c shows that nighttime luminosity increases rapidly in the first five years after splitting. Over time, this growth stabilizes and persists at 8 percent.

5.3 The Distributional Impacts

The aggregate results at the municipality level, albeit informative, are limited in illustrating the distributional implications of splitting *within* municipalities. For instance, the gains from splitting might be uniformly distributed across districts. Or they could be asymmetric, with applicant districts benefiting significantly more while other districts remain relatively unaffected. Understanding the distribution of economic activity is crucial for identifying the winners and, if any, losers of the policy.

Leveraging the granular structure of the nighttime luminosity data, we estimate the following difference-in-differences specification at the district level:

$$y_{dmst} = \alpha_d + \alpha_{st} + \sum_{\tau=-\underline{\tau}}^{\overline{\tau}} \beta_\tau Split_m \mathbf{1}[t - W_d = \tau] + \varepsilon_{dmst}, \tag{3}$$

in which subscripts d, m, s, and t stand for district, municipality, state, and time; and α_d represents district fixed effects. The remaining variables are similar to Equation (1) except that the subscripts represent districts rather than municipalities. As before, we normalize $\beta_{-1} = 0$ and the standard errors are two-way clustered at the state and split wave levels.

Figure 6a plots the dynamics of nighttime luminosity around splitting for applicant, headquarters, and remaining districts separately. Appendix Table D.6 reports the aggregate estimates.[19] We identify three main patterns. First, the pre-event coefficients are statistically close to zero, supporting the validity of the research design. Second, applicant districts experience a sharp increase in luminosity after splitting. The growth peaks at about 40 log points between 5 and 8 years later and stabilizes with a 34 log point (or 40 percent) increase 15 years after splitting. Third, the estimates for districts that did not request to split are much smaller. We find a negative and statistically insignificant change in luminosity for the remaining districts. Headquarters districts experience positive, albeit

[19]We add 0.1 to the average luminosity to ensure its log is defined for all districts.

much smaller, impacts of 6 percent.

Exploiting the extensive margin of luminosity, Figure 6b shows a 4 log points increase in pixels lit for applicant districts. Panel C of Appendix Table D.6 indicates that our estimates remain similar when measuring luminosity outside a 5 km radius around the town hall. This suggests that the growth in luminosity is widespread and not confined to the main urban area within the new municipality (Bluhm et al., 2023).

With the caveats that the Census tract-level data contain a much more limited set of outcomes and that we cannot test for pre-trends because the 1991 Census is the only pre-split data, we extend our district-level analysis to the Census records. Panel A of Appendix Table D.7 confirms that applicant districts primarily drive the gains in public services, while Panels B and C suggest that public services in the remaining and headquarters districts are relatively less affected. Our results together indicate that the gains are asymmetric, with districts that applied to split benefiting more from splitting.

5.4 Robustness Checks

We conduct some additional checks to ensure that our findings are robust to alternative definitions of outcomes, samples, and specifications. Appendix Table D.8 reports the robustness checks. For brevity, we limit our attention to district-level luminosity results from Equation (3) for applicant districts. Column (1) of Panel A replicates our benchmark result. In Column (2), we do not add 0.1 to the average luminosity so that its log is not defined for all districts. As an alternative approach to handle zeroes in the data, Column (3) applies inverse hyperbolic sine transformation to the average luminosity. Column (4) presents coefficients only for the 1997 wave to test whether the results are different across waves of splits. Because the process to split is usually lengthy, sometimes taking years, the timing of the 1996 CA is likely to be exogenous to our outcomes of interest for the 1997 wave, whose sample mostly consists of requests initiated between 1994 and 1996. We note that the point estimate is remarkably similar to Column (1). In addition, Appendix Figure D.4 examines the annual impacts of the 1997 wave for selected outcomes, confirming that the estimates for the longer pre-event period are statistically indistinguishable from zero. Column (5) controls for trends specific to local economies by adding micro region-by-year fixed effects.[20] Column (6) alternatively controls for baseline characteristics from Table 1

[20]Micro regions delineate local economies with similar socioeconomic and historical characteristics and are equivalent to commuting zones in the US.

interacted with year fixed effects, allowing for differential trends across municipalities with different initial characteristics. Panel B further shows that our results are robust to different choices of clustering the standard errors and to using the wild bootstrap-based test to account for the small number of clusters.

Two additional issues could threaten our main identification strategy. First, there may be concerns that the splitting treatment is correlated with other concurrent shocks unrelated to the creation of new municipalities, which could confound the estimated effects. For example, splitting might trigger new programs from state or federal governments, thereby affecting the outcomes of interest. We are not aware of such shocks occurring in Brazil. We also note that it is unlikely that the timing of any differential shocks affecting split and almost split municipalities coincides precisely with the timing of the splitting.

The second concern is that selection into splitting based on unobservable factors, such as economic growth potential, better organizational capacity, or connections with the state legislative, could bias our estimates. We propose a complementary research design to mitigate this concern. Before 1996, districts applying to split had to conduct local referenda and obtain approval by a simple majority. We leverage this rule in a difference-in-discontinuities design applied to the large and representative state of *Minas Gerais*, where referendum results are available. This approach compares districts that narrowly obtained the majority of necessary votes to split with those that did not.[21] Appendix E details the research design and confirms that both the difference-in-differences and difference-in-discontinuities strategies lead to qualitatively similar conclusions, thereby strengthening the validity of our main research design.

6 Drivers of Local Development

Our results indicate that splitting boosts local development by fueling the public sector and economic activity. We also find that these gains are driven by the successful applicant districts. Using observational data and key predictions from a simple model of public goods provision under splitting, outlined in Appendix Section A, this section assesses the extent to which these results are attributable to increased fiscal revenues or to the decentralization of decision-making power to new local governments.

[21] With an area larger than France, *Minas Gerais* is the second most populous and third richest state in Brazil. Its ethnic composition and geography are similar to the rest of the country.

6.1 The Role of Fiscal Revenues

We scrutinize the sources of financing for new local governments by investigating the impacts of splitting on fiscal performance, such as federal transfers and tax revenues. Using data on fiscal revenues, we estimate Equation (1), which directly compares split and almost split municipalities, before and after splitting. Figure 5d shows the dynamics of municipal revenues. Immediately after splitting, there is a sharp increase in revenues, a pattern that stabilizes over time.

Appendix Table D.5 (Columns (5)–(7)) presents the aggregate impacts. Column (5) shows a 15 percent increase in local revenues. In line with the institutional context, Column (6) reveals that this increase is driven by a rise in transfers from the federal government due to the funding allocation mechanism. In addition, Column (7) indicates an 12 percent increase in local tax revenues, although we cannot reject the null hypothesis of zero effect. Along with the absence of increased population inflow after splitting, this finding suggests a limited role for local taxation in enhancing fiscal capacity in weak states, underscoring the importance of non-taxes revenues in strengthening state capacity for peripheral regions.

The fact that splitting increases federal transfers to new municipalities implies an unintentionally large subsidy to fund their operations. A relevant question is to what extent our findings on public services and economic activity are driven by increased fiscal revenues. We propose two tests. First, we implement a "horse-race" approach, in which we add total revenues to the set of controls in Equation (1).[22] By holding revenues fixed when comparing split and almost split municipalities, we test whether the coefficient associated with splitting approaches zero if the increased transfers explain the gains in economic outcomes. Odd columns of Table 2 repeat selected baseline results. Even columns report the coefficients after controlling for total revenues. The changes in the point estimates indicate that increased revenues explain part, but not all, of our findings.

The second approach addresses the lack of information on fiscal revenues at the district level by assuming that, before splitting, municipal revenues are proportionally shared among districts based on population. Comparing Columns (1) and (2) of Table 3, the inclusion of predicted revenues, rather than actual revenues, at the district level in the set

[22]We control for revenues, rather than federal transfers, because there is little variation in transfers among almost split municipalities, as shown in Appendix Figure D.6d, and the correlation between revenues and transfers is strong ($\rho = 0.44$). The correlation between revenues and expenses ($\rho = 0.99$) is also strong.

of controls little alters the effects on luminosity: the point estimate decreases from 0.35 to 0.32. These results suggest that the increased revenues only explain part of the gains in public services and economic activity. Because the applicant districts gain administrative, fiscal, and political autonomy once they secede and become municipalities, we next discuss the role of decentralizing decision-making power in also explaining our results.

6.2 The Role of Decentralization of Decision-Making Power

In line with the new municipalities obtaining *de jure* decision-making power, Section 5 highlights that the *de facto* gains in public services are concentrated in activities for which local governments are expected to provide oversight, such as trash collection and primary education. We also find no evidence that these gains from splitting extend to activities also under the influence of federal and state governments, such as electricity, sanitation, and high school education. Although the main contribution of this paper is the reduced-form estimates of splitting on local development, we also test several theories of decentralization positing its implications for economic development. We note that this exercise only provides a suggestive glimpse into these theories due to data constraints.

The Mechanism of Curtailing Capture and Neglect. A key source of spatial inequality is that the decision-making process regarding the allocation of resources and of burdens often reflects the preferences of a few elite groups and the lack of policy priorities from local authorities, ultimately promoting capture and neglect. This is supported by a survey of Brazilian mayors in 1992, which confirms that neglect by parent local governments and geographical distance to the headquarters are the most common motivations for splitting (Bremaeker, 1993).[23] One of the premises of decentralization is to reduce the influence of capture and neglect in peripheral regions, advancing policies better aligned with local needs (Oates, 1972; Bardhan, 2002; Mookherjee, 2015).

One challenge in investigating whether capture and neglect occur is that these phenomena are difficult to measure. We propose a test that examines whether the gains in economic activity are stronger in districts with a higher propensity for capture and neglect

[23] Other works show that administrative remoteness in the form of geographical distance to the headquarters limits local development (Krishna and Schober, 2014; Asher et al., 2018). For instance, it may reduce the amount and quality of information about local needs available to the headquarters, leading to fewer public investments (Oates, 1999). High transportation costs and information frictions may also restrict the flow of services, as bureaucrats may travel less to remote areas and be less aware of citizens' preferences.

before splitting. These issues tend to be greater in areas that "are remote from centers of power; have low literacy; are poor; or have significant caste, race, or gender disparities" (Mansuri and Rao (2012), p.5). We examine heterogeneity in luminosity across these dimensions. In line with decentralization benefiting vulnerable areas, Columns (3) and (4) of Table 3 reveal that the gains in luminosity accrue to peripheral districts that were previously farther from their parent town halls and had lower urbanization rates in the baseline period. These findings suggest that this policy can serve the dual purpose of expanding the public sector in peripheral regions and freeing these regions from the capture and neglect of former governments.

The Mechanism of Politics. One argument against decentralization is the lack of policy coordination across jurisdictions, which can be detrimental when externalities are not internalized (Lipscomb and Mobarak, 2017). However, decentralization can also be beneficial in terms of increased political accountability, as local governments have incentives to tailor policies to local needs, increasing social welfare and influencing electoral outcomes (Seabright, 1996). Since elections are imperfect instruments of political accountability, we can assess the role of politics in explaining our results (Bordignon and Minelli, 2001).

Leveraging information from the electoral data available for early the 1990s, we scrutinize the electoral results across applicants and headquarters districts. Appendix Figure D.5 shows that, after splitting, applicants and headquarters districts often elect mayors from *different* parties. Immediately after splitting, this divergence is observed in about 75 percent of cases, increasing to nearly 85 percent two decades later. This finding speaks to the literature on the politics of decentralization (Grossman et al., 2017; Pierskalla, 2016), particularly to basic models of representative politics, where elected officials reflect local preferences for policies (Persson and Tabellini, 2002).[24]

We also examine whether splits are driven by the political alignment of local mayors with the state governor or by the mayor's political ideology, specifically left-wing affiliations. Leveraging limited data on state elections from 1986 and 1990 and on local elections from 1988 and 1992, we assess these factors. Appendix Table D.9 shows that political alignment between mayors and governors does not predict the likelihood of

[24]Unlike our context, in which districts can unilaterally request to split, Hassan (2016) and Gottlieb et al. (2019) model splitting as an endogenous distributive policy chosen by the incumbent politician. Because local elections are single-district, incumbent politicians may benefit from splits only to the extent that voters within the applicant district are in the opposition. We are unable to directly test these theories due to the lack of historical electoral data with information on vote shares and the level of electoral competition.

split requests or the success of these requests. While there is some evidence that left-wing mayors are more likely to request splits, we do not observe a similar pattern for successful splits. Therefore, the occurrence of splits does not appear to be influenced by state-level political exchanges or ideological biases.

The Lack of Migratory Responses. We test whether people "vote with their feet" by examining migratory responses to public goods provision (Tiebout, 1956). We investigate whether municipalities that split and experience improvements in public service delivery attract more individuals from elsewhere.[25] Appendix Table D.10 indicates no clear evidence of migration as a relevant margin of response to splitting in our context.

7 Discussion

7.1 The Spillover Effects

Our results indicate that subsidized voluntary splits lead to positive economic impacts for new municipalities, particularly for the applicant districts. An important distributional question is whether the municipalities that did not split experience negative impacts due to reductions in fiscal revenues from the federal transfer allocation mechanism.

To account for spillovers to the rest of the country, our first exercise exploits variations in the number of municipal splits within states. Appendix Figure D.6e shows that states with more splits experience larger losses in federal transfers ($\rho = -0.67$). Municipalities containing a split increased their share of federal transfers by 20.3 percent on average, while those not containing a split decreased their share by 13.7 percent on average. This motivates the correlation test between changes in federal transfers and selected outcomes.[26] Figure 7 shows a binned scatter plot of this correlation, with the point estimates

[25] An important caveat is that the 2000 Census is the first to collect information on migration across municipalities. We make a cross-sectional comparison between split and almost split municipalities around the 1997 splitting wave by running the following regression:

$$y_{ms} = \alpha_s + \beta \times Split_m + \varepsilon_{ms}, \tag{4}$$

in which y_{mt} stands for the fraction of residents in municipality m and state s in 2000 that declare having lived in another municipality in 1995; and α_s captures state fixed effects.

[26] Appendix Figure D.6 shows that, on average, a one-percentage-point increase in the population residing in new municipalities implies that non-split municipalities experience a 2.1 percentage points decrease

reported in the top left of each figure. For a one-percentage-point decrease in federal transfers, we can rule out negative effects greater than 15.5 percent for public jobs, 22.75 percent for private jobs, 10.42 percent for the number of establishments, and 2.67 percent for average luminosity at a 95 percent confidence level. Although the small sample size of 25 states prevents more conclusive statements, the point estimates and relatively flat relationships indicate that we cannot reject the null hypothesis of zero spillovers.

What could explain the lack of spillover effects in municipalities that did not split? This would be consistent with a model with decreasing returns to spending, in which local governments may engage in wasteful expenses with marginal value below the social cost of funds (Liebman and Mahoney, 2017). Even when the splitting process reduces the funds available to municipalities that did not split, they may reduce low-value spending and manage the remaining funds more efficiently. Because the social cost of these lost funds exceeds their social value, spending cuts may not be substantial enough to negatively impact economic outcomes.

7.2 Implications for the Local Economy

We also evaluate the impacts of splitting on the local economy by analyzing the associated cost per job and the local multiplier. As the effects of splitting combine components of FPM transfers and decentralization of decision-making power, we discuss how the magnitudes of these effects compare with recent estimates of FPM transfer multipliers in Brazil to indirectly assess the role of each component.

Cost per job. We estimate that splitting is associated with increases of about 15.8 percent in municipal public jobs. Considering the baseline mean of 604 jobs in the year before splitting, this translates to 95.65 additional municipal public jobs. At the same time, splitting leads to an increase of 36.75 percent in FPM transfers.[27] We calculate that each public job costs around US$ 3,635 in FPM transfers per year, which is significantly lower than the cost per job of US$ 8,000 from FPM transfers (Corbi et al., 2019). This estimate is also lower than the cost per job of US$9,799 for cash transfers in Brazil (Gerard et al., 2024). Our findings suggest that the returns from splitting in terms of cost per job are at least

in federal transfers.

[27] To make our estimates comparable to Corbi et al. (2019), we consider monetary values in Brazilian Reais (BRL) at constant 1998 prices, equivalent to US$1 in 2016 prices. Therefore, the increase in FPM transfers corresponds to an additional US$ 347,738.

twice as high as those from FPM transfers alone.

Local Multiplier. We also examine our findings through the lens of the local multiplier. One limitation is the lack of municipal GDP records prior to 2002, which requires strong assumptions to calculate municipal GDP during the 1990s. As an alternative, we use night light data to estimate the effect of splitting. We next compute the elasticity between nighttime light intensity and GDP in our sample to derive the implied effects of splitting on GDP. We find that splitting is associated with an average additional GDP of US$ 717,163.60 per municipality in a given year. This implies that splitting generates an output multiplier of 2.06 per year, obtained by dividing the additional GDP by the additional FPM transfer of US$ 347,738. An alternative methodology, relating output and employment multipliers as proposed by Chodorow-Reich (2019) and using the same calibrated parameters from Corbi et al. (2019), yields a local multiplier of 4.34 per year.[28]

To interpret the magnitude of our splitting multipliers, we compare them with previous studies. Using the methodology from Chodorow-Reich (2019), Corbi et al. (2019) show that FPM transfers generate a local multiplier ranging from 1.1 to 2.6, which is notably in line with our splitting multiplier. Chodorow-Reich (2019) documents that the median output multiplier from the literature on fiscal spending, drawn mainly from richer countries, is 1.9.[29] We interpret our estimates for splitting as consistent with the increase in FPM transfers being a major driver of the boost in local economic activity, though the decentralization of decision-making power also plays a role in these positive outcomes.

8 Conclusion

This paper provides comprehensive evidence of the short- and long-run impacts of one of the largest voluntary redistricting episodes worldwide. Exploiting sharp variations in

[28]We follow Chodorow-Reich (2019) and define the output multiplier μ^Y as:

$$\mu^Y = (1-\xi)(1+\chi)\frac{Y}{E}\mu^E, \tag{5}$$

such that $(1-\xi)$ is the share of labor in the production function, $(1-\chi)$ is the elasticity of hours per worker to total employment, $\frac{Y}{E}$ is the output and income multiplier, and μ^E is the employment spending multiplier (inverse of cost per job). We set $(1-\xi) = 0.666$, $\chi = 0.12$, and $\frac{Y}{E} = 21,152$ from Corbi et al. (2019).

[29]More broadly, our findings complement evidence from other policies beyond fiscal spending. In Brazil, Gerard et al. (2024) find that cash transfers generate an output multiplier of 1.49. Colonnelli and Prem (2022) find local multipliers ranging from 1.46 to 4.60 from anti-corruption programs that limit wasteful spending.

the number of municipalities in Brazil, we find that splitting through subsidized splitting generates positive impacts on the size of bureaucracy, public services delivery, and economic activity for new municipalities, without worsening economic outcomes for the rest of the country. The impacts are driven by applicant districts who voluntarily secede into new municipalities and are largest for peripheral and remote backward districts neglected by their former headquarters. Our findings indicate that increases in fiscal revenues and the decentralization of decision-making power enable peripheral regions to develop.

One limitation of this paper is that we cannot estimate other costs of splitting due to the lack of additional data. For instance, the literature has documented that revenue windfalls undermine government monitoring, exacerbate political corruption, and deteriorate the quality of politicians (Brollo et al., 2013; Boffa et al., 2016). Understanding whether this happens in the context of splitting would shed light on its potential pitfalls. Quantifying the economic costs of splitting would also help us understand its equity-efficiency trade-off. Lastly, fleshing out how governments are formed in new municipalities, what specific promises and investments they make, and how splitting affects political yardstick competition and representation is a next step worthy of its own paper. We view these examples as promising directions for future research.

References

Acemoglu, D., C. García-Jimeno, and J. A. Robinson (2015). State capacity and Economic Development: A Network Approach. *American Economic Review 105*(8), 2364–2409.

Alesina, A. and E. Spolaore (1997). On the Number and Size of Nations. *The Quarterly Journal of Economics 112*(4), 1027–1056.

Alesina, A. and E. Spolaore (2005). *The Size of Nations*. MIT Press.

Arretche, M. T. (2000). *Estado Federativo e Políticas Sociais: Determinantes da Descentralização*. Editora Revan São Paulo.

Asher, S., K. Nagpal, and P. Novosad (2018). The Cost of Distance: Geography and Governance in Rural India. *World Bank Working Paper*.

Ashraf, Q., Ö. Özak, and O. Galor (2010). Isolation and Development. *Journal of the European Economic Association 8*(2-3), 401–412.

Bai, Y. and R. Jia (2023). The Economic Consequences of Political Hierarchy: Evidence from Regime Changes in China, 1000–2000 CE. *The Review of Economics and Statistics 105*(3), 626–645.

Banco Central do Brasil (2025). Broad National Consumer Price Index (IPCA). Available at `https://www3.bcb.gov.br/sgspub/consultarvalores/consultarValoresSeries.do?method=consultarSeries&series=433`. Accessed on April 29, 2025.

Barankay, I. and B. Lockwood (2007). Decentralization and the Productive Efficiency of Government: Evidence from Swiss Cantons. *Journal of Public Economics 91*(5-6), 1197–1218.

Bardhan, P. (2002). Decentralization of Governance and Development. *Journal of Economic Perspectives 16*(4), 185–205.

Bazzi, S. and M. Gudgeon (2021). The Political Boundaries of Ethnic Divisions. *American Economic Journal: Applied Economics 13*(1), 235–66.

Besley, T. and A. Case (1995). Does Electoral Accountability Affect Economic Policy Choices? Evidence from Gubernatorial Term Limits. *The Quarterly Journal of Economics 110*(3), 769–798.

Besley, T. and T. Persson (2009). The Origins of State Capacity: Property Rights, Taxation, and Politics. *American Economic Review 99*(4), 1218–1244.

Besley, T. and T. Persson (2010). State Capacity, Conflict, and Development. *Econometrica 78*(1), 1–34.

Bianchi, N., M. Giorcelli, and E. M. Martino (2023). The Effects of Fiscal Decentralisation on Publicly Provided Services and Labour Markets. *The Economic Journal*.

Bluhm, R., C. Lessmann, and P. Schaudt (2023). The Political Geography of Cities: Capital cities, economic fundamentals, and urban growth.

Boffa, F., A. Piolatto, and G. A. Ponzetto (2016). Political Centralization and Government Accountability. *The Quarterly Journal of Economics 131*(1), 381–422.

Bolton, P. and G. Roland (1997). The Breakup of Nations: A Political Economy Analysis. *The Quarterly Journal of Economics 112*(4), 1057–1090.

Bordignon, M. and E. Minelli (2001). Rules Transparency and Political Accountability. *Journal of Public Economics 80*(1), 73–98.

Borusyak, K., X. Jaravel, and J. Spiess (2022). Revisiting Event Study Designs: Robust and Efficient Estimation. *Available at SSRN 2826228*.

Bremaeker, F. E. (1993). Os Novos Municípios: Surgimento, Problemas e Soluções. *Revista de Administração Municipal 40*(206), 88–99.

Brollo, F., T. Nannicini, R. Perotti, and G. Tabellini (2013). The Political Resource Curse. *American Economic Review 103*(5), 1759–1796.

Busso, M., J. Gregory, and P. Kline (2013). Assessing the Incidence and Efficiency of a Prominent Place Based Policy. *American Economic Review 103*(2), 897–947.

Cachatori, T. L. and A. A. Cigolini (2013). Emancipações Municipais no Brasil: Prognóstico sobre a Continuidade da Compartimentação do Espaço em Novos Municípios. *Revista Geonorte 4*(12), 730–747.

Callaway, B. and P. H. Sant'Anna (2021). Difference-in-Differences with Multiple Time Periods. *Journal of Econometrics 225*(2), 200–230.

Campante, F. R., Q.-A. Do, and B. Guimaraes (2019). Capital Cities, Conflict, and Misgovernance. *American Economic Journal: Applied Economics 11*(3), 298–337.

Carter, D. (2018). Terrain ruggedness and land cover: Improved data for most research designs. Available at https://doi.org/10.7910/DVN/WXUZBN, Harvard Dataverse, V1.

Caselli, F. and G. Michaels (2013). Do Oil Windfalls Improve Living Standards? Evidence from Brazil. *American Economic Journal: Applied Economics 5*(1), 208–238.

Cassidy, T. and T. Velayudhan (2022). Government Fragmentation and Economic Growth. *Available at SSRN 4038546*.

Chambru, C., E. Henry, and B. Marx (2024). The Dynamic Consequences of State Building: Evidence from the French Revolution. *American Economic Review 114*(11), 3578–3622.

Chen, X. and W. D. Nordhaus (2011). Using Luminosity Data as a Proxy for Economic Statistics. *Proceedings of the National Academy of Sciences 108*(21), 8589–8594.

Chodorow-Reich, G. (2019). Geographic Cross-Sectional Fiscal Spending Multipliers: What Have We Learned? *American Economic Journal: Economic Policy 11*(2), 1–34.

Coate, S. and B. Knight (2007). Socially Optimal Districting: A Theoretical and Empirical Exploration. *The Quarterly Journal of Economics 122*(4), 1409–1471.

Cohen, I. (2022). The Costs of Splitting: Administrative Unit Proliferation and Economic Growth in Uganda. *Available at SSRN 4120208*.

Colonnelli, E. and M. Prem (2022). Corruption and Firms. *The Review of Economic Studies 89*(2), 695–732.

Corbi, R., E. Papaioannou, and P. Surico (2019). Regional Transfer Multipliers. *The Review of Economic Studies 86*(5), 1901–1934.

Dahis, R. (2024). Cleaning the Relação Anual de Informações Sociais (RAIS) dataset, 1985-2020. Github repository available at https://github.com/rdahis/clean_RAIS.

Dahis, R. and C. Szerman (2025). Data and Code for: "Decentralizing Development: The Economic Impacts of Government Splits"'. Available at https://www.openicpsr.org/openicpsr/workspace?goToPath=/openicpsr/209961.

Data Basis (2022a). Brazilian Directories. Available at https://data-basis.org/dataset/33b49786-fb5f-496f-bb7c-9811c985af8e. Accessed on April 29, 2025.

Data Basis (2022b). Brazilian Elections. Available at https://data-basis.org/dataset/eef764df-bde8-4905-b115-6fc23b6ba9d6. Accessed on April 29, 2025.

Data Basis (2022c). Brazilian GDP. Available at https://data-basis.org/dataset/fcf025ca-8b19-4131-8e2d-5ddb12492347?table=fbbbe77e-d234-4113-8af5-98724a956943. Accessed on April 29, 2025.

Data Basis (2022d). Brazilian Population. Available at https://data-basis.org/dataset/d30222ad-7a5c-4778-a1ec-f0785371d1ca?table=0c279444-165b-41da-92cd-50fd7e66baa1. Accessed on April 29, 2025.

Data Basis (2022e). Sistema de Informações Contábeis e Fiscais do Setor Público Brasileiro (SICONFI). Available at https://data-basis.org/dataset/5a3dec52-8740-460e-b31d-0e0347979da0. Accessed on April 29, 2025.

de Chaisemartin, C. and X. D'Haultfœuille (2020). Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review 110*(9), 2964–96.

Duflo, E. (2001). Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment. *American Economic Review 91*(4), 795–813.

Dur, R. and K. Staal (2008). Local Public Good Provision, Municipal Consolidation, and National Transfers. *Regional Science and Urban Economics 38*(2), 160–173.

Ehrl, P. (2017a). Minimum Comparable Areas for the Period 1872-2010: An Aggregation of Brazilian Municipalities. *Estudos Econômicos 47*, 215–229.

Ehrl, P. (2017b). Minimum Comparable Areas for the Period 1872-2010: An Aggregation of Brazilian Municipalities. Code available at https://sites.google.com/site/philippehrl/research.

Faguet, J.-P. (2004). Does Decentralization Increase Government Responsiveness to Local Needs? Evidence from Bolivia. *Journal of Public Economics 88*(3-4), 867–893.

Faguet, J.-P. (2014). Decentralization and Governance. *World Development 53*, 2–13.

FAO-GAEZ (2021). Global Agro-Ecological Zoning version 4. Available at https://openknowledge.fao.org/handle/20.500.14283/cb4620en. Accessed on April 29, 2025.

Favero, E. (2004). *Desmembramento Territorial: O Processo de Criação de Municípios — Avaliação a Partir de Indicadores Econômicos e Sociais.* Ph. D. thesis, Universidade de São Paulo.

Ferraresi, M., G. Migali, and L. Rizzo (2018). Does Intermunicipal Cooperation Promote Efficiency Gains? Evidence from Italian Municipal Unions. *Journal of Regional Science 58*(5), 1017–1044.

Gadenne, L. (2017). Tax me, but Spend Wisely? Sources of Public Finance and Government Accountability. *American Economic Journal: Applied Economics*, 274–314.

Gadenne, L. and M. Singhal (2014). Decentralization in Developing Economies. *Annual Review of Economics 6*(1), 581–604.

Gendźwiłł, A., A. Kurniewicz, and P. Swianiewicz (2020). The Impact of Municipal Territorial Reforms on the Economic Performance of Local Governments: A Systematic Review of Quasi-Experimental Studies. *Space and Polity*, 1–20.

Gennaioli, N. and H.-J. Voth (2015). State Capacity and Military Conflict. *The Review of Economic Studies 82*(4), 1409–1448.

Gerard, F., J. Naritomi, and J. Silva (2024). Cash Transfers and the Local Economy: Evidence from Brazil.

Goodman-Bacon, A. (2021). Difference-in-Differences with Variation in Treatment Timing. *Journal of Econometrics 225*(2), 254–277.

Gottlieb, J., G. Grossman, H. Larreguy, and B. Marx (2019). A Signaling Theory of Distributive Policy Choice: Evidence from Senegal. *The Journal of Politics 81*(2), 631–647.

Grossman, G. and J. I. Lewis (2014). Administrative Unit Proliferation. *American Political Science Review 108*(1), 196–217.

Grossman, G., J. H. Pierskalla, and E. Boswell Dean (2017). Government Fragmentation and Public Goods Provision. *The Journal of Politics 79*(3), 823–840.

Hassan, M. (2016). A State of Change: District Creation in Kenya After the Beginning of Multi-Party Elections. *Political Research Quarterly 69*(3), 510–521.

Henderson, J. V., T. Squires, A. Storeygard, and D. Weil (2018). The Global Distribution of Economic Activity: Nature, History, and the Role of Trade. *The Quarterly Journal of Economics 133*(1), 357–406.

Henderson, J. V., A. Storeygard, and D. N. Weil (2012). Measuring Economic Growth from Outer Space. *American Economic Review 102*(2), 994–1028.

IBGE (2010a). Brazilian Demographic Census Microdata for 1991, 2000, and 2010. Available for purchase at https://loja.ibge.gov.br/catalogsearch/result/?q=censo+1991, https://loja.ibge.gov.br/censo-demografico-2000-microdados-da-amostra.html, and https://loja.ibge.gov.br/censo-demografico-2010-microdados-da-amostra-2623.html.

IBGE (2010b). Map of Brazilian Municipalities and Districts in 2010. Available at https://www.ibge.gov.br/geociencias/downloads-geociencias.html.

IBGE (2016). Genealogy of Municipalities.

IBGE (2025a). Broad National Consumer Price Index (IPCA). Available at https://www.ibge.gov.br/estatisticas/economicas/precos-e-custos/9256-indice-nacional-de-precos-ao-consumidor-amplo.html.

IBGE (2025b). Structure for the Código Brasileiro de Ocupações (CBO) 2002. Available at https://concla.ibge.gov.br.

Ipeadata (2025a). GDP of Brazilian States. Available at http://www.ipeadata.gov.br/Default.aspx.

Ipeadata (2025b). Monthly Federal Minimum Wage. Available at http://www.ipeadata.gov.br/Default.aspx.

Ipeadata (2025c). Physical Area of Brazilian Municipalities in 2010. Available at http://www.ipeadata.gov.br/Default.aspx.

Johnson, N. D. and M. Koyama (2017). States and Economic Growth: Capacity and Constraints. *Explorations in Economic History 64*, 1–20.

Klering, L. R., A. J. Kruel, and E. Stranz (2012). Os Pequenos Municípios do Brasil — Uma Análise a Partir de Índices de Gestão. *Análise–Revista de Administração da PUCRS 23*(1), 31–44.

Kline, P. and E. Moretti (2014). Local Economic Development, Agglomeration Economies, and the Big Push: 100 years of Evidence from the Tennessee Valley Authority. *The Quarterly Journal of Economics 129*(1), 275–331.

Kresch, E. P. (2020). The Buck Stops Where? Federalism, Uncertainty, and Investment in the Brazilian Water and Sanitation Sector. *American Economic Journal: Economic Policy 12*(3), 374–401.

Krishna, A. and G. Schober (2014). The Gradient of Governance: Distance and Disengagement in Indian Villages. *Journal of Development Studies 50*(6), 820–838.

Lassen, D. D. and S. Serritzlew (2011). Jurisdiction Size and Local Democracy: Evidence on Internal Political Efficacy from Large-Scale Municipal Reform. *American Political Science Review 105*(2), 238–258.

Li, X., Y. Zhou, M. Zhao, and X. Zhao (2020). A harmonized global nighttime light dataset 1992–2018. *Scientific Data 7*(1), 1–9.

Liebman, J. B. and N. Mahoney (2017). Do Expiring Budgets Lead to Wasteful Year-End Spending? Evidence from Federal Procurement. *American Economic Review 107*(11), 3510–3549.

Lima, R. C. d. A. and R. d. M. Silveira Neto (2018). Secession of Municipalities and Economies of Scale: Evidence from Brazil. *Journal of Regional Science 58*(1), 159–180.

Lipscomb, M. and A. M. Mobarak (2017). Decentralization and Pollution Spillovers: Evidence from the Re-Drawing of County Borders in Brazil. *The Review of Economic Studies 84*(1), 464–502.

Litschig, S. (2012). Are Rules-Based Government Programs Shielded from Special-Interest Politics? Evidence from Revenue-Sharing Transfers in Brazil. *Journal of Public Economics 96*(11-12), 1047–1060.

Litschig, S. and K. M. Morrison (2013). The Impact of Intergovernmental Transfers on Education Outcomes and Poverty Reduction. *American Economic Journal: Applied Economics 5*(4), 206–240.

Mansuri, G. and V. Rao (2012). Localizing Development: Does Participation Work?

Martínez-Vázquez, J., S. Lago-Peñas, and A. Sacchi (2017). The Impact of Fiscal Decentralization: A Survey. *Journal of Economic Surveys 31*(4), 1095–1129.

Mattos, E. and V. Ponczek (2013). Efeitos da Divisão Municipal na Oferta de Bens Públicos e Indicadores Sociais. *Revista Brasileira de Economia 67*, 315–336.

Michalopoulos, S. and E. Papaioannou (2014). National Institutions and Subnational Development in Africa. *The Quarterly Journal of Economics 129*(1), 151–213.

Mookherjee, D. (2015). Political Decentralization. *Annual Review of Economics 7*(1), 231–249.

Muendler, M. (2025). Conversion Tables Between CBO 1994 and ISCO 1988 Codes. Available at https://econweb.ucsd.edu/muendler/html/brazil.html#brazdata.

Narasimhan, V. and J. Weaver (2024). Polity Size and Local Government Performance: Evidence from India. Technical report.

Nunn, N. and D. Puga (2012). Ruggedness: The Blessing of Bad Geography in Africa. *Review of Economics and Statistics 94*(1), 20–36.

Oates, W. (1972). Fiscal Federalism. *New York: Harcourt, Brace, Jovanovich*.

Oates, W. E. (1999, September). An Essay on Fiscal Federalism. *Journal of Economic Literature 37*(3), 1120–1149.

Persson, T. and G. Tabellini (2002). *Political Economics: Explaining Economic Policy*. MIT press.

Pierskalla, J. H. (2016). Splitting the Difference? The Politics of District Creation in Indonesia. *Comparative Politics 48*(2), 249–268.

Pinkovskiy, M. and X. Sala-i Martin (2016). Lights, Camera… Income! Illuminating the National Accounts-Household Surveys Debate. *The Quarterly Journal of Economics 131*(2), 579–631.

PNUD (2013). Índice de Desenvolvimento Humano Municipal Brasileiro – 2013. Available at https://www.undp.org/pt/brazil/desenvolvimento-humano/publications/indice-de-desenvolvimento-humano-municipal-brasileiro-2013. Accessed on April 29, 2025.

Schönholzer, D. and C. Zhang (2017). Valuing Local Public Goods using Municipal Annexations. *Working Paper*.

Seabright, P. (1996). Accountability and Decentralisation in Government: An Incomplete Contracts Model. *European Economic Review 40*(1), 61–89.

Shenoy, A. (2018). Regional Development through Place-Based Policies: Evidence from a Spatial Discontinuity. *Journal of Development Economics 130*, 173–189.

Slattery, C. and O. Zidar (2020). Evaluating State and Local Business Incentives. *Journal of Economic Perspectives 34*(2), 90–118.

Stasavage, D. (2010). When Distance Mattered: Geographic Scale and the Development of European Representative Assemblies. *American Political Science Review 104*(4), 625–643.

Sun, L. and S. Abraham (2021). Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects. *Journal of Econometrics 225*(2), 175–199.

Tesouro Nacional (2025). Fundo de Participação dos Municípios (FPM) por município. Available at https://www.tesourotransparente.gov.br/temas/estados-e-municipios/transferencias-a-estados-e-municipios.

Tiebout, C. M. (1956). A Pure Theory of Local Expenditures. *Journal of Political Economy 64*(5), 416–424.

Tomio, F. R. d. L. (2002). A Criação de Municípios Após a Constituição de 1988. *Revista Brasileira de Ciências Sociais 17*, 61–89.

Tomio, F. R. d. L. (2005). Federalismo, Municípios e Decisões Legislativas: A Criação de Municípios no Rio Grande do Sul. *Revista de Sociologia e Política*, 123–148.

Treisman, D. (2007). *The Architecture of Government: Rethinking Political Decentralization*. Cambridge University Press.

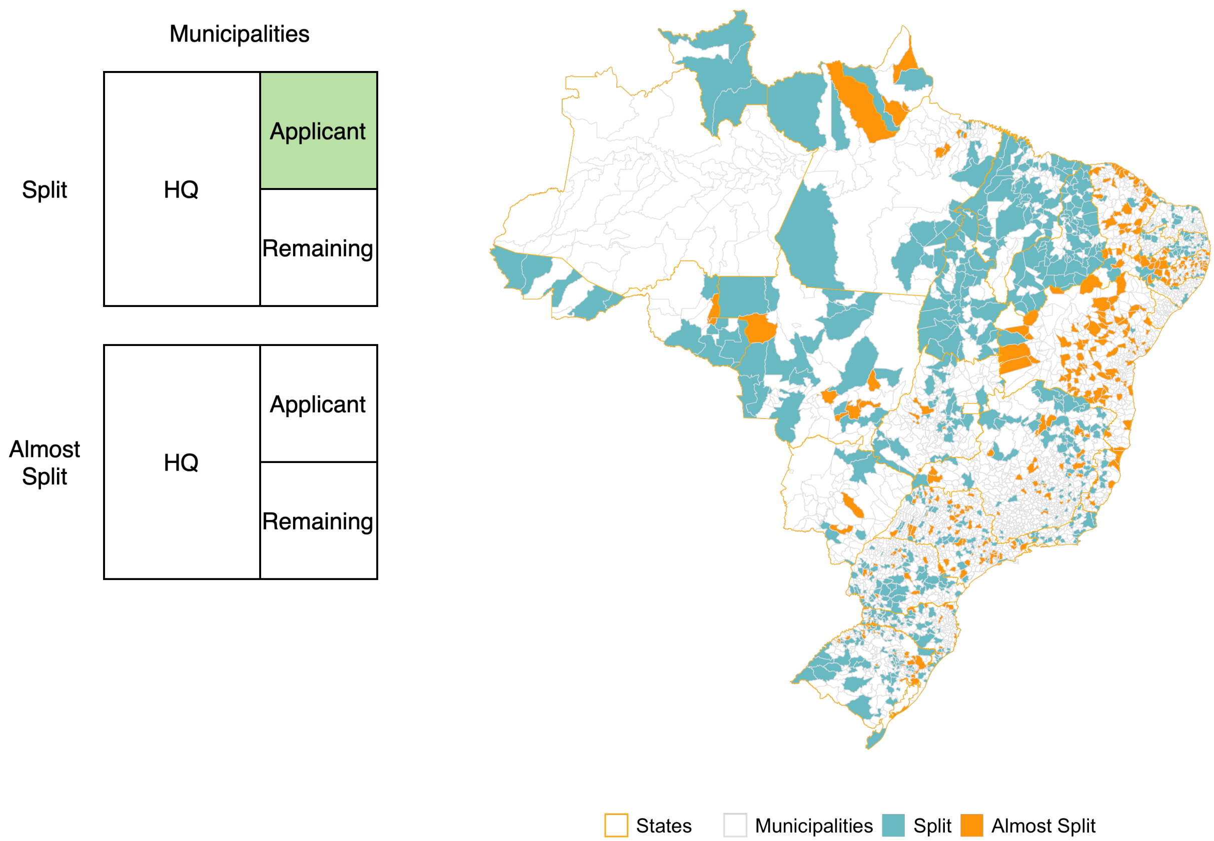
Tricaud, C. (2022). Better Alone? Evidence on the Costs of Intermunicipal Cooperation.

Weese, E. (2015). Political Mergers as Coalition Formation: An Analysis of the Heisei Municipal Amalgamations. *Quantitative Economics 6*(2), 257–307.

Zucco, C. and T. J. Power (2023). The Ideology of Brazilian Parties and Presidents: A Research Note on Coalitional Presidentialism Under Stress. *Latin American Politics and Society*.

9 Figures and Tables

Figure 1: Diagram Illustrating Splits and Map of Brazil

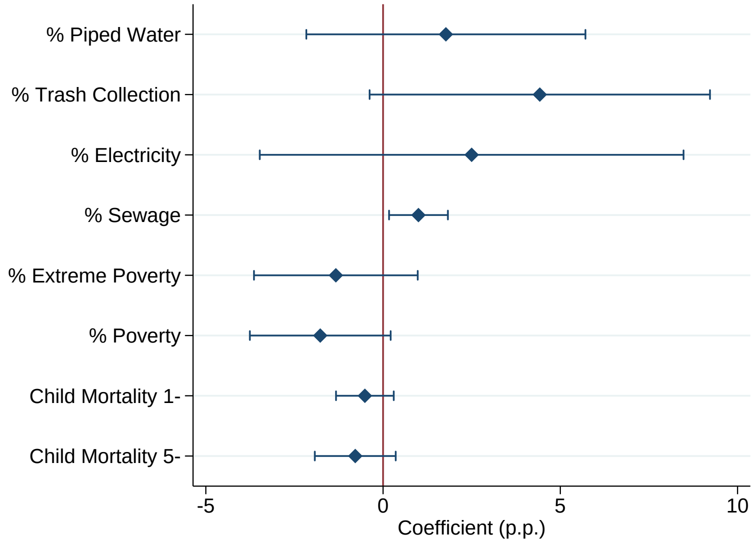


Note: On the left, the diagram illustrates the mechanics of split requests. Municipalities are divided into applicant, remaining, and headquarters districts. The green color highlights applicant districts that succeed at splitting. On the right, the map represents Brazil in 1991. Municipalities that split are colored blue and those that almost split are colored orange. More details can be found in Section 3.

Figure 2: Effects of Splitting on the Public Sector

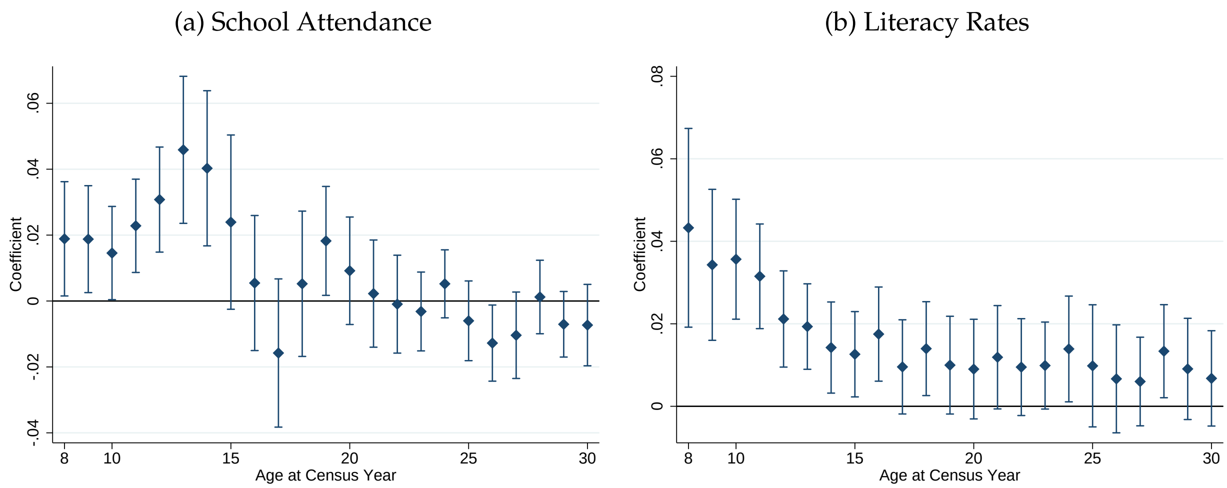
Note: This figure reports the annual effects of splitting on the public sector after estimating Equation (1). We consider the following dependent variables: log municipal capital expenditures, log municipal current expenditures, log total number of municipal jobs, and log average municipal wages. The omitted category is the year before splitting. Standard errors are two-way clustered at the state and split wave levels. Further details can be found in Appendix Table D.3.

Figure 3: Effects of Splitting on Public Services, Poverty, and Child Mortality



Note: This figure reports aggregate effects of splitting after estimating Equation (1). We consider the following dependent variables: household access to piped water, trash collection, electricity and sewage, extreme poverty, poverty, and child mortality (up to 1 and 5 years old) rates. Confidence intervals are 95 percent confidence intervals from regressions in which standard errors are two-way clustered at the state and split wave levels. Further details can be found in Appendix Table D.4.

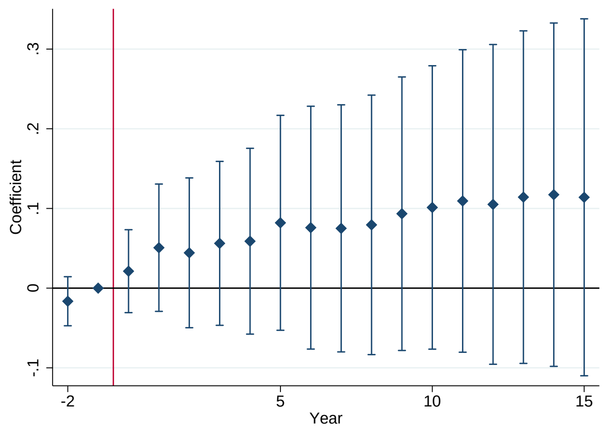
Figure 4: Heterogeneous Effects of Splitting on Education Outcomes Across Age



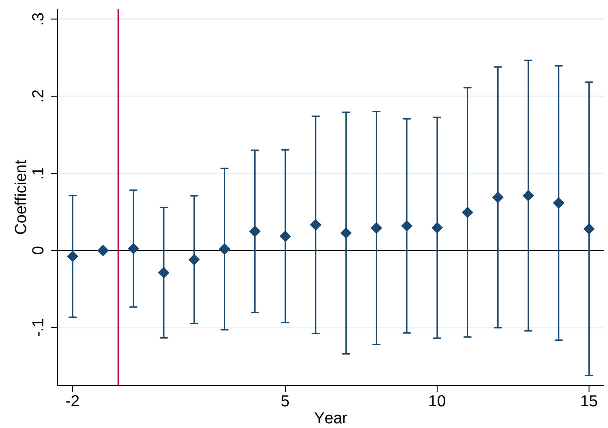
Note: This figure reports heterogeneous effects of splitting on school attendance (Panel (a)) and literacy rates (Panel (b)) after estimating Equation (2). Standard errors are two-way clustered at the state and split wave levels. Further details can be found in Appendix Figure D.1.

Figure 5: The Economic Effects of Splitting

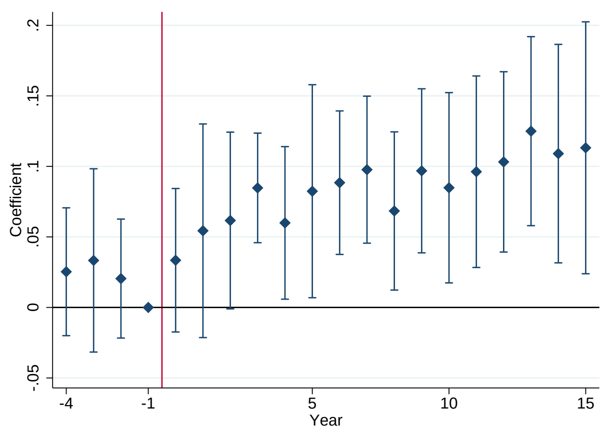
(a) Log Private Establishments



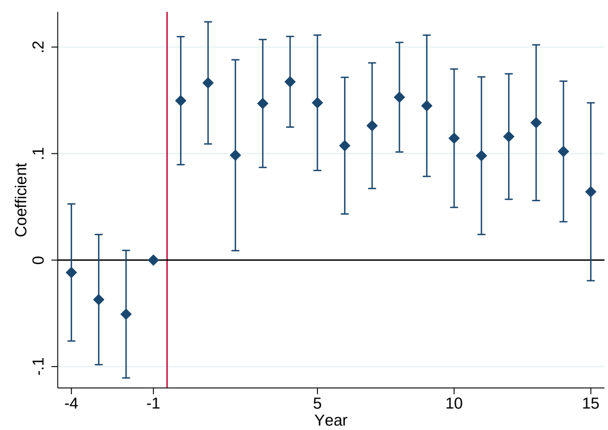
(b) Log Private Jobs



(c) Log Luminosity



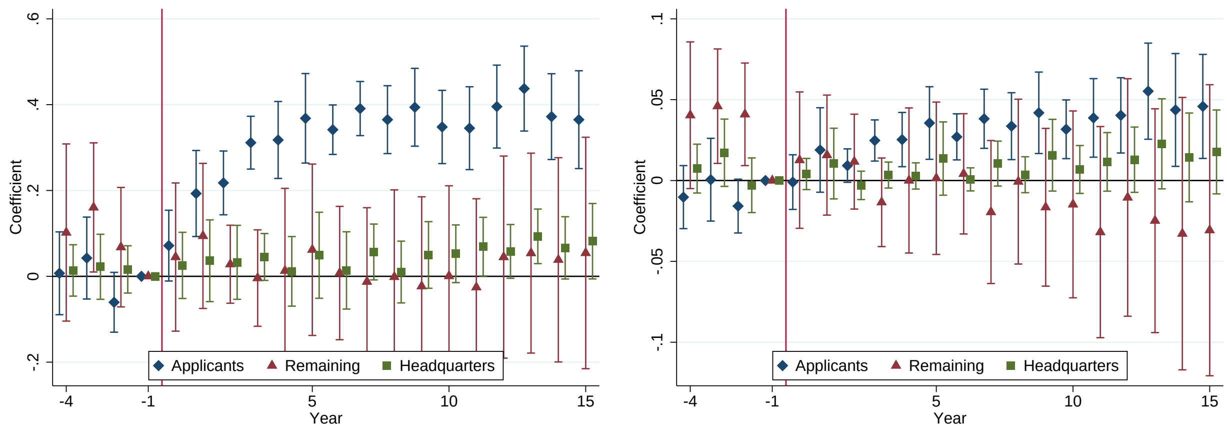
(d) Log Municipal Revenues



Note: Panels (a) and (b) of this figure report the annual effects of splitting on the private sector, captured by log total number of private establishments and log total number of private jobs. Panel (c) shows the annual effects of splitting on economic activity, captured by log average luminosity, while Panel (d) displays the annual effects for log municipal revenues. All results refer to Equation (1). The omitted category is the year before splitting. Standard errors are two-way clustered at the state and split wave levels. Further details can be found in Appendix Table D.5.

Figure 6: Distributional Effects of Splitting on Economic Activity

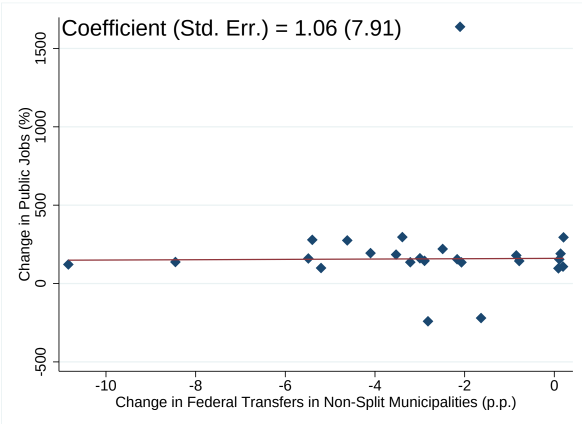
(a) Log Luminosity (b) % Pixels Lit



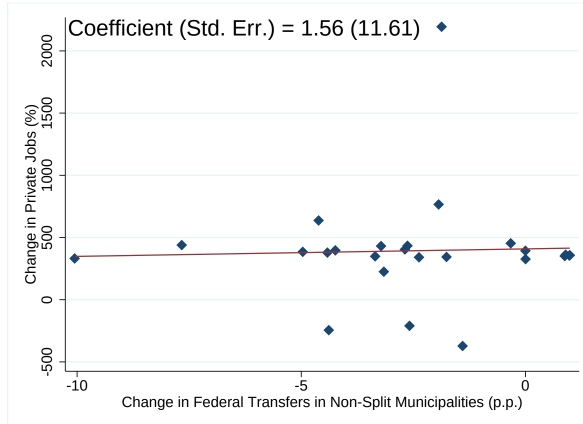
Note: This figure reports the annual effects of splitting on nighttime luminosity across districts after estimating Equation (3) separately for the applicant, remaining, and headquarters districts. Log average luminosity and indicator variable for whether the average is above zero are the dependent variables. The omitted category is the year before splitting. Standard errors are two-way clustered at the state and split wave levels. Further details can be found in Appendix Table D.6.

Figure 7: Spillover Effects of Changes in Federal Transfers

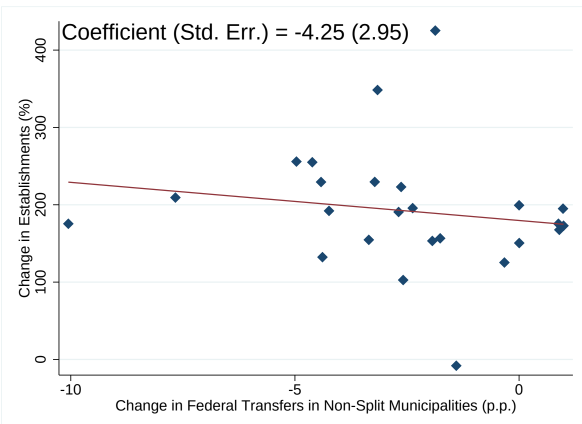
(a) Public Jobs



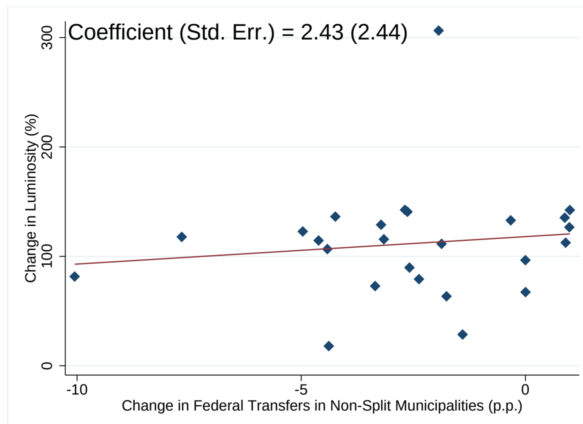
(b) Private Jobs



(c) Establishments



(d) Luminosity



Note: This figure reports correlations between changes in federal transfers (in percentage points) and selected outcomes (in percentage) for municipalities that did not split, weighted by population. Point estimates and standard errors are reported on the top left of each figure. We exploit variation in the number of splits within states after residualizing for region dummies. Changes in federal transfers are measured between 1996 and 1997. Outcomes of interest are percentage changes in the total number of public jobs, total number of private jobs, total number of establishments, and average luminosity 15 years after splitting. We exclude Distrito Federal and Roraima from the final sample.

Table 1: Baseline Descriptive Statistics at the Municipality Level

	Contains Applicant		Rest		*Differences*	
	Mean	SD	Mean	SD	Diff.	p-value
	(1)	(2)	(3)	(4)	(5)	(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 Attendance	13.1	9.7	17.4	14	-4.3	<0.01
Middleschool Attendance	88.1	10.7	89.7	11.5	-1.7	<0.01
High School Attendance	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
Log Distance to State Capital	5.4	.8	5.3	.8	.1	0.02
Log Income Per Capita	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			

Note: This table reports descriptive statistics in 1991 at the municipality level. We use information from the 1991 Demographic Census and the 1991 National Treasury data. See Section 3 for further details on data and sample construction of the municipality-level data.

Table 2: The Role of Fiscal Revenues

	% Trash Collection		% Sewage		% Poverty		Child Mortality 5-		Log Municipal Jobs		Log Private Establishments		Log Private Jobs		Log Luminosity	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Post x Split	4.45*	3.38	1.12***	0.76*	-1.92*	-2.43**	-0.82	-0.79	0.15	0.12	0.10	0.09	0.06	0.06	0.15***	0.14***
	(2.32)	(2.05)	(0.39)	(0.41)	(0.98)	(0.94)	(0.54)	(0.52)	(0.12)	(0.12)	(0.08)	(0.08)	(0.08)	(0.08)	(0.04)	(0.03)
Log Municipal Revenues		5.70**		1.89*		2.71**		-0.19		0.21***		0.02		0.01		0.07***
		(2.41)		(1.06)		(1.19)		(0.25)		(0.03)		(0.03)		(0.05)		(0.02)
Observations	1,325	1,325	1,325	1,325	1,325	1,325	1,325	1,325	6,950	6,950	7,033	7,033	7,033	7,033	8,131	8,131
R-squared	0.87	0.87	0.89	0.89	0.94	0.94	0.96	0.96	0.83	0.84	0.99	0.99	0.97	0.97	0.98	0.98
State-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean Pre-Split	63.73	63.73	96.04	96.04	42.47	42.47	38.70	38.70	5.82	5.82	4.64	4.64	6.94	6.94	-0.29	-0.29
SD Pre-Split	27.32	27.32	7.780	7.780	20.74	20.74	12.81	12.81	1.29	1.29	1.58	1.58	1.96	1.96	1.58	1.58

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the aggregate estimates of splitting on selected outcomes. Odd columns consider the baseline specification (further details can be found in Appendix Tables D.3 to D.5). Even columns further control for log municipal revenues. Standard errors are two-way clustered at the state and split wave levels.

Table 3: Heterogeneous Effects of Splitting on Economic Activity

	Log Luminosity			
	(1)	(2)	(3)	(4)
Post x Split	0.35***	0.32***	1.65***	0.79
	(0.03)	(0.03)	(0.28)	(0.51)
Log Revenues		0.05	0.05	0.05
		(0.03)	(0.03)	(0.03)
Post x Split x Log Population in 1991			-0.04	0.02
			(0.04)	(0.04)
Post x Split x Log Area			-0.13***	-0.25***
			(0.03)	(0.07)
Post x Split x Urbanization Rate in 1991			-0.01**	-0.01**
			(0.00)	(0.00)
Post x Split x Log Distance to Parent Townhall				0.24
				(0.16)
Post x Split x Log Distance to State Capital				0.07
				(0.07)
Observations	9,821	9,821	9,821	9,821
R-squared	0.96	0.96	0.96	0.96
District FE	✓	✓	✓	✓
State-Year FE	✓	✓	✓	✓
Controls-Time FE	✓	✓	✓	✓
Mean Pre-Split	-0.732	-0.732	-0.732	-0.732
SD Pre-Split	1.524	1.524	1.524	1.524

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the heterogeneous effects of splitting on log luminosity. Column (1) repeats the benchmark specification from Equation (3), while Column (2) adds log revenues to the set of controls. Revenues at the district level are imputed according to proportional population prior to splits. In Column (3), we include interaction terms with log population in 1991, log total area in 1991, and urbanization rate in 1991. Column (4) further adds interaction terms with log distance to the parent town hall and log distance to the state capital. Standard errors are two-way clustered both at the state and split wave levels.

Appendices

A Conceptual Framework

We sketch a simple framework to illustrate how splitting affects the provision of public services. Our model incorporates several features from our context and highlights the scope for several mechanisms studied in the paper, including neglect from the headquarters and fiscal incentives (Bolton and Roland, 1997; Dur and Staal, 2008).

We work with a one-period model. We assume that a municipality, which we refer to as municipality 1, is composed of two districts, A and B. The municipal population is immobile, and districts A and B have population α_A and α_B. Since there is no income heterogeneity within the district, all residents have income per capita y. Two sources of municipal revenues finance public goods g: income taxes τ and federal transfers $T(\cdot)$. In line with the institutional context described in Section 2, $T(\cdot)$ depends on the population size. We also assume that $T(\cdot)$ is weakly increasing and concave, while federal transfers per capita are weakly decreasing and convex in population size. The utility takes a quasi-linear form, $U_i = \theta_i \ln(g_i) + (1-\tau)y_i$, in which 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 regarding the allocation of public goods. When districts A and B form together a single 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. In other words, district A solves the following maximization problem:

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

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

In case of splitting, district B becomes a municipality and obtains decision-making power over its level of public goods, g_B^S. The maximization problem can be written as:

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

in which $T(\alpha_B)$ is the total federal transfers the new municipality receives. The parent municipality, now district A, chooses g_A and τ from an analogous maximization problem.

Comparing solutions of the maximization problems, we have:

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

1. ***(Capture and Neglect)*** *Its welfare was captured and neglected by the headquarters (lower λ);*

2. ***(Fiscal Incentives)*** *It is small in population size (lower α_B) and has:*

   (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, we extend our framework to introduce a second municipality representing the rest of the state, with population α_2. To capture the reallocation of federal transfers after a split, define T_i^U as the transfers that area i receives when municipality 1 does not split; and T_i^S as the transfers that area i receives when municipality 1 splits. Consistent with the Brazilian context, transfers are "zero-sum game", 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 integrated 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 how welfare changes after a split.

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

Proof. See Appendix Section B.2. □

The intuition behind Proposition 2 is straightforward. Due to 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 create substantial benefits to the latter. The welfare of district A changes little, either positively or negatively, depending on whether its transfers change or not. We directly test these predictions in Section 5 by separately evaluating the consequences of splitting for headquarters and non-headquarters districts.

B Proofs of Propositions

B.1 Proof of Proposition 1

Proof. 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 integrated policy choice problem (6), assuming there exists an interior optimum, we can solve the first-order condition:

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

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 integration:

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

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

Similarly, for Problem (7), we can show that:

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

District B unilaterally chooses to split if $U_B^S \geq U_B^U$. Substituting in Equations (9) and

(10), we can express the surplus condition as:

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

We can show that:

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

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

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

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

District B changes local tax rates from τ^U to τ_B^S after a split. This is equivalent to:

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

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

B.2 Proof of Proposition 2

Proof. If district B is relatively small $\left(\frac{\alpha_B}{\alpha_A} \to 0\right)$ and captured and neglected by its parent district ($\lambda \to 0$), and municipality 2 is relatively large $\left(\frac{\alpha_2}{\alpha_A + \alpha_B} \to \infty\right)$, we have that:

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

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

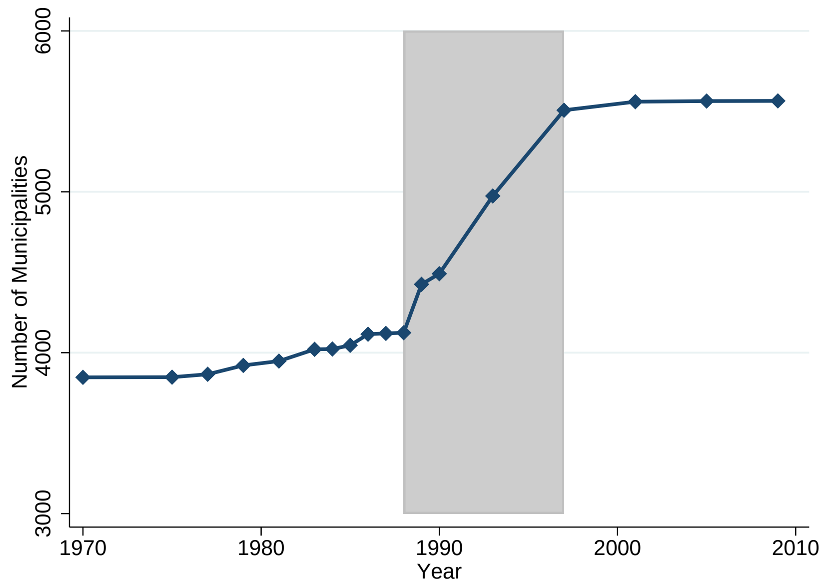
$$\Delta V_2 = \frac{T^S(\alpha_2) - T^U(\alpha_2)}{\alpha_2} \tag{16}$$

Given our assumptions, one can show that $\Delta V_A \to 0$, $\Delta V_B \to \infty$, $\Delta V_2 \to 0$. □

C Data Appendix

C.1 Figures

Figure C.1: Evolution of Total Number of Municipalities



Note: This figure shows the evolution of the number of municipalities between 1970 and 2010. The grey area highlights the period between the 1988 Federal Constitution and the 1996 CA.

C.2 Split Requests

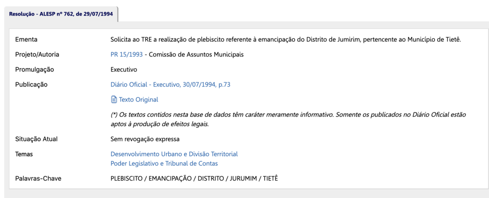
Using historical archives, we have constructed a novel dataset that includes all requests to split initiated by districts between 1989 and 1996. Prior to the 1996 CA, each state assembly had the discretion to set its own regulations on splitting, leading to substantial variation in records on split requests.

Brazil has 26 state legislative assemblies. For each state assembly, we searched for digitized historical records on split requests from the first half of the 1990s. We found 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 vary widely across states. Appendix Figure C.2 provides an example of the material available online. Appendix Figure C.3 shows the distributions of request and split years among applicant districts.

Below, we list the variables we constructed from the records for each state:

Figure C.2: Examples of Raw Material of Split Requests

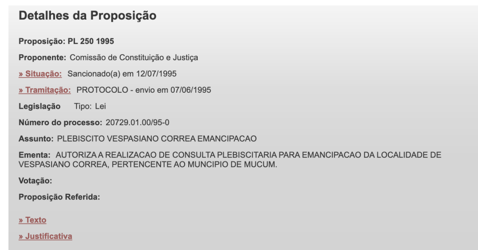
(a) São Paulo



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:

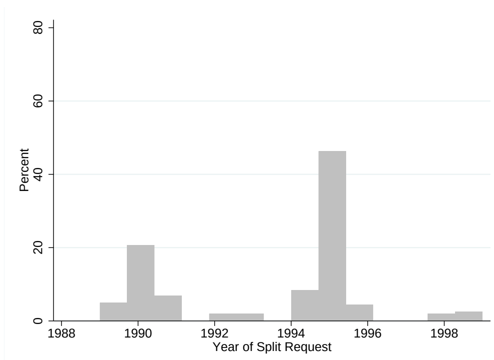
Proposição Referida:

» Texto

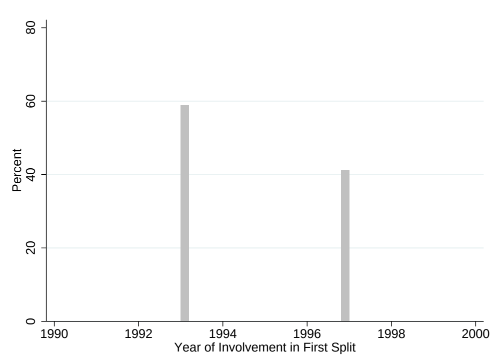
» Justificativa

Figure C.3: Histograms of Request and Split Years

(a) Year of *Request* among Almost Split Applicants



(b) Year of *Split* among Applicants



Amapá: Indicator for whether district has requested to split; indicator for whether district has the request approved; identification number of the split process; start date of the process; approval date of the referendum; 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; start date of the process; approval date of the referendum; 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; identification number of the split process; approval date of the referendum; and result of the referendum.

Mato Grosso: Indicator for whether district has requested to split; indicator for whether district has the request approved; identification number of the split process; start date of the process; 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; identification number of the split process; start date of the process; approval date of the referendum; and result of the referendum.

Pará: Indicator for whether district has requested to split; indicator for whether district has the request approved; identification number of the split process; start date of the process; approval date of the referendum; 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; identification number of the split process; start date of the process; 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; identification number of the split process; start date of the process; approval date of the referendum; 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; identification number of the split process; approval date of the referendum; and result of the referendum.

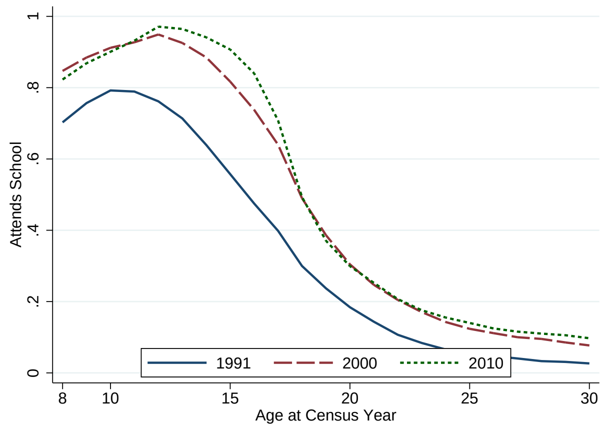
Santa Catarina: Indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; date when the request was archived; identification number of the split process; start date of the process; approval date of the referendum; 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; identification number of the split process; start date of the process; approval date of the referendum; and result of the referendum.

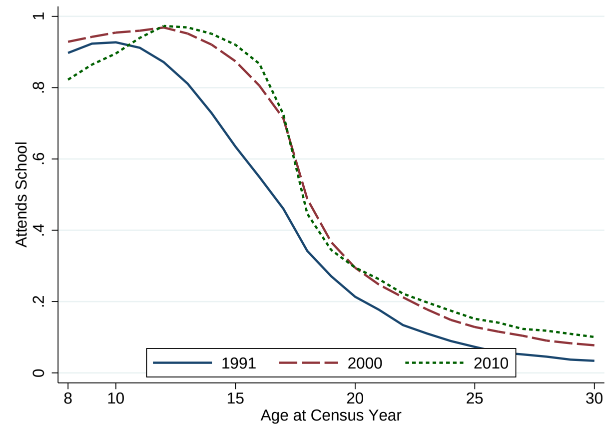
D Additional Results

Figure D.1: Heterogeneous Effects of Splitting on Education Outcomes: Raw Data

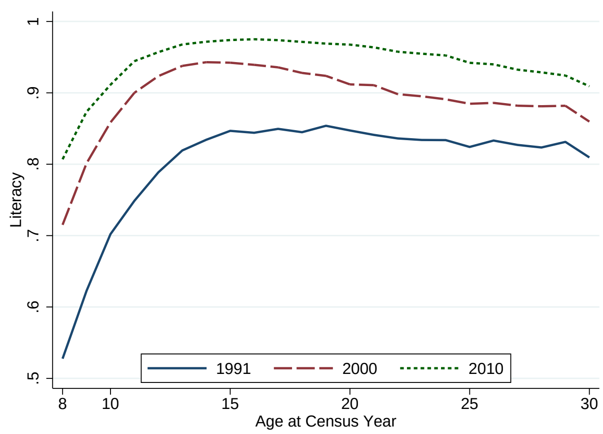
(a) School Attendance, Split Municipalities



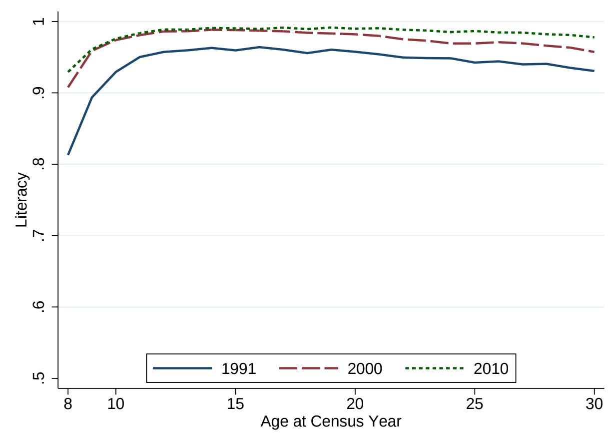
(b) School Attendance, Almost Split Municipalities



(c) Literacy Rates, Split Municipalities



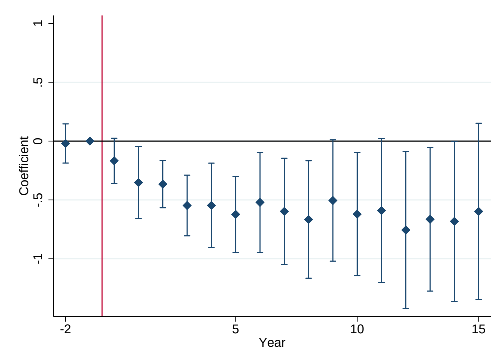
(d) Literacy Rates, Almost Split Municipalities



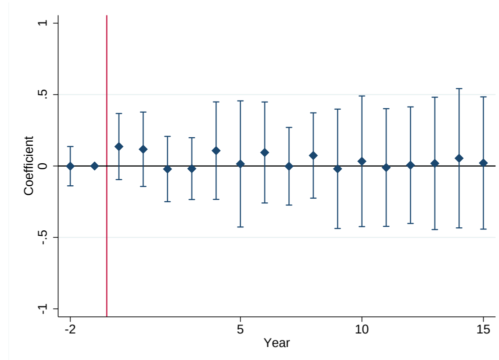
Note: This figure displays the raw data for school attendance and literacy rates from the sample described in Figure 4 for split and almost split municipalities, by census wave and age. The main data sources are the individual-level microdata from decennial Demographic Census from 1991, 2000, and 2010.

Figure D.2: Crowd-Out Effects of Splitting on Jobs

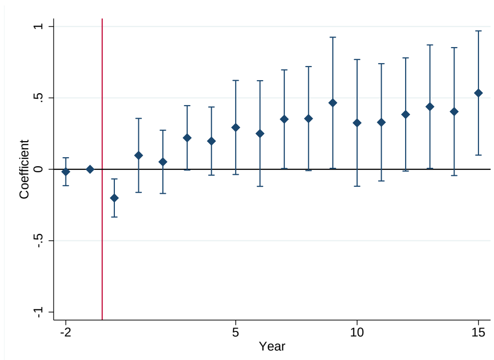
(a) Log Nonprofit Jobs in Education



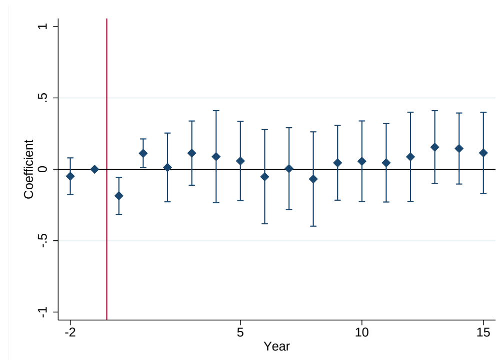
(b) Log Nonprofit Jobs in Health



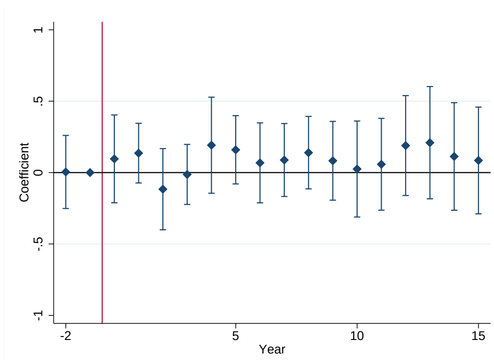
(c) Log Private Jobs in Education



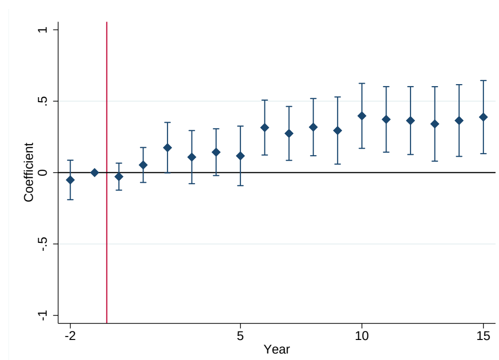
(d) Log Private Jobs in Health



(e) Log Public Jobs in Education



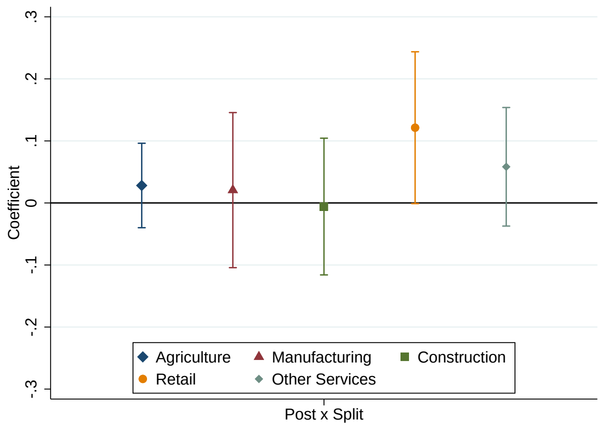
(f) Log Public Jobs in Health



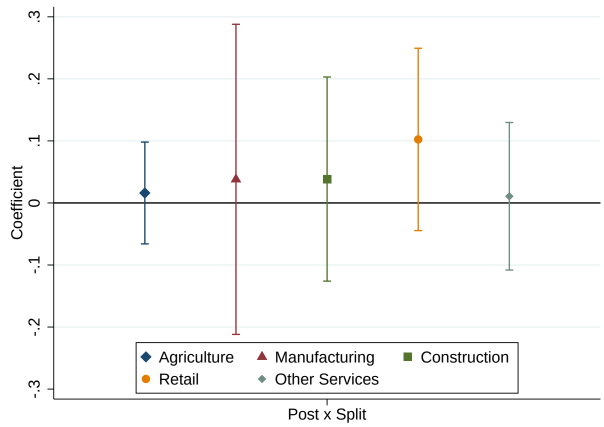
Note: This figure reports annual crowd-out effects of splitting on log number of jobs in nonprofit, private, and public sectors in education and health areas after estimating Equation (1). The omitted category is the year before splitting. Standard errors are two-way clustered at the state and split wave levels.

Figure D.3: Heterogeneous Effects of Splitting on the Private Sector

(a) Log Private Establishments



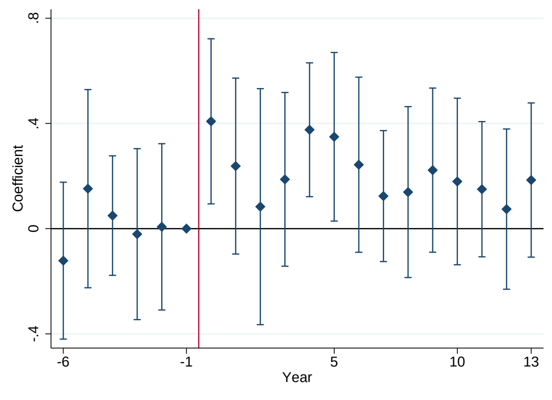
(b) Log Private Jobs



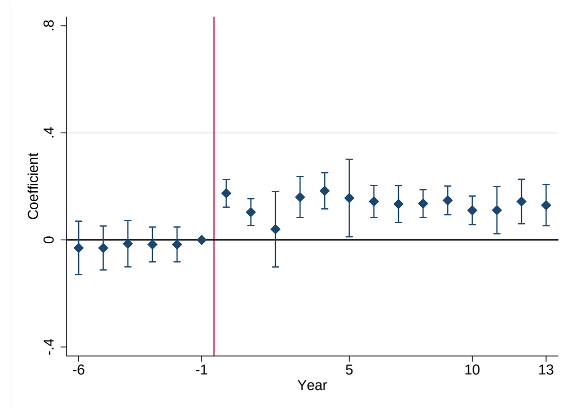
Note: This figure decomposes the aggregate effects of splitting on log private establishments and log private jobs from Columns (1) and (2) of Appendix Table D.5 across economic sectors.

Figure D.4: Effects of 1997 Splitting on Selected Outcomes

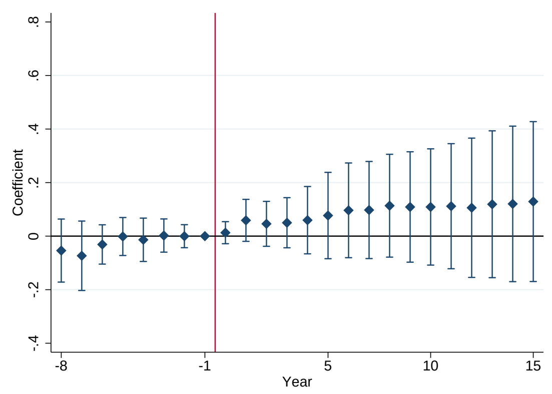
(a) Log Capital Expenditures



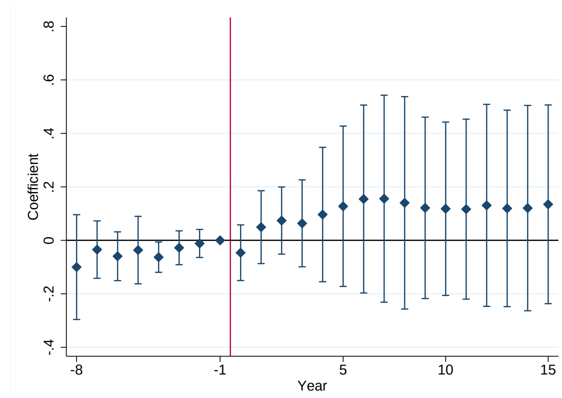
(b) Log Current Expenditures



(c) Log Establishments

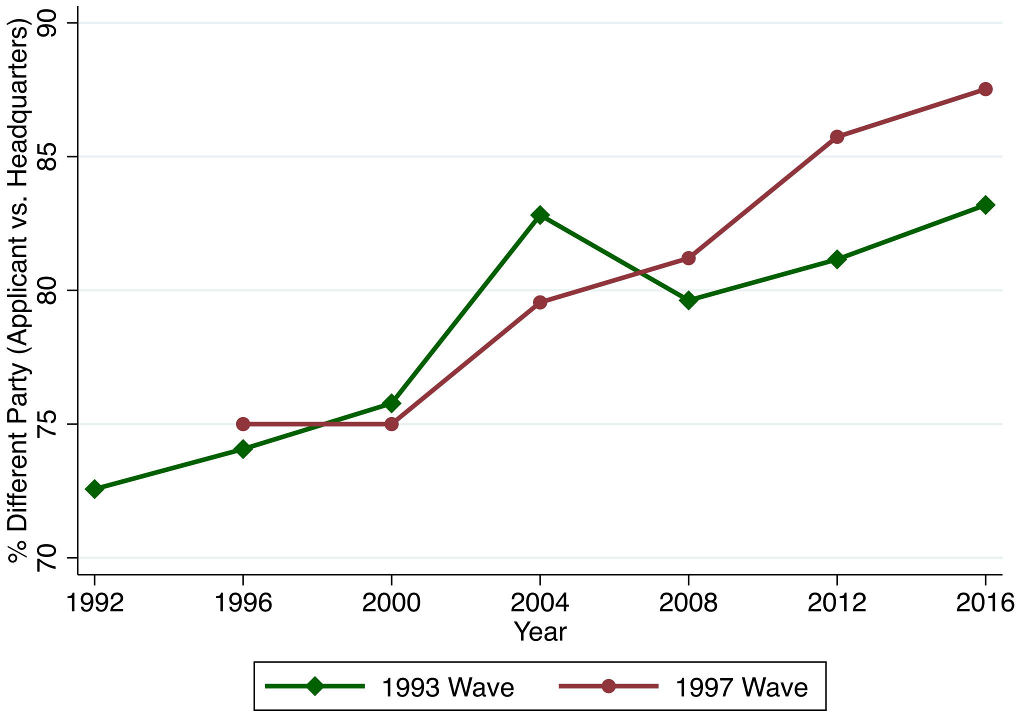


(d) Log Jobs



Note: This figure reports the annual effects of 1997 splitting after estimating Equation (1). We consider the following dependent variables: log municipal capital expenditures, log municipal current expenditures, log total number of establishments, and log total number of jobs. The omitted category is the year before splitting. Standard errors are two-way clustered at the state and split wave levels.

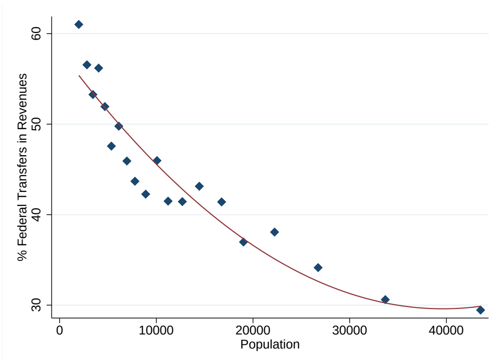
Figure D.5: Divergence in Political Preferences After Redistricting



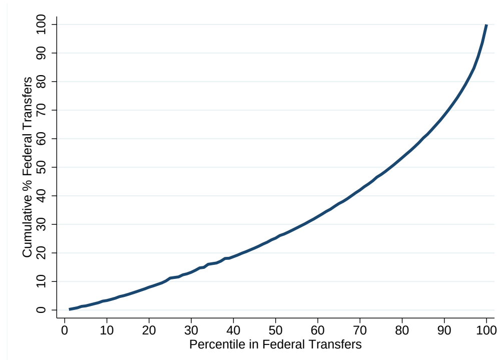
Note: This figure plots the percentage of municipalities in which the applicant and headquarters districts elected mayors from different parties after splitting. Because data on elections are only available at the municipality level, we only plot trends for municipalities that ultimately split.

Figure D.6: Distribution of Federal Transfers

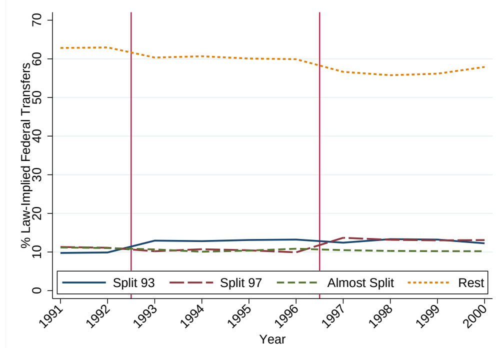
(a) Share of Federal Transfers Relative to Municipal Revenues in 1991



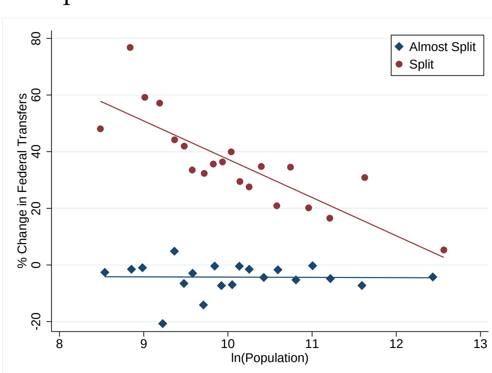
(b) Cumulative Distribution of Federal Transfers in 1991 (Bottom 50% Get ≈ 26%)



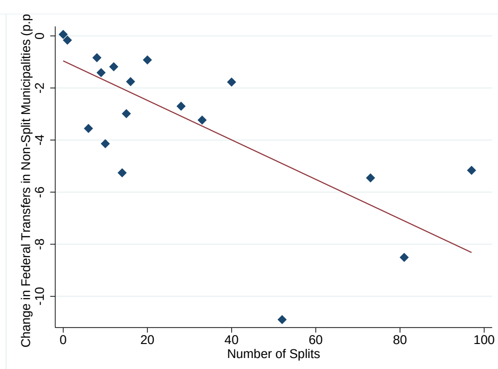
(c) Group Shares After Split Waves



(d) Change in Federal Transfers Across Groups



(e) Number of Splits and Losses in Federal Transfers



Note: This figure reports the patterns of revenues from federal transfers (FPM) over time, as described in Section 2. Panel (a) describes the share of municipal revenues from federal transfers across population bins in 1991. Panel (b) plots the distribution of federal transfers in 1991. Panel (c) plots the reallocation of federal transfers after the 1993 and 1997 split waves implied by the transfer allocation mechanism. Panel (d) illustrates how the gains in revenues from federal transfers accrue particularly to new municipalities with smaller population. Panel (e) shows the relationship between the number of splits and the losses in federal transfers in non-split municipalities.

Table D.1: Baseline Descriptive Statistics at the District Level

	In Sample						Rest				*Differences*			
	Applicant		Remaining		Headquarters		Periphery		Headquarters		(1)-(3)		(1)-(5)	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Diff.	p-value	Diff.	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Population (000's)	5.4	12.4	3.1	5.4	31.7	63.8	3.6	13.3	17.8	48.8	2.7	<0.01	-25.8	<0.01
% Urban Population	38.5	26.5	27.7	24.6	67.8	22.4	32.2	24.9	61.9	22.6	11.4	<0.01	-29.2	<0.01
% Male	51.8	1.3	52.3	1.8	50.3	1.2	52.3	1.9	51	1.3	-.5	<0.01	1.5	<0.01
% Literacy	65.8	11.6	65.1	12.2	70.3	9.5	64.3	11.2	68.9	8.6	.9	.26	-4.4	<0.01
% Piped Water	62.9	27	58.9	29	77.5	21.1	61.5	26.5	76	21	4.4	.02	-14.3	<0.01
% Sewage	19.9	25.5	16.2	22.9	40.5	30.5	17.3	22.5	38.4	30	4	.02	-20.2	<0.01
% Trash Collection	21.5	27.1	13.1	22.9	54.1	26.7	13.5	23.2	47.6	27.2	8.9	<0.01	-32.3	<0.01
Avg. Luminosity	1.8	5.8	1.4	5.2	3.1	6.5	1.7	7.2	2.4	6.7	.5	.18	-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
Log 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
Log Distance to State Capital	5.5	.8	5.4	.7	5.4	.8	5.2	.8	5.3	.8	.1	.13	.1	.3
Log Maize Suitability	8.7	.3	8.7	.3	8.6	.3	8.5	.3	8.5	.2	0	.99	0	.06
Log Wet Rice Suitability	8.6	.8	8.6	.5	8.7	.5	8.6	.9	8.6	.8	0	.57	0	.42
Log Soybean Suitability	7.7	.4	7.7	.2	7.7	.2	7.6	.8	7.7	.7	0	.44	0	.73
Log Wheat Suitability	6.5	2.9	6.8	2.8	6.6	2.8	6.5	3	6.5	2.9	-.2	.37	0	.9
Terrain Ruggedness	83.2	78.2	72.7	68.6	76.1	72.7	68.5	71.8	68.7	71.4	9.9	.06	6.9	.16
	N = 552		N = 324		N = 384		N = 912		N = 1777					

Notes: This table reports baseline descriptive statistics in 1991 at the district level. We use information from the 1991 Demographic Census, and the 1992 night lights, MapBiomas, FAO-GAEZ soil suitability, and terrain ruggedness data. See Section 3 for further details on data and construction of the district-level sample.

Table D.2: Baseline Descriptive Statistics at the District Level by Split Wave

	Applicants 1993	Applicants 1997	Split 1993	Split 1997	Almost Split 1993	Almost Split 1997	(2)-(1)		(4)-(3)		(5)-(3)		(6)-(4)	
	Mean (1)	Mean (2)	Mean (3)	Mean (4)	Mean (5)	Mean (6)	Dif. (7)	p (8)	Dif. (9)	p (10)	Dif. (11)	p (12)	Dif. (13)	p (14)
Population (000's)	5.73	4.95	4.94	4.32	9.8	6.88	-.78	.46	-.62	.27	-4.86	.03	-2.56	.05
% Urban Population	42.2	33.85	39.84	32.59	54.26	37.66	-8.35	0	-7.25	0	-14.42	0	-5.07	.17
% Male	51.79	51.76	51.78	51.74	51.87	51.8	-.04	.73	-.04	.77	-.09	.64	-.06	.78
% Literacy	68.54	62.41	68.23	61.28	70.12	65.84	-6.13	0	-6.96	0	-1.88	.2	-4.56	.02
% Piped Water	68.56	55.9	66.33	52.7	79.96	65.6	-12.66	0	-13.63	0	-13.63	0	-12.9	0
% Sewage	22.34	16.79	20.34	15.03	32.58	22.1	-5.55	.01	-5.31	.02	-12.24	0	-7.07	.04
% Trash Removal	25.44	16.7	21.41	14.74	46.06	22.66	-8.74	0	-6.67	.01	-24.65	0	-7.93	.03
Avg. Luminosity	1.97	1.51	1.23	.73	5.74	3.87	-.45	.36	-.49	.17	-4.52	0	-3.14	0
Area (000's km2)	.5	.61	.57	.69	.14	.35	.11	.4	.12	.43	.43	.03	.34	.19
Log Distance to Parent Townhall	2.97	2.96	3.05	3.05	2.56	2.69	-.01	.89	0	.95	.48	0	.36	0
Log Distance to State Capital	5.49	5.45	5.5	5.59	5.42	5.02	-.04	.57	.09	.18	.09	.46	.58	0
Log Maize Suitability	8.64	8.69	8.68	8.69	8.45	8.67	.04	.05	.01	.58	.23	0	.03	.46
Log Wet Rice Suitability	8.57	8.68	8.56	8.68	8.64	8.71	.11	.08	.12	.14	-.09	.58	-.03	.23
Log Soybean Suitability	7.7	7.74	7.7	7.73	7.71	7.76	.04	.27	.03	.46	-.01	.83	-.03	.69
Log Wheat Suitability	6.56	6.52	6.45	6.66	7.13	6.12	-.04	.88	.2	.48	-.67	.14	.54	.21
Terrain Ruggedness	86.16	79.44	95.21	83.22	39.84	67.99	-6.72	.32	-11.99	.12	55.37	0	15.23	.18
	N = 306	N = 246	N = 256	N = 185	N = 50	N = 61								

Note: This table reports baseline descriptive statistics at the district level by the split wave (1993 and 1997). We use information from the 1991 Demographic Census, the 1992 night lights, MapBiomas, FAO-GAEZ soil suitability, and terrain ruggedness data. See Section 3 for further details on data and construction of the district-level sample.

Table D.3: Effects of Splitting on the Public Sector

	Log Capital Expenditures	Log Current Expenditures	Log Municipal Jobs	Log Average Municipal Wages
	(1)	(2)	(3)	(4)
Post x Split	0.24***	0.16***	0.15	0.01
	(0.07)	(0.02)	(0.14)	(0.03)
Observations	8,803	8,807	7,063	7,063
R-squared	0.87	0.98	0.83	0.94
State-Year FE	✓	✓	✓	✓
Mean Pre-Split	13.67	15.33	5.80	8.19
SD Pre-Split	1.43	1.13	1.31	0.43

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the aggregate effects of splitting on the public sector. We consider the following dependent variables: log municipal capital expenditures, log municipal current expenditures, log total number of municipal jobs, and log average municipal wages. Standard errors are two-way clustered at the state and split wave levels.

Table D.4: Effects of Splitting on Public Services, Poverty, and Child Mortality

	% Piped Water	% Trash Collection	% Electricity	% Sewage	% Extreme Poverty	% Poverty	Child Mortality 1-	Child Mortality 5-
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post x Split	1.77	4.42*	2.50	1.00**	-1.33	-1.77*	-0.51	-0.78
	(1.89)	(2.31)	(2.87)	(0.40)	(1.11)	(0.96)	(0.39)	(0.55)
Observations	1,344	1,344	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	0.95	0.96
State-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean Pre-Split	71.18	63.51	81.33	96.10	19.62	42.81	32.33	38.64
SD Pre-Split	24.17	27.35	20.03	7.660	14.86	20.60	9.700	12.85

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the aggregate effects of splitting on public services, poverty, and child mortality. We consider the following dependent variables: household access to piped water, trash collection, electricity, sewage, extreme poverty, poverty, and child mortality (up to 1 and 5 years old) rates. Standard errors are two-way clustered at the state and split wave levels.

Table D.5: The Economic Effects of Splitting

	(1) Log Private Establishments	(2) Log Private Jobs	(3) Log Private Wages	(4) Log Luminosity	(5) Log Municipal Revenues	(6) Log Municipal Transfers	(7) Log Municipal Taxation
Post x Split	0.10	0.06	0.15**	0.08***	0.14***	0.16***	0.11
	(0.08)	(0.07)	(0.06)	(0.02)	(0.03)	(0.03)	(0.07)
Observations	7,152	7,152	8,925	8,276	8,809	8,808	8,808
R-squared	0.99	0.97	0.99	0.99	0.98	0.97	0.96
State-Year FE	✓	✓	✓	✓	✓	✓	✓
Mean Pre-Split	4.65	6.95	10.57	-0.04	15.50	15.49	12.77
SD Pre-Split	1.57	1.94	5.02	1.31	1.11	1.26	1.97

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the aggregate effects of splitting on the private sector, economic activity, and public finance. We consider the following dependent variables: log total number of private establishments, log total number of private jobs, log private wages, log average luminosity, log municipal revenues, log municipal transfers, and log municipal taxes. Standard errors are two-way clustered both at the state and split wave levels.

Table D.6: Distributional Effects of Splitting on Economic Activity

	Panel A: Log Luminosity		
	Applicants	Remaining	Headquarters
	(1)	(2)	(3)
Post x Split	0.34***	-0.07	0.06***
	(0.03)	(0.10)	(0.02)
Observations	10,122	5,964	6,987
R-squared	0.96	0.95	0.98
Mean Pre-Split	-0.724	-0.848	0.221
SD Pre-Split	1.527	1.425	1.415

	Panel B: % Pixels Lit		
	Applicants	Remaining	Headquarters
	(1)	(2)	(3)
Post x Split	0.04***	-0.04*	0.00
	(0.01)	(0.02)	(0.01)
Observations	10,122	5,964	6,987
R-squared	0.96	0.94	0.97
Mean Pre-Split	0.176	0.152	0.246
SD Pre-Split	0.289	0.256	0.290

	Panel C: Log Luminosity Outside 5km Town Hall Radius		
	Applicants	Remaining	Headquarters
	(1)	(2)	(3)
Post x Split	0.34***	-0.07	0.07***
	(0.03)	(0.10)	(0.02)
Observations	10,122	5,964	6,987
R-squared	0.96	0.95	0.98
Mean Pre-Split	-0.729	-0.848	0.196
SD Pre-Split	1.531	1.425	1.409

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the aggregate effects of splitting on economic activity separately for three groups of districts: applicant, remaining, and headquarters. Dependent variables for log average luminosity (Panel A), the percentage of pixels with luminosity above zero (Panel B), and log average luminosity outside a radius of 5km around the town hall (Panel C). Standard errors are two-way clustered at the state and split wave levels.

Table D.7: Distributional Effects of Splitting on Public Services

	% Piped Water	% Trash Removal	% Sewage	% Urban Population
	(1)	(2)	(3)	(4)
Panel A: Applicants				
Post x Split	9.93**	5.27***	0.53	2.88**
	(4.15)	(1.29)	(1.08)	(1.12)
Observations	1,656	1,656	1,656	1,656
R-squared	0.76	0.91	0.84	0.93
Mean Pre-Split	62.92	21.55	19.86	38.47
SD Pre-Split	26.97	27.14	25.48	26.50
Panel B: Remaining				
Post x Split	-1.51	-8.31*	-3.04	-0.97
	(4.91)	(4.01)	(3.76)	(3.49)
Observations	972	972	972	947
R-squared	0.78	0.85	0.77	0.94
Mean Pre-Split	58.86	13.06	16.20	27.68
SD Pre-Split	28.98	22.87	22.90	24.55
Panel C: Headquarters				
Post x Split	2.37	2.87***	1.49	1.77**
	(2.33)	(0.89)	(1.44)	(0.74)
Observations	1,149	1,149	1,149	1,149
R-squared	0.80	0.92	0.90	0.95
Mean Pre-Split	77.48	54.14	40.52	67.84
SD Pre-Split	21.10	26.73	30.51	22.45

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the aggregate effects of splitting on public services separately for three groups of districts: applicant (Panel A), remaining (Panel B), and headquarters (Panel C). We consider the following dependent variables: household access to piped water, trash collection and sewage, and share of urban population. Standard errors are two-way clustered at the state and split wave levels.

Table D.8: Robustness Checks: Effects of Splitting on Luminosity for Applicant Districts

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Specifications						
Post x Split	0.34***	0.45***	0.10***	0.32***	0.27***	0.36***
	(0.03)	(0.04)	(0.03)	(0.04)	(0.06)	(0.03)
Observations	10,122	9,530	10,122	4,920	10,122	10,122
R-squared	0.96	0.95	0.97	0.96	0.98	0.96
Choice	Benchmark	Log	IHS	1997 Wave	Microregion FE	Controls
Mean Pre-Split	-0.724	-0.929	0.707	-0.638	-0.724	-0.724
SD Pre-Split	1.527	2.052	0.999	1.507	1.527	1.527
Panel B: Standard errors						
Post x Split	0.34***	0.34***	0.34***	0.34***		
	(0.03)	(0.05)	(0.05)	(0.03)		
Observations	10,122	10,122	10,122	10,122		
R-squared	0.96	0.96	0.96	0.96		
Std Error Clustering	State-Split Wave	Municipality	Microregion	State		
Wild Bootstrap p-value	<0.01			<0.01		
Mean Pre-Split	-0.724	-0.724	-0.724	-0.724		
SD Pre-Split	1.527	1.527	1.527	1.527		
Number of Clusters	20	422	194	11		

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports several robustness checks for the aggregate estimates of splitting on economic activity, measured by log average luminosity, for applicant districts. Panel A shows that the results are robust to different choices of specifications, dependent variables, and samples. Column (1) repeats the benchmark specification from Equation (3). Column (2) does not add 0.1 to the average luminosity, while Column (3) applies inverse hyperbolic sine transformation to the average luminosity. Column (4) restricts the sample to districts involved in the 1997 wave. Column (5) adds micro region-by-year fixed effects to the set of controls. Column (6) controls for baseline characteristics from Appendix Table D.1 interacted with year fixed effects. Panel B shows that the results are robust to choices of clustering the standard errors. Column (1) refers to the standard choice of two-way clustering at the state and split wave levels. Columns (2), (3) and (4) consider clustering at the municipality, micro region and state levels, respectively. To account for the small number of clusters, Columns (1) and (4) additionally report wild bootstrap p-values.

Table D.9: The Politics of Splitting

	Some Applicant	Some Split	Some Applicant	Some Split
	(1)	(2)	(3)	(4)
Mayor and Governor from the Same Party	-0.00	0.04		
	(0.01)	(0.05)		
Left-Wing Mayor			0.05*	0.09
			(0.03)	(0.07)
Observations	3,144	338	3,148	340
R-squared	0.16	0.21	0.17	0.22
State and Election Year FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Mean Pre-Split	0.118	0.743	0.101	0.763
SD Pre-Split	0.323	0.438	0.301	0.426

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports political correlates of splitting decisions for the two waves of splitting in our data, 1993 and 1997. In Columns (1) and (3), the dependent variable is an indicator for having a split request in the municipality. In Columns (2) and (4), the dependent variable is an indicator variable for having split, and we restrict the sample to municipalities with some split request. We classify parties as left-wing following Zucco and Power (2023). Controls are log population, log area, log distance to the state capital, urbanization rate, Gini index, and the percentage of households with access to piped water and trash collection in 1991. We report robust standard errors.

Table D.10: Migration Effects of Splitting

	(1)	(2)	(3)
Split	0.16	0.19	-0.23
	(0.69)	(0.68)	(0.69)
Observations	220	220	220
R-squared	0.00	0.05	0.22
Controls	-	✓	✓
State FE	-	-	✓
Mean	9.8	9.8	9.8
SD	4.4	4.4	4.4

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the aggregate effects of splitting on migration after estimating Equation (4). Column (1) considers a regression without state fixed effects and baseline characteristics from Table 1 in the set of controls. Column (2) controls for baseline characteristics, while Column (3) further adds state fixed effects to the set of controls. We consider the fraction of residents who declare having lived in another municipality five years before as the dependent variable. The main data source is the decennial Demographic Census from 2000. We report robust standard errors.

E Difference-in-Discontinuities in Luminosity

Econometric Specification. Using the nighttime luminosity data at the district level, we estimate the following difference-in-discontinuities model in two stages:

$$Split_{m(d)} = \psi + \phi\mathbf{1}[RV_d \geq 50\%] + \kappa g(RV_d) + \eta_d \tag{17}$$

$$y_{dt} = \alpha_d + \alpha_t + \beta Split_d Post_{w(d)} + \gamma g(RV_d) Post_{w(d)} + X_{dt}\lambda + \varepsilon_{dt}. \tag{18}$$

From the first-stage Equation (17), we have that $Split_{m(d)}$ is an indicator variable for whether the municipality m with district d split after the referendum; RV_d represents the referendum vote share in favor of splitting in district d; $g(RV_d)$ is defined as a linear distance from the cutoff; and $\mathbf{1}[RV_d \geq 50\%]$ is an indicator for whether district d obtained at least half of votes in the referendum. The second-stage Equation (18) includes district and year fixed effects, α_d and α_t; and $Post_{w(d)}$, which is an indicator variable for the years after the wave-year w of splitting request. To account for fewer observations on the left side of the cutoff, our preferred specification considers a 15 percent bandwidth. The coefficient of interest, β, captures the effect of splitting. To support the validity of the research design, Appendix Table E.1 shows that most pre-referendum characteristics at the district level around the cutoff are continuous, except for population. To attenuate any bias in our estimates, we include interactions of 1991 population and year fixed effects as controls in the results below to allow for differential trends across levels of population.[30]

Panel (a) of Appendix Figure E.1 provides visual evidence of the first stage, confirming that having a simple majority determines splitting. Comparing applicant districts that barely obtained the majority of necessary votes to split to those that did not, Panel (a) of Appendix Figure E.2 displays a clear jump on the growth of log luminosity around the cutoff. Columns (1) and (3) of Appendix E.2 point to the Wald estimate of 28 percent (= 0.27/0.96). This effect is close to the difference-in-differences estimate restricted to the state of *Minas Gerais* (Column (4)). Concerning heterogeneity across districts, Panel (b) of Appendix Figure E.2 shows that the gains are driven by applicant districts.

[30] We use baseline characteristics from 1991. Panel (b) of Appendix Figure E.1 depicts the distribution of vote shares around the 50 percent cutoff. We note there are fewer districts with less than half of the voters.

Table E.1: Discontinuity Test on Pre-Referendum Characteristics

	Log Population	Log Area	Log Luminosity	Log Distance to Townhall
	(1)	(2)	(3)	(4)
Referendum Vote $\geq$ 50%	0.81***	0.18	0.58	-0.05
	(0.28)	(0.34)	(0.37)	(0.30)
Observations	50	50	50	50
R-squared	0.38	0.23	0.40	0.13
Mean Control	9.577	5.147	0.354	2.849

Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports point estimates from modified versions of Equation (17) to test for discontinuities in district-level characteristics prior to the referendum. We use these pre-referendum characteristics: log total population, log total area, log average luminosity, and log distance to the parent town hall.

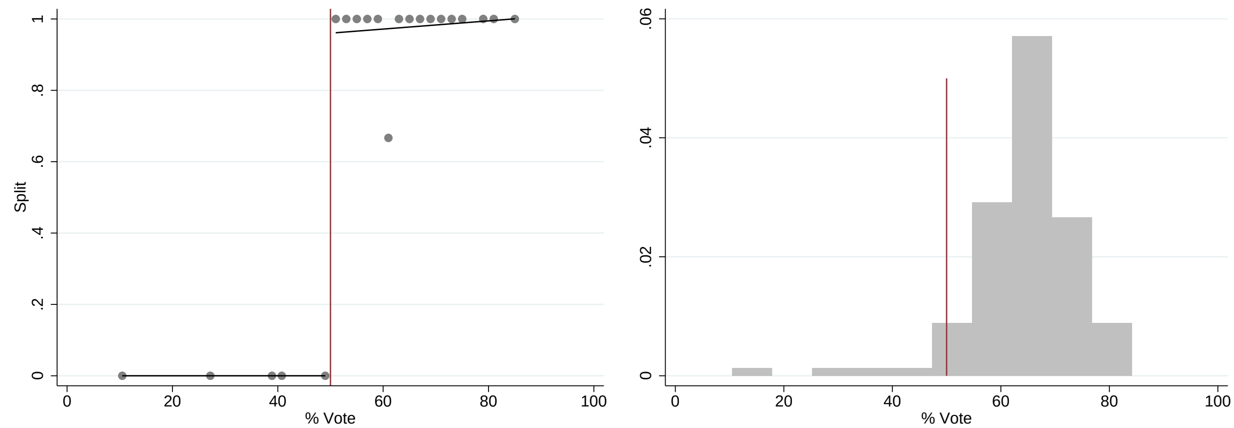
Table E.2: Effects of Splitting on Economic Acitivity

	First Stage	Reduced Form	Second Stage	DD
	(1)	(2)	(3)	(4)
Referendum Vote $\geq$ 50%	0.96***			
	(0.03)			
Post x Referendum Vote $\geq$ 50%		0.16***		
		(0.06)		
Post x Split			0.27***	0.26***
			(0.06)	(0.03)
Observations	50	985	985	2,422
R-squared	0.64	0.97	0.97	0.98
Mean Control	0	-1.001	-1.001	-0.802

Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports point estimates from the difference-in-discontinuities specification. Column (1) refers to the first stage from Equation (17), while Column (2) reports the reduced-form estimates. Column (3) refers to Equation (18). Column (4) speaks to the difference-in-differences estimates from Equation (3) restricted to the state of *Minas Gerais*. Except for Column (1), whose dependent variable is an indicator variable for splitting, the remaining dependent variables are log average luminosity.

Figure E.1: Referenda in *Minas Gerais*

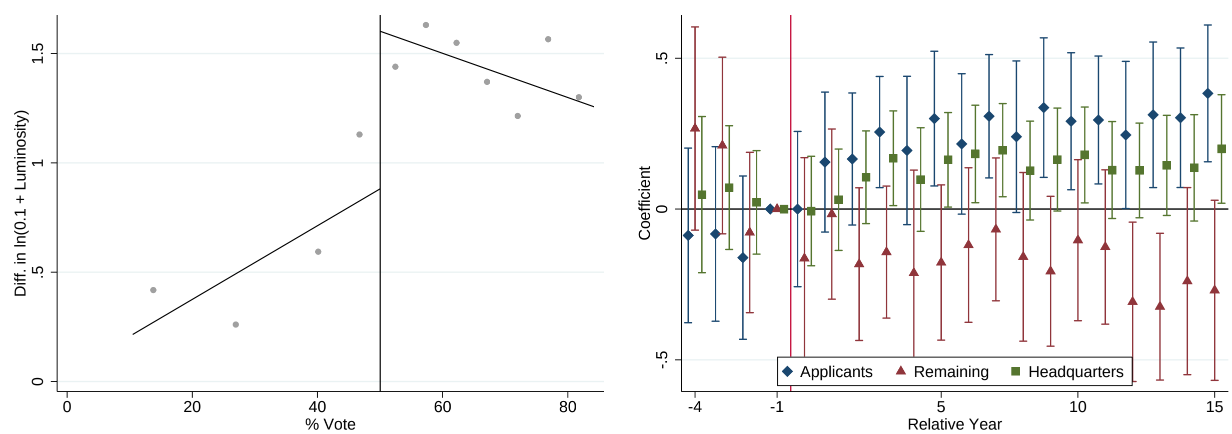
(a) The First Stage (b) Histogram of Vote Shares



Note: This figure describes the referendum data from Minas Gerais. Panel (a) plots the first stage of referendum votes on the likelihood of splitting. Panel (b) plots the distribution of vote shares. As described in Section 2, districts are required to obtain at least 50 percent turnout and votes in favor of splitting in the unilateral referendum as one of the steps to become a municipality.

Figure E.2: Effects of Splitting on Log Luminosity: Difference-in-Discontinuities

(a) Log Luminosity Growth (b) Log Luminosity: RD-DD Event-Study



Note: This figure reports results from specifications in Appendix Section E. Panel (a) plots the growth in log luminosity for applicant districts with share of votes from local referendum in favor of splitting below and above the approval cutoff of 50 percent. Panel (b) plots point estimates of the difference-in-discontinuities from Equation (18) for the applicant, headquarters, and remaining districts separately. The omitted category is the year before splitting.