Decentralizing Development: Evidence from Government Splits in Brazil*

Ricardo Dahis† Monash University

Christiane Szerman‡ University College London

July 3, 2023

Abstract

Changes in political boundaries aimed at devolving power to local governments are common in many countries. We examine the economic consequences of redistricting through the creation of smaller government units. Exploiting reforms that led to sharp variations in the number of government units in Brazil, we show that voluntary redistricting increases the size of the public sector, public services delivery, and economic activity in new local governments over the long-term. These benefits are not offset by losses elsewhere and are stronger in peripheral and remote backward areas that are neglected by their parent governments. We provide evidence that the decentralization of decision-making power boosts local development in disadvantaged areas beyond simply gains in fiscal revenues.

*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, Miguel Talamas, Edoardo Teso, Owen Zidar, 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, and at many conferences for comments and suggestions. We thank Renan Holanda and Cláudio Júnior for excellent research assistance, and Rudi Rocha for kindly sharing data. Christiane gratefully acknowledges financial support from the Economics Department and the Prize Fellowship in Social Sciences at Princeton University. This paper replaces an earlier version in Dahis and Szerman (2021).

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

‡Department of Economics, University College London. E-mail: c.szerman@ucl.ac.uk.

1 Introduction

Many countries have undertaken changes in political boundaries over the last 30 years with the goal of devolving power to local governments, and it is likely that more countries will follow suit (Grossman and Lewis, 2014). A large theoretical literature has put forward 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 it comes at the cost of proliferating new government units prone to capture by special interests and unable to fund their operations (Boffa et al., 2016). This trade-off could be made even worse if the new government units are too small to self-finance and require a subsidy from the rest of the country to pay the new bills (Alesina and Spolaore, 1997). This paper examines whether this is the case by assessing the long-run impacts of a large episode of redistricting through the creation of new local governments (henceforth "splitting").

Brazil provides an interesting empirical setting for studying the economic consequences of redistricting. First, the country is composed of municipalities that hold substantial administrative, fiscal, and political decision-making power. Municipalities consist of one or more districts with no decision-making power. Second, Brazil experienced one of the largest within-country splitting episodes worldwide. As a result of generous federal subsidies and lenient redistricting regulations between 1988 and 1996, the number of municipalities increased by 34 percent from 4,124 to 5,507. These newly-formed municipalities, previously districts, gained power and became responsible for roles designated to local governments, such as overseeing the provision of several public services, collecting local taxes, and managing fiscal revenues. Motivated by concerns about the rapid increase of new municipalities, a reform in 1996 curbed their proliferation. Features of the institutional context create useful quasi-experimental variations for identification. Third, the availability of rich information on public service delivery, economic activity, and fiscal performance provides an opportunity to evaluate the short- and long-run impacts.

Quantifying the economic impacts of redistricting is challenging because splits across the world are typically voluntary and reflect choices, and these choices may reflect underlying characteristics that also affect local development. In our context, areas requesting to split are less developed and neglected by their parent local governments. They 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 split to those that did not is unlikely to identify the causal impacts of redistricting.

To overcome this challenge and to document and characterize the selection into redistricting, we build a new dataset containing areas that requested to split. We collect and classify digitized historical archives of requests between 1988 and 1996. Our difference-in-differences design compares areas with ratified requests to untreated areas whose requests to split were not approved due to political reasons or the 1996 reform. Because *almost split* areas applied and failed to split for reasons unrelated to specific factors that could also affect local development, they form a counterfactual to those that ultimately split. We document that treated and control areas exhibit similar level and trends over various economic outcomes prior to splitting, lending support to the causal interpretation of our difference-in-differences estimates.

We find that redistricting improves public services delivery. Relative to their counterfactual, municipalities that split experience higher capital (e.g., machinery and buildings) and current (e.g., payroll and administrative costs) expenses by 27 and 15 percent in order to establish new local governments. This expansion of the public sector translates into improvements in public services delivery, including 1 and 4.4 percent increases in household access to sewage and trash collection. Consistent with higher levels of educational infrastructure, heterogeneity across age from the individual-level Census data also reveals that younger and, therefore, more exposed cohorts show the greatest improvements in school attendance and literacy rates.

Having demonstrated that redistricting leads to the growth of public sector and public services delivery, we turn to examine its effects on economic activity beyond the public sector. Using matched employer-employee records, we find no evidence of a similar expansion in the private sector. These aggregate impacts, however, mask substantial heterogeneity across economic sectors. We find an increase in new establishments in the retail and services sectors, suggesting some degree of structural transformation towards nontradable sectors. In addition, satellite records of light density at night confirm that municipalities that split experience an increase in economic activity (Chen and Nordhaus, 2011; Henderson et al., 2012; Pinkovskiy and Sala-i Martin, 2016; Henderson et al., 2018). Exploiting the fine spatial resolution, we disaggregate the municipality-level effect by estimating the effects for districts that voluntarily and involuntarily split. We uncover relevant distributional impacts: the increase in luminosity is concentrated in districts that voluntarily applied to split, whereas the remaining districts that involuntarily split are not affected.

Our main findings hold for a variety of robustness checks, including alternative definitions of outcomes, samples, and specifications with a large number of flexible controls to account for observable baseline differences between treated and control areas. We also observe similar patterns when breaking down our results by waves of splitting, strengthening the internal validity of our findings. Despite the robustness of our difference-in-differences findings, there remains the concern that unobserved factors influence our estimates. To address this concern, we propose a novel research design. Before 1996, areas requesting to split were required to conduct local referenda and obtain approval by simple majority. We utilize this rule in a difference-in-discontinuities design applied to Minas Gerais, a representative state where referendum results are available. By comparing areas that barely obtained the majority of necessary votes to split with those that did not, we document qualitatively similar results.

What can explain the gains in public services delivery and economic activity? One explanation is that redistricting results in higher fiscal revenues. Municipalities that split experience an average increase of 14 percent in revenues after splitting due to higher transfers from the federal government to the new local governments. These extra revenues might be spent on bureaucracy and infrastructure, leading to mechanical increases in public services and economic activity. The second possibility is that redistricting leads to the decentralization of decision-making power to new local governments. With administrative, fiscal, and political autonomy, they might be able to better address local needs.

We show that higher decision-making autonomy explains our results beyond simply gains in fiscal revenue. 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 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 and remote backward areas with more constraints to state capacity. This is one of the major goals of decentralization in developing countries (Bardhan, 2002). Third, turning to historical electoral data, we find that, after splitting, new municipalities elect politicians affiliated with different political parties than politicians from their parent municipalities. This suggests that new municipalities are better able to implement policies that reflect local preferences, another key premise of decentralization (Oates, 1972). Fourth, two complementary pieces of evidence weaken the role of extra revenues as the primary driver of local development. Our mediation analysis shows that extra revenues only account for a small portion of the improvements in economic outcomes. Additional ancillary exercises also reveal that, although areas that did not voluntarily split experience changes in fiscal revenues, their economic outcomes remain unchanged.

A final analysis investigates whether the policy imposes a negative burden to the rest of the country due to losses in resources. Leveraging state-level variation in loss of revenues from federal transfers caused by splitting, we find that municipalities without changes in political boundaries remain unaffected. Therefore, our results reject that redistricting reforms are a "zero sum game" among Brazilian municipalities, in which gains in economic activity in some areas come at the expense of other areas. Our findings are also consistent with the idea that lost revenues may have previously subsidized wasteful expenses with marginal value below the social costs of funds (Liebman and Mahoney, 2017), and the reallocation of resources may decrease low-value spending in areas that did not voluntarily split and raise aggregate welfare.

Our findings have relevant policy implications. First, while this paper does not quantify the optimal size of local governments, our results support that settings with large government units, like the Brazilian case, can benefit from subsidized and voluntary splits. We find that new municipalities drive the gains in public services and economic activity. At the same time, we find no clear evidence that the policy is detrimental to the rest of the country. It does not mean, however, that more splits are always advantageous. Second, we show that the benefits are largest for peripheral, remote backward areas, suggesting that redistricting through splitting can strengthen state capacity and achieve one of the main goals of decentralization: make local governments more responsive and efficient in promoting long-run development (Bardhan, 2002).

This paper contributes to several lines of research. At a broader level, it speaks to an extensive literature, theoretical and empirical, studying the causes and consequences of decentralization through redistricting.[1] Building on the seminal works of Tiebout (1956)

[1]We note that other terms have also been used to study decentralization (Oates, 1972, 1999; Bardhan,

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. Our contribution to empirical evidence on the actual consequences of redistricting is threefold. First, rather than leveraging cross-country or cross-sectional variations, we overcome the endogeneity of redistricting by proposing two complementary quasi-experimental approaches that generate qualitatively similar results. Second, we provide novel evidence on the distributional incidence of redistricting because our granular data allow us to examine heterogeneous impacts across and within areas involved in border changes, voluntarily and involuntarily (Gendźwiłł et al., 2020).[2] Third, the time horizon of the data permits an assessment of how the local impacts evolve over time. The richness of the data also allows us to quantify the impacts beyond the programs administrated by local governments, including whether higher public expenditures crowd-in the private sector, and to adjudicate between mechanisms hypothesized by theories of decentralization.[3]

The fact that peripheral, remote backward areas benefit the most from redistricting contributes to a large literature on the effects and spillovers 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). Related to the public sector, our context points that administrative remoteness and neglect from the headquarters regions correlate with regional inequality, and that splitting mitigates these frictions and gener-

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 reforms (Coate and Knight, 2007; Boffa et al., 2016; Bazzi and Gudgeon, 2021), amalgamations (Weese, 2015), and municipal cooperation and annexation (Schönholzer and Zhang, 2017; Ferraresi et al., 2018; Tricaud, 2022)

[2]In a recent review of papers studying on the economic consequences of border reforms, Gendźwiłł et al. (2020) illustrate that the main focus in this literature has been developed countries. We note that our setting, characterized by low state capacity, can also be informative to developing countries that have recently undertaken changes in political boundaries.

[3]Lima and Silveira Neto (2018) also compare municipalities that split and did not split to examine the impacts of splitting on fiscal outcomes in Brazil. Relative to this paper, we propose new empirical strategies that corroborate the difference-in-differences approach, examine both the average and distributional implications for local development, and scrutinize the mechanisms. At the national level, our results contribute 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).

ates subsequent positive and persistent economic consequences to peripheral regions.[4] 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). We advance this literature by evaluating the implications of reducing administrative remoteness.

Our empirical findings build on the literature investigating the determinants of state capacity (Besley and Persson, 2009, 2010; Acemoglu et al., 2015; Gennaioli and Voth, 2015; Johnson and Koyama, 2017). The within-country policy experiment we study changes the nature of the agency problems within government that limit state capacity (Mookherjee, 2015; Banerjee et al., 2020) and enables us to show how the size of local government influences economic outcomes. Evidence that the creation of new local governments can serve the dual purpose of expanding state capacity, like growing bureaucracy and implementing policies targeted to local conditions, in peripheral regions, and freeing these regions from capture of former governments is a novel result. Interestingly, these benefits do not require increased expenditures from higher levels of government, like the federal or state governments, or shifts in economic activity from one area to another.[5] This is relevant from a policy perspective given that countries often need to improve state capacity under severe budget constraints.

The rest of the paper is organized as follows. Sections 2 and 3 describe the institutional background and the sources of data. Sections 4 and 5 present the empirical strategy and the main results. In Section 6, we analyze potential mechanisms underlying the main effects. Section 7 discusses the net effects of redistricting. Section 8 concludes.

[4]Outside the public sector, several papers have documented that the distance between headquarters and branches in multi-location firms represents an important source of wage inequality across space (Gumpert et al., 2022; Hazell et al., 2022; Kleinman, 2022).

[5]Our finding that the growing bureaucracy is associated with improvements in public services delivery is also related to a large literature on the personnel economics of the state (Evans and Rauch, 1999; Finan et al., 2017; Besley et al., 2022; Dahis et al., 2023).

2 Institutional Background

2.1 The Role of Municipal Governments

Brazil has three tiers of government holding administrative, fiscal, and political power: federal, state, and municipal governments. Municipalities are the smallest government units with decision-making power. Each municipality is divided into one or more districts, which are purely administrative subdivisions and do not have any political autonomy. No district belongs to two different municipalities.

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).[6] Since 1988, municipalities are responsible for overseeing the provision of several public services, including primary education, basic health care, sanitation, trash collection, and street lighting services. Municipalities share mandate on provision of certain public services, such as sanitation and health care. For other services, like primary education, they are the sole provider. In addition, municipalities have fiscal autonomy to collect and manage local taxes (e.g., property and service taxes) and to 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 councillors.[7] In January after elections, the elected officials take office.

[6] As opposed to *horizontal* decentralization, which consists of government units breaking into smaller ones with the number of tiers and the allocation of functions held constant, *vertical* decentralization implies the creation of new tiers of government or the transfer of functions from a higher tier to a lower one. In addition, administrative decentralization generally indicates that different government tiers execute various functions and policies. Superior tiers may reverse and overrule decisions made by lower ones. Political decentralization includes *appointment* decentralization, *decision-making* decentralization, or *constitutional* decentralization. Fiscal decentralization involves decision-making decentralization on taxation and expenditure matters (Ebel and Yilmaz, 2002; Treisman, 2007).

[7] In municipalities with fewer than 200,000 voters, there is a single-round system, and 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 most votes wins. Municipal councillors are elected through an open-list proportional representation system.

2.2 The Creation of New Municipalities

The 1988 Federal Constitution also granted to states the authority to establish their own 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 formally request to state assembly the creation of 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 put forward for voting in the state legislature; (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 ultimately becoming municipalities.

In light of the rapid rise in number of new municipalities and the subsequent concerns that such splits were inefficient and driven by local patronage, the Congress enacted the Constitutional Amendment 15/1996 (henceforth "1996 CA"). The 1996 CA reassigned to the federal government the authority to regulate over the creation and amalgamation of municipalities. Three major changes stand out. First, districts requesting to split have to conduct a referendum with the entire municipality, not only with the applicant districts, and obtain approval from the majority of voters. Second, the federal government requires evidence of fiscal sustainability from the applicant districts. Third, redistricting would depend on a further, albeit never enacted, legislation by the federal government. As a result, the 1996 CA induced a *de facto* halt in the creation of new municipalities and left various split requests from districts open and unapproved.

Once the request to split is approved, the applicant district (or group of applicant districts) is established as a new municipality after the ensuing municipal elections, when the elected mayor and municipal councillors take office. Consistent with flexible redistricting regulations, Figure 1 displays a 34 percent increase in the number of municipalities between 1989 and 1997, jumping from 4,124 to 5,507. We note two two main waves of splitting before the 1996 CA, both in 1993 and 1997, right after municipal elections.

2.3 The Reasons for Splitting

Many factors have contributed to the redistricting process in Brazil. We highlight two factors: neglect from the headquarters and fiscal incentives. Several past studies have put forward large disparities in the provision of public services across districts within a municipality as a key driver of split requests (Cachatori and Cigolini, 2013; Klering et al., 2012). In a survey with mayors in 1992, Bremaeker (1993) confirms that most respondents reported neglect by local governments (63 percent) and large territorial size of local governments (24 percent) as the main reasons for splitting.

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 has the following allocation mechanism: every year, 22.5 percent of total revenues from federal income and industrial product taxes are reserved for FPM. Each state receives a block grant to be shared between its municipalities, implying that transfers are zero-sum within state. Each municipality then obtains a share determined by a concave step-wise population-based formula that assigns coefficients to population brackets. In practice, the formula disproportionately benefits small municipalities because transfers per capita are larger in lower population brackets. Municipalities within the same state and bracket obtain the same amount of transfers.[8] In addition, 15 percent of FPM transfers are earmarked for education and health each, and the rest is unearnmarked (Brollo et al., 2013). On average, federal transfers account for between 30 and 60 percent, whereas local taxation and fees represent 5 percent of total municipal revenues.

When splits occur and new municipalities are established, they start receiving FPM transfers. Most splits are concentrated in small municipalities, pointing to fiscal incentives as a relevant driver of split requests. We note, however, that the net change in FPM transfers for the headquarters and remaining areas is unclear because it depends on several factors: the allocation of funds within the municipality prior to splitting, the curvature of the FPM curve, and the quantity of splits within the state.

To evaluate the different forces at play and their empirical predictions, Appendix Section A outlines a simple conceptual framework in which the municipal headquarters choose the allocation of public goods across districts within the municipality. The model

[8] 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.

illustrates two key predictions to motivate our empirical analysis. First, districts applying to split may benefit more from it if they are neglected by local governments or experience larger fiscal incentives. Second, the consequences of splitting for headquarters districts and the rest of the country may be negligible.

3 Data

This paper uses a newly collected data on split requests, together with different sources of spatial and administrative data, to capture public service delivery, economic activity, and fiscal performance margins along which redistricting impacts local development.[9]

Split Requests. We gather information on the official creation dates of municipalities, along with their parent municipalities prior to split, from the Brazilian Institute of Geography and Statistics (IBGE). To catalog split requests, we collect and classify historical archives of such split requests. The availability, level of detail, and quality of these archives largely vary across states because state assemblies set their own redistricting requirements prior to the 1996 CA. The final data include split requests, regardless of their final approval status, initiated by districts from 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 Brazilian population and 63 percent of splits between 1989 and 1996. The remaining states do not provide public records on split requests. Section C of the Appendix describes the data collection in detail.

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, which include information on turnout and percentage of valid votes in favor of splitting. We validate information from reports by cross-checking them with our data on split requests.

Demographic Census. 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

[9]To our knowledge, there is no historical data on housing prices.

decennial Brazilian Demographic Census, and are only available at a decadal frequency. We use the 1991, 2000, and 2010 versions sourced from IBGE and the Atlas of Human Development (United Nations Development Program, 2013). For our baseline specification, we aggregate data at the municipality level because district identifiers are not available. We also use individual-level microdata on literacy and school attendance when exploiting variation across birth cohorts.

Formal Labor Market. We draw labor market information from the annual matched employer-employee data, the *Relação Anual de Informações Sociais* (RAIS), carried out by the Brazilian Ministry of Labor. The data cover the entire formal sector between 1995 and 2018, and provide a rich set of worker, job, and establishment characteristics. We use worker-level data to calculate the total number of employees and establishments in the public and private sectors at the municipality level. We also generate these variables by economic sector (i.e., agriculture, mining, manufacturing, construction, retail, and services) and areas (e.g., education and health). An important caveat is that we are not able to examine the impacts on the informal economy due to the lack of data covering the informal sector prior to 2000.[10]

Night Lights. To measure economic activity, we use satellite imagery of night-time lights organized by the U.S. National Oceanographic and Atmospheric Administration (NOAA) and the National Geophysical Data Center (NGDC).[11] The annual data consist of grids with integer values ranging from 0 (no light) to 63 that record the intensity of lights between 1992 and 2013.[12] Taking advantage of its granularity, we construct district-level information on the intensive and extensive margins of luminosity, measured by the weighted average of lights across grids within a district and whether this average is above zero. To quantify regional inequality, we calculate a luminosity Gini index from variation across pixels within each district.

[10] The 2000 Census is the first edition with information on the informal sector. According to the Ipeadata, the informal sector accounted for 56 percent of total employment in 1992.

[11] Intensity of night lights measures both outdoor and some indoor use of lights. Henderson et al. (2012) and Henderson et al. (2018) show that night lights are a good proxy for long-term GDP growth. This is useful in our context because there is no data on economic activity at the district level, and data on electricity consumption are only available for more recent years.

[12] A grid cell captures a 30 arc-second output pixel or 0.86 square kilometers at the Equator.

Local Government Expenditures and Revenues. We collect information on expenditures and revenues at the municipality level from the Brazilian National Treasury. Available since 1989, the data details revenue sources (e.g., local taxation and intergovernmental transfers) and expenditure categories (e.g., capital and current expenses).[13]

Other data. We rely on minor sources of data to conduct our analyses. We gather geographic characteristics, such as soil suitability from FAO-GAEZ and terrain ruggedness from Shaver et al. (2019). To disentangle several mechanisms behind the results, we also use municipal-level electoral data from the Superior Electoral Court (TSE). Between 1988 and 1996, we only observe the elected mayor's name and party. Other information, like the list of mayoral candidates and vote shares, started to be reported in 2000.

Municipality-Level Sample. To build our main estimation sample, we begin with 4,298 municipalities from the 1991 Demographic Census and keep those that meet three criteria: (1) municipalities that belong to one of the 11 states with records on split requests; (2) municipalities with either a single split event or with districts having split requests between 1989 and 1996 to avoid multiple events; and (3) municipalities that are not state capitals because they also serve as the headquarters of state governments. The restrictions yield a final municipality-level sample of 448 municipalities.[14]

District-Level Sample. Due to data availability, we perform most of our analysis at the municipality level. We note, however, that the nighttime luminosity information are available at the district level, permitting additional insights on differences within and across municipalities. Starting with 8,855 districts from the 1991 Demographic Census, we apply restrictions similar to before to construct a district-level sample.[15] The restric-

[13]To our knowledge, there is no data on local tax rates during the 1990s. Anecdotal evidence, however, suggest that changes in local tax rates are uncommon and negligible.

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

[15]We keep districts that meet the following criteria: (1) districts that belong to one of the 11 states with records on split requests; (2) districts that do not belong to state capitals; (3) municipalities with requests to split initiated by districts, as opposed to areas smaller than districts, 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.

tions leave us with a final sample of 1,259 districts. We next classify them into three groups: (1) *applicant* districts, which correspond to peripheral districts that requested to split; (2) *remaining* districts that contain peripheral districts that did not request to split but were located in municipalities where some district did so; and (3) *headquarters* districts, which cover districts serving as headquarters in municipalities that have a district requesting to split. Such classification leads to a final district-level sample of 552 applicants, 325 remaining, and 382 headquarters districts.

4 Empirical Strategy

We begin this section by characterizing the selection into redistricting. We then explain how we construct a credible counterfactual to areas that split, and discuss the difference-in-differences design we use to examine the effects of splitting.

4.1 Who Applies to Split?

To examine how municipalities select into splitting, Table 1 presents summary statistics of baseline characteristics in 1991 for the sample at the municipality level. The numbers confirm that, prior to 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. The exceptions are that municipalities with applicant districts are bigger in population and area, have slightly lower levels of public services, and receive lower share of federal transfers relative to total revenues. In contrast to countries like Indonesia or India (Pierskalla, 2016; Bazzi and Gudgeon, 2021), differences in racial and religious composition are small in magnitude, ruling out social fragmentation as an important driver of splitting.

We also examine how districts select into applying to split. Our district-level sample corroborates that districts requesting to split are less developed than other parts of the country. Comparing baseline characteristics between applicant (Column (1)) and headquarters districts (Column (5)), Table 2 shows that applicant districts display, on average, worse economic and demographic conditions before splitting. They are also smaller in population and area, less urban, and located farther from their parent town halls.[16]

[16]Interestingly, we find that applicant districts are larger and more developed than remaining ones (Col-

4.2 Identification

Our goal is to examine how redistricting affects economic performance. To mitigate concerns related to selection into splitting, our estimation sample includes municipalities with an application to split. We then define municipalities containing a district that applied and failed to split as the control group. We note that these *almost split* municipalities form a credible counterfactual to those that ultimately split.[17] With initially good chances of approval, their requests were not sanctioned for reasons unrelated to specific factors also affecting economic performance. Reasons include vetoes from state legislative committees or governors, referenda without support from the majority, and the 1996 CA, which left requests initiated in 1994 and 1995 open without enough time to conclude the multi-stage process outlined in Section 2.2.[18]

The treatment group consists of municipalities that split. The control group with almost split municipalities includes never-treated units (i.e., units that applied to but never split) and excludes not-yet-treated units (i.e., units that applied to and split after the waves of 1993 and 1997). This division implies that the sample of 448 municipalities contains 324 split units and 124 almost split units. Figure 2 plots split and almost split municipalities. We highlight two patterns. First, requests to split are geographically scattered. Second, despite some degree of clustering due to redistricting regulations at the state level, we note a large geographical variation in split and almost split events.

We also apply similar classifications to our district-level sample of 552 applicants (441 split and 111 almost split units); 325 remaining (261 split and 64 almost split units); and 382 headquarters districts (292 split and 90 almost split units). Appendix Table D1 presents means for districts' baseline characteristics in 1991 across treatment status and split waves (1993 and 1997). Relative their almost split counterparts, districts that split have, on average, smaller population, larger area, and are located farther from their parent town halls. We also observe some degree of negative selection into splitting over time

umn 3). Columns (7)–(10) also display summary statistics for districts outside the estimation sample. Districts who are not involved in splits are similar to those who are in most dimensions excluding population, area, urbanization, and terrain ruggedness.

[17]Figure 2 presents a simple diagram that compares split (blue) and almost split (orange) municipalities, as well as illustrates how municipalities are divided into applicant, remaining, and headquarters districts.

[18]Table 5 of Tomio (2005) provides statistics about which stage the requests to split were denied in the state of Rio Grande do Sul. Out of 398 requests, 64 percent of them ultimately passed; 10 percent were still left open; 13 percent were rejected in legislature commissions; 5 percent were rejected by the legislature's plenary; 6 percent were vetoed by the state governor; and 2 percent had the majority of local referendum voting against the split.

since districts involved in the latter wave have worse economic conditions.

4.3 Main Econometric Specification

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

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

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 the 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). 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 dynamics effects of splitting relative to that year. The variable $Post_{mt} \equiv \mathbf{1}[t \geq W_m]$ indicates time periods after the municipality's wave-year. Standard errors are two-way clustered both 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. The inclusion of almost split municipalities assuages concerns related to event-study specifications that only rely on the variation in the timing of treatment (Goodman-Bacon, 2021; Borusyak et al., 2022). In addition, because our data only contain two waves of splits, our results are also unlikely to be affected by issues on the variation in the timing of treatment raised by the recent difference-in-differences literature (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021). Indeed, our robustness checks show similar patterns when we break down our results by waves of splitting.

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 test this assumption by assessing whether the pre-event coefficients of interest are statistically indistinguishable from zero.

Even restricting the sample to municipalities that applied to split and attesting parallel 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, the inclusion of municipality fixed effects α_m mitigates concerns related to time-invariant characteristics of municipalities that might 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 provide reassuring evidence that we estimate the impacts of redistricting rather than the impacts of unobservable municipality-specific factors. Fourth, one of our robustness checks accounts for heterogeneous initial characteristics that can also influence the 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.

To further rule out unobservable factors influencing our estimates and validate our findings, we leverage an extra feature from the institutional context. Prior to 1996, districts requesting to split had to conduct local referenda and obtain approval by simple majority. As a robustness check, we complement our difference-in-differences approach with a difference-in-discontinuities design exploiting final results from local referenda. Section 5.3 shows that both empirical approaches generate qualitatively similar estimates.

5 Main Results

We start by documenting how new municipalities set up local governments and to what extent redistricting improves public services delivery. We then show that redistricting has positive economic impacts beyond the public sector. We also find relevant distributional consequences: applicant districts drive the gains in economic activity, whereas the remaining and headquarters districts remain unaffected along this margin.

5.1 Setting up New Local Governments: The Effects on Public Services

Bureaucracy in the Public Sector. We examine how new municipalities set up new local governments. Panels (a) and (b) of Figure 3 show $\hat{\beta}_\tau$, along with 95 percent confidence intervals, after estimating Equation (1) for selected variables capturing local expenses. Appendix Table D2 displays the aggregate impacts. The pre-event coefficients are statistically equal to zero, supporting the assumption that both split and almost split have similar pre-split trends. Following splitting, treated municipalities experience a sharp increase in public expenses.

Panel (a) of Figure 3 displays results for capital expenditures per capita. These expenses, which account for 16 percent of total municipal expenditures, refer to purchases of machinery, vehicles, buildings, and the like. We find a spike at around 40 percent in the year of splitting, followed by a stable increase of around 27 percent over the next 15 years. Panel (b) reports the results for current expenditures, which represent 84 percent of municipal expenditures and capture maintenance and operation costs of providing public service (e.g., payroll and administrative costs). Following splitting, current expenditures in treated municipalities increase by about 15 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 installation and entrance costs. Strict rules prohibiting indiscriminate hiring in the public sector also explain the stable trends in current expenditures, mostly payroll costs, after splitting.

We also turn to the richness of the matched employer-employee RAIS data to validate the previous findings. We quantify the impacts on the size of the bureaucracy in the public sector, measured by the number of public employees and total payroll in the local government. Panels (c) and (d) of Figure 3 and Appendix Table D2 report that splitting is associated with an average increase of around 23 percent in both dimensions. At the same time, we find no changes in public employment at the state and federal governments, confirming that the growth of the public sector exclusively comes from new municipalities.

Public Services Delivery. We next investigate to what extent the growth of the public sector influences public services delivery. Although the decennial Census data permit a rich analysis of multiple margins along which splitting affects public services delivery, an important caveat is that we are not able to directly test for pre-trends because we can only use one data point, the 1991 Demographic Census, prior to the 1996 CA. We note that the lack of pre-trends for other outcomes with higher frequency from alternative data sources, like the RAIS data, helps alleviate this concern.

Figure 4 and Appendix Table D3 report coefficients after estimating Equation (1). We document that household access to trash collection and sewage increases by 4.4 and 1 percent (the former is significant at the 10 percent level), whereas we do not find significant impacts on household access to piped water and electricity. Interestingly, the impacts are weaker for public services whose mandate on provision is shared with the state and federal governments, such as water and sanitation sectors. These results are consistent with shared mandates generating lower investments in these public services due to uncertainty about which level of government is ultimately responsible for their provision (Kresch, 2020).[19]

We also employ a complementary empirical approach to estimate additional margins of response. Because pre-split data on public goods from the early 1990s, such as education and health infrastructure, are unavailable, we propose an indirect test exploiting variation in splitting across municipalities and birth cohorts (Duflo, 2001). If splitting causes an increase in the stock of schools, and both the year of birth and municipality of residence determine exposure to it, then younger cohorts more exposed to splitting would experience higher levels of schooling relative to older, less exposed cohorts.

Exploiting the Census data at the individual level and adapting Equation (1) to consider heterogeneity by age, we estimate the following specification :

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

in which y_{imst} represents outcomes for person i in municipality m, state s, and year t; α_{st}, $\alpha_{k(i)m}$, $\alpha_{k(i)t}$ are state-time, age-municipality, and age-time fixed effects, respectively; $Split_{mt}$ is an indicator variable for whether the municipality m split, and takes values equal to zero for $t = 1991$ and equal to one for years $t \in \{2000, 2010\}$ in municipalities that split; and $\mathbf{1}[k(i) = \tau]$ are dummies for each age. The term X_i refers to a vector of individual controls, such as gender, race, religion, and nationality. Standard errors are two-way clustered both at the state and split wave levels. The key identifying assumption is that educational outcomes for birth cohorts in split and almost split municipalities would have followed parallel trends if no splitting had occurred for treated municipali-

[19] Using municipal-level Census data, we find that municipalities that split experience higher literacy rates and years of education. We also find increases in preschool and middle school attendance, while we do not observe a 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, whereas state governments are in charge of high schools.

ties.

Consistent with higher investments in educational infrastructure, we document that younger cohorts from municipalities that split experience higher gains in school attendance and literacy rates. Figure 5 displays the $\hat{\beta}_\tau$, along with 95 percent confidence intervals. Panel (a) shows that splitting is associated with increases ranging between 2 and 5 percentage points in school attendance, whereas Panel (b) points to increases of up to 5 percentage points in literacy rates for individuals below 16 years.[20] Using RAIS data, Appendix Figure D3 further shows a crowd-out of employment from non-profit to government organizations in the educational sector, confirming that the higher levels of education come from increased public investments after splitting.

5.2 Beyond the Public Sector: The Effects 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 the RAIS data, we estimate Equation (1), which directly compares the number of private establishments and jobs in the formal sector in treated and control municipalities, before and after splitting shocks. Panels (a) and (c) of Figure 6 and Appendix Table D4 illustrate the dynamic and aggregate impacts around splitting. The point estimates are positive, but we cannot reject null effects, implying that the private sector does not expand in the same degree as the public sector. The aggregate results, however, mask substantial heterogeneity across economic sectors. Panels (b) and (d) of Figure 6 indicate some degree of structural transformation towards nontradable sectors since the majority of new establishments come from the retail and, to a lesser extent, services sectors.

Nighttime Luminosity. Thus far, the empirical results point to 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 satellite-recorded nighttime lights (Chen and Nordhaus, 2011; Henderson et al., 2012; Pinkovskiy and Sala-i Martin, 2016; Henderson et al., 2018). We note that the lack

[20]These effects are not bounded by top censoring of school attendance or literacy rates. To put the numbers into perspective, we plot the average school attendance and literacy rates for different years and ages in Appendix Figure D2. The rates are far from 100 percent in 1991: school attendance peaks at 82 percent (at the age of 11), whereas the maximum of literacy rate is 87 percent (at the age of 15).

of evidence of improvements in household access to electricity in Section 5.1 indicates that nighttime lights are unlikely driven by street lights. Similar to the previous findings, Panel (a) of Figure 7 shows that nighttime luminosity quickly grows in the first five years after splitting. Over time, the growth becomes stable and persistent, with an increase of around 12 percent.

The aggregate results at the municipality level, however, are limited in illustrating the distributional implications of redistricting *within* municipalities. For instance, the gains from redistricting may be uniformly distributed across districts. Or, alternatively, the gains may be asymmetric and exclusively driven by successful applicant districts, whereas other districts remain unaffected. Understanding the overall distribution of economic activity is key to shedding light on 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:

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

in which subscripts d, m, s, and t stand for district, municipality, state, and year; and α_d represents district fixed effects. The remaining variables are similar to Equation (1) with the exception that, rather than municipalities, the subscripts represent districts. As before, standard errors are two-way clustered both at the state and split wave levels.

Panel (b) of Figure 7 plots the dynamics of nighttime luminosity around splitting separately for the applicant, headquarters, and remaining districts, whereas Appendix Table D5 reports the aggregate estimates.[21] We highlight three main patterns. First, the pre-event coefficients are statistically close to zero, lending credibility to the research design. Second, applicant districts strikingly experience a sharp growth in luminosity right after splitting. The growth peaks at about 40 log points between 5 and 8 years later, and becomes stable, with a 34 log points (or 39 percent) increase 15 years after splitting. Third, the estimates for the remaining and headquarters districts, who are involved in involuntary splits, are statistically insignificant. Exploiting additional margins of luminosity, Panels (c) and (d) of Figure 7 indicate a 4 percent increase in lit pixels, which capture the extensive margin of luminosity, and a 3 percent decline in luminosity Gini index for applicant districts. The latter is consistent with a decline in spatial inequality. We find no

[21] We add 0.1 to the average luminosity so that its log is defined for all districts.

significant changes in the intensive margin for the remaining districts. If anything, the headquarters districts experience *positive*, albeit much smaller, impacts along this margin. These results indicate no shifts in economic activity from headquarters and remaining districts to successful applicant districts and, therefore, reject that the policy is a "zero sum game" among districts. Instead, they point to aggregate welfare gains.

5.3 Robustness Checks

We conduct some additional checks to ensure that our findings are robust to alternative definitions of outcomes, samples, and specifications. Table 3 reports the robustness checks. For brevity, we limit our attention to district-level 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. The point estimate is remarkably similar to Column (1). Column (5) controls for trends specific to local economies by adding micro region-by-year fixed effects.[22] Column (6) alternatively controls for baseline characteristics from Table 1 interacted with year fixed effects, permitting 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.

Two additional issues could threat our main identification strategy. First, one could be concerned that the splitting treatment may be correlated with other concurrent shocks, confounding the estimated effects. For instance, splitting may result in new government programs, and thereby affecting the outcomes of interest. We are not aware of such shocks in Brazil. We also note that it is unlikely that the exact timing of differential shocks happening in split and almost split municipalities coincides with the timing of the splitting.

The second concern is that selection into splitting along unobservable factors, such as

[22] Micro regions delineate local economies with similar socioeconomic and historical characteristics and are equivalent to commuting zones in the US. In addition, results for remaining and headquarters districts as well as other outcomes are available upon request.

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. Prior to 1996, districts applying to split had to conduct local referenda and obtain approval by simple majority. We leverage this rule in a difference-in-discontinuities design applied to a large and representative state, Minas Gerais, where referendum results are available, to compare districts that barely obtained the majority of necessary votes to split to those that did not.[23] Appendix E describes the research design in detail and confirms that both the difference-in-differences and difference-in-discontinuities strategies lead to qualitatively similar conclusions, strengthening the validity of our main research design.

6 Drivers of Local Development

Our results point that redistricting boosts local development by fueling bureaucracy in the public sector, public services, and economic activity. We also find evidence that these gains are driven by successful applicant districts. Using observational data and key predictions from a simple model of public goods provision under redistricting outlined in Appendix Section A, this section assesses to what extent our results can be attributed to larger fiscal revenues or to decentralization of decision-making power to new local governments.

6.1 The Role of Fiscal Revenues

We scrutinize the sources of financing new local governments by investigating the impacts of redistricting on fiscal revenues, 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 8 shows the dynamics of local revenues around splitting events. The pre-event coefficients are statistically equal to zero, lending support to our empirical strategy. Immediately after splitting, there is a sharp increase in revenues, a pattern that becomes stable over time.

Appendix Table D6 displays the aggregate impacts. Column (1) indicates an increase

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

of 14 percent in local revenues after splitting. Consistent with the institutional context, Column (2) points that this result is predominantly driven by the increase in federal transfers per capita due to the funding allocation mechanism. Concerning the impacts on tax revenues per capita, Column (3) shows a growth of 11 percent, though we cannot reject a null effect. Along with the lack of increased inflow of population following splitting, this finding indicates a limited role of local taxation as an asset to fiscal capacity in weak states (Balan et al., 2022), reinforcing the importance of non-taxes revenues in building state capacity for peripheral regions.

The fact that splitting leads to increases in federal transfers to newly created municipalities implies an unintentional large subsidy to fund their operations, like infrastructure and bureaucracy. An important question is to what extent our previous findings on public services and economic activity are driven by the increased fiscal revenues. We propose two exercises. First, we implement a "horse-race" approach, in which we add total expenses to the set of controls from Equation (1).[24] By holding expenses fixed when comparing split and almost split municipalities, we test whether the coefficient associated with splitting approaches zero in case the increased transfers explain the positive impacts on economic outcomes. Odd columns of Table 4 replicate selected baseline results, whereas even columns report the coefficients after controlling for total expenses. The small changes in the point estimates and in the R-squared values indicate a limited role of increased expenses on explaining our main findings.

The second approach overcomes the lack of information on fiscal revenues at the district level by assuming that, prior to redistricting, municipal expenditures and revenues are proportionally shared among districts based on population. Comparing Columns (1) and (2) of Table 5, the inclusion of predicted expenditures, rather than actual expenditures, at the district level in the set of controls barely alters the effects on luminosity: the point estimate declines from 0.34 to 0.30. These results together suggest that the increased fiscal revenues do not explain the bulk 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 decentralization of decision-making power in justifying our results.

[24]We control for total expenses, rather than total federal transfers, because there is little variation in transfers among municipalities that almost split, as shown in Appendix Figure D1d, and expenses strongly correlate with revenues ($\rho = 0.99$). We also observe a strong correlation between total expenses and federal transfers ($\rho = 0.44$).

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, like trash collection and primary education. In addition, we find no evidence that such gains of 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, with the limitation that this exercise only allows a suggestive glimpse into theories due to data constraints.

The Mechanism of Curtailing Capture and Neglect. A key source of inequality across space is that the decision-making process regarding the allocation of resources and burdens usually 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 with Brazilian mayors in 1992 confirming that neglect by parent local governments and geographical distance to the headquarters are the most common motivations for splitting (Bremaeker, 1993).[25] One of the premises of decentralization is to curtail the influence of capture and neglect in peripheral regions, advancing policies better aligned with local needs (Oates, 1972; Bardhan, 2002; Mookherjee, 2015).

One challenge to investigating whether this happens is that capture and neglect are difficult to measure. We propose an indirect test that examines whether the gains in economic activity are stronger in areas with higher propensity to capture and neglect prior to splitting. Because they 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 different dimensions. In line with decentralization benefiting vulnerable areas, Columns (3) and (4) of Table 5 reveal that the gains in luminosity accrue to peripheral areas previously located

[25] Other works have shown that administrative remoteness in form of geographical distance to the headquarters limits the provision of public services and local development more broadly (Krishna and Schober, 2014; Asher et al., 2018). For instance, it may reduce the amount and the 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 prone to observing citizens' preferences.

farther from their parent town halls and with 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 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 fail to be internalized (Lipscomb and Mobarak, 2017). However, decentralization can also be beneficial in terms of increased political accountability, since local governments have incentives to tailor policies aligned with the local needs, increasing social welfare and influencing electoral outcomes (Seabright, 1996). Because elections are imperfect instruments of political accountability, we can assess the role of politics in explaining our findings (Bordignon and Minelli, 2001).

Leveraging limited information from the electoral data during the 1990s, we scrutinize the electoral results across applicants and headquarters districts. Figure 9 shows that, in most times, the applicants and headquarters districts elect mayors from *different* parties following redistricting. Immediately after splitting, we observe this divergence for about 75 percent of results, and this pattern grows to nearly 85 percent two decades later. This finding speaks to the literature studying the politics of decentralization (Grossman et al., 2017; Pierskalla, 2016), especially to basic models of representative politics, in which elected officials reflect local preferences for policies (Persson and Tabellini, 2002).[26,27]

The Lack of Migratory Responses. We test whether people "vote with their feet" by examining migratory responses to public good provision (Tiebout, 1956). Specifically, we investigate whether municipalities that split and, as a result, experience improvements in public services delivery attract more individuals from elsewhere.[28] Table D7 indicates no

[26]For instance, Myerson (2006) illustrates that decentralization may increase yardstick competition between jurisdictions, raising the quality of politicians and improving the chances of selecting capable administrators. Boffa et al. (2016) argue that centralization has the advantage of combining regions with diverse informed voters, which increases the average level of information and limits rent-seeking.

[27]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 on the level of electoral competition.

[28]An important caveat is that the 2000 Census is the first edition to collect information on migration across municipalities. We rely on the question of whether the individual lived in a different municipality

evidence of migration as a relevant margin of response to redistricting in our context.

7 The Net Effects

Thus far, our results indicate that subsidized voluntary splits bring positive economic impacts for new municipalities. At the same time, we do not find evidence that other areas who involuntarily split are also affected along these margins. An important question for the distributional consequences of this policy is whether the municipalities that did not split ultimately experience negative impacts due to losses in fiscal revenues resulting from the allocation mechanism of the federal transfers.

To account for spillovers to the rest of the country, our first exercise exploits variation in the number of municipal splits within states. Appendix Figure D1e shows that states with more splits experience larger losses in federal transfers ($\rho = -0.67$). It motivates the correlation test between changes in federal transfers and selected outcomes, including public and private jobs, number of establishments, and average luminosity.[29] Table 6 and Appendix Figure D4 indicate no evidence that lower revenues are associated with worse measures of local development. The only exception is an increase, although small in magnitude, in luminosity.

What can explain the lack of spillover effects onto municipalities that did not split? We argue that the findings are very consistent with a model with decreasing returns to spending, in which local governments engage in wasteful expenses whose marginal value is below social cost of funds (Liebman and Mahoney, 2017). Even when the redistricting process implies less funds available to municipalities that did non split, they may hold back low-value spending and manage these funds more efficiently. Because the social cost of these lost funds exceed their social value, the spending cuts may not be large enough to worsen economic outcomes.

five years before to make a cross-sectional comparison between split and almost split municipalities around the 1997 wave of splitting. We run the following specification:

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

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

[29] Appendix Figure D1 shows that, on average, a one-percentage-point increase in population residing in new municipalities implies that non-split municipalities experience a 2.1 percentage points decrease in federal transfers.

Our second exercise is a back-of-the-envelope calculation asking what the return-per-dollar of transfers should to be for municipalities that did not split to generate a positive social value of redistricting and the subsequent reallocation of federal transfers. The change in social value can be written as:

$$\begin{aligned}\Delta V &= \Delta V_R + \Delta V_N + \Delta V_{NS} \\ &= \Delta T_R \times R_R \times \alpha_R + \Delta T \times (R_N \times \alpha_N - R_{NS} \times \alpha_{NS}),\end{aligned}$$

in which subscript R stands for the groups of remaining and headquarters districts that split; N stands for new municipalities; and NS stands for non-split municipalities; α_i represents population shares for each group $i = R, N, NS$; ΔT denotes the change in percentage points in the reallocation of federal transfers by the time of splitting; ΔT_R captures the change in federal transfers for remaining and headquarters districts; and R_i indicates the total returns-per-dollar of federal transfers for each group i.

The reallocation of federal transfers is still beneficial if $\Delta V > 0$. To uncover the maximum value of returns for non-split municipalities, $\bar{R}_{NS}$, needed to justify the redistricting process and the resulting reallocation of federal transfers, we map the returns-per-dollar in remaining and headquarters districts, R_R, and in new municipalities, R_N, into the district-level effects on luminosity from Section 5.2. We obtain the population shares, α_i, and the change in transfers, ΔT, from the data. We back out a "break-even" value $\bar{R}_{NS}$ lower than or equal to 6.2 percent that still leads to positive aggregate benefits, $\Delta V > 0$.[30] An analogous approach using the effects on school attendance and literacy rates points to a maximum value of returns, $\bar{R}_{NS}$, of 13 percent.

We then compare the "break-even" values $\bar{R}_{NS}$ to the *actual* values of R_{NS} in Brazil from the literature. For instance, exploiting exogenous variations in population cutoffs generated by the allocation mechanism, Corbi et al. (2019) find that a one-percentage-point increase in federal transfers generates around 0.18 and 0.21 percent increases in the total public and private employment. Litschig and Morrison (2013) document returns of 0.35 percent for school attendance and 0.2 percentage points for literacy rates. In Appendix F, we leverage a similar regression discontinuity approach to estimate the effects

[30]We use the following numbers: $R_R \approx 0$, $\alpha_R \approx 22.2$ percent, $\Delta T \approx 5.5$ percentage points, $R_N \approx 34/32 = 106.25$ percent, $\alpha_N = 4.3$ percent, and $\alpha_{NS} = 73.4$ percent.

of federal transfers on selected economic outcomes for the sample of non-split municipalities. We find little evidence of local multiplier effects. Benchmarking the returns from the literature and from our estimation sample to the "break-even" values, our findings point to positive impacts of the redistricting episode on the aggregate social value.

8 Conclusion

This paper provides comprehensive evidence of the short- and long-run impacts of one of the largest voluntary redistricting episodes worldwide (Grossman and Lewis, 2014). Leveraging rich administrative and spatial data, along with reforms generating sharp variations in the number of municipalities in Brazil, we find that redistricting 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 remote districts neglected by their former headquarters. Our findings indicate that splitting promotes decentralization of decision-making power and enables peripheral regions to develop.

This paper offers new policy-relevant insights for countries debating on how much autonomy and resources to devote to peripheral, poor and remote areas as part of their decentralization reforms. The gains in fiscal revenues for new municipalities allow us to shut down one often-hypothesized pitfall of redistricting: their limited fiscal capacity to self-finance. We also do not find evidence that the resulting losses of scale and revenues impose a visible burden to the rest of the country, likely due to the decreasing returns to spending. Therefore, our context of subsidized and voluntary splits shines a positive light on decentralization reforms, often classified as "cautionary tales" (Kremer et al., 2003).

One limitation of this paper is that we are not able to estimate the costs of redistricting 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 redistricting would shed light on its pitfalls. Quantifying the economic costs of redistricting would also advance our understanding of its equity-efficiency trade-off embedded in a general equilibrium framework for a welfare analysis. Lastly, fleshing out exactly 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., R. Baqir, and C. Hoxby (2004). Political Jurisdictions in Heterogeneous Communities. *Journal of Political Economy 112*(2), 348–396.

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.

Balan, P., A. Bergeron, G. Tourek, and J. L. Weigel (2022). Local Elites as State Capacity: How City Chiefs Use Local Information to Increase Tax Compliance in the Democratic Republic of the Congo. *American Economic Review 112*(3), 762–97.

Banerjee, A., E. Duflo, C. Imbert, S. Mathew, and R. Pande (2020). E-Governance, Accountability, and Leakage in Public Programs: Experimental Evidence from a Financial Management Reform in India. *American Economic Journal: Applied Economics 12*(4), 39–72.

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., R. Burgess, A. Khan, and G. Xu (2022). Bureaucracy and Development. *Annual Review of Economics 14*, 397–424.

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*.

Blom-Hansen, J., K. Houlberg, S. Serritzlew, and D. Treisman (2016). Jurisdiction Size and Local Government Policy Expenditure: Assessing the Effect of Municipal Amalgamation. *American Political Science Review 110*(4), 812–831.

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.

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

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.

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

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

Dahis, R., L. Schiavon, and T. Scot (2023, 03). Selecting Top Bureaucrats: Admission Exams and Performance in Brazil. *The Review of Economics and Statistics*, 1–47.

Dahis, R. and C. Szerman (2021). Development via Administrative Redistricting: Evidence from Brazil. *Available at SSRN 3125757*.

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.

Ebel, R. D. and S. Yilmaz (2002). *On the Measurement and Impact of Fiscal Decentralization*, Volume 2809. World Bank Publications.

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

Evans, P. and J. E. Rauch (1999). Bureaucracy and Growth: A Cross-National Analysis of the Effects of "Weberian" State Structures on Economic Growth. *American Sociological Review*, 748–765.

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.

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.

Finan, F., B. A. Olken, and R. Pande (2017). The Personnel Economics of the Developing State. *Handbook of Economic Field Experiments 2*, 467–514.

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.

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.

Gumpert, A., H. Steimer, and M. Antoni (2022). Firm Organization with Multiple Establishments. *The Quarterly Journal of Economics 137*(2), 1091–1138.

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.

Hazell, J., C. Patterson, H. Sarsons, and B. Taska (2022). National Wage Setting. Technical report, National Bureau of Economic Research.

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.

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

Kleinman, B. (2022). Wage Inequality and the Spatial Expansion of Firms. Technical report.

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.

Kremer, M., S. Moulin, and R. Namunyu (2003). Decentralization: A Cautionary Tale. *Poverty Action Lab Paper 10*.

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.

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.

Myerson, R. (2006). Federalism and Incentives for Success in Democracy.

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.

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.

Shaver, A., D. B. Carter, and T. W. Shawa (2019). Terrain Ruggedness and Land Cover: Improved Data for Most Research Designs. *Conflict Management and Peace Science 36*(2), 191–218.

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.

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.

United Nations Development Program (2013). Atlas of Human Development in Brazil.

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

9 Tables

Table 1: Baseline Descriptive Statistics at the Municipality Level

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

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

Table 2: Baseline Descriptive Statistics at the District Level

	District-Level 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	3.2	5.5	31.7	64	3.6	13.1	17.9	48.9	2.6	<0.01	-25.8	<0.01
% Urban Population	.4	.3	.3	.2	.7	.2	.3	.2	.6	.2	.1	<0.01	-.3	<0.01
% Male	.5	0	.5	0	.5	0	.5	0	.5	0	0	<0.01	0	<0.01
% Literacy	.7	.1	.7	.1	.7	.1	.6	.1	.7	.1	0	.3	0	<0.01
% Piped Water	.5	.3	.5	.3	.5	.4	.5	.3	.5	.4	0	.53	-.1	<0.01
% Sanitation	.6	.4	.7	.3	.6	.4	.6	.3	.6	.4	0	.05	0	.45
% Trash Removal	.1	.2	.1	.2	.3	.3	.1	.2	.3	.3	0	.03	-.2	<0.01
Avg. Luminosity	1.8	5.8	1.4	5.2	3.1	6.5	1.9	8.1	2.5	7.3	.5	.19	-1.2	<0.01
Area (000's km2)	.5	1.5	.3	.5	.9	2.5	.3	.9	.6	1.5	.3	<0.01	-.4	<0.01
Log Distance to Parent Town Hall	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	.9	5.3	.8	.1	.09	0	.35
Log Maize Suitability	8.7	.3	8.7	.3	8.6	.3	8.5	.3	8.5	.2	0	.98	0	.06
Log Wet Rice Suitability	8.6	.8	8.6	.5	8.7	.5	8.6	.9	8.6	.8	0	.58	0	.42
Log Soybean Suitability	7.7	.4	7.7	.2	7.7	.2	7.6	.8	7.7	.7	0	.47	0	.67
Log Wheat Suitability	6.5	2.9	6.8	2.7	6.6	2.8	6.5	3	6.5	2.9	-.2	.35	0	.85
Terrain Ruggedness	83.2	78.2	72.8	68.5	76.2	72.7	68.6	71.7	68.7	71.4	9.7	.06	6.9	.17
	N = 552		N = 325		N = 382		N = 916		N = 1772					

Notes: This table reports 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 sample construction of the district-level data.

Table 3: Robustness Checks: The Effects of Redistricting on Luminosity for Applicant Districts

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Specifications						
Post x Split	0.34***	0.48***	0.10***	0.33***	0.25***	0.25***
	(0.03)	(0.05)	(0.03)	(0.04)	(0.07)	(0.02)
Observations	10,122	9,616	10,122	4,920	10,122	10,122
R-squared	0.96	0.95	0.97	0.97	0.98	0.97
Choice	Benchmark	Log Lumin.	IHS Lumin.	1997 Wave	Micro region FE	Controls
Mean Pre-Split	-0.761	-1.023	0.688	-0.686	-0.761	-0.761
SD Pre-Split	1.525	2.090	0.998	1.506	1.525	1.525
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	Micro region	State		
Mean Pre-Split	-0.761	-0.761	-0.761	-0.761		
SD Pre-Split	1.525	1.525	1.525	1.525		
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 redistricting on economic activity, measured by log 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, whereas Column (3) applies inverse hyperbolic sine transformation to the average luminosity. Column (4) further 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 Table 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 both at the state and split wave levels. Columns (2), (3), and (4) consider clustering at the municipality, micro region, and state levels, respectively.

Table 4: The Role of Fiscal Revenues

	Log Municipal Jobs		% Trash Collection		% Sewage		Log Luminosity		Log Private Establishments		Log Private Jobs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Split	0.24***	0.21***	4.50*	3.39	1.11***	0.67	0.12**	0.11**	0.06	0.06	0.07	0.06
	(0.04)	(0.04)	(2.33)	(2.09)	(0.39)	(0.41)	(0.04)	(0.04)	(0.07)	(0.07)		(0.06)
Log Expenditures		0.20***		5.76**		2.27**		0.07***		0.04		0.04
		(0.02)		(2.25)		(0.96)		(0.01)		(0.03)		(0.04)
Observations	7,179	7,179	1,324	1,324	1,324	1,324	8,106	8,106	7,229	7,229	7,229	7,229
R-squared	0.85	0.86	0.87	0.87	0.89	0.89	0.98	0.98	0.99	0.99	0.97	0.97
State-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean Pre-Split	5.98	5.98	63.73	63.73	96.03	96.03	-0.05	-0.05	5.39	5.39	7.16	7.16
SD Pre-Split	1.35	1.35	27.35	27.35	7.78	7.78	1.59	1.59	1.49	1.49	2.02	2.02

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the aggregate estimates of redistricting on selected outcomes representing public sector, economic activity, and private sector. Odd columns replicate baseline results (further details can be found in Appendix Tables D2 to D4), whereas even columns control for log total expenditures. Standard errors are two-way clustered both at the state and split wave levels.

Table 5: Heterogeneous Effects of Redistricting on Economic Activity

	Log Luminosity			
	(1)	(2)	(3)	(4)
Post x Split	0.34***	0.30***	1.71***	0.89*
	(0.03)	(0.04)	(0.27)	(0.46)
Log Expenditures		0.07**	0.07**	0.07**
		(0.04)	(0.03)	(0.03)
Post x Split x Log Population in 1991			-0.04	0.01
			(0.04)	(0.04)
Post x Split x Log Area			-0.14***	-0.28**
			(0.04)	(0.08)
Post x Split x Urbanization Rate in 1991			-0.53**	-0.51**
			(0.20)	(0.19)
Post x Split x Log Distance to Parent Town Hall				0.26*
				(0.15)
Post x Split x Log Distance to State Capital				0.06
				(0.07)
Observations	9,821	9,821	9,821	9,821
R-squared	0.96	0.96	0.96	0.96
State-Year FE	✓	✓	✓	✓
District FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean Pre-Split	-0.77	-0.77	-0.77	-0.77
SD Pre-Split	1.52	1.52	1.52	1.52

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports heterogeneous effects of redistricting on log luminosity. Column (1) considers the benchmark specification from Equation (3), whereas Column (2) adds log expenditures to the set of controls. 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.

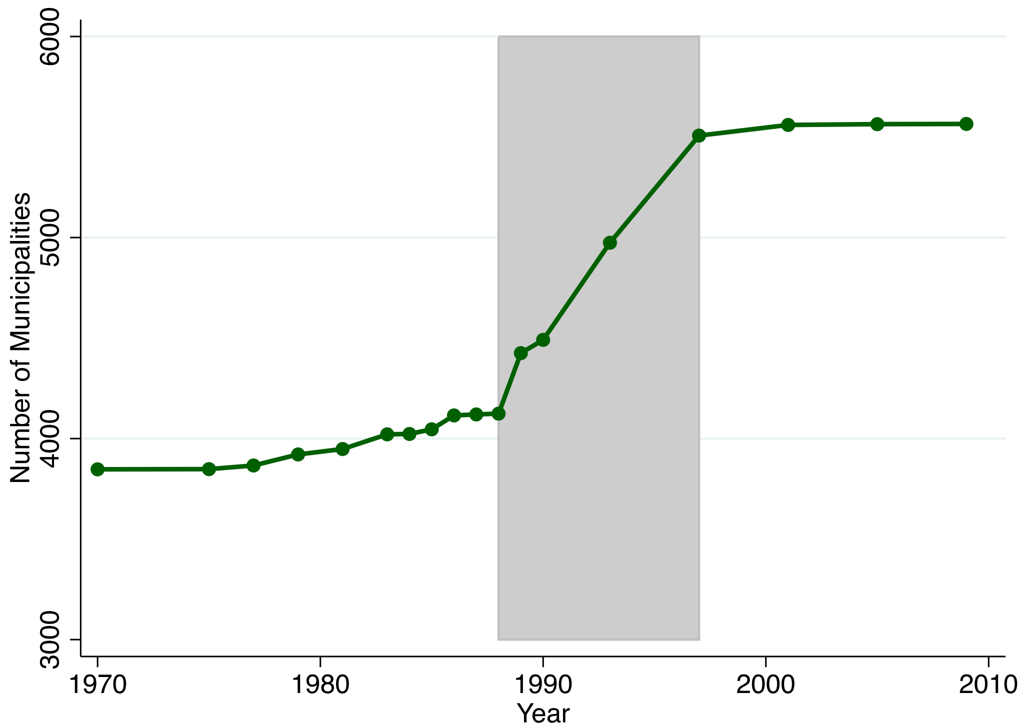
Table 6: Spillover Effects of Changes in Federal Transfers on the Rest of the Country

	Public Jobs	Private Jobs	Establishments	Luminosity
	(1)	(2)	(3)	(4)
Federal Transfers	3.82	-5.23	0.63	3.48*
	(8.40)	(3.76)	(11.07)	(1.97)
Observations	25	25	25	25
R-squared	0.38	0.68	0.46	0.54
Region FE	✓	✓	✓	✓
Split Wave	1997	1997	1997	1997
Mean Outcome Change	226.7	189.8	366.4	117.4
SD Outcome Change	403.9	133.7	442.5	59.62

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports correlations between changes in federal transfers (in percentage points) and selected outcomes (in percentage) for municipalities that did not split. To capture changes in federal transfers, we exploit variation in the number of splits within states after residualizing for region dummies. Outcomes of interest are percentage changes in total number of public jobs, number of private jobs, number of establishments and average luminosity 15 years after splitting. We exclude *Distrito Federal* and *Roraima* from the final sample.

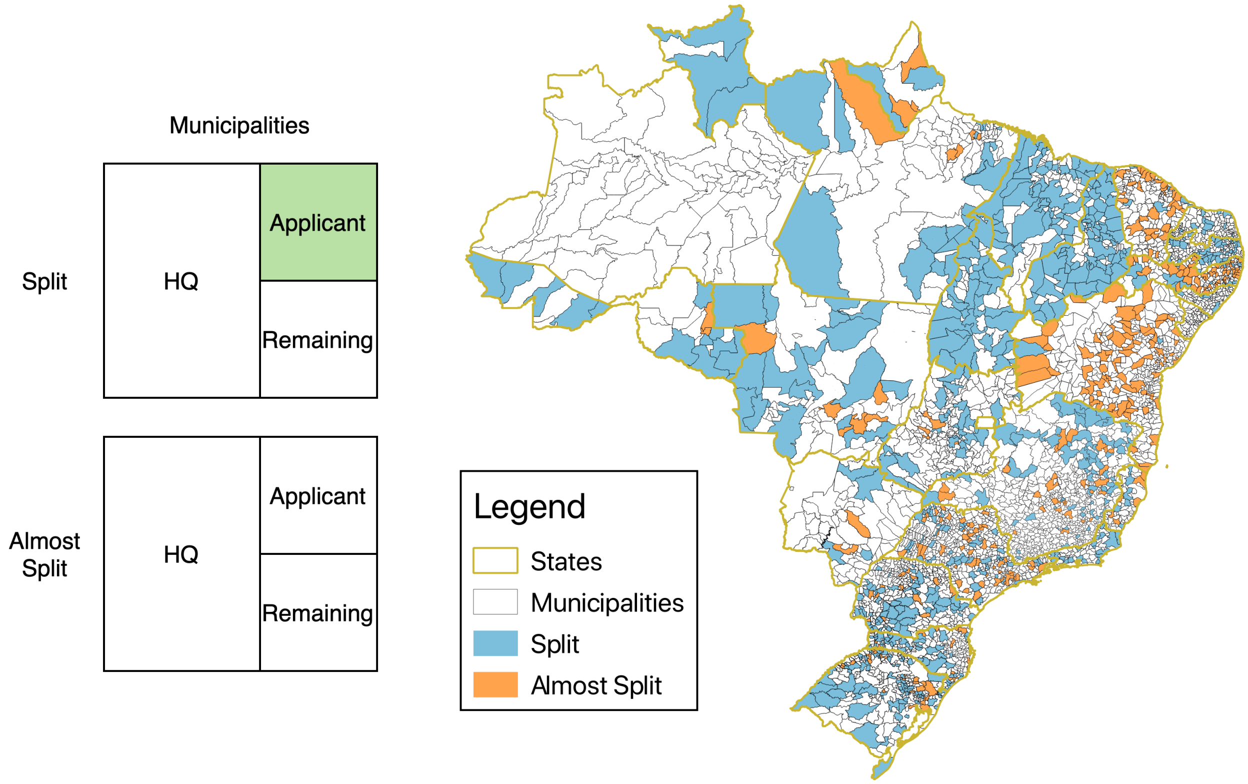
10 Figures

Figure 1: Evolution of Total Number of Municipalities



Note: This figure shows the evolution of the total number of municipalities in Brazil between 1970 and 2010. New municipalities are established in the beginning of election terms after obtaining approval to split. The grey area highlights our period of study: the period between the enactments of the 1988 Federal Constitution and the 1996 Constitutional Amendment. Information on splits are obtained from the Brazilian Institute of Geography and Statistics (IBGE).

Figure 2: Diagram Illustrating Splits and Map of Brazil

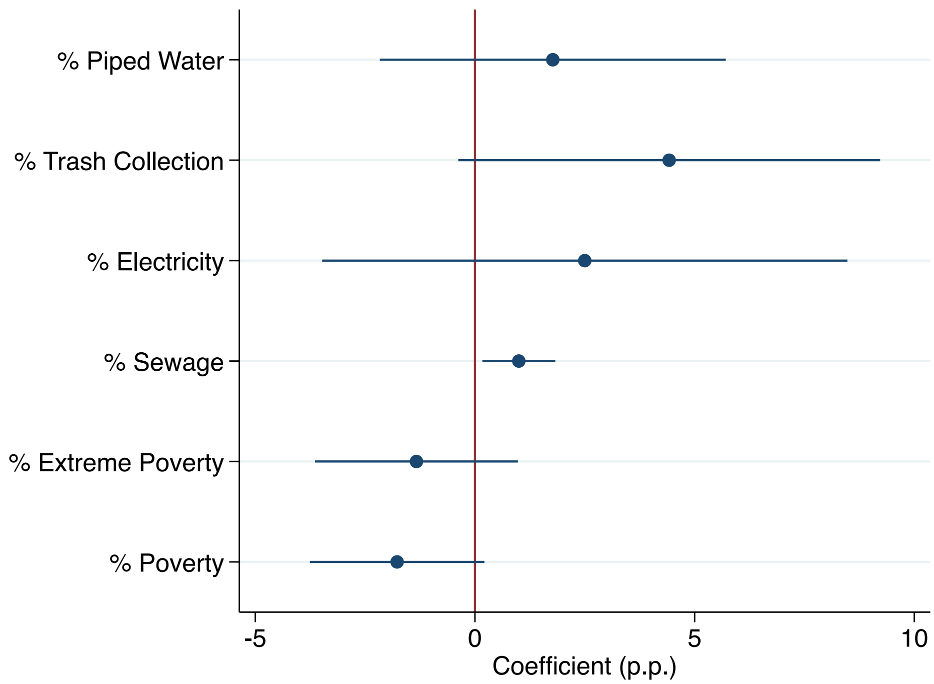


Note: On the left, the diagram illustrates the structure of split requests from our sample. Municipalities are divided into applicant, remaining and headquarters districts. The green color highlights applicant districts that succeed at splitting. On the right, the map represents Brazil in 1991. Municipalities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

Figure 3: Effects of Redistricting on Bureaucracy in the Public Sector

Note: *** p<0.01, ** p<0.05, * p<0.1. This figure reports the annual effects of redistricting on the setup of new local governments after estimating Equation (1). We consider the following dependent variables: log municipal capital expenditures per capita, log municipal current expenditures per capita, log total number of municipal jobs, and log municipal wages. The main data sources are the Brazilian National Treasury between 1989 and 2018 and the annual RAIS data between 1995 to 2018. We use information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO) to classify jobs and economic sectors. The omitted category is the year before splitting. Standard errors are two-way clustered both at the state and split wave levels. Further details can be found in Appendix Table D2.

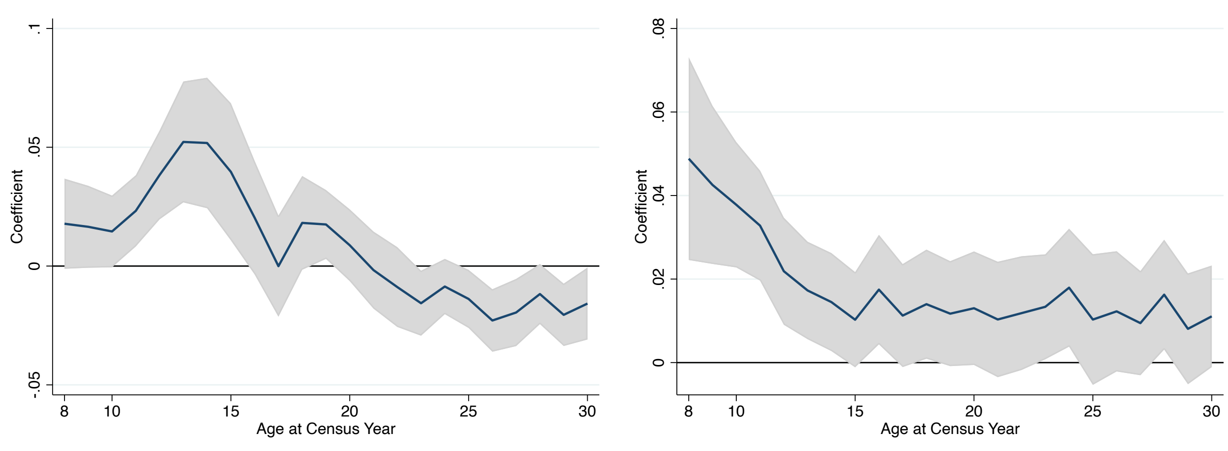
Figure 4: Effects of Redistricting on Public Services and Poverty



Note: This figure reports the aggregate effects of splitting on public services and poverty measures after estimating Equation (1). We consider the following dependent variables: household access to piped water, trash collection, electricity, sewage, extreme poverty and poverty rates. The main data sources are the decennial Demographic Census from 1991, 2000 and 2010. Standard errors are two-way clustered both at the state and split wave levels. Further details can be found in Appendix Table D3.

Figure 5: Heterogeneous Effects of Redistricting on Education Outcomes Across Age

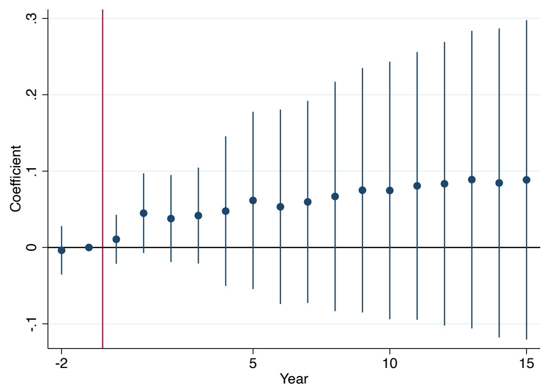
(a) School Attendance (b) Literacy Rates



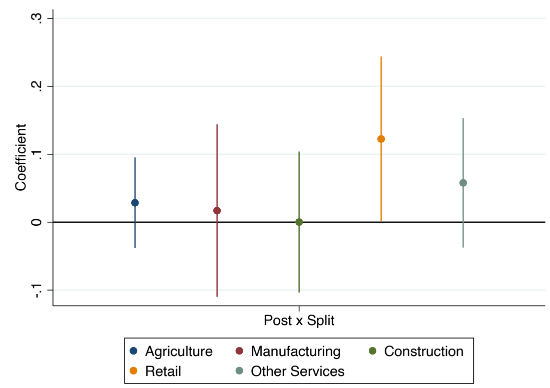
Note: This figure reports heterogeneous effects of redistricting on school attendance (Panel (a)) and literacy rates (Panel (b)) after estimating Equation (2). The main data sources are the decennial Demographic Census microdata from 1991, 2000 and 2010. Standard errors are two-way clustered both at the state and split wave levels. Further details can be found in Appendix Figure D2.

Figure 6: Effects of Redistricting on the Private Sector

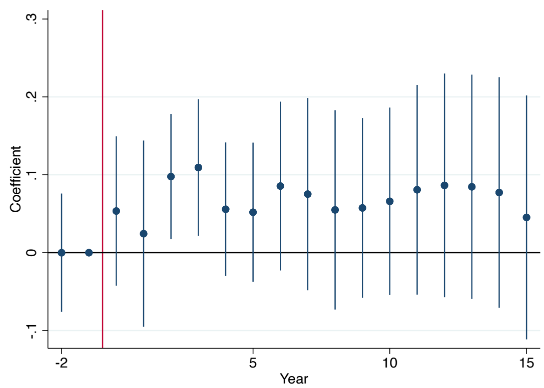
(a) Log Private Establishments



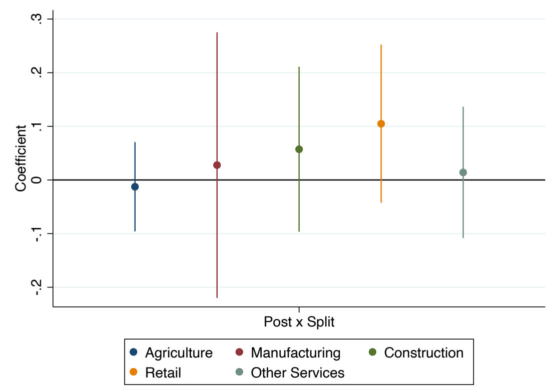
(b) Log Private Establishments



(c) Log Private Jobs



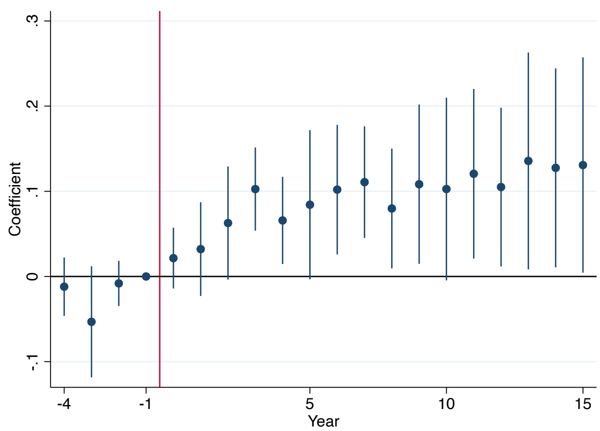
(d) Log Private Jobs



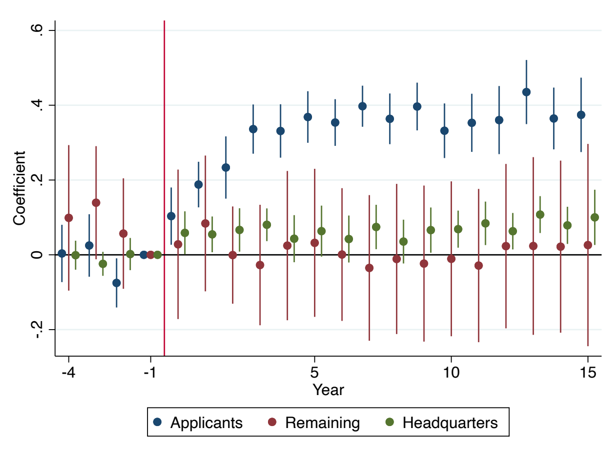
Note: Panels (a) and (c) of this figure report the annual effects of redistricting on log total number of establishments and log total number of jobs, both in the private sector, after estimating Equation (1). The omitted category is the year before splitting. Panels (b) and (d) report the aggregate effects of redistricting for the same outcomes across economic sectors. The main data sources are the annual RAIS data between 1995 to 2018. We also use information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO) to classify jobs and economic sectors. Standard errors are two-way clustered both at the state and split wave levels. Further details can be found in Appendix Table D4.

Figure 7: Effects of Redistricting on Economic Activity

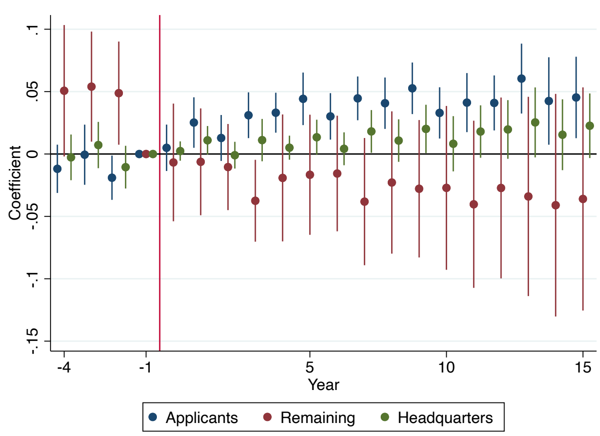
(a) Log Luminosity: Municipality Level



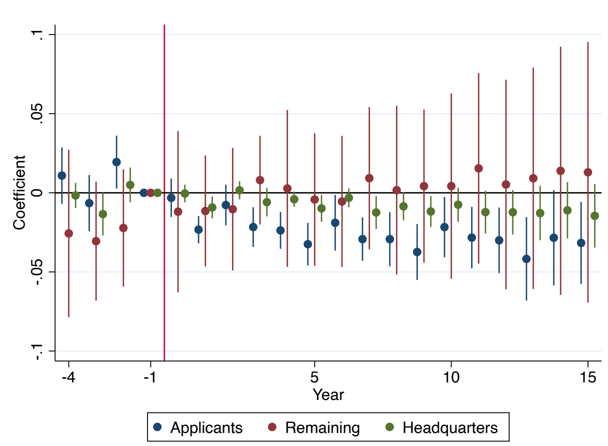
(b) Log Luminosity: District Level



(c) % Pixels Lit: District Level

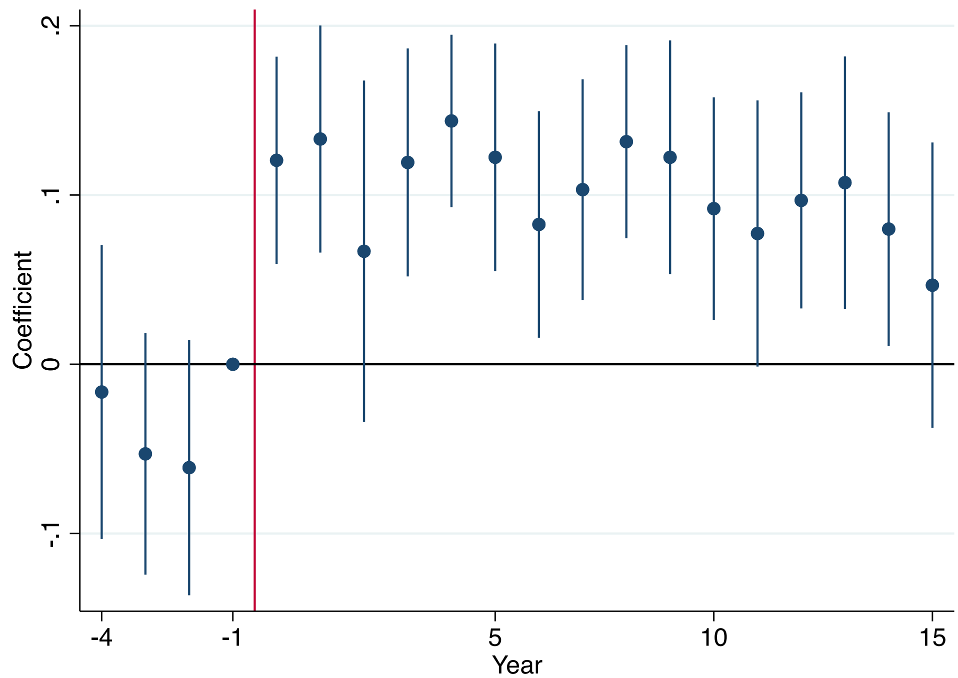


(d) Luminosity Gini: District Level



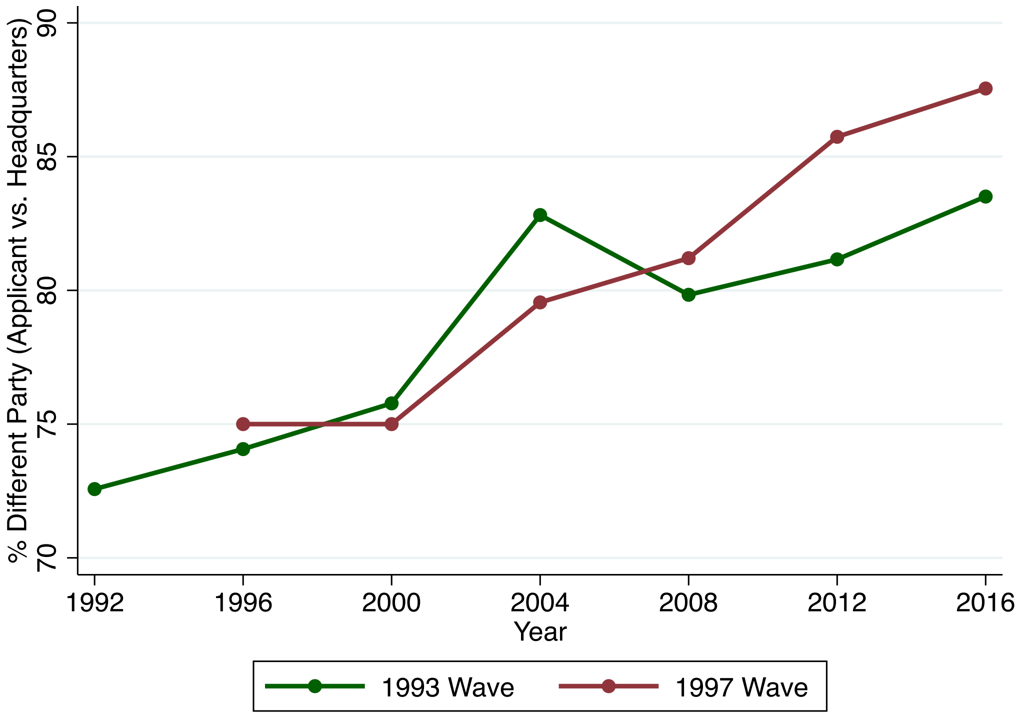
Note: This figure reports the annual effects of redistricting on economic activity. Panel (a) refers to point estimates after estimating Equation (1) with log luminosity as the dependent variable. Panels (b), (c) and (d) display point estimates after estimating Equation (3) separately for applicant, remaining, and headquarters districts, considering log average luminosity, indicator variable for whether the average is above zero, and luminosity Gini index as the dependent variables. The main data sources are the nighttime luminosity data between 1992 to 2013. The omitted category is the year before splitting. Standard errors are two-way clustered both at the state and split wave levels. Further details can be found in Appendix Table D5.

Figure 8: Effects of Redistricting on the Local Revenues



Note: This figure reports the annual effects of redistricting on log local revenues per capita after estimating Equation (1). The main data sources are the Brazilian National Treasury data between 1989 to 2018. The omitted category is the year before splitting. Standard errors are two-way clustered both at the state and split wave levels. Further details can be found in Appendix Table D6.

Figure 9: 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.

Appendices

A Conceptual Framework

We sketch a simple framework to illustrate how redistricting through 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). Appendix Section B presents further details and proofs.

We work with a one-period model. We assume that a municipality, which we refer 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. There is no income heterogeneity within the district, implying that all residents have income per capita y. Two sources of municipal revenues finance public goods g: income taxes τ and federal transfers $T(\cdot)$. Consistent 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, whereas 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:

$$\max_{g_A, g_B, \tau} \quad (1-\lambda)\alpha_A U_A + \lambda \alpha_B U_B \quad \text{subject to} \quad g_A + g_B \leq \tau y + T(\alpha_A + \alpha_B), \tag{5}$$

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

In 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:

$$\max_{g_B, \tau} \quad \alpha_B U_B \quad \text{subject to} \quad g_B \leq \tau \alpha_B y_B + T(\alpha_B), \tag{6}$$

in which $T(\alpha_B)$ represents the amount of federal transfers the new municipality receives. The parent municipality, which now only consists of 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 of splitting, we extend our framework to introduce a second municipality with population α_2. This municipality represents the rest of the state. To capture the reallocation of federal transfers after a split, define T_i^U as the amount of transfers that area i receives when municipality 1 is integrated; and T_i^S as the amount of 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 $\left(\frac{\alpha_B}{\alpha_A} \to 0\right)$ and neglected by its parent district $(\lambda \to 0)$, and if municipality 2 is relatively large $\left(\frac{\alpha_2}{\alpha_A+\alpha_B} \to \infty\right)$, then (i) ΔV_A is small, (ii) ΔV_B is positive and large, and (iii) ΔV_2 is negative and small.*

Proof. See Appendix Section B.2. □

The intuition behind Proposition 2 is straightforward. Because of decreasing returns to spending, for a given configuration of population sizes and neglect by the headquarters district, the transfers moved from municipality 2 to district B may do little harm to the former and create substantial benefits to the latter. Welfare of district A changes little, either positively or negatively, depending on whether its transfers change or not.[31] We directly test these predictions in Section 5 by separately evaluating the consequences of splitting for headquarters and non-headquarters districts.

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

[31] We abstract away from agglomeration effects. In a setting accounting for them, this result could be further exacerbated (Kline and Moretti, 2014).

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 (5), assuming there exists an interior optimum, we can solve the first-order condition:

$$\frac{g_B^U}{g_A^U} = \frac{\lambda}{1-\lambda}\frac{\alpha_B}{\alpha_A}\frac{\theta_B}{\theta_A} \tag{7}$$

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:

$$g_A^U = (1-\lambda)\alpha_A\theta_A\frac{y}{\overline{y}} \qquad g_B^U = \lambda\alpha_B\theta_B\frac{y}{\overline{y}} \qquad \tau^U = \frac{\overline{\theta}}{\overline{y}} - \frac{T(\alpha_A+\alpha_B)}{y} \tag{8}$$

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 (6), we can show that:

$$g_A^S = \alpha_A\theta_A \qquad g_B^S = \alpha_B\theta_B \qquad \tau_A^S = \frac{\theta_A}{y_A} - \frac{T(\alpha_A)}{\alpha_A y_A} \qquad \tau_B^S = \frac{\theta_B}{y_B} - \frac{T(\alpha_B)}{\alpha_B y_B} \tag{9}$$

District B unilaterally chooses to split if $U_B^S \geq U_B^U$. Substituting in Equations (8) and

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

$$
\begin{aligned}
G(\lambda, \alpha_A, \alpha_B, \theta_A, \theta_B, y_A, y_B, T) &\equiv U_B^S - U_B^U \\
&= \theta_B[\ln(g_B^S) - \ln(g_B^U)] + (\tau^U - \tau_B^S)y_B \\
&= \theta_B \ln\left(\frac{\overline{y}}{\lambda y}\right) + \left(\frac{\overline{\theta}}{\overline{y}} - \frac{\theta_B}{y_B} + \frac{T(\alpha_B)}{\alpha_B y_B} - \frac{T(\alpha_A+\alpha_B)}{y}\right) y_B \\
&\geq 0
\end{aligned} \tag{10}
$$

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

$$
\begin{aligned}
H(\lambda, \alpha_A, \alpha_B, \theta_A, \theta_B, y_A, y_B) &\equiv g_B^S - g_B^U \\
&= \alpha_B\theta_B - \frac{\lambda\alpha_B\theta_B y}{\overline{y}} \\
&= \frac{(1-2\lambda)\alpha_A\alpha_B\theta_B y_A}{\overline{y}} \geq 0
\end{aligned} \tag{11}
$$

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:

$$\begin{aligned}\tau_B^S - \tau^U &= \frac{\theta_B}{y_B} - \frac{\overline{\theta}}{\overline{y}} + \frac{T(\alpha_A+\alpha_B)}{y} - \frac{T(\alpha_B)}{\alpha_B y_B} \\ &= \frac{(1-\alpha)\alpha_A\alpha_B y[\theta_B y_A - \theta_A y_B] + \overline{y}[\alpha_B y_B T(\alpha_A+\alpha_B) - yT(\alpha_B)]}{\alpha_B y_B y \overline{y}}\end{aligned} \tag{12}$$

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:

$$\Delta V_A = \theta_A \ln\left(\frac{\overline{y}}{(1-\lambda)y}\right) + \left(\frac{\overline{\theta}}{\overline{y}} - \frac{\theta_A}{y_A} + \frac{T(\alpha_A)}{\alpha_A y_A} - \frac{T(\alpha_A+\alpha_B)}{y}\right) y_A \tag{13}$$

$$\Delta V_B = \theta_B \ln\left(\frac{\overline{y}}{\lambda y}\right) + \left(\frac{\overline{\theta}}{\overline{y}} - \frac{\theta_B}{y_B} + \frac{T(\alpha_B)}{\alpha_B y_B} - \frac{T(\alpha_A+\alpha_B)}{y}\right) y_B \tag{14}$$

$$\Delta V_2 = \frac{T^S(\alpha_2) - T^U(\alpha_2)}{\alpha_2} \tag{15}$$

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

C Data Construction

C.1 Splitting Requests

This appendix contains a detailed description of the data on split requests used in this paper. From historical archives, we construct a novel dataset that contains all requests to split initiated by districts between 1989 and 1996. Prior to the 1996 CA, each state assembly had discretion to set its own regulation over splitting, leading to substantial variation in records on split requests.

Brazil has 26 state legislative assemblies. For each state assembly, we search for digitized historical records on split requests during the first half of the 1990s. We find records for twelve states: Amapá, Amazonas, Espírito Santo, Goiás, Mato Grosso, Minas Gerais, Pará, Paraná, Rio Grande do Sul, Rondônia, Santa Catarina, and São Paulo. The availability and quality of the data widely vary across states. Figure C1 provides an example of the material available online, whereas Figure C2 depicts the distributions of request and split years among applicant districts. We list the variables we construct from the records for each state below:

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.

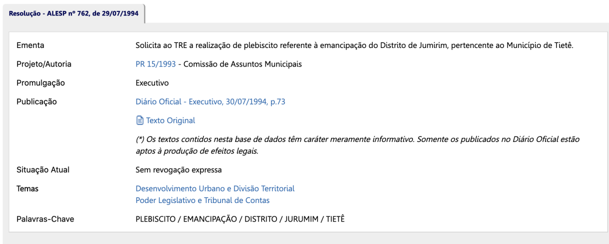
Rondônia: indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; date when the request was archived; identification number of the split process; approval date of the referendum; and result of the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

Figure C1: Examples of Raw Material of Split Requests

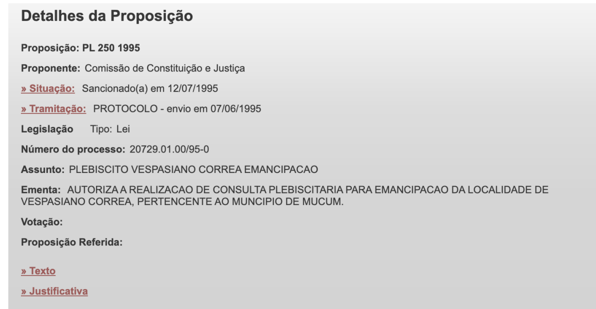
(a) São Paulo



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:

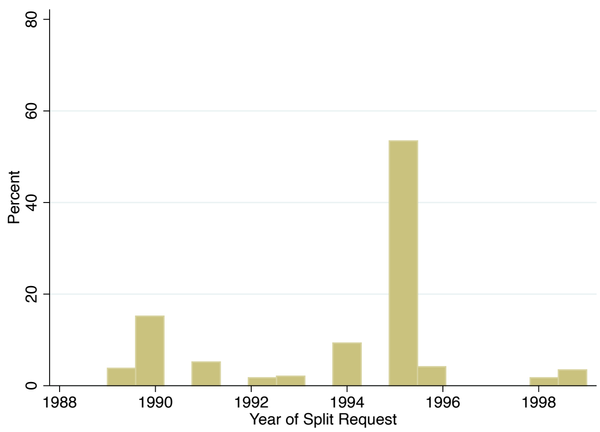
Proposição Referida:

» Texto

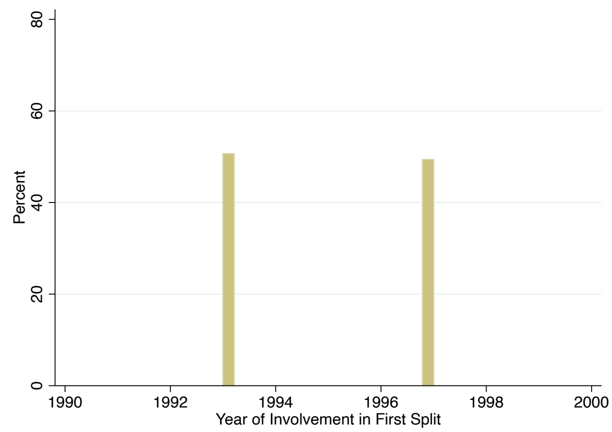
» Justificativa

Figure C2: Histograms of Request and Split Years

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



(b) Year of *Split* among Applicants



D Additional Results

Table D1: Descriptive Statistics at the District Level by Split Wave

	Applicants		Split		Almost Split		(2)-(1)		(4)-(3)		(5)-(3)		(6)-(4)	
	1993	1997	1993	1997	1993	1997								
	Mean	Mean	Mean	Mean	Mean	Mean	Dif.	p	Dif.	p	Dif.	p	Dif.	p
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(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	.34	.4	.33	.54	.38	-.08	0	-.07	0	-.14	0	-.05	.17
% Male	.52	.52	.52	.52	.52	.52	0	.73	0	.77	0	.64	0	.78
% Literacy	.69	.62	.68	.61	.7	.66	-.06	0	-.07	0	-.02	.2	-.05	.02
% Piped Water	.45	.45	.5	.43	.17	.52	0	.92	-.07	.02	.34	0	-.08	.07
% Sanitation	.58	.63	.65	.63	.22	.64	.06	.06	-.02	.56	.43	0	-.01	.76
% Trash Removal	.1	.1	.1	.09	.05	.14	0	.76	-.02	.38	.05	.07	-.05	.08
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 Town Hall	2.97	2.96	3.05	3.05	2.56	2.69	-.01	.92	.01	.93	.48	0	.36	0
Log Distance to State Capital	5.49	5.45	5.5	5.6	5.42	5.02	-.04	.59	.09	.17	.08	.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 descriptive statistics at the district level by split wave. We use information from 1991 Demographic Census, 1992 night lights, MapBiomas, FAO-GAEZ soil suitability, and terrain ruggedness data. The variables are: total population (in thousands), shares of urban and male population, literacy rates, share of households with access to piped water, sanitation and trash removal, average luminosity, total area (in thousands km^2), log distance to town hall, log distance to state capital, log soil suitability for different crops (maize, wet rice, soybean and wheat), and log terrain ruggedness. See Section 3 for further details on data and sample construction of the district-level data.

Table D2: Effects of Redistricting on Bureaucracy in the Public Sector

	Log Expenditures p.c. (1)	Log Capital Expenditures p.c. (2)	Log Current Expenditures p.c. (3)	Log Municipal Jobs (4)	Log Municipal Wages (5)
Post x Split	0.15***	0.27***	0.15***	0.23***	0.22***
	(0.03)	(0.08)	(0.03)	(0.07)	(0.05)
Observations	8,110	8,105	8,109	7,270	7,270
R-squared	0.97	0.71	0.93	0.85	0.90
State-Year FE	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓
Mean Pre-Split	5.79	3.86	5.66	5.99	15.56
SD Pre-Split	0.74	1.14	0.77	1.23	1.54

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the aggregate effects of redistricting on the setup of new local governments. We consider the following dependent variables: log total municipal expenditures per capita, log municipal capital expenditures per capita, log municipal current expenditures per capita, log total number of municipal jobs, and log municipal wages. The main data sources are the annual Brazilian National Treasury data between 1989 to 2018 and the annual RAIS data between 1995 to 2018. We also use information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO) to classify jobs and economic sectors. Standard errors are two-way clustered both at the state and split wave levels.

Table D3: Effects of Redistricting on Public Services and Poverty

	% Piped Water (1)	% Trash Collection (2)	% Electricity (3)	% Sewage (4)	% Extreme Poverty (5)	% Poverty (6)
Post x Split	1.77	4.42*	2.50	1.00**	-1.33	-1.77*
	(1.89)	(2.31)	(2.87)	(0.40)	(1.11)	(0.96)
Observations	1,344	1,344	1,344	1,344	1,344	1,344
R-squared	0.89	0.87	0.83	0.89	0.89	0.94
State-Year FE	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓
Mean Pre-Split	71.18	63.51	81.33	96.10	19.62	42.81
SD Pre-Split	24.17	27.35	20.03	7.66	14.86	20.60

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the aggregate effects of redistricting on public services and poverty. We consider the following dependent variables: household access to piped water, trash collection, electricity, sewage, extreme poverty and poverty rates. The main data sources are the decennial Demographic Census from 1991, 2000 and 2010. Standard errors are two-way clustered both at the state and split wave levels.

Table D4: Effects of Redistricting on the Private Sector

	Log Private Establishments (1)	Log Private Jobs (2)	Log Private Wages (3)
Post x Split	0.06	0.07	0.08
	(0.07)	(0.06)	(0.05)
Observations	7,348	7,348	7,348
R-squared	0.99	0.97	0.97
State-Year FE	✓	✓	✓
Municipality FE	✓	✓	✓
Mean Pre-Split	5.39	7.16	16.75
SD Pre-Split	1.49	2.01	2.09

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the aggregate effects of redistricting on the private sector. We consider the following dependent variables: log total number of establishments, log total number of jobs, and log wages, all measured in the private sector. The main data sources are the annual RAIS data between 1995 to 2018. We also use information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO) to classify jobs and economic sectors. Standard errors are two-way clustered both at the state and split wave levels.

Table D5: Effects of Redistricting on Economic Activity: Heterogeneity Across Districts

	Panel A: Log Luminosity		
	Applicants (1)	Remaining (2)	Headquarters (3)
Post x Split	0.34***	-0.05	0.07***
	(0.03)	(0.10)	(0.02)
Observations	10,122	5,893	6,947
R-squared	0.96	0.96	0.98
State-Year FE	✓	✓	✓
District FE	✓	✓	✓
Mean Pre-Split	-0.76	-0.86	0.18
SD Pre-Split	1.52	1.43	1.43

	Panel B: % Pixels Lit		
	Applicants (1)	Remaining (2)	Headquarters (3)
Post x Split	0.04***	-0.06**	0.01
	(0.01)	(0.02)	(0.01)
Observations	10,122	5,893	6,947
R-squared	0.96	0.94	0.97
State-Year FE	✓	✓	✓
District FE	✓	✓	✓
Mean Pre-Split	0.18	0.16	0.24
SD Pre-Split	0.29	0.26	0.29

	Panel C: Luminosity Gini		
	Applicants (1)	Remaining (2)	Headquarters (3)
Post x Split	-0.03***	0.02	-0.01
	(0.01)	(0.02)	(0.01)
Observations	9,616	5,052	6,918
R-squared	0.96	0.93	0.97
State-Year FE	✓	✓	✓
District FE	✓	✓	✓
Mean Pre-Split	0.83	0.84	0.84
SD Pre-Split	0.24	0.21	0.18

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the aggregate effects of redistricting on luminosity separately for three groups of districts: applicant, remaining, and headquarters. We consider the following dependent variables for each panel: log average luminosity, indicator variable for whether the average is above zero, and average luminosity Gini index. Standard errors are two-way clustered both at the state and split wave levels.

Table D6: Effects of Redistricting on Local Revenues

	Log Revenues p.c. (1)	Log Federal Transfers p.c. (2)	Log Local Taxation p.c. (3)
Post x Split	0.15***	0.32***	0.11
	(0.04)	(0.02)	(0.10)
Observations	8,110	8,086	8,109
R-squared	0.93	0.90	0.90
State-Year FE	✓	✓	✓
Municipality FE	✓	✓	✓
Mean Pre-Split	5.79	4.70	2.91
SD Pre-Split	0.74	0.73	1.33

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the aggregate effects of redistricting on local revenues. We consider the following dependent variables: log revenues per capita, log federal transfers per capita, and log local taxation per capita. The main data sources are the annual Brazilian National Treasury data between 1989 to 2018. Standard errors are two-way clustered both at the state and split wave levels.

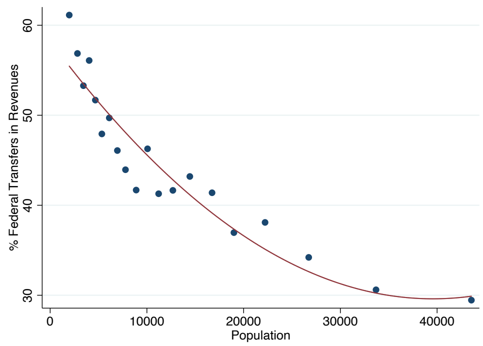
Table D7: Effects of Redistricting on Migration

	(1)	(2)	(3)
Split	0.08	1.00*	0.75
	(0.68)	(0.59)	(0.59)
Observations	220	220	220
R-squared	0.00	0.33	0.45
Controls	-	✓	✓
State FE	-	-	✓
Mean Control	9.8	9.8	9.8
SD Control	4.4	4.4	4.4

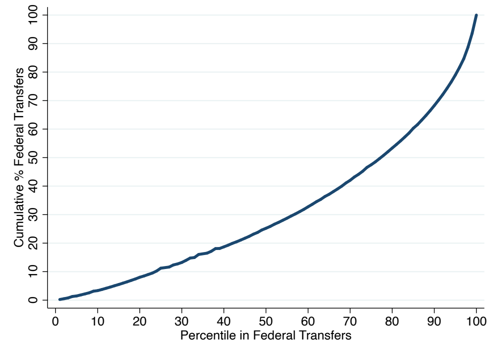
Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports the aggregate effects of redistricting on migration after estimating Equation (4). Column (1) considers a regression without state fixed effects and baseline characteristics from Table 1 in the set of controls. Column (2) controls for baseline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

Figure D1: Distribution of Federal Transfers

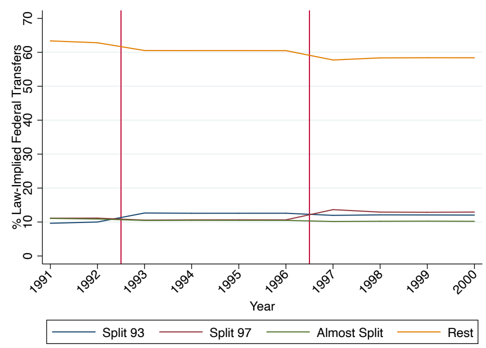
(a) Share of Federal Transfers Relative to Municipal Revenues in 1991



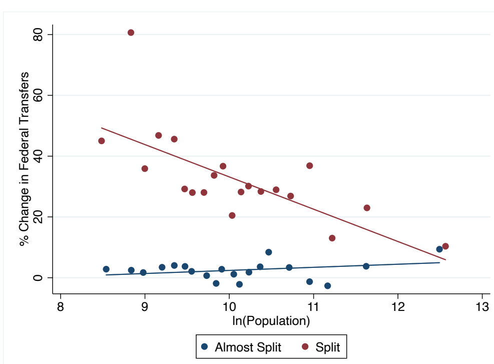
(b) Cumulative Distribution of Federal Transfers in 1991 (Bottom 50% Get ≈ 26%)



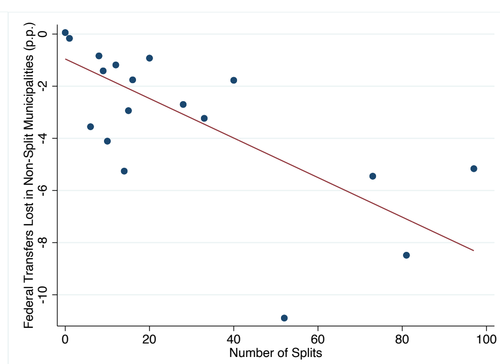
(c) Group Shares After Split Waves



(d) Change in Federal Transfers Across Groups



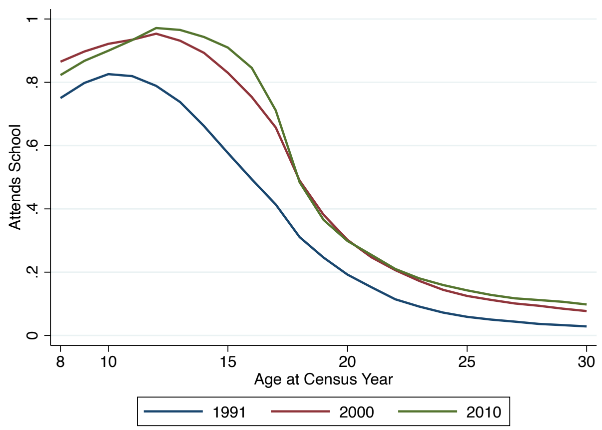
(e) Number of Splits and Losses in Federal Transfers



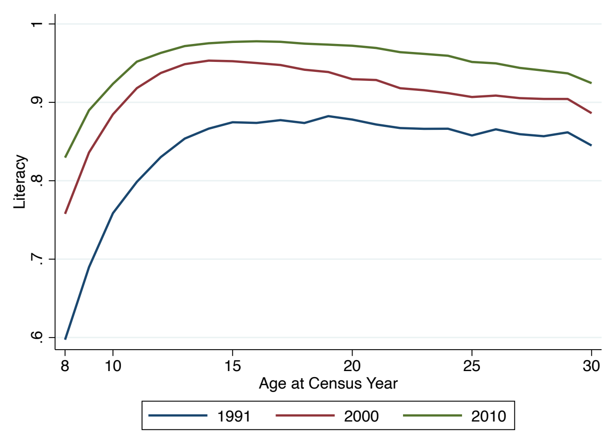
Note: This figure reports the patterns of revenues from federal transfers (FPM) over time, as described in Section 2. Panel (a) describes the share of municipal revenues from federal transfers for small municipalities in 1991. Panel (b) plots the distribution of federal transfers in 1991. Panel (c) plots 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.

Figure D2: Education Outcomes: Raw Data

(a) School Attendance



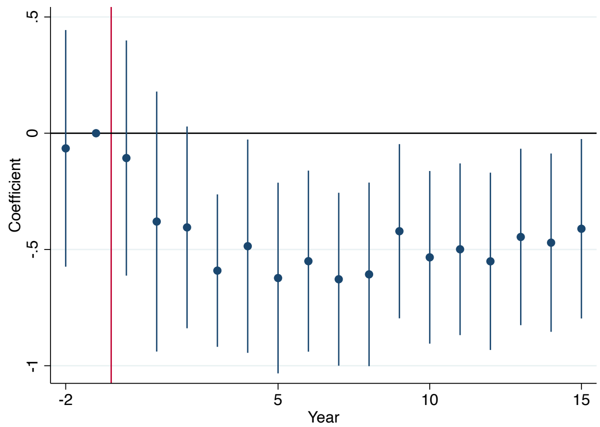
(b) Literacy Rates



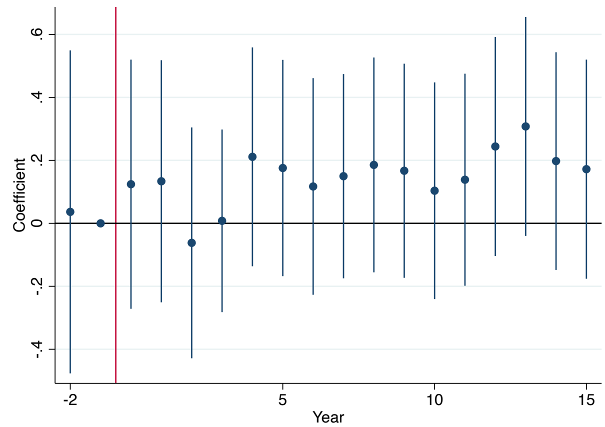
Note: This figure displays the raw data for school attendance and literacy rates from the sample described in Figure 5, by year and age. The main data sources are the decennial Demographic Census microdata from 1991, 2000 and 2010.

Figure D3: Crowd-Out Effects on Jobs in Education

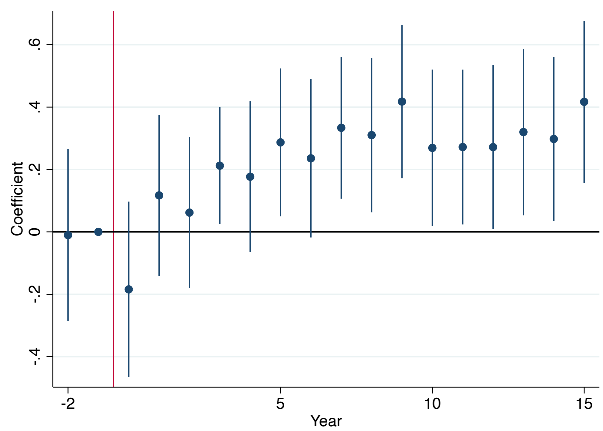
(a) Log Nonprofit Jobs



(b) Log Private Sector Jobs



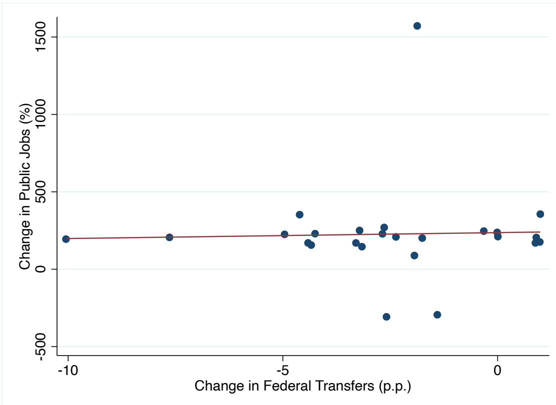
(c) Log Public Sector Jobs



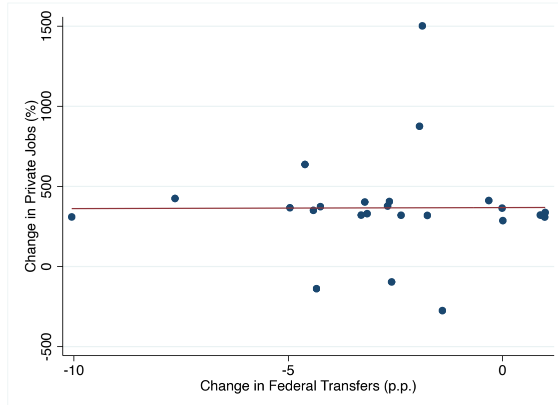
Note: This figure reports annual crowd-out effects of redistricting on jobs in the nonprofit, private, and public sectors in the education area after estimating Equation (1). The omitted category is the year before splitting. The main data sources are the annual RAIS data between 1995 to 2018. We also use information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO) to classify jobs and economic sectors. Standard errors are two-way clustered both at the state and split wave levels.

Figure D4: Spillover Effects of Changes in Federal Transfers on the Rest of the Country

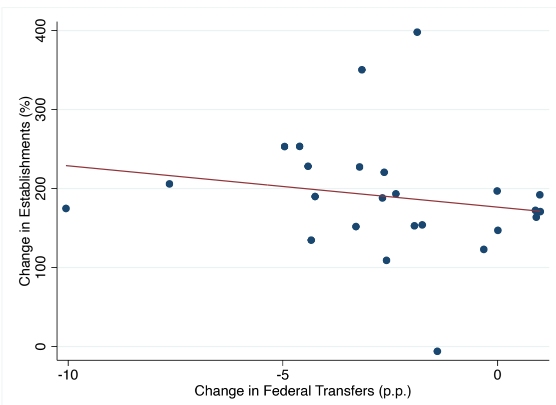
(a) Public Jobs



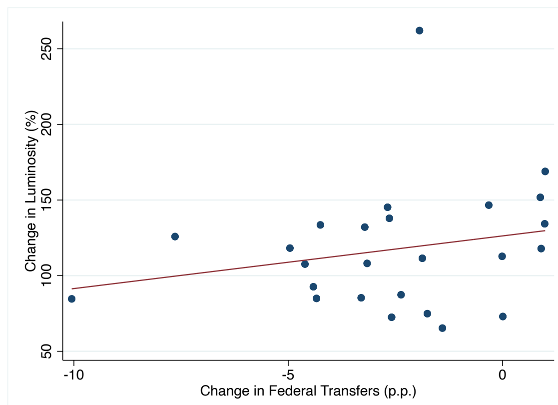
(b) Private Jobs



(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. To capture changes in federal transfers, we exploit variation in the number of splits within states after residualizing for region dummies. Outcomes of interest are percentage changes in total number of public jobs, number of private jobs, number of establishments and average luminosity 15 years after splitting. We exclude Distrito Federal and Roraima from the final sample. Further details can be found in Table 6.

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:

$$Split_{m(d)} = \psi + \phi \mathbf{1}[RV_d \geq 50\%] + \kappa g(RV_d) + \eta_d \tag{16}$$

$$y_{dt} = \alpha_d + \alpha_t + \beta Split_d Post_{w(d)} + \gamma g(RV_d) Post_{w(d)} + X_{dt}\lambda + \varepsilon_{dt}. \tag{17}$$

From the first-stage Equation (16), 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 (17) 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 E1 shows no evidence of discontinuities in pre-referendum characteristics at the district level around the cutoff.[32]

Panel (a) of Appendix Figure E1 provides a visual evidence of the first-stage, confirming that reaching the 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 E2 displays a clear jump on the growth of log luminosity around the cutoff. In terms of magnitude, Columns (1) and (3) of Appendix E2 point to the Wald estimate of 23 percent (= 0.22/0.96). 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 E2 shows that the gains are driven by applicant districts.

[32] We use baseline characteristics from 1991. Panel (b) of Appendix Figure E1 depicts the distribution of vote shares around the 50 percent cutoff and points that there are fewer districts with less than half of voters.

Table E1: Discontinuity Test on Pre-Referendum Characteristics

	(1) Log Population	(2) Log Area	(3) Log Luminosity	(4) Log Distance Town Hall
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	3.12	5.71	-1.43	3.12
SD Control	0.63	0.95	1.54	0.63

Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports point estimates from modified versions of Equation (16) to test for discontinuities in district-level characteristics prior to the referendum. We use the following pre-referendum characteristics: log total population, log total area, log average luminosity, and log distance to the parent town hall.

Table E2: Effects of Redistricting on Log Luminosity

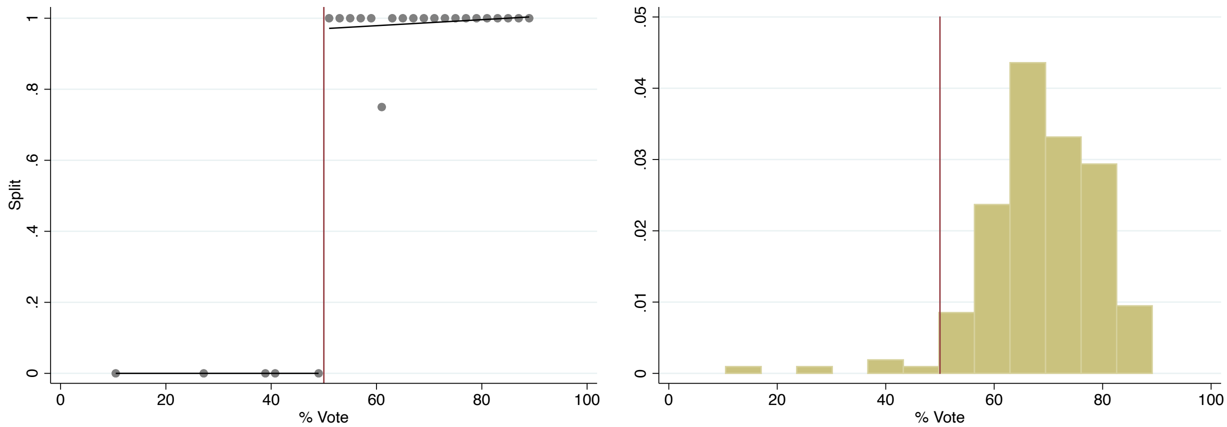
	First Stage (1)	Reduced Form (2)	Second Stage (3)	DD (4)
Referendum Vote $\geq$ 50%	0.96***			
	(0.03)			
Post x Referendum Vote $\geq$ 50%		0.13**		
		(0.06)		
Post x Split			0.21***	0.26***
			(0.05)	(0.03)
Observations	50	985	985	2,422
R-squared	0.64	0.98	0.98	0.98
District FE	-	✓	✓	✓
Mean Control	0	-1.61	-1.61	-1.41
SD Control	0	2.16	2.16	2.26

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports point estimates from the difference-in-discontinuities specification applied to the nighttime luminosity data. Column (1) refers to the first stage from Equation (16), whereas Column (2) reports the reduced-form estimates. Column (3) refers to Equation (17). Column (4) speaks to the difference-in-difference estimates from Equation (3) restricted to the state of *Minas Gerais*. Except for Column (1), in which the dependent variable is an indicator variable for whether there is a split, the remaining dependent variables are log average luminosity.

Figure E1: Referenda in *Minas Gerais*

(a) The First Stage

(b) Histogram of Vote Shares

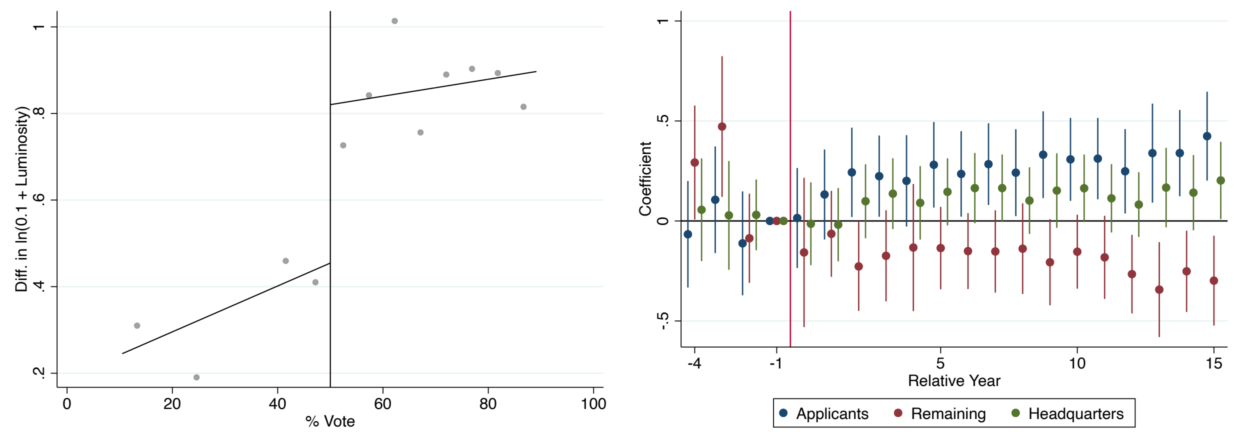


Note: This figure describes the referendum data from the state of *Minas Gerais*. Panel (a) plots the first stage of referendum votes on the likelihood of splitting. Panel (b) plots the distribution of vote shares from referenda. As described in Section 2, districts are required to obtain at least 50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

Figure E2: Effects of Redistricting on Log Luminosity: Difference-in-Discontinuities

(a) Log Luminosity Growth

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



Note: This figure reports results from specifications described 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 (17) for the applicant and headquarters districts separately. The omitted category is the year before splitting.

F The Effects of Federal Transfers on Economic Outcomes

As described in Section 2, the allocation mechanism of FPM transfers consists of fixed block grants to states. Each state then allocates a block grant to municipalities through a population-based formula that assigns coefficients to population brackets. Put differently, define FPM_m^s as the amount of FPM transfers that municipality m in state s receives in a given year. The allocation mechanism formula is the following:

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

in which FPM^s is the amount of FPM transfers allocated to state s; λ_m is the FPM coefficient of municipality m based on its population; and $\frac{\lambda_m}{\sum_{m \in s} \lambda_m}$ is the within-state share of state FPM transfers FPM^s allocated to municipality m in a given year.

The population-based formula implies that the coefficients λ_m generate multiple population cutoffs. Since most Brazilian municipalities have population around the first cutoff of 10,189 inhabitants, we restrict our attention to it. Following Brollo et al. (2013), Litschig and Morrison (2013), and Corbi et al. (2019), we compare municipalities that received less FPM transfers (because they are barely located on the left side of the population cutoff) with those that received more FPM transfers (barely located on the right side). We estimate the following regression discontinuity specification:

$$y_{mst} = \alpha_{st} + g(P_{m,t-1}) + \beta T_m + \varepsilon_{mst}, \tag{18}$$

in which y_{mt} stands for selected outcomes for municipality m and state s in year t, such as log total public jobs, log total private jobs, log total establishments, and log average luminosity; α_{st} are state-by-year fixed effects; $g(\cdot)$ is a linear distance from the lagged population $P_{m,t-1}$; and T_m is an indicator variable for treated municipalities located on the right side of the population cutoff.

Column (1) of Table E3 displays the first stage: on average, non-split municipalities on the right side receive 14 percent more FPM transfers relative to those on the left side of the population cutoff. Nonetheless, Columns (2) to (5) indicate that the higher FPM transfers do not translate into better economic outcomes for these municipalities.

Table E3: Effects of Federal Transfers on Selected Economic Outcomes

	Log Transfers (in Millions) (1)	Log Public Jobs (2)	Log Establishments (3)	Log Private Jobs (4)	Log Luminosity (5)
RD Estimate	0.14***	-0.01	-0.01	-0.02	-0.05
	(0.01)	(0.05)	(0.07)	(0.09)	(0.08)
Observations	9,976	11,236	11,747	11,592	14,144
State-Year FE	✓	✓	✓	✓	✓
Optimal Bandwidth (%)	4.2	7	3.3	6.1	4.3

Note: *** p<0.01, ** p<0.05, * p<0.1. This table reports point estimates from Equation (18) that capture the effects of federal transfers (FPM transfers) on selected economic outcomes. Column (1) refers to the first stage, in which the dependent variable is the log total federal transfers. Columns (2) to (5) report reduced-form estimates considering the following set of dependent variables: log total number of public jobs, log total number of private jobs, log total number of establishments, and log average luminosity.