Decentralizing Development*

Ricardo Dahis† Christiane Szerman‡

August 31, 2020

JOB MARKET PAPER

Click here for the latest version

Abstract

This paper studies how decentralization affects local development. We exploit an episode of a remarkable increase in the number of new municipalities in Brazil to understand the long run impacts of municipal splits. Using a rich panel of spatial and administrative data, we first show that splits are initiated by poor and rural districts motivated by fiscal incentives and neglect from their parent municipality. When using areas whose request to split was never approved as a control group, we find that splitting increases economic activity and public service delivery in new municipalities. Parent municipalities remain unaffected. We document that increases in fiscal revenues and reduced physical remoteness from their administrators are the main channels explaining the results. Our findings suggest that decentralization in the presence of equalizing intergovernmental grants may effectively promote local development.

*Ricardo thanks Seema Jayachandran, Nancy Qian, and Chris Udry for their invaluable advice. We also thank Lori Beaman, Nicola Bianchi, Prashant Bharadwaj, Ernesto Dal Bó, Leah Boustan, Mariana Carvalho, Henry Farber, Thomas Fujiwara, David Lee, Riccardo Marchingiglio, Alexandre Mas, Ameet Morjaria, Joris Mueller, Christopher Neilson, Paul Niehaus, Jacopo Ponticelli, Miguel Talamas, Edoardo Teso, Owen Zidar and participants at the Northwestern Development Lunch, EMCON 2017, MIEDC 2018, UCSD (OMG, Joint Lunch and GSRS), UCB for comments and suggestions. We thank Renan Holanda and Cláudio Júnior for excellent research assistance, and Rudi Rocha for kindly sharing data.

†Economics Department, Northwestern University. Email: rdahis@u.northwestern.edu

‡Economics Department, Princeton University. Email: cszerman@princeton.edu

1 Introduction

The ideal architecture of government has long been studied and discussed by scholars and policymakers. The structuring of government into multiple tiers has been widely implemented through multiple waves of decentralization worldwide, particularly in developing countries (Bardhan, 2002; World Bank, 2004). Yet, despite its importance, the question of whether decentralization promotes the provision of public goods and local development remains unsettled.[1] We investigate this question empirically.

In this paper, we study one of the most common ways to promote decentralization: by creating new jurisdictions within a government tier.[2] The consequences of generating new and smaller subnational units are not obvious as it entails various trade-offs (Treisman, 2007). For instance, smaller jurisdictions may generate losses of scale and efficiency in the provision of public goods, although they may also better serve populations with heterogeneous preferences (Oates, 1972; Alesina and Spolaore, 1997). The creation of new jurisdictions may induce competition across areas, placing pressures on the provision of local public goods to attract residents (Tiebout, 1956). Decentralization can also undermine accountability in uniformed areas (Boffa et al., 2016). In contexts with equalizing intergovernmental grants, horizontal decentralization generates reallocation of funds to new jurisdictions, which may itself affect local development (Oates, 1999; Kline and Moretti, 2014).

We argue that Brazil provides a compelling setting for examining how splitting affects local development for at least three reasons. First, Brazil is a large developing country where local jurisdictions (or municipalities) hold significant political, administrative and fiscal autonomy and decision-making power. In our setting, the Brazilian municipalities are granted responsibility of providing public services[3] and discretion to collect taxes and manage revenues. In practice, becoming a new municipality implies granting more autonomy and decision-making power to the area.

[1] For reviews of the literature, see Rondinelli et al. (1983), Oates (1999), Bardhan (2002), Alesina and Spolaore (2003), Treisman (2007), Gadenne and Singhal (2014), Mookherjee (2015), Rodden and Wibbels, eds (2019) and Gendźwiłł et al. (2020).

[2] Throughout the paper, we refer the process of creating new jurisdictions as splitting. Other terms used in the literature for this phenomenon are administrative unit proliferation (Grossman and Lewis, 2014; Pierskalla, 2019) and government fragmentation (Grossman et al., 2017).

[3] The Brazilian municipalities are responsible for crucial areas, including health care, primary education, land use, public transport, sanitation, trash collection, and street lighting.

Second, the Brazilian context presents several sources of variation that allows us to overcome several empirical challenges typically associated with splitting. In the first half of the 1990s, the country experienced a remarkable 23 percent increase in the number of new municipalities in a short period of time, followed by an abrupt reform in 1996 that stopped the wave of splitting. Moreover, we observe the universe of districts requesting split, including those who succeeded and those who did not. Third, in addition to the availability of high-quality, granular and extensive sub-national data, the timing of splitting provides an opportunity to investigate whether decentralization induces persistent impacts for parent and child municipalities that effectively split. Many studies suggest that interventions may have long lasting effects on local development. As the waves of splitting peak in the 1990s, we examine the dynamic impacts of horizontal decentralization for least two decades following the splits and disentangle the mechanisms underlying our main results.

In order to causally identify effects of decentralization on local development, our main empirical strategy consists of a difference-in-differences (DD) framework comparing areas that effectively split to those that *almost* split around the year of requesting to split. Given that requesting to split is not random and likely reflects positive selection, simply comparing areas that split to those that did not would yield upwards-biased estimates. To identify counterfactuals for areas that split, we leverage exogenous variation in what requests were approved. This comes both from idiosyncrasies of each request and from the enactment of the constitutional amendment in 1996 by the Congress that imposed substantial barriers to create new municipalities. This reform immediately stopped the wave of splitting and left many areas with split requests unapproved or never approved. Using a novel data set on unapproved split requests built from historical archives, we construct a set of locations that *almost* split. These locations constitute our control group for those that effectively split in the first half of the 1990s.

The event studies show no sign of pre-trends, reassuring the causal interpretation of the difference-in-differences estimates. Confounding shocks that could threaten our strategy would need to exactly coincide with splitting. Although it is unlikely that effects of splitting are driven by omitted factors, we develop a complementary empirical strategy. Prior to 1996, in order to become a municipality, the district applying to break off was required to conduct a local referendum and obtain approval from at least half of voters. We exploit a regression discontinuity (RD) design embedded in local referendums: districts with more than 50 percent of voters in favor of splitting are more likely to split than those below this threshold. Using detailed data on referendum results available for a state statistically representative of the whole country, *Minas Gerais*, we use RD results to further probe the robustness of our main difference-in-differences results.

Exploiting multiple sources of data, spanning from high spatial resolution to administrative data, our difference-in-differences results indicate sizable benefits from splitting. Our preferred specification suggests that splitting is associated with around 17 percent increase in economic growth, proxied by night lights, and 20 percent increase in total urban area for new, child municipalities. In both outcomes, the effects sharply grow in the first five years and stabilize afterwards. We also find that new municipalities experience a 4 percent growth in the total number of lit pixels and a 3 percent decrease in Gini coefficient of lights in comparison to areas that *almost* split, suggesting gains in extensive margin in night lights and reductions in spatial inequality, respectively. These patterns persist 15 years after splitting, and they are driven both by government investment in public lighting and by private-sector economic activity.

The RD estimates qualitatively follow the difference-in-differences results. We show that districts that barely win the referendum and ultimately split experience an average increase in night lights by about 48 percent relative to those that barely lose. We decompose the average effect into annual estimates in a difference-in-discontinuities framework and document a remarkably similar pattern to our event study estimates. Interestingly, in both empirical strategies, we do not find any impact for parent municipalities, suggesting that they remain largely unaffected by the splitting process.

Turning to the Census data, we document that municipalities that split experience large and persistent improvements in multiple dimensions: urban growth, literacy rates, school attendance, and provision of public services. In comparison to the control group, the fraction of households having piped water, trash collection, electricity and sewage increase by around 6-14 percent. Poverty rate declines by 7 percent. In additional cohort analyses exploiting the timing of birth around splits, we find that school attendance grows by around 4-7 percent for individuals aged 15 or younger up to 15 years after splitting.

Taking advantage of administrative matched employer-employee data covering the entire formal sector, we further show evidence of splitting leading to significant and positive impacts on the local labor market and on the provision of public services. We find that the size of the government, as measured by the number of municipal public jobs, increases by 21 percent, while average total employment in the private sector remains stable over time. The total number of establishments increases by 6 percent after splitting. Our results mask important heterogeneity revealing that municipalities go through structural transformation. We find that employment and firm growth are large in the services sector, but there are slightly negative effects on agriculture and extraction industries. Finally, as an indication that municipalities successfully improve public goods provision, we show that public employment in education and health grows by 13 and 10 percent, respectively, after splitting. This change is accompanied by crowd-out of local education nonprofit employment.

We dig deeper into the mechanisms behind our results. First, we show that ethnic, racial, and religious divisions are unlikely to be one of the determinants of splitting in our context. This allows us to rule out one of the most common drivers of the creation of new jurisdictions in the literature (Pierskalla, 2016; Dunning, 2019; Bazzi and Gudgeon, 2020). Second, we find no evidence of increasing migration into new municipalities, ruling out the channel of individuals ”voting with their feet” (Tiebout, 1956) and endogenous sorting of citizens in response to splitting. Third, we document that the impacts on economic activity are stronger in areas located farther away from the parent town hall and with larger share of rural population, indicating that splitting alleviates neglect by administrative headquarters. More remote areas benefit more by the creation of new municipalities as the physical distance to new administrative headquarters tends to be lower in comparison to the old ones. Fourth, we document that splitting is followed by a country-wide redistribution of federal transfers by 5.5 percentage points, or approximately 1 billion dollars, to child municipalities, indicating that our findings for new municipalities are partially driven by gains in fiscal revenues. On the other hand, we find no evidence that splitting affects economic activity and other outcomes in parent municipalities – including those that experienced losses in federal transfers, which suggests aggregate efficiency gains.

On the cost side, new municipalities spike up capital investment and, to a lesser degree, current expenditures immediately after splitting (Lima and Silveira Neto, 2018). The increase in local expenditures to run new administrative units, however, is substantially smaller than the aggregate gains in economic activity and public services. Parent municipalities remain unaffected by splitting. Our findings suggest that the gains from decentralization accrue to new municipalities, particularly the distant and rural ones.

Lastly, we directly explore to what extent are our findings explained simply by the observed growth in fiscal revenues for split municipalities. We estimate a horse-race difference-in-differences regression similar to our main specification, but where we directly control for revenues per capita. Conceptually, this exercise holds fixed the resources channel and estimates the residual effect of splitting on economic activity, i.e. gains in autonomy, reduction in administrative remoteness, etc. We find that fiscal revenues only partially explain our main results. Our conclusion is that other mechanisms discussed in the text are at play in our findings.

This paper makes contributions to several strands of literature. An extensive body of research studies different aspects of multi-tier governance and federalism, albeit with distinct terminology: decentralization (Oates, 1972, 1999; Bardhan, 2002; Hooghe and Marks, 2003; Treisman, 2007; Faguet, 2004, 2014; Gadenne and Singhal, 2014; Mookherjee, 2015; Rodden and Wibbels, eds, 2019), border reforms (Coate and Knight, 2007; Boffa et al., 2016; Schoenholzer, 2018; Gendźwiłł et al., 2020), amalgamations (Fox and Gurley, 2006; Weese, 2015; Egger et al., 2017), municipal cooperation (Ferraresi et al., 2018; Tricaud, 2019), the size of nations (Bolton and Roland, 1997; Alesina and Spolaore, 1997, 2003; Lassen and Serritzlew, 2011), and administrative unit proliferation (Green, 2010; Pierskalla, 2016; Grossman and Lewis, 2014; Grossman et al., 2017; Pierskalla, 2019). The majority of empirical papers study effects of amalgamations on fiscal outcomes, such as spending on administrative costs, with a few notable exceptions (Gendźwiłł et al., 2020).[4] Effects on public service provision are studied by Reingewertz (2012) in Israel, by Grossman et al. (2017) in Africa, and by Mattos and Ponczek (2013) and Lima and Silveira Neto (2018) in Brazil. To the best of our knowledge, we are first to study an event of horizontal decentralization with credible identification, providing separate results for child and parent municipalities, and using detailed data on economic activity and public service provision.

Second, we contribute to the literature on place-based policies and effects of public spending. Kline and Moretti (2014) discuss under what conditions may place-based policies have net positive welfare effects. Shenoy (2018) concludes that the robust growth observed in the new state of Uttarakhand in India was caused by massive place-based spending. The specific literature on Brazil estimates fiscal multipliers and other effects of spending exploiting exogenous variation in intergovernmental grants (Litschig, 2012; Brollo et al., 2013; Litschig and Morrison, 2013; Corbi et al., 2019). We estimate returns to

[4] Lockwood (2002) and Besley and Coate (2003) develop theoretical models comparing centralized versus decentralized provision of public goods in political economy settings.

intergovernmental grants exploiting the reallocation of resources to underserved regions (Grossman et al., 2017). Our results are consistent with agglomeration economies driven not by industrial growth, but by government structure and services.

Lastly, we contribute to the literature and debate about the number of municipalities in Brazil (Gomes and MacDowell, 2000; Tomio, 2002, 2005; Cachatori and Cigolini, 2012, 2013; Mattos and Ponczek, 2013; Lipscomb and Mobarak, 2017; Lima and Silveira Neto, 2018). While the current consensus is that splits were almost uniformly bad because new municipalities were small and fiscally unsustainable, we find novel evidence to the contrary. We show that splits were likely a net positive, in agreement with Klering et al. (2012), who find that smaller municipalities show better governance indicators. This finding informs the policy trade-off Brazil faces, which has been actively deliberated upon in Congress in recent years.

The rest of the paper is organized as follows. We describe the institutional background in Section 2. We detail our data sources, identification strategies and sample selection in Section 3. Results follow in Section 4, and Section 5 discusses to what extent fiscal revenues explain our findings. Section 6 concludes.

2 Institutional Background

2.1 Decentralization and the Role of Local Governments

The organizational structure in Brazil is characterized by three tiers of government with fiscal, administrative and political responsibilities: federal, states and municipalities. The country is divided into 26 states and one federal district. By 1988 there were 4124 municipalities, the smallest unit of government carrying decision-making power. The country is also comprised of 10,283 districts, which do not have political or administrative autonomy. One municipality may have one or more districts, although no district belongs to two different municipalities.

Most of current structure dates back to 1988, when the Federal Constitution was enacted, representing an important advancement towards fiscal federalism and decentralization of political power and financial resources (Arretche, 2000; Favero, 2004). In addition to being responsible for providing land use, public transport, water, sanitation, trash collection and street lighting services, the Federal Constitution endowed responsibility for health care and primary education to municipalities.[5] They were also granted the same political and administrative autonomy as state and federal governments, including power to collect and manage local taxes and discretion to administer their own revenues.

The decentralization process in our context[6] involves additional political, administrative and fiscal responsibilities in the roles of municipal governments (Treisman, 2007).[7] Following Hooghe and Marks (2003), municipalities in Brazil are *type-1* jurisdictions as they are general-purpose for holding a large range of functions, are durable[8], and are characterized by non-intersecting boundaries.

Municipal governments are formed by a mayor and a city council, both elected every four years. In municipalities with more than 200,000 voters, mayors are elected through majority rule, while municipalities with total number of voters below this threshold use plurality rule to elect mayors. City councilors are elected using an open list proportional representation system. Elections are typically held in October and elected mayors and city councilors take office in January. Since 1988, they have been vital in shaping local public policies as a result of increasing decentralization in the provision of public goods.

2.2 The Creation of New Municipalities

Until 1988, the number of municipalities remained stable. This changed after the 1988 Federal Constitution as states were granted authority to establish their own rules and criteria regarding the creation and amalgamation of municipalities (Brandt, 2010). The requirements varied across states, though generally involved territorial contiguity, a minimum population, and some level of urban development for new municipalities. The process to create a new municipality consisted of multiple stages: (i) local leaders or state politicians had to formally request the creation a new municipality to state assembly; (ii)

[5]A minimum fraction of total revenues is required by law to be spent on health and education. There are also constraints over personnel expenditures.

[6]It is worth emphasizing that this decentralization process is different from *vertical* decentralization, which refers to the creation of new tiers of government or to transfer of functions from a higher tier to a lower one.

[7]We adopt terminology from Treisman (2007). Administrative decentralization refers to the various government functions and policies that are executed by different tiers. Superior tiers may reverse and overrule decisions of lower ones. Political decentralization is the same as *appointment* decentralization, *decision-making* decentralization, or *constitutional* decentralization. Fiscal decentralization involves decision-making decentralization on matters of taxation and expenditure (Ebel and Yilmaz, 2002).

[8]Type-1 jurisdictions are generally durable in the sense that creating, abolishing or adjusting new jurisdictions is costly and rare Hooghe and Marks (2003).

state legislative's committee responsible for evaluating this request approved it; (iii) state legislative authorized a local referendum in the applicant district, though state governor could veto it; (iv) if the majority of voters in the referendum voted for splitting, the request was put forward for voting in state legislative; (v) thereafter the state and federal governments had to approve it, although either of them had the discretion to veto it (Tomio, 2002). These vetoes were rare, and the country experienced an unprecedented increase in the number of districts requesting to split and breaking off to become municipalities in the first half of the 1990s.

In 1996, the National Congress curbed the creation of new municipalities by enacting the Constitutional Amendment 15/1996 (henceforth "1996 CA"), which removed from states and assigned to federal government the authority to regulate splits and amalgamations. The process to create a new municipality was similar to before except for several changes. First, districts requesting to split had to conduct a referendum involving the entire municipality, including other districts, and obtain approval from the majority. Second, the applicant district started to be required to present more evidence that, in the case of becoming a new municipality, it would by financially sustainable (Klering et al., 2012). In practice, the federal government virtually stopped the splitting process through the 1996 CA, inducing a halt in the creation of new municipalities and producing a sizable number of districts with unapproved requests due to stricter rules.[9]

The successful applicant district (or group of applicant districts) was only officially established as municipality after municipal elections when mayors and city councilors were elected. Figure 1 displays the evolution of the number of municipalities with a noticeable jump from 4,124 in 1988 to 5507 municipalities in 1997, an increase by around 23% percent. We observe two main waves of splitting, in 1993 and 1997, the years immediately after municipal elections. These split requests, however, were approved before the 1996 CA.

2.3 The Reasons for Splitting

Considering the set of push and pull factors raised in the literature explaining border reforms, we highlight two major factors in the Brazilian context: administrative negligence and fiscal incentives.

[9] By 2008 there were more than 800 districts with open requests for splitting. See here.

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

The second relevant factor involves fiscal incentives from the distribution of federal transfers. The *Fundo de Participação dos Municípios* (henceforth "FPM") is the main mechanism through which federal government transfers money to municipalities. Introduced in 1965, FPM was largely modified by the 1988 Federal Constitution (Tesouro Nacional, 2018).[10]

For the purposes of this paper, five features of FPM design are important. First, allocation is calculated mechanically as a function of population.[11] Second, transfers are zero-sum within state. Each state receives a block grant to be shared between its municipalities. Third, municipalities in the same bracket within state should get the exact same amount, independently of their exact population. Fourth, the scheme disproportionately benefits smaller municipalities in terms of population. As we show in Figure C.2a, the revenue share from FPM grows nonlinearly with decreasing municipality population, although this group receiving a small share of total transfers (e.g. the bottom half municipalities received only 26% of transfers, in Figure C.2b). Fifth, when a municipality splits, while the amount of transfers likely increases both in total and for the applicant district, it may not change for the parent district. The net effect depends on (i) what was the allocation of funds within municipality prior to split, (ii) how nonlinear the FPM curve is, and (iii) how many splits there are in total within the state.[12]

[10]The reform was implemented by legislation in 1981, although it was only rectified by the new Constitution

[11]Several papers exploit the function's discontinuities to estimate effects of federal transfers on different outcomes (Litschig, 2012; Brollo et al., 2013; Litschig and Morrison, 2013; Gadenne, 2017; Corbi et al., 2019).

[12]Consider the following framework. Suppose a municipality is composed of two districts A and B, with population of sizes α_A and α_B, respectively. District B considers splitting into a new municipality. Before the split, FPM revenues are $R_A = w_A f(p_A + p_B)$ and $R_B = w_B f(p_A + p_B)$, where $w_A + w_B = 1$. Weights w_A and w_B are shares of total revenue received by each district, which may be proportional to population (i.e. $w_A = \frac{\alpha_A}{\alpha_A + \alpha_B}$) or not. Splitting generates higher revenues for A if $w_A > \frac{f(\alpha_A)}{f(\alpha_A + \alpha_B)}$, and for B if $w_B > \frac{f(\alpha_B)}{f(\alpha_A + \alpha_B)}$. We first conclude that total revenues likely increase (because $f(\alpha_A) + f(\alpha_B) > f(\alpha_A + \alpha_B)$), and that revenues for B also increase (because w_B is small and f is increasing and concave around small α_B). Revenues for A may increase if w_A is sufficiently low and if $\frac{f(\alpha_A)}{f(\alpha_A + \alpha_B)}$ is sufficiently high; and vice-versa.

In Appendix A, we outline a simple model illustrating the expected effects of splitting on public goods provision. We incorporate the main features of the Brazilian context into the model to generate testable predictions which we test in our empirical analysis. We model a municipality composed of two districts. District A is headquarters, and district B considers splitting off into a new municipality. When united as a single jurisdiction, headquarters chooses levels of public good provision for both districts to maximize a weighed sum of districts' welfare subject to a budget made of local taxation and federal transfers. If district B splits, it chooses its level of public goods subject to its individual budget constraint. The model predicts that district B is more likely to request to split when it: (i) is neglected by its headquarters, (ii) is small and gains substantial transfers if split, (iii) has high preferences for public goods and a large tax base. The model also suggests that incentives to split are commensurate with potential gains in public goods provision. Therefore, gains would be larger gains in districts that effectively split in comparison to those that did not.

3 Empirical Strategy

3.1 Data

3.1.1 Splitting Requests

We catalog the splitting requests made by districts between 1989 and 1996.[13] We retrieve this information from two sources. First, we use the list of municipalities disclosed the Brazilian Institute of Geography and Statistics (IBGE), which provides an overview of the creation of municipalities. We obtain information on which and when municipalities have been created and officially established together with which municipalities they have belonged to before their creation.

Second, we manually collect and classify historical archives on splitting requests by

Since municipalities receive constant amounts within state and population bracket, the latter comparison depends on whether district A moves to a lower bracket after the split. Lastly, changes in f also depend on the total number of splits within state because one split lowers the total amount available to every other municipality.

[13] There are districts that request to split even after the 1996 CA, although these requests are unlikely to be approved. The National Confederation of Municipalities (CNM) documented 418 requests on the queue to get approval in 2008.

districts from twelve states.[14] Prior to the 1996 CA, each state assembly set their own additional rules to handle splitting requests. The availability and quality of the data vary across states. The level of detail of these requests, encompassing information like submission and vote dates and whether they have been vetoed, also differs by state. We describe the data construction process in further detail in Appendix Section B. The final data includes the universe of split requests made by districts between 1989 and 1996, including those that passed and those that did not.

In addition, to implement our Regression Discontinuity analysis, we manually scrape legislative reports and collect individual referendum results for the state of Minas Gerais (MG). This is only state that provides reliable and complete information on splitting referendums, including information on turnout, percentage of valid votes and percentage of voters in favor of splitting. We crosscheck and validate information from reports regarding the result (split or not) with our data collection on split requests.

3.1.2 Outcome Data

We examine the long-run impact of splitting on local development by combining different sources of spatial and administrative data, which we describe in detail below.

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

[14] We obtain the information for the following states: Amapá, Amazonas, Espírito Santo, Goiás, Mato Grosso, Minas Gerais, Pará, Paraná, Rio Grande do Sul, Rondônia, Santa Catarina, and São Paulo. The remaining fourteen states do not provide public information about splits.

[15] In particular, the grid represents every 30 arc-second output pixel, equivalent to 0.86 square kilometers at the Equator.

[16] As Henderson et al. (2012) and Henderson et al. (2018) describe, intensity of night lights measures both outdoor and some indoor use of lights. The authors show that night lights are a good proxy for GDP growth over the long-term growth. This is particularly useful in our context as information on economic activity is not collected at the district level and data on electricity consumption only contain more recent information.

Land Use. We use data about land use from the *MapBiomas* project. The data set classifies Landsat 30m^2-resolution images from 1985 to 2018 into land use categories. We aggregate images to construct annual information at the district level on the extent of land covered by urban structure, forestry, agriculture, pasture, and others.

Demographic Census. We use decennial Census data from IBGE and from the Atlas of Human Development in Brazil (United Nations Development Programme, 2013) to get demographic characteristics at the individual and municipality levels in 1991, 2000 and 2010, including population size, urbanization, education, health, access to public services, and income. We additionally collect individual-level microdata on educational attainment, health, and income. While these data contain municipality identifiers, they do not have district identifiers.[17]

Formal Labor Market. We use the matched employer-employee database, the *Relação Anual de Informações Sociais* (RAIS), carried out by the Brazilian Ministry of Economy. The data covers the formal sector[18] (Dix-Carneiro, 2014; Alvarez et al., 2018) and includes a rich set of worker (e.g. education, age, gender, race, etc.), job (e.g. contract duration, hours worked, occupation, etc.) and establishment (e.g. payroll, location, economic sector, etc.) characteristics. We use individual level data to generate information on total employment in the public and private sectors at the municipality level. We also generate information on total employment by economic sector such as education and health.

Public Finance. Information on revenues and expenditures at the municipality level are available since 1989 and obtained from the Brazilian National Treasury (Finbra). The data contains details on revenue sources, including local taxation and federal transfers, and expenditure categories, including capital and current expenses. We construct an annual panel at the municipality level describing the composition of revenues and expenditures.

Additional Sources. Our analysis also includes the following minor sources of data: (i) soil suitability from FAO-GAEZ; and (ii) terrain ruggedness from Carter (2018).

3.1.3 Sample Selection.

We take several steps to build our final estimation sample. Starting from a sample of 10,283 districts listed in the 2010 Census, we first restrict it to districts meeting the follow-

[17] We harmonize variables across years with material provided by the Data Zoom project.

[18] It does not cover informal labor, some categories of self-employed businesses and of public employees.

ing criteria: (i) districts that belong to one of twelve states with records on split requests; (ii) districts in municipalities with a single split event or split requests between 1989 and 1996; and (iii) districts that do not belong to state capitals. These restrictions leave us with a sample of 2,259 districts.

We then group these districts into four different sets: (i) *split applicant*, which contains districts that requested to split and had such request approved, becoming new municipalities; (ii) *split remaining*, which includes districts that did not apply to split but were located in municipalities that lose territories due to the creation of new municipalities; (iii) *almost split applicant*, which comprises districts that unsuccessfully requested to split; and (iv) *almost split remaining*, which encompasses other non-applicant districts from municipalities that had at least one district with a denied split request. This division leads to 990 applicant (817 split and 173 almost split) and 1,269 remaining (964 split and 305 almost split) districts. Figure 2 plots these districts, already classified into four groups, using the Brazilian map. There are two noteworthy patterns: split requests are geographically scattered and, despite some geographical clustering, there is large variation in where split and almost split events happen.

Because some data are only available at the municipality level[19], we apply similar restrictions to municipalities.[20] Starting from the universe of 4,298 municipalities, the restrictions yields a final sample of 468 municipalities (344 split and 124 almost split). Due to the nature of the geographic unit, we are not able to distinguish between applicant and remaining districts within a municipality.

3.2 Selection into Splitting and Summary Statistics

Before outlining our empirical strategy, we discuss how districts select into splitting and how that relates to our analysis. Given our institutional context described in Section 2, districts that split are more likely to be less developed than other parts of the country, which is corroborated by our data. Therefore, any simple comparison between districts

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

[20] In particular, we keep municipalities meeting the following criteria: (i) municipalities that belong to one of twelve states with records on split requests; (ii) municipalities with a single split event or with districts having split requests between 1989 and 1996; and (iii) municipalities that do not belong to state capitals.

that split and those that did not is likely to deliver biased estimates.

Our empirical analysis uses districts that failed to become new municipalities to construct a set of counterfactual districts. We argue that, although requesting to split is not random, if we restrict our sample to the group of applicant districts, the timing of splitting is likely to be exogenous to our outcomes of interest. The approval process itself was long and consisted of multiple steps characterized by idiosyncrasies that would vary the request's duration and its ultimate outcome, e.g. legislative commissions denying the request, the referendum not returning a majority, or the state governor vetoing the request. Moreover the sudden enactment of the 1996 Constitutional Amendment left a substantial number of requests open. Indeed, a large fraction of requests made by almost split districts was started only in 1995. In addition, our main identification strategy, which we outline in detail in the next section, does not require balance on covariates. It relies on parallel trends between split and almost split areas before the splitting events.

In Table 1, we compare districts' average characteristics before the first wave of splits for three groups: (i) applicant districts, including split and almost split applicant (Column 1); (ii) remaining districts, including split and almost split remaining (Column 3); and (iii) the rest of districts that never had requests to split (Column 5). We document that applicant districts are, on average, smaller in terms of area and population, more rural, less populated and urban, and located farther away from the municipality's town hall and the state capital than non-applicant and never split districts. Overall, Table 1 confirms that districts that applied to split, regardless of having succeeded or not, were economically less developed.

In contrast to other countries, such as Indonesia (Pierskalla, 2016; Bazzi and Gudgeon, 2020) or India (Dunning, 2019), ethnic, racial, and religious divisions across districts are unlikely to be one of the core causes for splitting. Accordingly, Table 3 indicates no significant patterns of differential racial or religious fragmentation in Brazilian municipalities in 1991,[21] before the waves of splitting, allowing us to shut down one of the most common drivers of splitting in the literature.

[21] We draw this information from the decennial Census.

3.3 Regression Specification

This paper uses a difference-in-differences strategy to estimate the impacts of decentralization on districts that request to split and on remaining districts in the municipality that lose territory due to splitting. More precisely, we estimate two specifications comparing districts that split with districts that almost split, before and after each district's attempted split date.

We build a balanced panel of districts fixing their geographical boundaries over time.[22] Moreover, from our sample selection choices, we exclude any districts not ever involved in split requests. To systematically test for the presence of pre-trends in our various outcomes, we allow for time-specific coefficients before treatment. Formally, once only for applicant districts and once only for remaining districts, we estimate

$$y_{dt} = \alpha_d + \alpha_{s(d)t} + \sum_{\tau=-\underline{\tau}}^{\overline{\tau}} \beta_\tau Split_{m(d)} \mathbf{1}[t - E_{m(d)} = \tau] + \boldsymbol{X}_{dt}\boldsymbol{\lambda} + \varepsilon_{dt} \tag{1}$$

where y_{dt} represents an outcome for district d in year t. We include district fixed effects α_d to control for district-specific non-time-varying characteristics, and state-time fixed effects $\alpha_{s(d)t}$ to control for any state-specific time effects affecting districts equally. These include aggregate trends in economic growth, inflation, or other state-level policies. $Split_{m(d)}$ indicates whether the municipality m went through a split or not. The variable $E_{m(d)}$ represents the term-year of split request for municipality m. For example, if the request happened in 1995, we code $E_{m(d)}$ as 1997.[23] The choices of starting lead $\underline{\tau}$ and last lag $\overline{\tau}$ vary according to what is available for each outcome of interest. The vector $\boldsymbol{X}_{dt}$ contains an array of time-varying controls, including all baseline variables used in Table 2 interacted with time fixed effects. We cluster standard errors ε_{dt} at the municipality $m(d)$ level.

To complement our analysis, since part of our data is not available disaggregated at the district level, we estimate similar regressions but at the municipality level. Despite not being able to directly test for sorting within municipality in this specification, we extrapolate from our district-level regression results to assume that patterns are driven

[22] A balanced panel alleviates potential worries with districts non-randomly appearing and disappearing from the data.

[23] This step groups variation in split request dates into only 1993 and 1997. We make this choice because our definition of treatment is not requesting to split, or having the split approved by a certain date, but the changes in governance and autonomy that start only after the start of the following election term.

by applicant districts, and not remaining districts. Specifically, we estimate

$$y_{mt} = \alpha_m + \alpha_{s(m)t} + \sum_{\tau=-\underline{\tau}}^{\overline{\tau}} \beta_\tau Split_m \mathbf{1}[t - E_m = \tau] + \mathbf{X}_{mt}\boldsymbol{\lambda} + \varepsilon_{mt} \tag{2}$$

where y_{mt} represents outcomes for municipality m in time t. All other variables are analogously defined as for Equation (1).

From Equations (1) and (2), our coefficients of interest are the set of β_τ's, which capture the dynamics effects of splitting relative to the year before the event time. We normalize $\beta_{-1} = 0$ such that all estimates are relative to the year right before treatment (i.e. 1992 or 1996). Identification relies on two assumptions. First, we assume selection-into-applying bias is on average equal within applicant districts. Second, the timing of splitting is uncorrelated with the outcomes of interest, conditional on the set of fixed effects and controls. The key identifying assumption is districts' (municipalities') outcomes for treated and control units would have followed parallel trends in $\tau \geq 0$ if no splitting had occurred.

We provide three indirect tests of our identification hypotheses. First, we test to what extent baseline characteristics predict splitting, both for applicant and for remaining districts. We estimate the following linear probability model:

$$Split_d = \alpha_{s(d)} + \mathbf{X}_d\boldsymbol{\beta} + \varepsilon_d \tag{3}$$

where $Split_d$ refers to whether a district d split, and $\mathbf{X}_d$ contains a host of characteristics. We include state fixed effects because each state had a different splitting policy, as described in Section 2. We further distinguish districts by what split wave they were in, either 1993 or 1997, to test whether there was differential selection over time. Table 2 shows that most of our covariates do not statistically significantly predict splitting. The relevant exceptions are logs of luminosity, area, distance to parent townhall and to the state capital. In our estimation we flexibly saturate regressions with interactions of all baseline characteristics with time dummies to control for any differential trends by observables.

Second, we empirically test for the presence of pre-trends in every regression we have the data to do so. As we describe in detail in Section 4, we find little or no evidence of pre-trends across specifications. This reassures us that districts involved in splitting were not in differential trends when compared to those not splitting, before treatment. Lastly, we complement our main empirical results with a difference-in-discontinuities exercise (Grembi et al., 2016). We describe it in more detail in Section 3.3, but we emphasize here that this strategy yields qualitatively similar results.

4 Main Results

In this Section, we estimate the effects of municipal splits on a variety of outcomes. We start by focusing on results about economic activity and the provision of public goods and services. We then shed light on various mechanisms to help explain the main results. Furthermore, we specify and detail results on education provision of children at school age. Lastly, we discuss a validation exercise in which we exploit the fact that a necessary condition for splits is for district to get at least 50% of the local referendum vote.

4.1 Effects on Economic Activity and Public Services

We report the first estimated coefficients from Equation (1) in Figure 3. Vertical lines represent the 95%-confidence intervals. Each set of estimates corresponds to a difference-in-differences specification: one for applicant districts and one for remaining districts. Figure 3a shows results for log of average luminosity in the district, as a proxy for economic activity. Figure 3b shows results for a measure of urban agglomeration city structure, namely log of total area covered by pixels labeled urban. Both Figures consistently present three patterns.

First, coefficients for leads, before treatment, are all close to zero and statistically insignificant. This reassures us that our estimates are not driven by existing trends in luminosity and urbanization. Second, we observe growth that picks up right after splitting for applicant districts. This growth peaks in 5-8 years after split at about 30% and levels off after that. In a less conservative exercise, we plot in Figure C.3 the same regressions, but without all extra controls. We find qualitatively similar effects. Coefficients are more precisely estimated and more stable.

Third, for remaining districts, we observe no effects on luminosity and small negative effects on urbanization. This finding is evidence against theoretical arguments implying symmetric effects, such as a loss of scale, or both types of districts changing political equilibria to better represent the district's median voter preferences (Alesina and Spolaore, 1997; Bolton and Roland, 1997). It is also evidence against a prediction of negative spillovers, with resources (both capital and labor) being transferred from headquarter districts to new municipalities, with losses to the former. We discuss this point in more detail below.

Table 4 provides a precise estimate of the effects of splitting on log of average luminosity. Columns 1 to 3 refer to regressions for applicant districts, while columns 4 to 6 refer to remaining. Columns 1 and 4 do not include state-year fixed effects to benchmark our difference-in-differences strategy. Columns 2 and 5 report estimates from specifications without extra time-varying controls. For applicants the average effect of splitting on luminosity is 25%, while for remaining districts that effect is zero. In columns 3 and 6 we include our list of controls, and the effect lowers to 17% for applicants, still significant at 1% level, and remains zero for remaining.[24]

In theory, splitting could impact districts differently according to starting size, location, and other relevant characteristics. For example, one may expect smaller districts to benefit the most from becoming municipalities by enjoying the largest returns to fiscal spending, or from having their particular policy demands met. We test these questions for applicant districts in Table 5. Columns 2 and 3 test for measures of scale and distance to larger urban centers. However, results show no relevant heterogeneity by baseline characteristics we have available.

Beyond the average growth for a given district, we are also interested in to what extent it is driven by extensive or intensive margins. In Figure 4 we study how two measures of the spatial distribution of activity change with splits. In particular, these are measured by the % of pixels with Digital Number (DN) luminosity above zero, and by a luminosity Gini index. Both graphs show similar patterns: not only are previously-lit pixels getting brighter, but also luminosity reaches new pixels at a 4% growth. This phenomenon is strong enough such that the concentration of luminosity indeed decreases. Lastly, we find no statistically significant results for remaining districts.

To study other dimensions of impacts, we estimate Equation (2) without leads and averaging lags using data from three waves of Census data in 1991. This has the advantage of validating our previous findings with administrative data, and of detailing govern-

[24]We use insights from Oster (2019) to assess the bias due to unobservables using the sensitivity of the treatment to added controls. Under the assumption of proportional selection, we find in Table C.1 that selection in unobservables would need to be about 28% of selection in observables to explain our baseline estimate for applicant districts.

ment policy reactions to splitting. Each coefficient in Figure 5 corresponds to a separate regression with coefficient standardized by the outcome's mean and standard deviation in 1991. Bars represent 95%-confidence intervals. Results show that, when compared to almost splits, municipalities with splits have urban growth of 9% and no significant change in population composition. There are gains in literacy rates of 9%, and large increases in school attendance for children in preschool and middle school. Interestingly, we find no effects for high school attendance. This is reassuring given that municipalities are constitutionally responsible for education only up to middle school. Moreover, we find no significant effects on health as measured by life expectancy or child mortality. On the other hand, public service provision improves: we find a growth in the percentage of households served by piped water, trash collection, electricity and sewage of about 6-14%.

Since households self-declare access to electricity, we deduce that our main effects on luminosity discussed above are at least partially driven by public investment. We discuss this channel in more detail below.

Our similar-in-magnitude results about access to electricity from the Census (i.e. 10%) and about average nighttime luminosity from satellite imaging (i.e. 8% at the municipality level) point to little change in luminosity being driven by economic growth as discussed by economic

align

Our results in access to electricity

Since households

8% in luminosity from satellites

10% from Census

Finally, we find marginally significant drops in poverty of 7%.

4.2 Cohort-Analysis in Education

To further detail the changes municipalities go through after splits, we would ideally have direct evidence on the provision of public goods, such as the construction of schools or hospitals. This, however, is unfeasible for Brazil because spatial data of this quality is available only from the 2000s onwards. Nevertheless, one exercise we can implement is testing whether educational attainment changes for children at ages particularly affected by improvements in education provision. This is also useful to make sure that the effects at municipality-level are not driven by composition effects or migration.

We make use of Census microdata between 1991 and 2010 to estimate the effects of municipality splits exploiting variation in children's age in the spirit of Duflo (2001). In particular, we exploit three sources of variation: (i) municipalities with a split versus an almost split, (ii) time before and after splits, and (iii) the age distribution of individuals surveyed. We estimate the differential trends in educational outcomes for a given age group across municipalities that split versus almost split. This strategy can control for three sources of confounding variation: age-specific within municipalities, municipality-specific trends, and state-age-specific trends. Our identification assumption is that, absent a split, birth cohorts would have had the same education trend in split versus almost-split municipalities. Formally, we estimate a triple-differences (DDD) model

$$y_{imkt} = \alpha_{mk} + \alpha_{mt} + \alpha_{s(m)kt} + \sum_{\tau=5}^{31} Split_{mt}\mathbf{1}[t-k=\tau]\beta_\tau + \boldsymbol{X}_{imkt}\lambda + \varepsilon_{imkt} \tag{4}$$

where y_{imkt} represents outcomes for person i in municipality m, birth cohort k and year t. We include all two-way fixed effects to flexibly control for municipality-cohort, municipality-time and state-cohort-time variation. The variable $Split_{mt}$ takes value zero for $t = 1991$ everywhere and one for years $t \in \{2000, 2010\}$ in municipalities that split. The vector $\boldsymbol{X}_{imkt}$ includes person-cohort-varying controls, such as fully saturated sets of dummies for the person's gender, race, religion, nationality. We cluster standard errors ε_{imkt} at the municipality level. Finally, we normalize the last $\beta_{31} = 0$, such that all estimates are relative to individuals 31 years old. Our coefficients of interest are the β_τ's, which identify the effect of municipality splits on education outcomes for a person of age τ at time of each Census.

We present our main results in Figure 6. We plot the raw data in panels (a) and (c) for comparison. Panels (b) and (d) display our DDD estimates on school attendance and literacy rates. Gray areas represent 95%-confidence intervals. We find precisely estimated growth of 4-6 p.p. on school attendance for individuals in age groups 15 years old or younger. Moreover, we find small but statistically insignificant effects on literacy rates. The excluded group (individuals aged 31 in almost split municipalities in 1991) shows an average 3.1% school attendance and 93.3% literacy. These results are consistent with municipalities investing in education for kids up to 15 years old, exactly as we would expect given that high school is under the jurisdiction of states. Municipalities make progress in universalizing schooling but not as much in raising education quality, at least during the time period we observe. This story is in line with prior research on the history of the Brazilian education system.

4.3 Mechanisms

In this Section we explore in detail a variety of mechanisms that explain how municipalities involved in splits grow and better provide public services. To do so, we make use of the rich panel data set we described in Section 3.1.

4.3.1 Public Finance

The discussion in Section 2 made clear that fiscal incentives played an important role in motivating districts to apply for splitting. Additionally, the institutional rules governing federal transfers predicted gains in revenues for new small municipalities. We directly test this story by estimating Equation (2) with data on municipality's public finance. The main results are reported in Figure 7. We find large increases in both revenues and expenditures immediately after the municipality splits.

More specifically, revenues per capita grow by about 16% after splitting. This effect is directly accounted for by a growth in federal transfers per capita in panel (c) of about 35%. Notably, as shown in panel (d), revenues from local taxation remain constant after splitting.[25] This lack of crowd-out suggests that local governments were constrained in revenues relative to their desired level of public good provision, and that they do not internalize the marginal cost of raising intergovernmental grants.

Accordingly, we find in panels (e) and (f) that capital expenditures spike up by 54% when compared to the comparison year, from a base 16% of total expenditures, while current expenditures plateau at around 10% growth, from a baseline 84% total expenditures. These findings are consistent with municipalities using the extra revenue to invest in urban infrastructure and public goods, e.g. new government buildings, schools, and street

[25] We do not observe local tax rates. Therefore, this result may be mechanically explained either by (i) an increase in economic activity but lower tax rate, or (ii) a similar tax rate and local taxable income.

lighting (Lima and Silveira Neto, 2018).[26]

4.3.2 Bureaucratic Capacity and Structural Transformation

We can also shed light on the patterns of state capacity and public bureaucracy by measuring how employment changes after splits. We aggregate numbers from RAIS taking advantage of data on employers' unique identifying code and the data set's classification of occupations and establishments into economic sectors. In Figure 8 we plot the coefficients from Equation (2) on the number of public and private jobs in the municipality. Consistent with the current expenditures results discussed above, we find a increase of around 45% in the number of public employees. This is driven specifically by hires at the municipality level, not state or federal.

To examine other dimensions of economic activity, we show that on average the number of jobs in the private sector does not change, while the number of establishments active in the municipality grow by 8%. However, the average effects masks important heterogeneity. In Figure 9 we estimate the simple difference-in-differences effect of splitting measuring activity by economic sector. In all three graphs, for number of establishments, jobs and total wages received, the pattern we observe is that retail and services show a positive change. Agriculture and oil, mining and metals show an imprecisely estimated negative effect, while manufacturing and construction show an imprecisely estimated positive and small effect. The growth in retail and services is consistent with a rise in demand for urban services by a larger local bureaucracy. This finding stands in contrast to the theoretical motivation of place-based policies of enjoying agglomeration economics sustained by industrial development (Kline and Moretti, 2014).

4.3.3 Crowding Out Nonprofits

Moreover, our context and data allow for testing whether government presence and the provision of public services crowds out the local nonprofit sector. Recent work by Nyqvist et al. (2019) and Deserranno et al. (2020) suggests that the opposite result may occur: in poor rural communities, where government personnel is scarce, governments strategi-

[26]Our results are potentially opposite to what Gadenne (2017) finds also in the context of Brazil, i.e. governments can successfully fund public goods from intergovernmental grants. Our setting, however, does not provide a direct test to her theory because we do not explore exogenous sources of variation in local taxation revenue.

cally react to NGO entry and reallocate health providers to other regions. We show in Figure 10 a significant 50-60% drop in employment in local nonprofits in the education sector after municipality splits. This drop happens while government employment in education grew by 30-40% from baseline levels before split, and while private sector employment stayed relatively constant. We interpret this as evidence that a positive shock to the supply of education by governments render NGOs less necessary. These, in turn, reduce the size of their activities in the new municipality.

4.3.4 The Cost of Administrative Remoteness

One recurring argument in favor of decentralization has been that administrative remoteness, as measured by the physical distance between areas and their governing headquarters, reduces state capacity and causes underdevelopment by distancing citizens from administrators (Bardhan, 2002; Mansuri and Rao, 2013; Krishna and Schober, 2014; Asher et al., 2018; Brinkerhoff et al., 2018). This could be explained by transportation costs affecting both the supply side (e.g. bureaucrats not travelling to remote parts of the municipality) and the demand side (e.g. inhabitants of remote areas not expressing their political demands to politicians located in the town hall).

We test whether distance to the municipality's town hall, which we refer to as "administrative remoteness", affects economic development. In our setting districts that split into new municipalities also erect a new town hall building within its borders. This causes a sharp reduction in the distance to the closest town hall. We study the following exercise. Restricting attention to applicant districts that split, we can measure if there is a positive relationship between growth in luminosity after splitting and the reduction in distance to the town hall. A positive slope, i.e. districts with larger reductions in distance growing relatively more, holding observable characteristics constant, would suggestive evidence of costs of administrative remoteness ultimately curbing economic activity. And this is precisely what we find in Figure 11. When controlling for state fixed effects and all baseline characteristics shown in Table 1, we find that a 1% reduction in distance to the town hall correlates with a 3.43% growth in luminosity over a period of 15 years. This slope coefficient is statistically significant at 1% level.

4.3.5 Sorting and Migration

One important aspect of decentralization reforms is the possibility of individuals ”voting with their feet” (Tiebout, 1956). If municipalities attract mobile individuals by splitting and offering new combinations of local taxation and public goods provision, and if this systematically attracts more qualified and richer individuals, then our main results could be driven by sorting.

To test this mechanism, we leverage Census microdata from 2000, which was the first in Brazil to collect data on cross-municipality migration. Our outcome of interest is the percentage of residents living in a different municipality five years before. Since we only observe variation around one wave of split requests, we restrict attention to the second wave before 1997. We estimate a cross-section comparison between split and almost split municipalities. In particular, we estimate

$$y_m = \alpha_{s(m)} + \beta Split_m + \boldsymbol{X}_m \boldsymbol{\lambda} + \varepsilon_m \tag{5}$$

where y_m represents the percentage of residents in municipality m in 2000 declaring having lived in a different municipality in 1995. We include state fixed effects $\alpha_{s(m)}$ and a vector of controls $\boldsymbol{X}_m$ as in (2).

We find no evidence of migration into new municipalities. First, Table 6 documents that municipalities which split have no significantly different percentage of 5-year migrants as compared to almost split municipalities. Column 1 estimates Equation (5) without state fixed effects, while column 2 includes it. Column 1 shows a marginally significant coefficient of 1.0, but Column 2 shows a small-in-magnitude and insignificant coefficient of 0.75. Neither coefficient amounts to more than about 10% of the sample average, and 23% of a standard deviation. Second, our result in Figure 5 shows that population levels do not change over a period of 15 years after a split.

4.4 Robustness Checks

In this Section we discuss robustness checks we perform to our main results. We start by probing our main specification with changes to definitions of outcomes, the sample, and the level at which we include fixed effects. In Table 7 Column 2 we show that our results remain qualitatively the same if we do not include any of the interacted controls described in Section 3.3. To account for composition effects potentially driven by districts showing zero luminosity in some years (and thus being dropped in a log regression in Column 1), we re-estimate regression (1) using ln(0.01 + Avg. Luminosity) as outcome. Results remain qualitatively the same.

Additionally, to explore the fact that splitting was halted abruptly by the 1996 Constitutional Amendment, we restrict attention to only districts whose request was made between 1994 and 1996. Since the administrative process to split may take up to years, we explore idiosyncratic variation in whether or not a given district had its request not approved by the time of the Amendment as exogenous variation in treatment. We re-estimate regression (1) for only this subgroup and show that results are robust, both in Table 7 Column 4 and in Figure C.4. Finally, to flexibly control for any within-state geographical clustering of shocks to economic activity, we also control for micro-region-by-year fixed effects in Column 5.[27] Results remain similar to baseline.

Moreover, from our description in Section 2, a question could remain open, e.g. to what extent is our variation in treatment (split versus almost split) actually exogenous. Despite the evidence from Table 2 showing that, conditional on state fixed effects, baseline characteristics do not significantly predict splitting, one may still expect that districts' unobservable characteristics correlate with who gets a request approved. For example, districts with more organizational capacity or more political connections in the state's legislative may more effectively pressure for approvals. And to the extent that such unobservables may also be positively correlated with potential gains of splitting, this would cause our estimated effects to be biased upwards.

To alleviate this concern, we manually collect data on individual referendums and use variation below and above the 50% cutoff for electoral approval to supplement our main difference-in-differences results with a difference-in-discontinuities (RD-DD) exercise (Grembi et al., 2016). With data for the state of *Minas Gerais*, we argue that districts being just above or just below the approval cutoff is as-good-as-random. We show the first stage of this exercise in Figure D.5a. It is clear from the graph that, in our data, splitting is an almost deterministic function of reaching a majority vote in referendums.

Despite the low number of observations to the left of the cutoff, as shown in Figure D.5b, we can proceed to estimate a fuzzy RD-DD specification. In particular, we estimate

[27] In Brazil there are 558 micro-regions and only 26 states, according to IBGE.

two stages in:

$$\begin{aligned} Split_{m(d)} &= \psi + \phi \mathbf{1}[V_d \geq 50\%] + g(V_d) + \boldsymbol{X}_d \boldsymbol{\omega} + \eta_{dt} \\ y_{dt} &= \alpha_d + \alpha_t + \beta Split_{m(d)} Post_{m(d)t} + g(V_d) Post_{m(d)t} + \boldsymbol{X}_{dt} \boldsymbol{\lambda} + \varepsilon_{dt} \end{aligned} \tag{6}$$

where $Split_{m(d)}$ indicates whether a municipality was split, and V_d represents the referendum vote share. We follow Gelman and Imbens (2019) and estimate $g(V_d)$ as a local-linear polynomial. In our second-stage equation, we leverage time variation in outcomes y_{dt} to add district and time fixed effects, α_d and α_t, and interact district-level variables with $Post_{m(d)t}$. The list of covariates $\boldsymbol{X}_{dt}$ is as in Equation (1). Due to low power on the left side of the cutoff, we pick a bandwidth of 15%. Our main outcome of interest is ln(Luminosity). Our coefficient of interest is β, which captures the effect of splitting among districts that have a referendum.

As a first standard check on RD design validity, we test for continuity at the cutoff for covariates. We estimate our first stage in Equation (6) for covariates at baseline before a split event. Table D.2 documents that, with the exception of % Forest Area being marginally significant, a referendum being above 50% vote in favor is not correlated with any baseline district characteristic such as distance to parent townhall, area, baseline luminosity, and land use variables.

We present our main results in several ways. First, in Figure 12 panel (a), we show the reduced-form raw growth of ln(Luminosity) for applicant districts on both sides of the cutoff, before and after split request events. The figure documents a visually clear jump with a slightly negative slope to the right, but which remains high as the vote margin grows above 80%. In panel (b) we plot event-study coefficients similar to Figure 3. The pattern displayed is remarkably similar to our main results. Finally, in Appendix Table D.3 we display the coefficients estimated from Equation (6). Column 1 repeats the pattern of Figure D.5a of almost deterministic treatment status from referendum results. Column 2 estimates the reduced-form regression of ln(Luminosity) on the district obtaining a majority in the referendum. The resulting coefficient 0.29 is statistically significant at 1%. Column 3 estimates the second stage and shows a statistically significant coefficient of 0.48. This is larger than both the reduced form in Column 2, and the difference-in-differences estimate in Column 4. We obtain the Instrumental-Variables (IV) Wald estimate from dividing 0.48 by 0.97, which equals 0.495. This estimate is 98% larger than our main DD effect from Table 4. This difference may be driven by particularities of *Minas*

Gerais but also from noise still present in our smaller sample in this exercise.

5 Is It Just More Money?

Our discussion so far suggest that the observed effects in economic activity and public service delivery are driven both by (i) a positive shock to municipal revenues and (ii) a reduced-form aggregate change in *productivity*, i.e. producing larger amounts of output for any given amount of revenues. In this Section we investigate to what extent can the increase in revenues explain the range of positive effects we estimate in Section 3.

In a horse-race between both variables, i.e. splitting and the amount of revenues, we test whether controlling for local revenues changes the magnitude and statistical significance of our main coefficient of interest. Formally, we compare municipalities that split versus others that almost split but holding fixed the amount of revenues the area receives. We extend Equation (2) and estimate

$$y_{mt} = \alpha_m + \alpha_{s(m)t} + \beta Split_m \times Post_{mt} + \gamma \ln(\text{Revenues p.c.})_{mt} + \boldsymbol{X}_{mt}\boldsymbol{\lambda} + \varepsilon_{mt} \tag{7}$$

where y_{mt} represents outcomes for municipality m in time t. All other variables are analogously defined as for Equation (2). Our coefficients of interest are β and γ, which capture the effects of splitting and revenues on outcomes after a split.

Table 8 presents our main findings. Across four outcomes approximating economic activity and public sector size, we find that revenues correlate positively and significantly with outcomes but barely reduce the magnitude and significance of $Split \times Post$. Odd columns report basic difference-in-differences effects similar to those presented in Section 4. We find that splitting impacts average luminosity, the number of establishments, the number of local public jobs, but on average does not affect private sector jobs. Even columns report results controlling for ln(Revenues p.c.). Coefficients for $Split \times Post$ decrease in magnitude by about 14%, but significance remains unchanged. Notably, fiscal revenues do not correlate with the number of establishments or private sector jobs, suggesting that government spending has limited multiplier effects on the rest of the local economy.

6 Conclusion

This paper provides the cleanest test of the effects of horizontal decentralization to date. Exploiting a window of time of exceptionally low requirements for municipality splits, during which Brazil experienced a 23% growth in number of units, we estimate the impacts of the creation of new municipalities. We manually collect data on the universe of split requests and assemble a rich panel of districts and municipalities over time. Our estimates suggest that new municipalities benefit both from newly acquired autonomy but also from an inflow of revenues via federal transfers. We find significant improvements in economic activity, urbanization, the provision of public services, employment, and some degree of structural transformation. These benefits appear to not come at the expense of remaining districts: we find no evidence of negative spillovers for districts in the mother municipality. Our results suggest that countries may substantially raise regional development in small and isolated areas by reallocating funds to where their marginal return is high.

We provide new evidence to the debate in Brazil about the benefits and costs of more municipalities. The consensus in the press and public debate argues that the new small municipalities created in the 1990s were inefficient and expensive for the small population they account for. Although it may still be true that costs are relatively high, we show robust findings that benefits were also large. This serves as input to the debate that is currently going on in Congress.[28]

Despite having made significant progress in answering the question we set out to in the introduction, this paper leaves various interesting paths open for future research. For instance, we left open the various political stories that may drive parts of our results. As more complete data becomes available for elections in the 1990s at the district level, one could study how splits affect political yardstick competition, representation, and policy implementation (Grossman et al., 2017). Moreover, it would be important to study in more detail how exactly local governments act, what specific investments they make, and how this could translate into policy advice to other areas of the country.

[28] For example, see here, here, and here.

References

Alesina, Alberto F. and Enrico Spolaore, "On the Number and Size of Nations," *Quarterly Journal of Economics*, 1997, *112* (4), 1027–1056.

_ and _ , *The Size of Nations*, 1st ed., MIT Press, 2003.

Alvarez, Jorge, Felipe Benguria, Niklas Engbom, and Christian Moser, "Firms and the decline in earnings inequality in Brazil," *American Economic Journal: Macroeconomics*, 2018, *10* (1), 149–189.

Arretche, Marta, *Estado Federativo e Políticas Sociais: Determinantes da Descentralização*, 1st ed., Revan, 2000.

Asher, Sam, Karan Nagpal, and Paul Novosad, "The Cost of Distance: Geography and Governance in Rural India," 2018.

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

Bazzi, Samuel and Matthew Gudgeon, "The Political Boundaries of Ethnic Divisions," 2020.

Besley, Timothy and Stephen Coate, "Centralized versus decentralized provision of local public goods: A political economy approach," *Journal of Public Economics*, 2003, *87* (12), 2611–2637.

Boffa, Federico, Amedeo Piolatto, and Giacomo A. M. Ponzetto, "Political Centralization and Government Accountability," *Quarterly Journal of Economics*, 2016, *131* (1), 381–422.

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

Brandt, Cristina Thedim, "A criação de municípios após a Constituição de 1988: O impacto sobre a repartição do FPM e a Emenda Constitucional nº 15, de 1996," *Revista de Informação Legislativa*, 2010, *47* (187), 59–76.

Bremaeker, François E. J. De, "Os Novos Municípios: Surgimento, Problemas e Soluções," *Revista de Administração Municipal*, 1993, *206.*

Brinkerhoff, Derick W., Wetterberg Erik, and Erik Wibbels, “Distance, services, and citizen perceptions of the state in rural Africa,” *Governance*, 2018, *31*, 103–124.

Brollo, Fernanda, Tommaso Nannicini, Roberto Perotti, and Guido Tabellini, “The Political Resource Curse,” *American Economic Review*, 2013, *103* (5), 1759–1796.

Cachatori, Thiago L. and Adilar A. Cigolini, “Emancipações Municipais no Brasil,” *Revista Geonorte*, 2013, *7* (1), 730–747.

_ and Adilar Antonio Cigolini, “Análise do Processo de Criação de Municípios no Brasil,” in “XXII Colóquio Internacional de Geocrítica” Bogotá 2012.

Carter, David, “Terrain ruggedness and land cover: Improved data for most research designs,” 2018.

Coate, Stephen and Brian Knight, “Socially Optimal Districting: A Theoretical and Empirical Exploration,” *Quarterly Journal of Economics*, 2007, *122* (4), 1409–1471.

Corbi, Raphael, Elias Papaioannou, and Paolo Surico, “Regional Transfer Multipliers,” *Review of Economic Studies*, 2019, *86*, 1901–1934.

de Andrade Lima, Ricardo Carvalho and Raul da Mota Silveira Neto, “Secession of municipalities and economies of scale: Evidence from Brazil,” *Journal of Regional Science*, 2018, *58* (1), 159–180.

de Limas Tomio, Fabricio Ricardo, “Instituições, Processo Decisorio e Relações Executivo-Legislativo nos Estados.” PhD dissertation, Universidade Estadual de Campinas 2002.

de Limas Tomio, Fabrício Ricardo, “Federalismo, Municípios e Decisões Legislativas: A Criação de Municípios no Rio Grande do Sul,” *Revista de Sociologia e Política*, 2005, (24), 123–148.

de Mello, Diogo Lordello, “A Multiplicação dos Municípios no Brasil,” *Revista de Administração Municipal*, 1992, *39* (203).

Deserranno, Erika, Aisha Nansamba, and Nancy Qian, “Aid Crowd-Out: The Effect of NGOs on Government-Provided Public Services,” *NBER Working Paper*, 2020, *w26928.*

Dix-Carneiro, Rafael, “Trade Liberalization and Labor Market Dynamics,” *Econometrica*, 2014, *82* (3), 825–885.

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

Dunning, Thad, "Decentralization and Ethnic Diversity," in Jonathan A. Rodden and Erik Wibbels, eds., *Decentralized Governance and Accountability*, Cambridge University Press, 2019, chapter 11, pp. 248–272.

Dur, Robert and Klaas Staal, "Local public good provision, municipal consolidation, and national transfers," *Regional Science and Urban Economics*, 2008, *38* (2), 160–173.

Ebel, Robert D. and Serdar Yilmaz, "On the Measurement and Impact of Fiscal Decentralization," *World Bank Policy Research Working PaperPolicy Research Working Paper*, 2002, *2809*.

Egger, Peter H, Marko Koethenbuerger, and Gabriel Loumeau, "Local Border Reforms and Economic Activity," 2017.

Ehrl, Philipp, "Minimum comparable areas for the period 1872-2010: an aggregation of Brazilian municipalities," *Estudos Econômicos*, 2017, *47* (1), 215–229.

Faguet, Jean-Paul, "Does decentralization increase government responsiveness to local needs? Evidence from Bolivia," *Journal of Public Economics*, 2004, *88*, 867–893.

—, "Decentralization and Governance," *World Development*, 2014, *53* (0), 2–13.

Favero, Edison, "Desmembramento Territorial: O Processo de Criação de Municípios - Avaliação a Partir de Indicadores Cconômicos e Sociais." PhD dissertation, Universidade de São Paulo 2004.

Ferraresi, Massimiliano, Giuseppe Migali, and Leonzio Rizzo, "Does intermunicipal cooperation promote efficiency gains? Evidence from Italian municipal unions," *Journal of Regional Science*, 2018, *58* (5), 1017–1044.

Fox, William F. and Tami Gurley, "Will Consolidation Improve Sub-National Governments?," 2006.

Gadenne, Lucie, "Tax Me, but Spend Wisely? Sources of Public Finance and Government Accountability," *American Economic Journal: Applied Economics*, 2017, *9* (1), 274–314.

— and Monica Singhal, "Decentralization in Developing Economies," *Annual Review of Economics*, 2014, *6* (1), 581–604.

Gelman, Andrew and Guido Imbens, "Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs," *Journal of Business and Economic Statistics*, 2019, *37* (3), 447–456.

Gendźwiłł, Adam, Anna Kurniewicz, and Paweł Swianiewicz, "The impact of municipal territorial reforms on the economic performance of local governments. A systematic review of quasi-experimental studies," *Space and Polity*, 2020, pp. 1–20.

Gomes, Gustavo Maia and Maria Cristina MacDowell, "Descentralização Política, Federalismo Fiscal e Criação de Municípios: O que É Mau para o Econômico nem sempre é Bom para o Social," 2000.

Green, Elliott, "Patronage, district creation, and reform in Uganda," *Studies in Comparative International Development*, 2010, *45* (1), 83–103.

Grembi, Veronica, Tommaso Nannicini, and Ugo Troiano, "Do fiscal rules matter?," *American Economic Journal: Applied Economics*, 2016, *8* (3), 1–30.

Grossman, Guy and Janet I. Lewis, "Administrative Unit Proliferation," *American Political Science Review*, 2014, *108* (01), 196–217.

— , Jan H Pierskalla, and Emma Boswell Dean, "Government Fragmentation and Public Goods Provision," *Journal of Politics*, 2017, *79* (3).

Henderson, J. Vernon, Adam Storeygard, and David N. Weil, "Measuring Economic Growth from Outer Space," *American Economic Review*, 2012, *102* (2), 994–1028.

— , Tim Squires, Adam Storeygard, and David Weil, "The Global Distribution of Economic Activity: Nature, History, and the Role of Trade," *Quarterly Journal of Economics*, 2018, *133* (1), 357–406.

Hooghe, Liesbet and Gary Marks, "Unraveling the Central State, but How? Types of Multi-level Governance," *American Political Science Review*, 2003, *97* (2), 233–243.

Klering, Luis Roque, Alexandra Jochims Kruel, and Eduardo Stranz, "Os pequenos municípios do Brasil – uma análise a partir de índices de gestão," *Análise*, 2012, *23* (1), 31–44.

Kline, Patrick and Enrico Moretti, "People, Places, and Public Policy: Some Simple Welfare Economics of Local Economic Development Programs," *Annual Review of Economics*, 2014, *6* (1), 629–662.

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

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

Lipscomb, Molly and Ahmed Mushfiq Mobarak, "Decentralization and Pollution Spillovers: Evidence from the Re-drawing of County Borders in Brazil," *Review of Economic Studies*, 2017, *84* (1), 464–502.

Litschig, Stepan and Kevin M. Morrison, "The impact of intergovernmental transfers on education outcomes and poverty reduction," *American Economic Journal: Applied Economics*, 2013, *5* (4), 206–240.

Litschig, Stephan, "Are rules-based government programs shielded from special-interest politics? Evidence from revenue-sharing transfers in Brazil," *Journal of Public Economics*, 2012, *96* (11-12), 1047–1060.

Lockwood, Ben, "Distributive politics and the costs of centralization," *Review of Economic Studies*, 2002, *69* (2), 313–337.

Mansuri, Ghazala and Vijayendra Rao, *Localizing Development: Does Participation Work?*, World Bank, 2013.

Mattos, Enlinson and Vladimir Ponczek, "Efeitos da Divisão Municipal na Oferta de Bens Públicos e Indicadores Sociais," *Revista Brasileira de Economia*, 2013, *67* (3), 315–336.

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

Nyqvist, Martina Björkman, Andrea Guariso, Jakob Svensson, and David Yanagizawa-Drott, "Reducing child mortality in the last mile: Experimental evidence on community health promoters in Uganda," *American Economic Journal: Applied Economics*, 2019, *11* (3), 155–192.

Oates, Wallace E., *Fiscal Federalism*, Edward Elgar Publishing, 1972.

— , "An Essay on Fiscal Federalism," *Journal of Economic Literature*, 1999, *37* (September), 1120–1149.

Oster, Emily, "Unobservable Selection and Coefficient Stability: Theory and Evidence," *Journal of Business and Economic Statistics*, 2019, *37* (2), 187–204.

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

— , "The Proliferation of Decentralized Governing Units," in Jonathan A. Rodden and Erik Wibbels, eds., *Decentralized Governance and Accountability*, Cambridge University Press, 2019, chapter 6, pp. 115–143.

Reingewertz, Yaniv, "Do municipal amalgamations work? Evidence from municipalities in Israel," *Journal of Urban Economics*, 2012, *72* (2-3), 240–251.

Rodden, Jonathan A. and Erik Wibbels, eds, *Decentralized Governance and Accountability*, 1 ed., Cambridge University Press, 2019.

Rondinelli, Dennis A., John R. Nellis, and G. Shabbir Cheema, "Decentralization in Developing Countries: A Review of Recent Experience," 1983.

Schoenholzer, David S., "Essays on State Capacity and Local Public Goods." PhD dissertation, University of California Berkeley 2018.

Shenoy, Ajay, "Regional development through place-based policies: Evidence from a spatial discontinuity," *Journal of Development Economics*, 2018, *130*, 173–189.

Tesouro Nacional, "O que Você Precisa Saber Sobre Transferências Fiscais da União," Technical Report 2018.

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

Treisman, Daniel, *The Architecture of Government: Rethinking Political Decentralization*, 1st ed., Cambridge University Press, 2007.

Tricaud, Clemence, "Better Alone? Evidence on the Costs of Intermunicipal Cooperation," 2019.

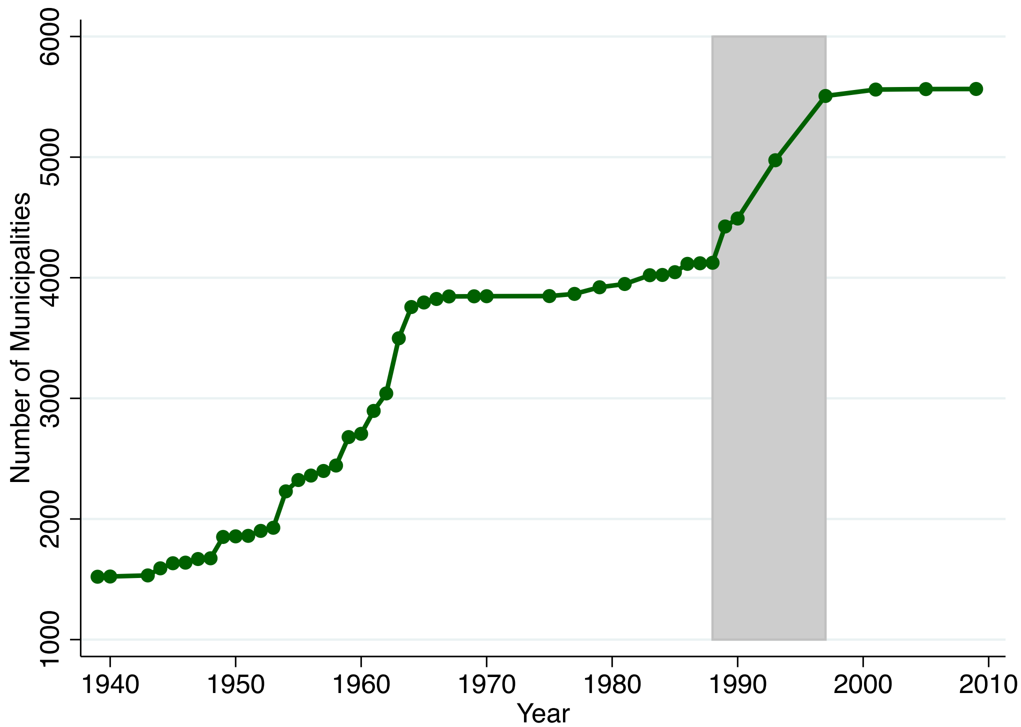
United Nations Development Programme, "Atlas of Human Development in Brazil," 2013.

Weese, Eric, "Political mergers as coalition formation: An analysis of the Heisei municipal amalgamations," *Quantitative Economics*, 2015, *6* (2), 257–307.

World Bank, "Making Services Work for Poor People," *World Development Report*, 2004.

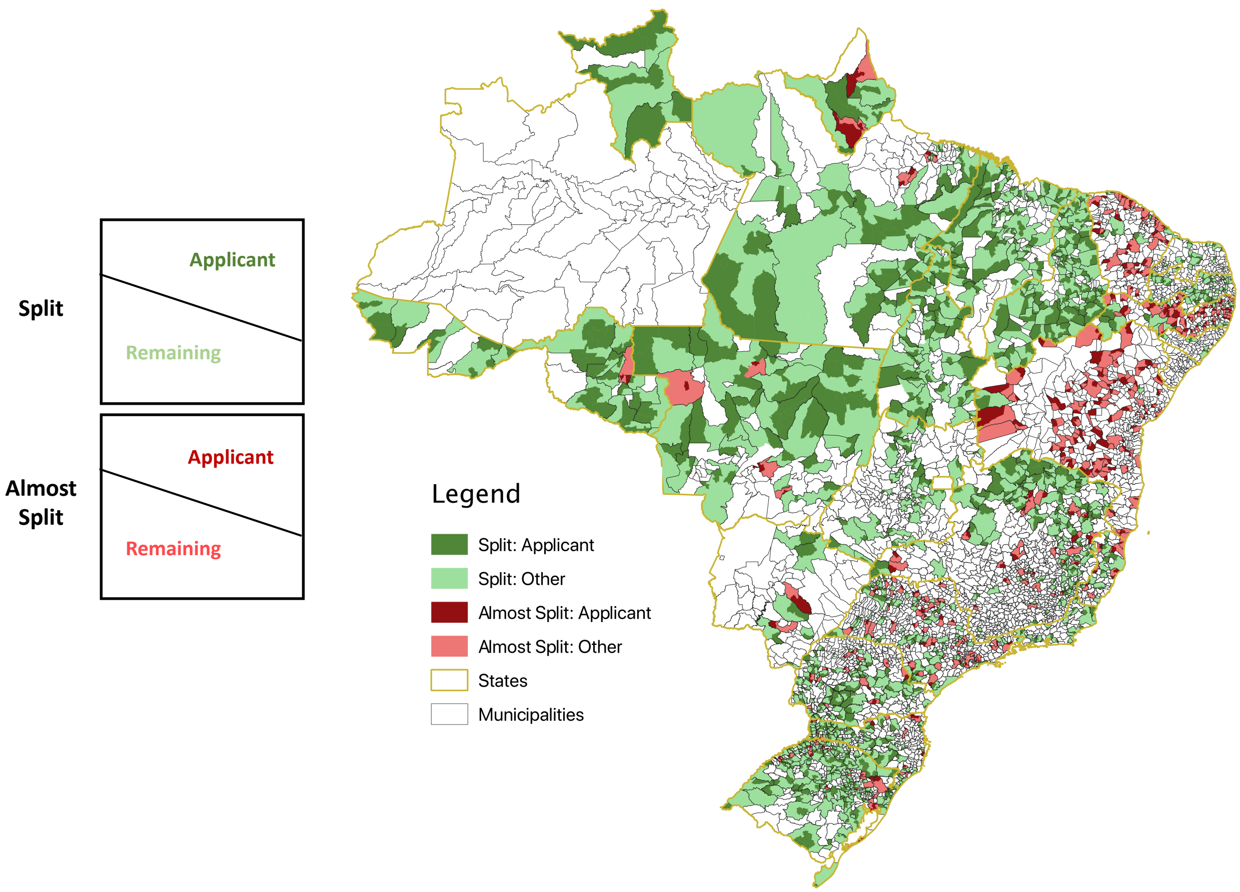
7 Figures

Figure 1: Number of Municipalities Over Time



Notes: This figure plots the total number of municipalities in Brazil. A municipality is officially established in the start of the election term next to the final approval of a split request. The gray bar highlights out period of study, between the 1988 Federal Constitution and the 1996 Constitutional Amendment.

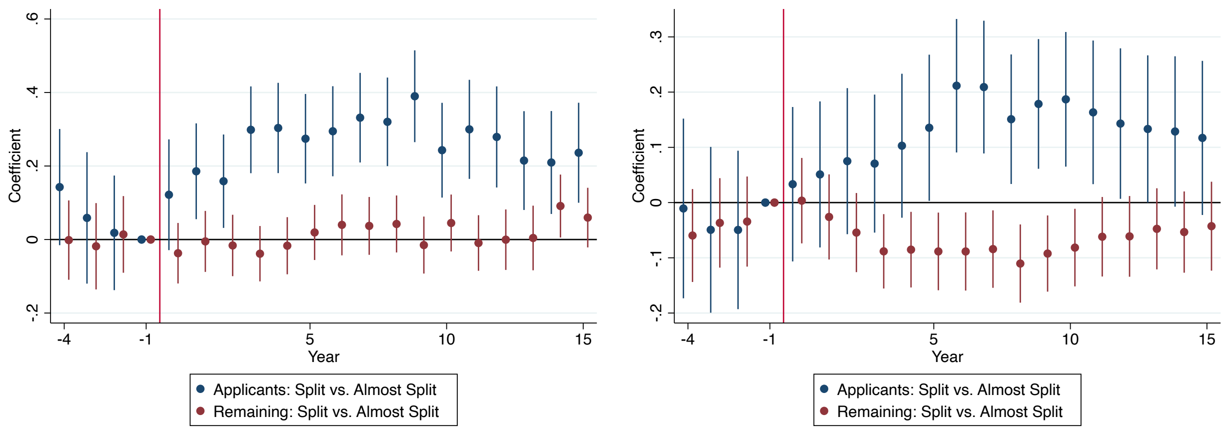
Figure 2: Diagram and Map of Samples



Notes: The diagram on the left pictures the standard structure of split requests in our sample of Brazilian municipalities. Districts are colored: dark green represents split applicants, dark red represents almost split applicants, light green represents split remaining, and light red represents almost split remaining. The map on the right represents Brazil in 2010, after the waves of splits since 1991 as described in Section 2.

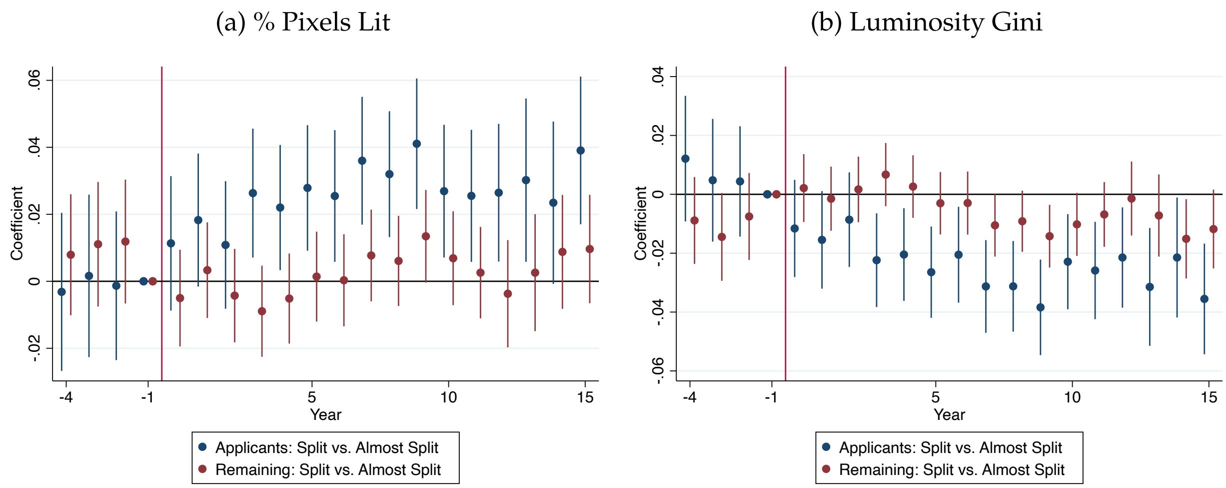
Figure 3: Economic Activity

(a) ln(Luminosity) (b) ln(Urban Area)



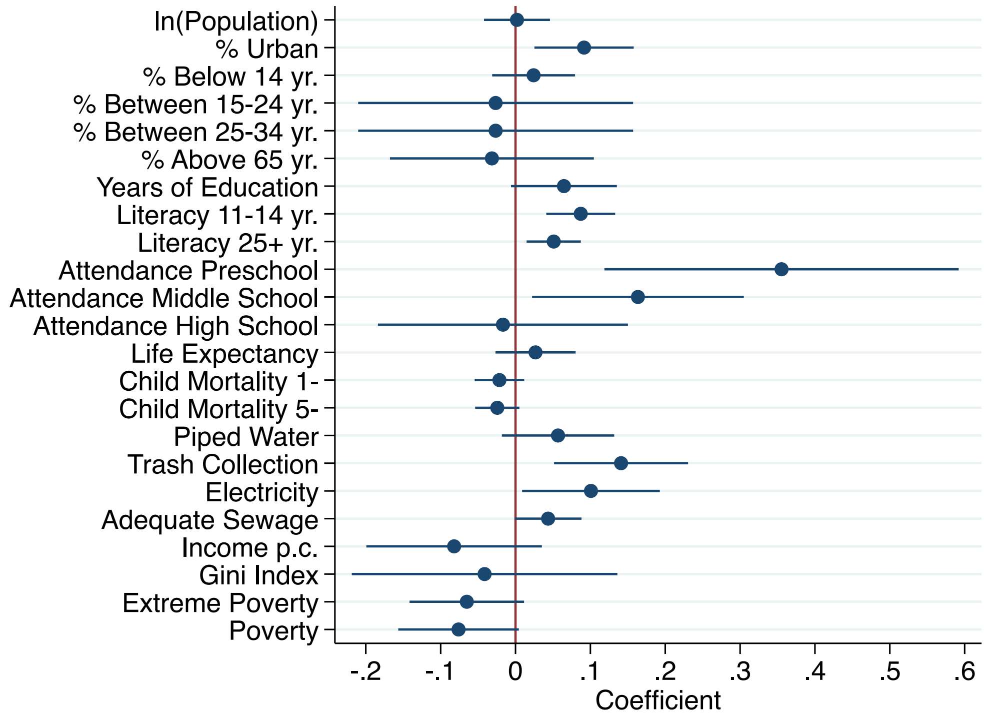
Notes: Results from estimation of Equation (1) in Section 4. Each set of coefficients is from one differences-in-differences equation, one for applicant and another for remaining districts. Controls interacted with time fixed effects include ln(Area), ln(Distance to Parent Town Hall), ln(Distance to State Capital), soil suitability for maize, wet rice, soybean, and wheat, terrain ruggedness, ln(Luminosity) in 1992, land use shares in 1991 for urban, agriculture, pasture and forest. Bars represent the 95%-confidence intervals.

Figure 4: Distribution of Economic Activity



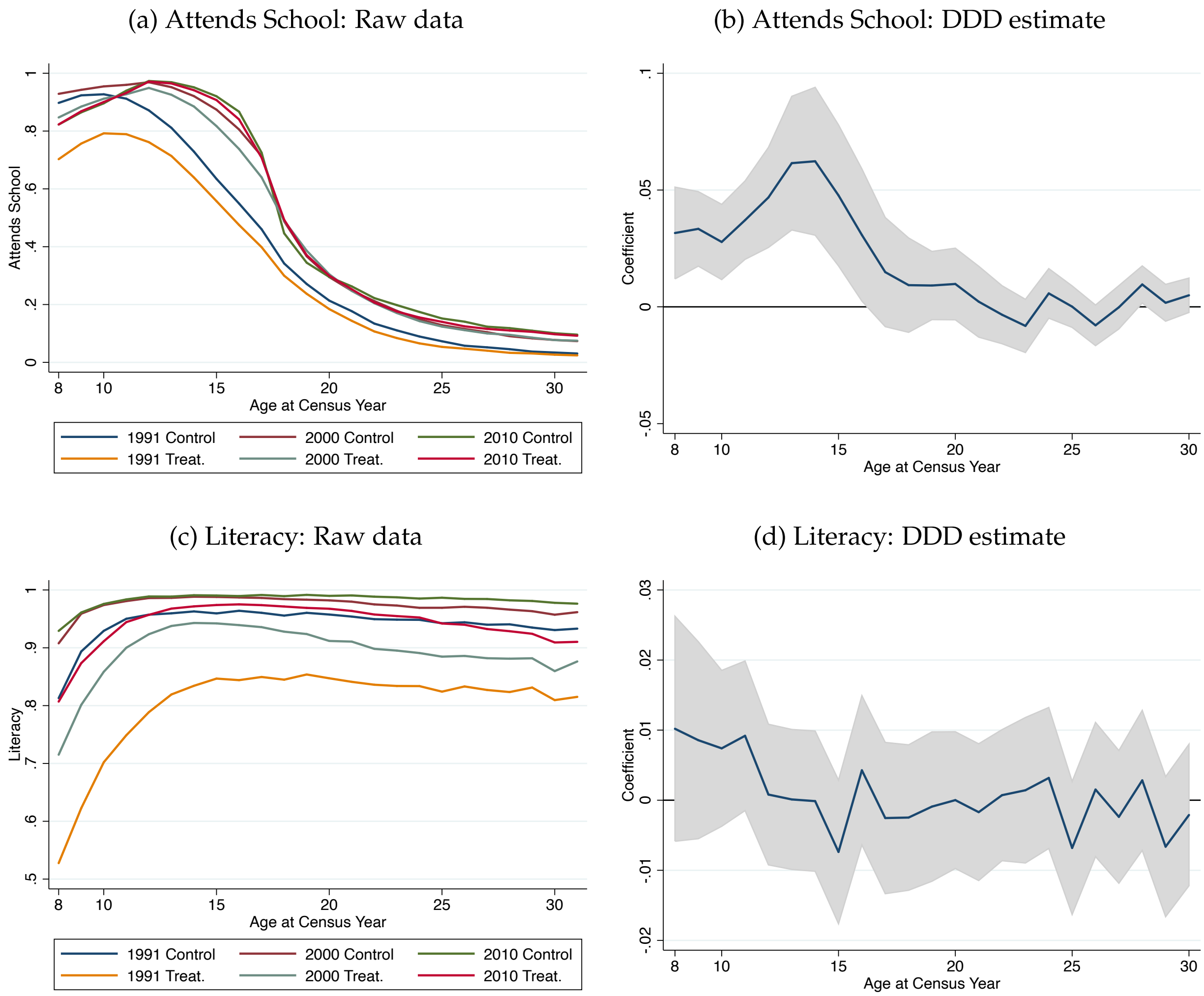
Notes: Results from estimation of Equation (1) in Section 4. Each set of coefficients is from one differences-in-differences equation, one for applicant and another for remaining districts. Controls interacted with time fixed effects include ln(Area), ln(Distance to Parent Town Hall), ln(Distance to State Capital), soil suitability for maize, wet rice, soybean, and wheat, terrain ruggedness, ln(Luminosity) in 1992, land use shares in 1991 for urban, agriculture, pasture and forest. Bars represent the 95%-confidence intervals.

Figure 5: Demographic and Public Service Provision Standardized Outcomes



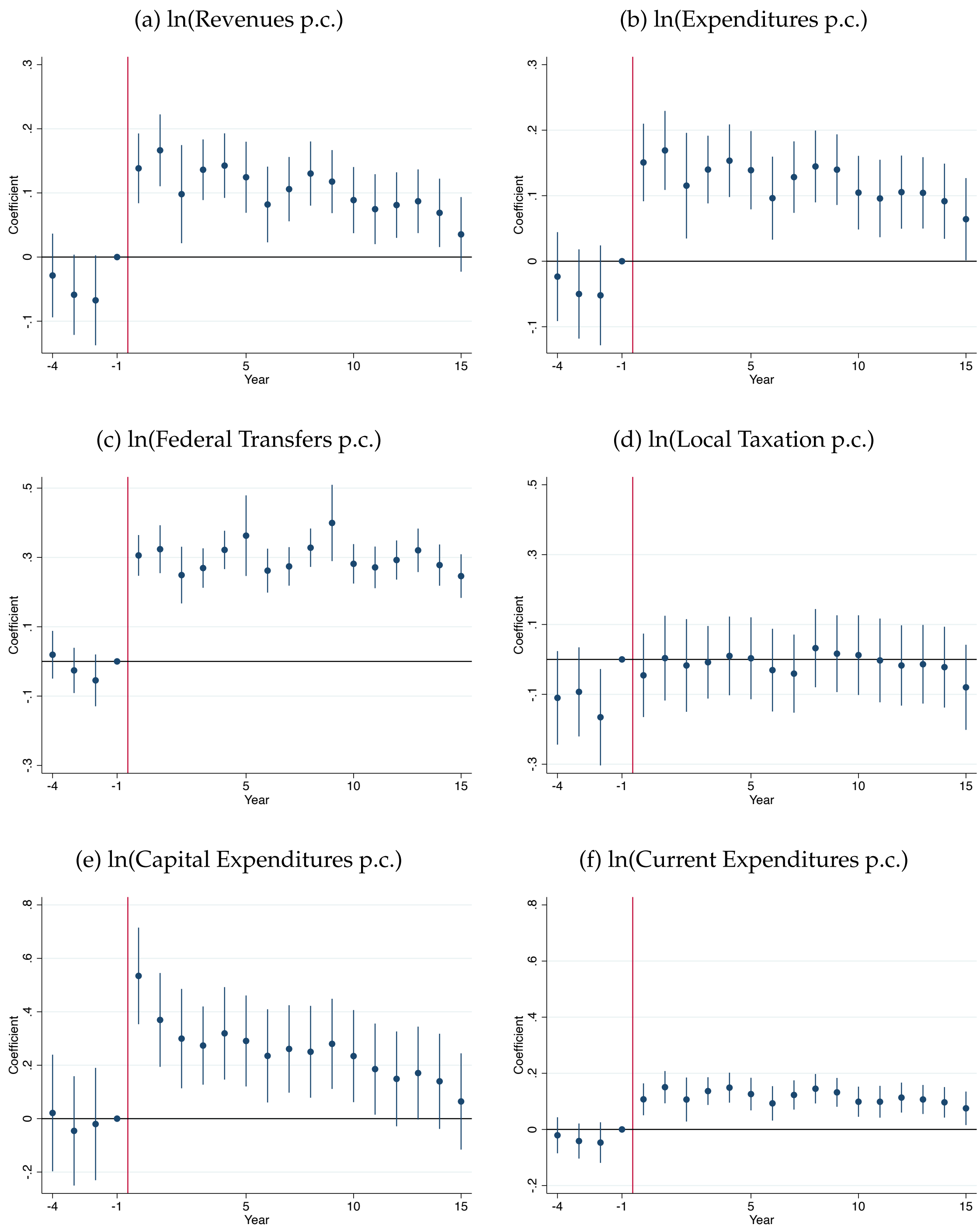
Notes: Results from estimation of a simplified version of Equation (2) in Section 4. Each coefficient is calculated from a regression on the given standardized outcome. Main data sources are the Demographic Census of 1991, 2000 and 2010. Bars represent the 95%-confidence intervals.

Figure 6: Education Outcomes from Triple-Differences



Notes: Results from estimation of the triple-differences Equation (4) in Section 4. Panels (a) and (c) display the raw data by treatment (split) vs. control (almost split), year and age group. On panels (b) and (d) we normalize $\beta_{31} = 0$. Main data sources are the Demographic Census microdata from of 1991, 2000 and 2010. Questions about educational attainment are only asked for individuals age 5 or older. Gray areas represent the 95%-confidence intervals.

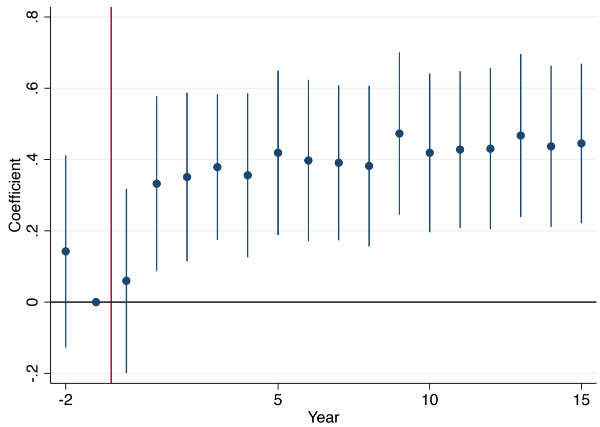
Figure 7: Fiscal Revenues and Expenditures



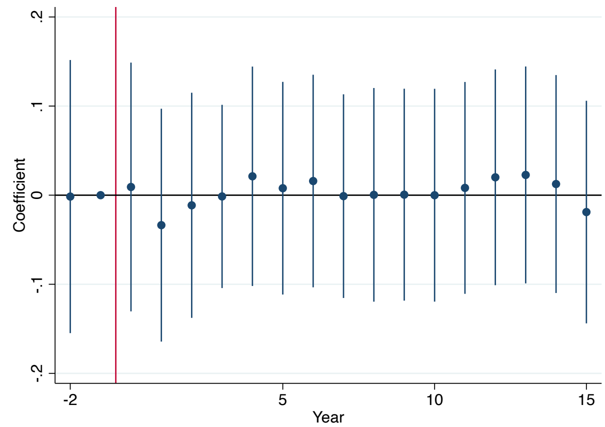
Notes: Results from estimation of Equation (2) in Section 4. Main data source is the *Finanças Brasileiras* (Finbra) data set for 1989 to 2018. Controls interacted with time fixed effects include number of districts in 1991, ln(Population) in 1991, % Urban in 1991, ln(Area), ln(Distance to State Capital), ln(Income p.c.) in 1991, and HHI for race and religion in 1991. Bars represent the 95%-confidence intervals.

Figure 8: Employment

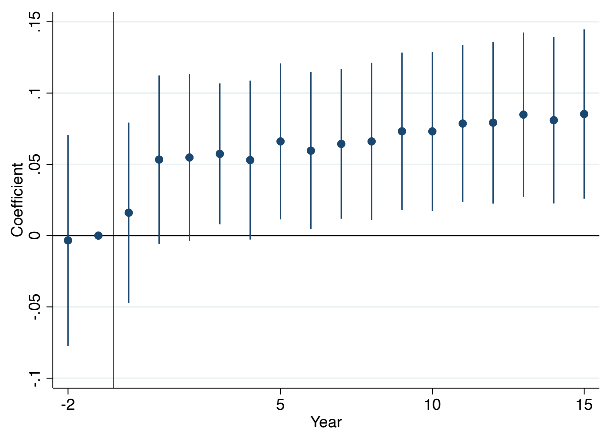
(a) ln(Public Jobs)



(b) ln(Private Jobs)

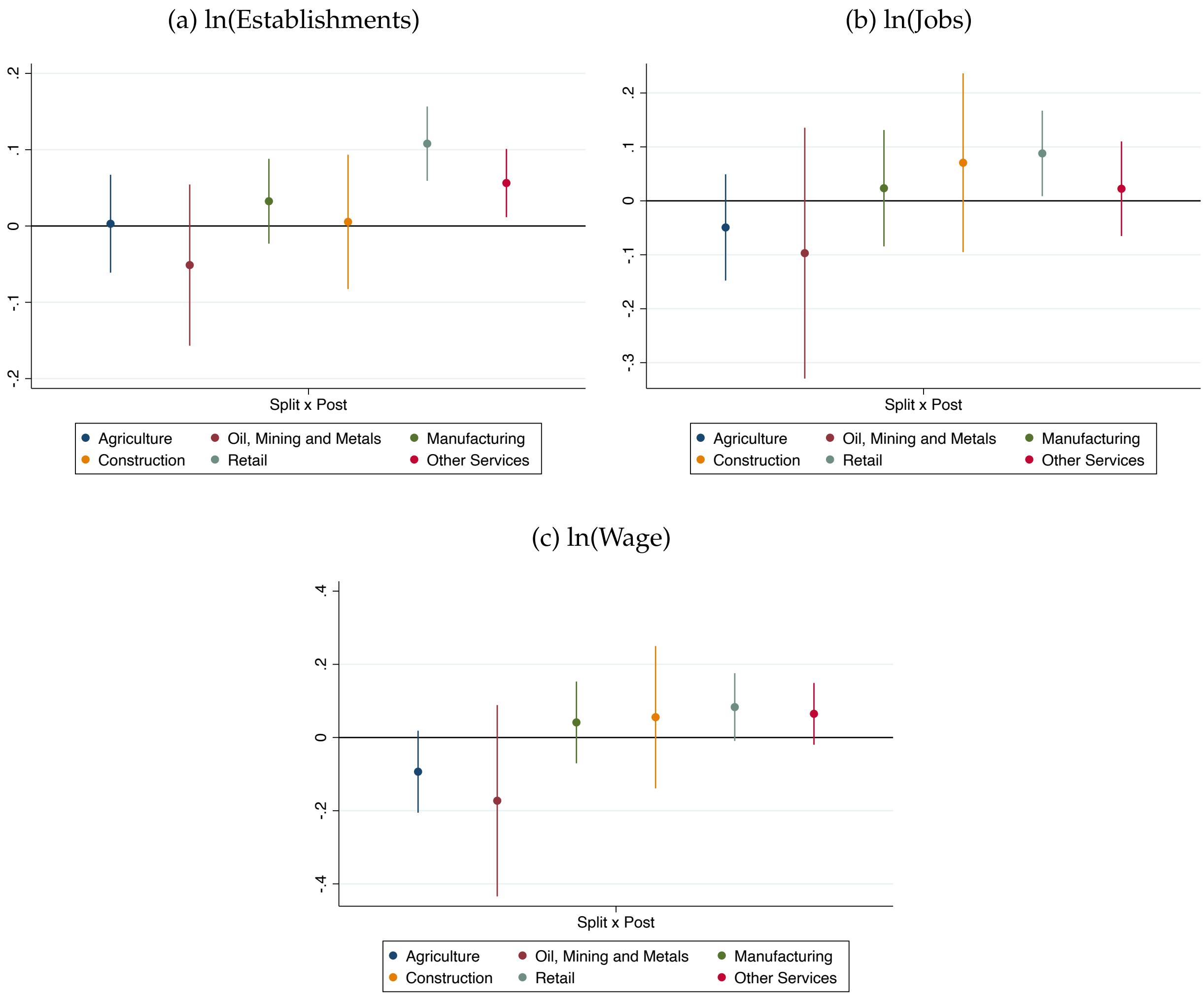


(c) ln(Establishments)



Notes: Results from estimation of Equation (2) in Section 4. Main data source is the *Relação Anual de Informações Sociais* (RAIS) data set for 1985 to 2018. We classify jobs and establishments according to sector with information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO). Controls interacted with time fixed effects include number of districts in 1991, ln(Population) in 1991, % Urban in 1991, ln(Area), ln(Distance to State Capital), ln(Income p.c.) in 1991, and HHI for race and religion in 1991. Bars represent the 95%-confidence intervals.

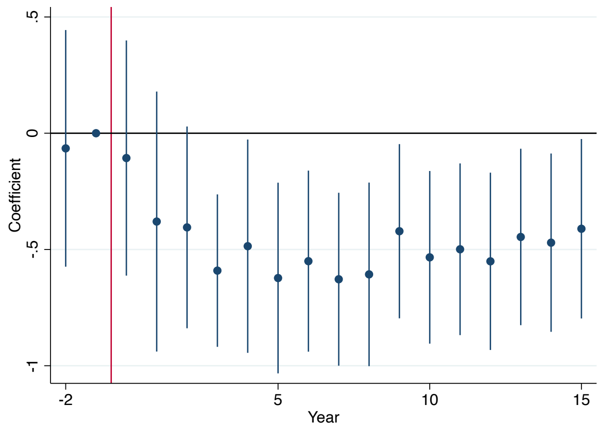
Figure 9: Employment by Sector



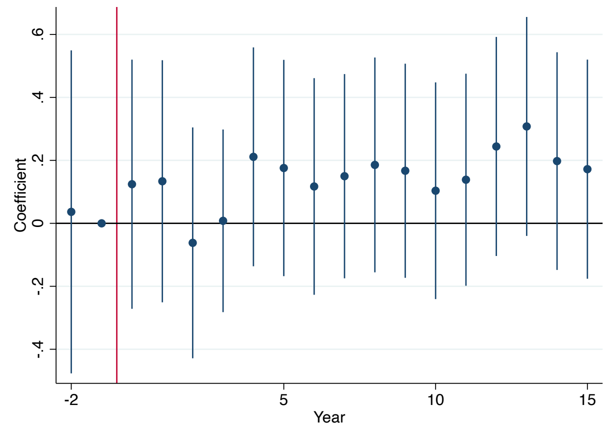
Notes: Results from estimation of a simplified version of Equation (2) in Section 4. Main data source is the *Relação Anual de Informações Sociais* (RAIS) data set for 1985 to 2018. We classify jobs and establishments according to sector with information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO). Grouping of economic sectors is taken from Alvarez et al. (2018). Controls interacted with time fixed effects include number of districts in 1991, ln(Population) in 1991, % Urban in 1991, ln(Area), ln(Distance to State Capital), ln(Income p.c.) in 1991, and HHI for race and religion in 1991. Bars represent the 95%-confidence intervals.

Figure 10: Crowd-out of Nonprofit Jobs in Education

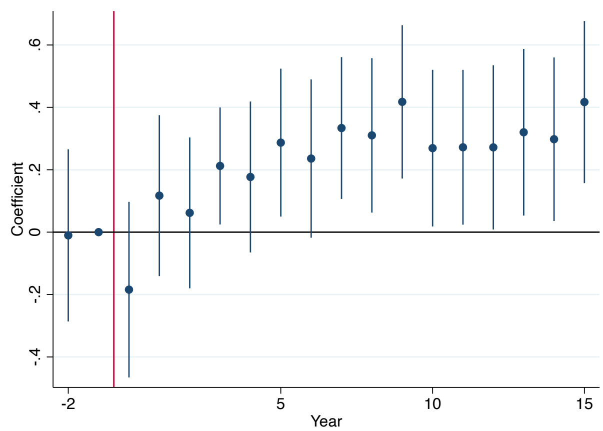
(a) ln(Nonprofit Jobs)



(b) ln(Private Sector Jobs)

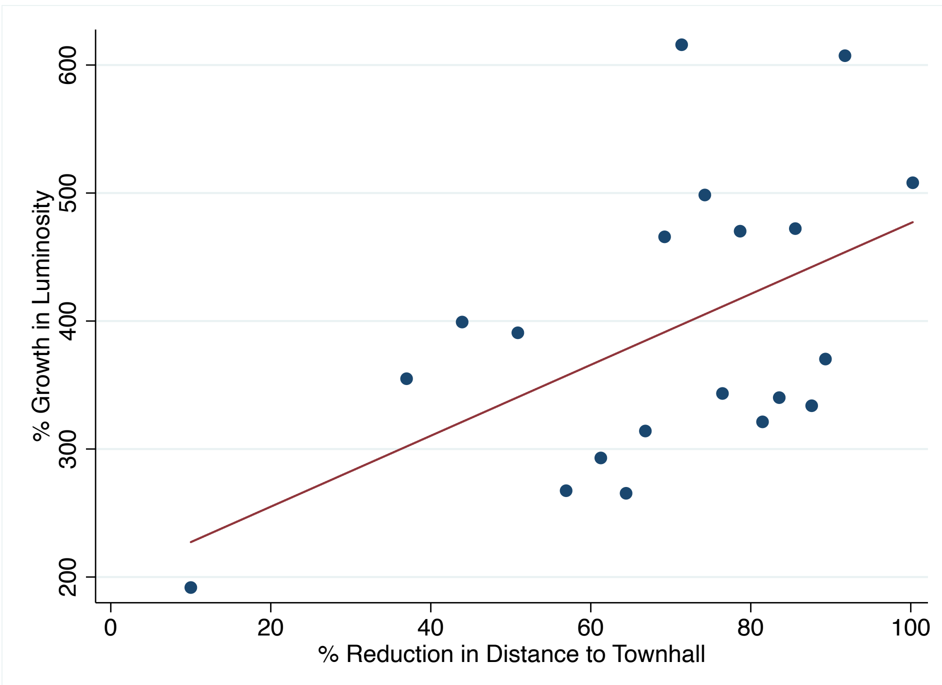


(c) ln(Public Jobs)



Notes: Results from estimation of Equation (2) in Section 4. Main data source is the *Relação Anual de Informações Sociais* (RAIS) data set for 1985 to 2018. We classify jobs and establishments according to sector with information from the *Classificação Nacional de Atividades Econômicas* (CNAE) and *Classificação Brasileira de Ocupações* (CBO). Controls interacted with time fixed effects include number of districts in 1991, ln(Population) in 1991, % Urban in 1991, ln(Area), ln(Distance to State Capital), ln(Income p.c.) in 1991, and HHI for race and religion in 1991. Bars represent the 95%-confidence intervals.

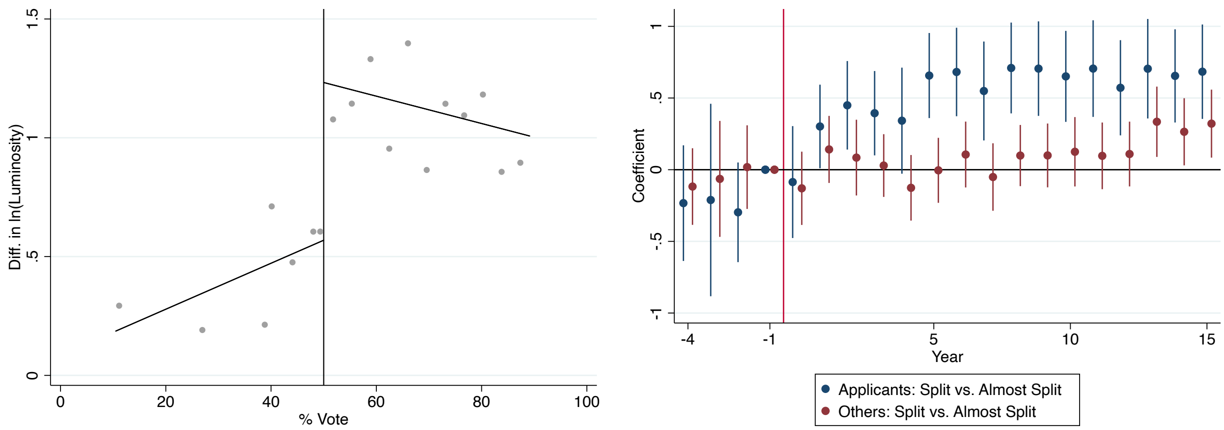
Figure 11: Reducing the Cost of Distance to the Municipality's Town Hall



Notes: This figure plots a binned scatter across (i) % growth in luminosity over 15 years and (ii) % reduction in distance to townhall after split. Controls include state fixed effects, ln(Area), ln(Distance to State Capital), soil suitability for maize, wet rice, soybean, and wheat, terrain ruggedness, ln(Luminosity) in 1992, land use shares in 1991 for urban, agriculture, pasture and forest.

Figure 12: Difference-in-Discontinuities

(a) ln(Luminosity) Growth (b) ln(Luminosity) RD-DD Event-Study



Notes: Results from estimation described in Section 4. Panel (a) plots the growth in ln(Luminosity) for districts below and above the plebiscite approval cutoff of 50%. Panel (b) plots the Differences-in-Discontinuities results. Each set of coefficients is from one differences-in-differences equation, one for applicant and another for remaining districts. Controls interacted with time fixed effects include ln(Area), ln(Distance to Baseline Townhall), ln(Distance to State Capital), soil suitability for maize, wet rice, soybean, and wheat, terrain ruggedness, ln(Luminosity) in 1992, land use shares in 1991 for urban, agriculture, pasture and forest. Bars represent the 95%-confidence intervals.

8 Tables

Table 1: Baseline Descriptive Statistics in Levels - District Across Samples

	Split		Almost Split		Rest		(1)-(3)		(3)-(5)	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	Dif. (7)	P-value (8)	Dif. (9)	P-value (10)
Population (000's)	5.71	14.51	17.97	43.99	13.79	51.73	−12.26	0	−8.08	0
% Urban Population	39.45	26.78	46.6	30.32	51.2	27.24	−7.15	0	−11.75	0
% Male	51.8	1.35	51.36	1.86	51.48	1.66	0.44	0	0.31	0
ln(Luminosity)	−1.15	2.15	−0.7	1.94	−0.66	1.75	−0.45	0	−0.5	0
% Urban Pixels	0.9	4.77	1.11	4.07	1.62	8.16	−0.21	0.27	−0.72	0.01
% Forest Pixels	33.86	26.63	32.18	25.85	27.01	24.4	1.68	0.13	6.85	0
% Pasture Pixels	30.78	26.87	31.04	27.06	44.12	29.76	−0.26	0.82	−13.34	0
% Agriculture Pixels	26.91	27.87	27.81	27.59	19.93	23	−0.9	0.44	6.98	0
ln(Area)	5.23	1.39	5.49	1.4	5.39	1.18	−0.27	0	−0.17	0
ln(Dist. Baseline Townhall)	2.73	0.72	2.27	1.05	1.9	1.04	0.46	0	0.83	0
ln(Dist. State Capital)	5.47	0.76	5.38	0.76	5.26	0.8	0.09	0.01	0.21	0
ln(Dist. Sea)	5.4	1.2	5.34	1.24	5.29	1.25	0.07	0.21	0.11	0.02
ln(Maize Suitability)	8.66	0.28	8.64	0.29	8.53	0.26	0.02	0.1	0.13	0
ln(Wet Rice Suitability)	8.58	0.96	8.64	0.63	8.59	0.81	−0.05	0.11	−0.01	0.8
ln(Soybean Suitability)	7.68	0.53	7.7	0.37	7.67	0.7	−0.01	0.53	0.01	0.59
ln(Wheat Suitability)	6.05	3.34	6.18	3.24	6.49	2.94	−0.13	0.36	−0.44	0
Terrain Ruggedness	82.05	80.22	76.81	71.34	69.21	71.43	5.24	0.1	12.84	0
	N = 990		N = 1269		N = 3221					

Notes: Table reports characteristics from various data sources at district level (1991 Demographic Census, 1992 nighttime luminosity, MapBiomas classification of land use, FAO-GAEZ soil suitability, and Carter (2018) terrain ruggedness). Samples are defined in Sections 2 and 3.1.

Table 2: Baseline Selection Into Splitting - District Level

	Applicants			Remaining		
	All	1993	1997	All	1993	1997
	(1)	(2)	(3)	(4)	(5)	(6)
ln(Luminosity)	-0.01***	-0.01**	-0.02***	-0.01**	0.00	-0.01
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)
% Urban Pixels	-0.00	-0.00	-0.01	-0.01*	-0.01	-0.01**
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)
% Forest Pixels	-0.00	0.00	-0.00*	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
% Pasture Pixels	-0.00*	0.00	-0.01***	-0.00	0.00	-0.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
% Agriculture Pixels	-0.00***	-0.00	-0.01***	-0.00*	-0.00	-0.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
ln(Area)	0.06***	0.05***	0.07***	0.03***	0.00	0.05**
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
ln(Dist. Parent Townhall)	-0.20***	-0.10***	-0.29***	-0.03**	0.01	-0.04*
	(0.03)	(0.03)	(0.04)	(0.01)	(0.02)	(0.02)
ln(Dist. State Capital)	0.12***	0.02	0.19***	0.10***	0.02	0.15***
	(0.03)	(0.03)	(0.04)	(0.02)	(0.03)	(0.04)
ln(Dist. Sea)	-0.02	-0.02	-0.03	-0.03**	-0.01	-0.05**
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)
ln(Maize Suitability)	0.10	0.26***	-0.03	0.12*	0.21***	0.14
	(0.07)	(0.08)	(0.12)	(0.07)	(0.08)	(0.11)
ln(Wet Rice Suitability)	-0.02	-0.01	-0.05	-0.01	-0.00	-0.01
	(0.01)	(0.01)	(0.04)	(0.02)	(0.02)	(0.04)
ln(Soybean Suitability)	-0.00	0.01	0.01	0.08**	0.01	0.07
	(0.02)	(0.02)	(0.05)	(0.03)	(0.09)	(0.05)
ln(Wheat Suitability)	0.00	0.01	-0.01	0.01*	0.02**	0.02**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Terrain Ruggedness	0.00	-0.00	-0.00	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	986	504	481	1,255	655	600
R-squared	0.20	0.29	0.26	0.19	0.25	0.22
State FE	✓	✓	✓	✓	✓	✓
Mean	0.828	0.897	0.755	0.760	0.875	0.635
SD	0.378	0.304	0.431	0.427	0.331	0.482

Notes: Table reports results from Equation (3) on baseline characteristics from various data sources at district level (1991 Demographic Census, 1992 nighttime luminosity, MapBiomas classification of land use, FAO-GAEZ soil suitability, and Carter (2018) terrain ruggedness). Columns (1) to (3) restrict observations to applicant districts, while columns (4) to (6) restrict it to remaining districts, defined in Sections 2 and 3.1. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Baseline Descriptive Statistics at Municipality Level

	Split		Almost Split		Rest		(1)-(3)	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	Dif. (7)	P-value (8)
Number of Districts	2.79	1.69	3.02	1.66	1.56	1.05	−0.23	0.19
% Urban Districts	42.78	37.54	53.17	35.62	57.57	44.02	−10.39	0.01
ln(Population)	9.92	0.85	10.27	1.1	9.16	1.04	−0.35	0
% Urban Population	54.91	22.97	67.1	24.23	59.53	22.76	−12.19	0
ln(Income p.c. CZ)	10.27	0.45	10.48	0.47	10.31	0.43	−0.2	0
ln(Area)	6.84	1.25	6.39	0.91	5.96	1.07	0.46	0
ln(Dist. State Capital)	5.47	0.76	5.15	1	5.3	0.76	0.32	0
% Male	50.92	1.52	50.41	1.42	51.16	1.6	0.51	0
% Literacy 5+	75.77	11.61	79.39	8.21	76.7	9.2	−3.62	0
Years of Education	3.41	0.93	3.73	0.8	3.45	0.78	−0.32	0
% White	65.12	25.52	68.09	22.17	64.84	23.58	−2.97	0.25
% Black or Brown	34.21	25.44	30.94	22.27	34.54	23.53	3.27	0.21
HHI Race	64.56	14.44	63.23	13.08	62.15	14.87	1.34	0.37
% Catholic	85.96	9.28	84.95	7.52	87.8	9.01	1.01	0.28
% Evangelical	10.11	7.58	10.09	6.37	8.28	7.06	0.02	0.98
HHI Religion	76.07	13	73.83	11.6	78.99	12.33	2.24	0.09
Infant Mortality 1-	33.73	13	32.83	11.46	33.66	11.67	0.9	0.5
Fertility Rate	3.19	0.96	3.01	0.76	3.05	0.83	0.18	0.06
Life Expectancy	66.42	3.66	66.81	3.39	66.47	3.54	−0.39	0.3
% Piped Water	67.54	25.08	78.67	19.76	74.55	21.66	−11.13	0
% Trash Collected	39.52	26.05	54.48	27.75	45.73	26.6	−14.96	0
% Active	53.12	7.53	54.17	5.84	52.52	6.36	−1.04	0.16
Gini Index	0.56	0.05	0.56	0.05	0.55	0.06	0	0.97
ln(Federal Transfers p.c.)	2.79	0.45	2.63	0.58	3.16	0.62	0.16	0
% Federal Transfers	39.6	16.82	32.06	16.13	43.63	18.5	7.54	0
	N = 344		N = 124		N = 1925			

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

Table 4: Effects on ln(Luminosity)

	Applicants			Remaining		
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Split	0.33***	0.25***	0.17***	0.07***	0.00	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	15,950	15,949	15,909	20,604	20,604	20,544
R-squared	0.94	0.95	0.96	0.96	0.96	0.96
State-Year FE	-	✓	✓	-	✓	✓
Controls-Time FE	-	-	✓	-	-	✓

Notes: Table reports point estimate results from Equation (1). All Columns include district fixed effects. Columns (1) to (3) restrict observations to applicant districts, while columns (4) to (6) restrict it to remaining districts, defined in Sections 2 and 3.1. Controls interacted with time fixed effects include ln(Area), ln(Distance to Parent Townhall), ln(Distance to State Capital), soil suitability for maize, wet rice, soybean, and wheat, terrain ruggedness, ln(Luminosity) in 1992, land use shares in 1991 for urban, agriculture, pasture and forest. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5: Heterogeneity in ln(Luminosity) for Applicant Districts

	(1)	(2)	(3)	(4)	(5)
Post x Split	0.17***	0.20***	0.20***	0.23***	0.27***
	(0.02)	(0.04)	(0.04)	(0.04)	(0.05)
Post x Split x ln(Area)		-0.01			-0.07*
		(0.01)			(0.04)
Post x Split x ln(Dist. Parent Townhall)			-0.00		0.16**
			(0.03)		(0.06)
Post x Split x ln(Dist. State Capital)			0.02		0.04
			(0.02)		(0.03)
Post x Split x ln(Dist. Sea)			-0.03		-0.06***
			(0.02)		(0.02)
Post x Split x ln(Pop. 1991)				0.01**	0.02
				(0.01)	(0.02)
Post x Split x % Urban 1991				-0.38***	-0.37***
				(0.07)	(0.08)
Observations	15,612	15,612	15,612	9,314	9,314
R-squared	0.96	0.96	0.96	0.96	0.96

Notes: Table reports point estimate results from Equation (1). All Columns include district and state-year fixed effects. Columns (1) to (3) restrict observations to applicant districts, while columns (4) to (6) restrict it to remaining districts, defined in Sections 2 and 3.1. Controls interacted with time fixed effects include ln(Area), ln(Distance to Parent Townhall), ln(Distance to State Capital), soil suitability for maize, wet rice, soybean, and wheat, terrain ruggedness, ln(Luminosity) in 1992, land use shares in 1991 for urban, agriculture, pasture and forest. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 6: Migration

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

Notes: Results from estimation of Equation (5) in Section 4. Controls include number of districts in 1991, ln(Population) in 1991, % Urban in 1991, ln(Area), ln(Distance to State Capital), ln(Income p.c.) in 1991, and HHI for race and religion in 1991. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 7: Robustness for Applicant Districts

	Baseline	No Controls	ln(0.01 +)	Only 93 Wave	Only 97 Wave	More FEs
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Split	0.17***	0.25***	0.20***	0.20*	0.20***	0.24***
	(0.02)	(0.02)	(0.04)	(0.10)	(0.04)	(0.03)
Observations	15,920	15,960	18,168	7,687	8,233	15,920
R-squared	0.96	0.95	0.90	0.96	0.96	0.97
State-Year FE	✓	✓	✓	✓	✓	-
Controls-Time FE	✓	-	✓	✓	✓	✓
Microregion-Year FE	-	-	-	-	-	✓

Notes: Table reports point estimate results from Equation (1). All Columns include district fixed effects. Controls interacted with time fixed effects include ln(Area), ln(Distance to Parent Townhall), ln(Distance to State Capital), soil suitability for maize, wet rice, soybean, and wheat, terrain ruggedness, ln(Luminosity) in 1992, land use shares in 1991 for urban, agriculture, pasture and forest. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 8: Horse-Race Between Splitting and Revenues

	ln(Luminosity)		ln(Establishments)		ln(Public Jobs)		ln(Private Jobs)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Split x Post	0.06***	0.05***	0.06***	0.05***	0.27***	0.24***	-0.00	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.06)	(0.07)	(0.04)	(0.04)
ln(Revenues p.c.)		0.05***		0.01		0.13***		0.04
		(0.01)		(0.01)		(0.03)		(0.03)
Observations	7,872	7,745	7,348	7,228	7,293	7,178	7,348	7,228
R-squared	0.99	0.99	0.99	0.99	0.86	0.86	0.97	0.97
State-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls-Time FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Results from estimation of Equation (7) in Section 5. Controls interacted with time fixed effects include number of districts in 1991, ln(Population) in 1991, % Urban in 1991, ln(Area), ln(Distance to State Capital), ln(Income p.c.) in 1991, and HHI for race and religion in 1991. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendices

A Model 58

B Data Construction 61

C Additional Results 64

D Details for Difference-in-Discontinuities Regression 67

A Model

Consider a municipality composed of two districts, A and B. District A is the municipality headquarters, i.e. it holds decision-making power over both districts if they are unified. District B considers splitting from A to become a new municipality. A perfectly immobile population mass of 1 is divided into $1-\alpha$ living in A and α living in B (Bolton and Roland, 1997).

Municipalities have two sources of revenue: local taxation (via a distortionary tax τ) and federal transfers $f(\cdot)$ as function of population size (Dur and Staal, 2008). For simplicity we assume local tax rate τ is held fixed regardless of whether districts are unified or split. Moreover, to map the Brazilian context as described in Section 2, we assume $\frac{f'(x)}{x} < 0$ and $\frac{f''(x)}{x} > 0$. We assume no income inequality within district, i.e. the representative agent has income per capita y_i.

When united, district A maximizes a Pareto-weighted sum of utilities by choosing levels of public good provision in both districts. For simplicity, the unit price of public goods is assumed to be one. District A solves

$$\max_{g_A, g_B} (1-\lambda)(1-\alpha)U_A + \lambda \alpha U_B \quad \text{s.t.} \quad g_A + g_B \leq \tau y + f(1) \tag{8}$$

where $y \equiv (1-\alpha)y_A + \alpha y_B$.

If district B chooses to split, it solves a standard public good provision problem.

$$\max_{g_B} \alpha U_B \quad \text{s.t.} \quad g_B \leq \tau \alpha y_B + f(\alpha) \tag{9}$$

where $f(\alpha)$ represents a new level of federal transfers calculated exogenously given population size α.

We summarize the model's predictions in the following result.

Proposition 1. *District B is more likely to split if*

1. *(Neglect) its welfare was neglected by the headquarters (low λ);*
2. *(Fiscal Incentives) it is small (low α) and gains in federal transfers are large;*
3. *(Local Preferences) it has high preference for public goods (high θ_B);*

4. *(Scale) it has a large local tax base (high y_B) or its headquarters has a small local tax base (low y_A);*

5. *(Local Taxation) tax rate (τ) is higher or lower, depending on parameters;*

Proof. As is standard in the literature, for each district we assume a representative agent with quasi-linear utility of the form $U_i = \theta_i h(g) + c_i$. The agent's private spending is $c_i = (1-\tau)y_i$.

From the unified policy choice problem (8), assuming there exists an interior optimum, we can solve the First Order Condition (FOC) for

$$\frac{h'(g_A^U)}{h'(g_B^U)} = \frac{\lambda}{1-\lambda}\frac{\alpha}{1-\alpha}\frac{\theta_B}{\theta_A} \tag{10}$$

In the particular case of $h(\cdot) = \ln(\cdot)$, we can solve for a closed-form levels of public good provision under unification:

$$g_A^U = \frac{(1-\lambda)(1-\alpha)\theta_A}{\overline{\theta}}[\tau y + f(1)] \quad g_B^U = \frac{\lambda\alpha\theta_B}{\overline{\theta}}[\tau y + f(1)] \tag{11}$$

where $\overline{\theta} \equiv (1-\lambda)(1-\alpha)\theta_A + \lambda\alpha\theta_B$

Under the same functional form assumptions, from problem (9), it is straightforward that

$$g_A^S = \tau(1-\alpha)y_A + f(1-\alpha) \quad g_B^S = \tau\alpha y_B + f(\alpha) \tag{12}$$

Thus, district B unilaterally chooses to split if $U_B^S \geq U_B^U$. By monotonicity of the utility function, this condition boils down to $g_B^S \geq g_B^U$. Substituting in Equations (11) and (12), we express the surplus condition as:

$$G(\lambda, \alpha, \theta_A, \theta_B, \tau, y_A, y_B, f) \equiv g_B^S - g_B^U = \tau\alpha y_B + f(\alpha) - \frac{\lambda\alpha\theta_B}{\overline{\theta}}[\tau y + f(1)] \geq 0 \tag{13}$$

With simple algebra we can show that

1. $\frac{\partial G}{\partial \lambda} \leq 0$.

2. $\frac{\partial G}{\partial \alpha} \leq 0$ if gains in transfers are large enough.

3. $\frac{\partial G}{\partial \theta_A} \geq 0$ and $\frac{\partial G}{\partial \theta_B} \geq 0$.

4. $\frac{\partial G}{\partial \tau} \lesseqgtr 0$ depending on parameters.

5. $\frac{\partial G}{\partial y_A} \leq 0$ and $\frac{\partial G}{\partial y_B} \geq 0$.

$\square$

B Data Construction

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

Brazil has 26 state legislative assemblies.[29]. For each state assembly, we search for digitized historical records on split requests from the first half of the 1990s. We find records for twelve states: Amapá, Amazonas, Espírito Santo, Goiás, Mato Grosso, Minas Gerais, Pará, Paraná, Rio Grande do Sul, Rondônia, Santa Catarina, and São Paulo. The availability and quality of the data widely vary across states. We exemplify the online material we have access to in Figure B.1 below.

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

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

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

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

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

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

Minas Gerais. indicator for whether district has requested to split; indicator for whether

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

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

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

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

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

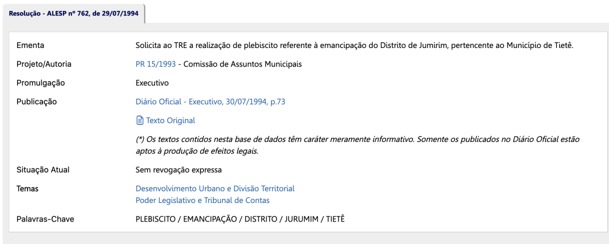
Rondônia. indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; date when the request was archived; id of the split process; date when the plebiscite was approved; and result of the plebiscite.

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

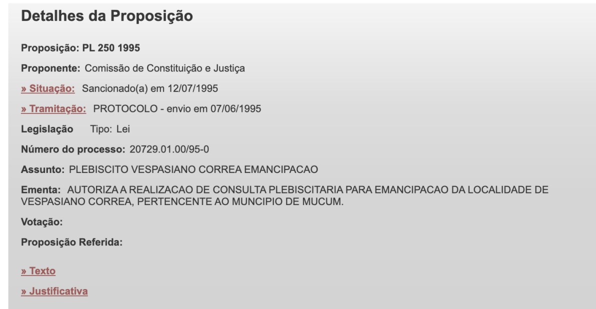
São Paulo. indicator for whether district has requested to split; indicator for whether district has the request approved; indicator for whether the request was archived; id of the split process; date when the process began; date when the plebiscite was approved; and result of the plebiscite.

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

(a) São Paulo

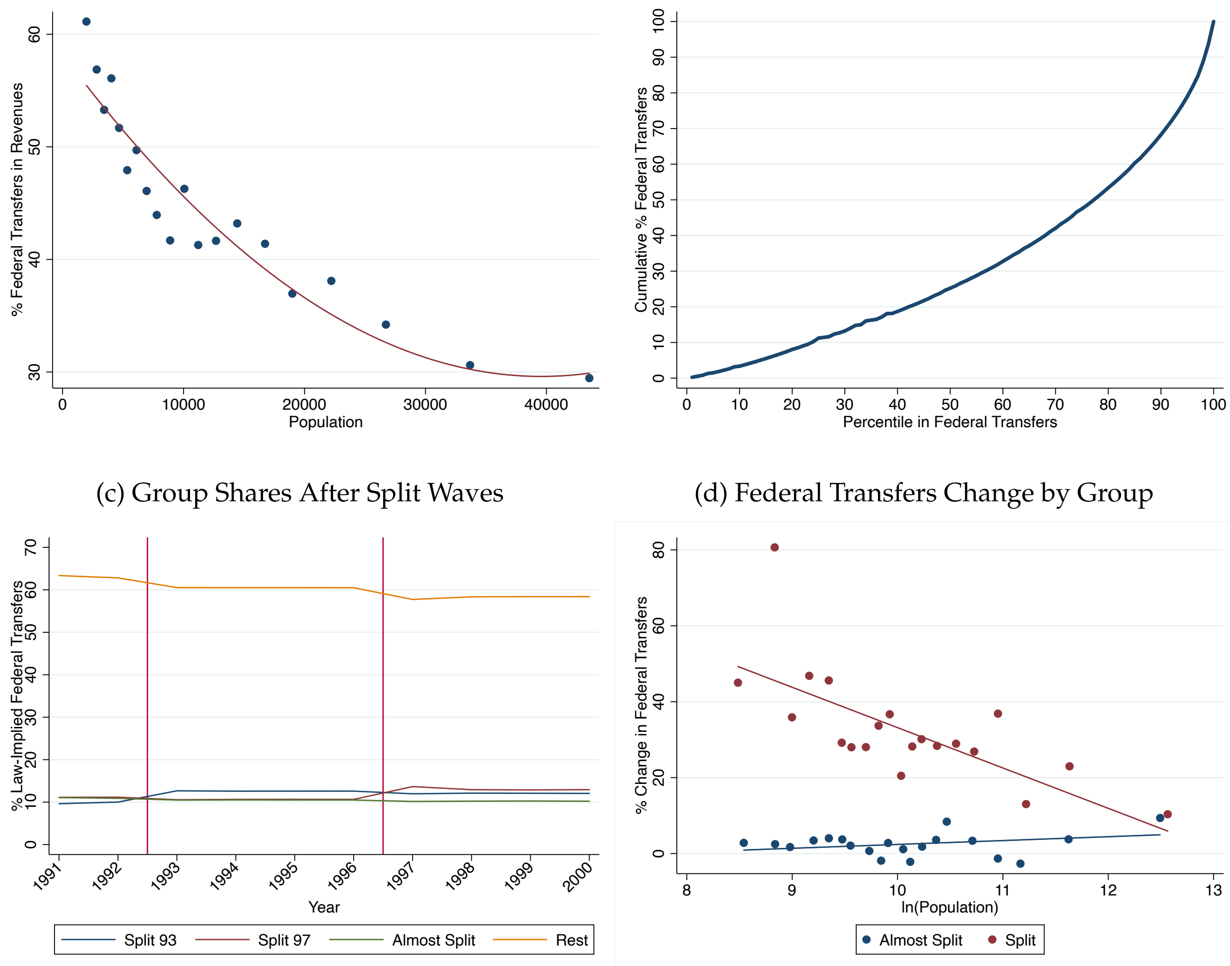


(b) Rio Grande do Sul



C Additional Results

Figure C.2: Distribution of Federal Transfers



(a) % Transfers from Municipality Revenues 1991

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

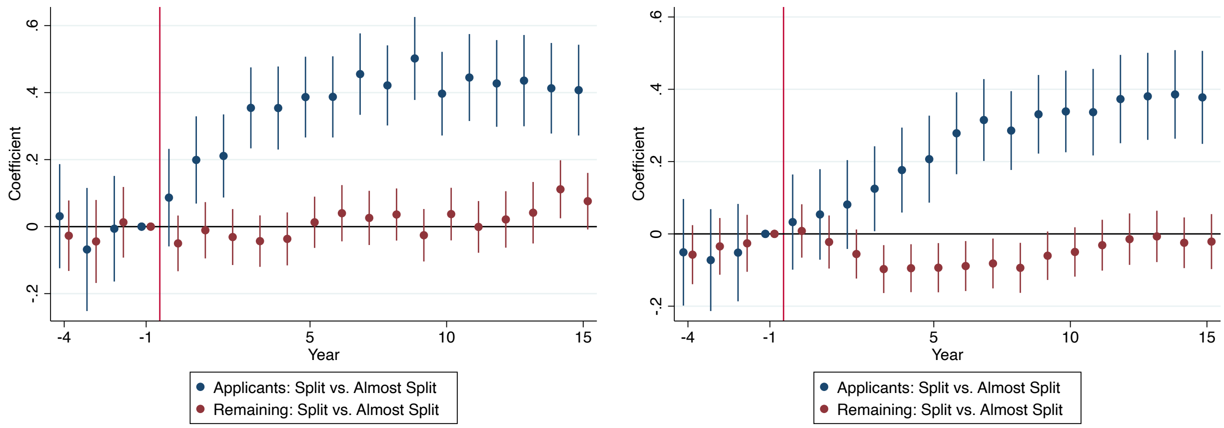
(c) Group Shares After Split Waves

(d) Federal Transfers Change by Group

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

Figure C.3: Economic Activity, without Controls

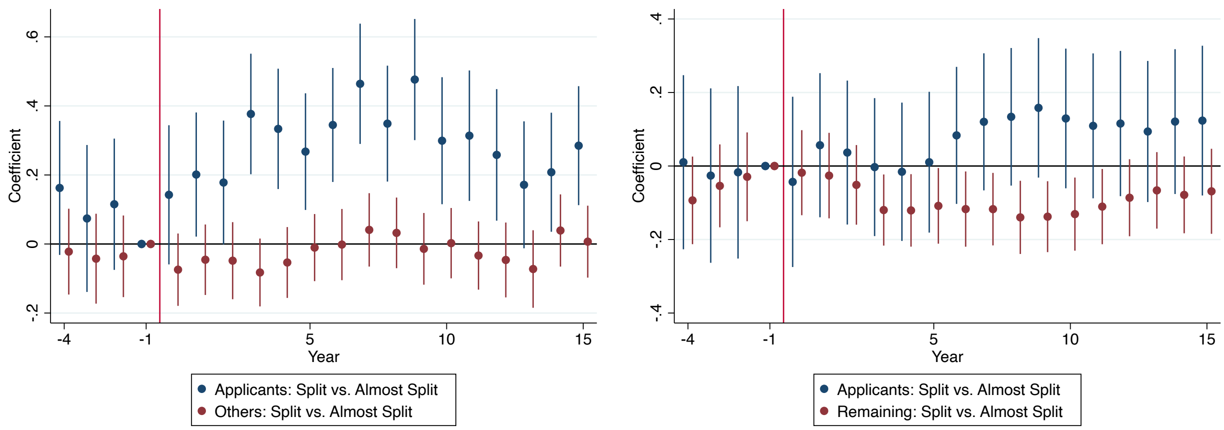
(a) ln(Luminosity) (b) ln(Urban Area)



Notes: Results from estimation of Equation (1) in Section 4. Each set of coefficients is from one difference-in-differences equation, one for applicant and another for remaining districts. Bars represent the 95%-confidence intervals.

Figure C.4: Economic Activity, Only 1997 Wave

(a) ln(Luminosity) (b) ln(Urban Area)



Notes: Results from estimation of Equation (1) in Section 4 restricting observations to districts requesting so split between 1994 and 1996. Each set of coefficients is from one difference-in-differences equation, one for applicant and another for remaining districts. Controls interacted with time fixed effects include ln(Area), ln(Distance to Baseline Townhall), ln(Distance to State Capital), soil suitability for maize, wet rice, soybean, and wheat, terrain ruggedness, ln(Luminosity) in 1992, land use shares in 1991 for urban, agriculture, pasture and forest. Bars represent the 95%-confidence intervals.

Table C.1: Oster (2019) Correction

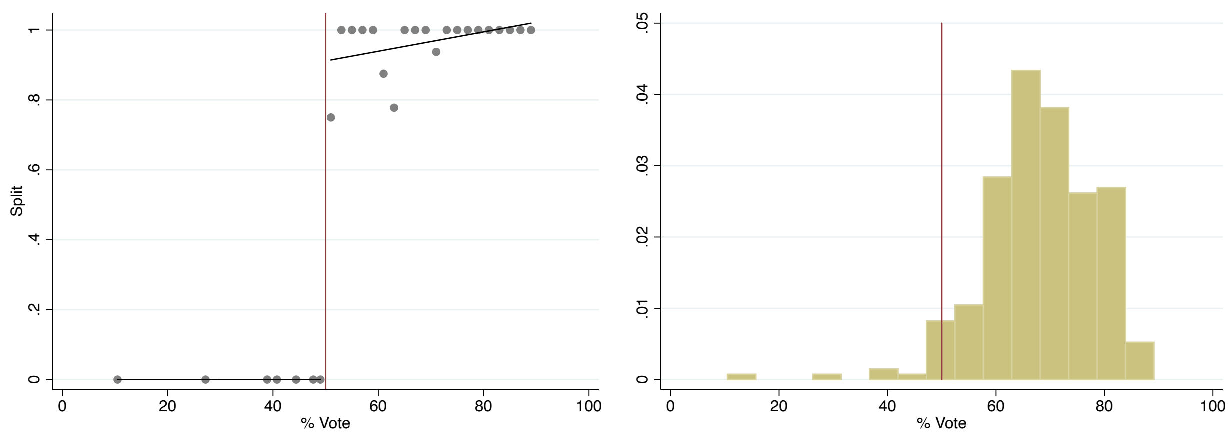
	Baseline		β		δ
Applicant: Post × Split	.166	5.5	2.05	1.36	.28
Other: Post × Split	-.011	-.15	1.02	.21	-.18
δ		1	2	5	
β					0
R_{max}		1	1	1	1

Notes: Results from the procedure proposed by Oster (2019) for estimates from Equation (1).

D Details for Difference-in-Discontinuities Regression

Figure D.5: Referendums in *Minas Gerais* State

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



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

Table D.2: Discontinuity Test on Covariates

	ln(Dist. Townhall) (1)	ln(Area) (2)	ln(Luminosity) (3)	% Urban (4)	% Pasture (5)	% Agric. (6)	% Forest (7)
Referendum Vote $\geq$ 50%	-0.36	0.79	-1.54	3.47	-26.65	-5.43	34.51*
	(0.59)	(0.83)	(2.49)	(3.91)	(21.26)	(8.17)	(19.13)
Observations	93	93	93	93	93	93	93
R-squared	0.02	0.10	0.21	0.15	0.03	0.04	0.05

Notes: Estimates from Equation (6). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table D.3: Fuzzy Difference-in-Discontinuities on ln(Luminosity)

	First Stage (1)	Reduced Form (2)	Second Stage (3)	DD (4)
Referendum Vote $\geq$ 50%	0.97***			
	(0.23)			
Post x Referendum Vote $\geq$ 50%		0.29***		
		(0.08)		
Post x Split			0.48***	0.29***
			(0.07)	(0.04)
Observations	93	1,648	1,648	3,882
R-squared	0.39	0.96	0.96	0.96
District FE	-	✓	✓	✓
Controls-Year FE	-	✓	✓	✓

Notes: Estimates from Equation (6). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.