Government Fragmentation and Economic Growth*

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Abstract

We estimate the impact of local government fragmentation on economic activity in Indonesia over 2000–2014, when the number of districts increased by 50 percent. Exploiting idiosyncratic variation in the timing of district splits, we find that fragmentation reduces district GDP in the short term despite large increases in central transfers. The GDP decline is larger in "child" districts that acquire a new capital and government. Furthermore, splitting districts focus spending on administration without improving public services or reducing red tape and corruption. The downsides of fragmentation due to economies of scale and low bureaucratic capacity outweigh potential upsides.

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1 Introduction

Decentralization—the devolution of responsibilities to subnational governments—is a policy choice that carries significant implications for economic activity and welfare. Centralized systems exploit economies of scale in the provision of public services, are better positioned to internalize policy spillovers, and feature fewer, more capable politicians and bureaucrats. Decentralized systems, on the other hand, can better accommodate diverse citizen preferences, incorporate information about local conditions, or promote accountability via interjurisdictional competition. While greater accountability or improved public service provision are valuable goals on their own, the stated aim of most decentralization policy is to enhance economic prosperity through these channels.[1] However, the net impact of decentralization on economic growth remains an open question.

This paper examines a prevalent feature of decentralization reforms across the developing world: the creation of new local governments. Similar to the transfer of responsibilities from central to local authorities, government fragmentation expands the number of jurisdictions responsible for policymaking, albeit while keeping the roles of different tiers of government unchanged. Consequently, many advantages and disadvantages of decentralization apply with equal force to the proliferation of local governments.[2]

We study an extraordinary period of government fragmentation in Indonesia between 2000 and 2014, during which the number of districts increased from 341 to 514 with the stated goal of accelerating regional economic development (Regulation No. 129/2000). Exploiting idiosyncratic variation in the timing of district splits generated by two national moratoria, we find that fragmentation reduces economic growth in the short run. Splitting districts experience a cumulative loss of 18 percent of GDP over five years following a split, compared to non-splitting districts. We observe a similar decline when using household survey data

[1] See, for example, World Bank (1999), United Nations (2009), and International Monetary Fund (2009).

[2] A distinct but related phenomenon is political party fragmentation, e.g., many jurisdictions controlled by different political parties. This type of fragmentation can also result in policymaking that neglects spillover effects (Magontier, Solé-Ollé and Viladecans-Marsal, 2022).

on expenditure as well as nighttime luminosity data. This decline is remarkable considering that splits lead to a large increase in central transfers. Based on a conservative multiplier assumption, the fiscal shock alone should result in a 17 percent cumulative *increase* in GDP.

To examine whether the results mask general equilibrium effects, we compare the effects of doubling the number of districts at three levels of aggregation: original district (2000 borders), province-by-island, and province. We find that the negative growth effects are more pronounced at higher levels of geography. While these effects are not statistically different from one another, they rule out the possibility that splits simply reshuffle economic activity across space. District proliferation lowered nationwide GDP in Indonesia.

We consider several mechanisms through which local government fragmentation can affect growth. Theory motivated by developed country settings emphasizes the roles of tax competition and sorting by preferences over the level of public services.[3] These mechanisms are unlikely to be important in our setting, as Indonesian districts are extremely limited in their ability to tax income and property—a common situation in the developing world (Gadenne and Singhal, 2014). However, districts do collect business licensing fees and receive shared revenue from business taxes administered by the central government. Therefore, government fragmentation could increase competition for mobile firms, limit rent-seeking, and improve the business environment (Brennan and Buchanan, 1980; Fisman and Gatti, 2002; Arikan, 2004). District proliferation could also promote accountability via stiffer yardstick competition, in which voters judge the quality of local politicians by comparing their outcomes to those of similarly situated jurisdictions (Besley and Case, 1995).[4] On the other

[3]Increasing the number of jurisdictions can facilitate sorting by taste (Tiebout, 1956), which can either increase growth (Brueckner, 1999, 2006) or decrease growth (Benabou, 1993, 1996), depending on the composition of the resulting heterogeneous communities. Government fragmentation can also increase competition for mobile capital, leading to lower capital tax rates and higher growth (Hoyt, 1991; Hatfield, 2015). See Agrawal, Hoyt and Wilson (2022) for a review of the literature on decentralization and growth.

[4]However, in the model of Boffa, Piolatto and Ponzetto (2016), fragmentation *reduces* accountability due to heterogeneity in voter information and diminishing marginal benefits of information. The relationship between decentralization and accountability is ambiguous in the model of Bardhan and Mookherjee (2000).

hand, fragmentation could impede growth if there are economies of scale in the provision of public goods (Oates, 1972; Alesina and Spolaore, 1997), or if bureaucratic capacity falls because the new governments are staffed with less qualified civil servants.

We find that bureaucratic capacity and economies of scale are important mechanisms, but accountability is not. GDP falls both in parent districts, which retain the original government, and in newly created child districts, but the decline is larger in child districts.[5] The difference in economic impact on parent and child districts mirrors the difference in quality of their civil servants, who are tasked with the majority of local public service provision. Child districts tend to have younger, less educated civil servants. The quality of the bureaucracy therefore matters but does not explain all of the decline. Economies of scale also play a role. Splitting districts devote a greater share of expenditure to administrative expenses and fail to improve public services—despite the increase in central transfers. Bureaucratic capacity also contributes to the disappointing results for public services. District splits lead to delays in the execution of physical capital spending plans, with only child districts struggling to implement plans following a split. By contrast, the hypothesized gains in accountability never materialize. Following a split, firms report paying bribes at higher rates, and there are no improvements in the regulatory environment. Unsurprisingly, firm productivity does not increase.

The baseline model nonparametrically controls for region-by-year effects, so that identification hinges on a conditional parallel trends assumption: within each region, GDP would have grown at the same rate in splitting and non-splitting districts in the absence of splitting. This assumption could be violated if districts select into splitting based on recent economic shocks. However, institutional factors deprive districts of precise control over the timing of splits, lessening this concern. Furthermore, splitting and non-splitting districts grew at the same rate, on average, prior to the split.

The conditional parallel trends assumption could also be violated if baseline characteristics that matter for growth are unbalanced for splitting and non-splitting districts within

[5]More precisely, GDP falls more in child districts relative to the expected path of GDP given the fiscal multiplier effect of splits.

a region. To address this concern, we show that the results are robust to controlling for time-varying effects of baseline ethnic fractionalization, urbanization, age structure, and education.

A final, unrelated problem is that the timing of splits varies across districts. As a result, estimates based on a restrictive two-way fixed effects model may not recover a reasonably weighted causal effect if treatment effects are heterogeneous across time or districts. We circumvent this problem by estimating separate difference-in-differences estimands for each splitting cohort and time period. We then report the weighted average of the cohort-specific estimates.[6]

Our setting provides several advantages. First, the massive scale of the reforms taking place in the world's fourth-largest country makes this an important episode to study. If decentralization and fragmentation are to be recommended as a rule, it is necessary to understand what effect these policies can have at a wide scale. Second, two national moratoria on splitting generated idiosyncratic variation in the timing of splits, strengthening our difference-in-differences design. Much of the earlier work on decentralization and growth relies on cross-country comparisons across very different contexts and with confounding events around decentralization episodes. Finally, Indonesia is one of the few developing countries that provides a consistent GDP series at the second subnational level. Research on decentralization and local development in other settings has relied on imperfect proxies such as nighttime luminosity.

Our results are informative for the policy debate over administrative unit redistricting. Local governments have proliferated in many parts of the developing world and Eastern Europe with the goal of promoting economic development (Swianiewicz, 2010; Grossman and Lewis, 2014; Grossman, Pierskalla and Dean, 2017).[7] Our analysis shows that the disruption caused by the creation of new governments can more than offset the fiscal benefits enjoyed by splitting units, reducing growth in the short run. To avoid the pitfalls of fragmentation, policymakers should pay special attention to the quality of the new government personnel

[6] Our approach draws on the estimators of Wooldridge (2021) and Sun and Abraham (2021).

[7] Notable examples include Brazil, Nigeria, and Vietnam.

and should consider how government scale will impact public good provision.

This paper contributes to multiple literatures. First, it contributes to the literature on administrative unit redistricting. This literature focuses on public expenditure and service delivery, not growth (see Gendźwiłł, Kurniewicz and Swianiewicz (2021) for a review). However, there are two recent exceptions. Dahis and Szerman (2023) find that municipal splits in Brazil improve public service provision and increase nighttime luminosity in new municipalities, but have no impact on private-sector employment. By contrast, Cohen (2024) finds that district splits in Uganda harm the quality of infrastructure and reduce nighttime luminosity in new districts. Prior research on district splitting in Indonesia has focused on public services (Lewis, 2017; Singhania, 2022), ethnic conflict (Bazzi and Gudgeon, 2021), and deforestation (Burgess, Hansen, Olken, Potapov and Sieber, 2012; Alesina, Gennaioli and Lovo, 2019).[8] In contrast to these papers, we focus on economic growth. Related work in political science studies the political motivations behind administrative unit splitting in the developing world (e.g., Grossman and Lewis, 2014; Pierskalla, 2016). Research in developed country settings focuses on the effect of amalgamations on public expenditure.[9]

Second, our paper contributes to the literature on decentralization and growth. Cross-country analyses are challenging due to endogeneity concerns and the difficulty of finding a single measure of decentralization that summarizes intergovernmental relations and can be consistently measured for all countries (Oates, 1993; Rodden, 2004). In response to these challenges, recent work uses within-country variation in the number of local governments in contexts very different from our own. Exploiting cross-sectional variation in the number of local governments in a metropolitan area, Stansel (2005) and Hatfield and Kosec (2013)

[8]Our findings on public services are consistent with those of Lewis (2017), who finds that splits do not improve public service delivery. We build on their results by using a longer time horizon and a larger set of indicators of public service provision. Singhania (2022) focuses on the interaction effect between splitting and direct mayoral elections, using a more limited set of years.

[9]See, for example, Reingewertz (2012), Breuille and Zanaj (2013), and Blom-Hansen, Houlberg, Serritzlew and Treisman (2016). Erlingsson, Mörk and Klarin (2021) provide a rare study of municipal splitting in a developed country setting.

find that fragmentation increases growth in the United States. Similarly, Zhang, Sun, Cai and Wang (2019) find that fragmentation increases local growth in China, but only when starting from a low level of fragmentation. In those studies the local governments had been operating for a significant amount of time, whereas we study the creation of new governments.

Finally, our paper is related to research documenting the downsides of decentralization in practice in developing countries. Lipscomb and Mobarak (2017) find that decentralization exacerbates water pollution externalities in Brazil. Also examining Brazil, Kresch (2020) shows that uncertainty over the responsibilities of different tiers of government can deter public investment. Our paper, by contrast, underscores the importance of bureaucratic capacity and scale for realizing the potential gains from fragmentation.

In Section 2 we describe the institutional context of Indonesia's decentralization reforms. Section 3 describes our dataset, which combines data on districts, households, villages, and manufacturing firms. Section 4 then explains the empirical strategy, and Section 5 presents the results. In Section 6 we examine potential mechanisms behind the results. Section 7 compares our results to those of previous studies and discusses potential implications for long-run growth. Section 8 provides concluding remarks.

2 Empirical Context

2.1 Indonesia's Decentralization Reforms

Following the resignation of autocratic ruler Suharto in 1998, Indonesia transitioned to democracy and instituted a series of political and fiscal reforms. Indonesia is currently divided into 34 provinces, the first tier of subnational government, which mostly play a coordinating role. Districts, the second tier of subnational government, are categorized as either urban districts (*kota*) or rural districts (*kabupaten*), however political institutions and fiscal responsibilities are the same for both types. Subdistricts (*kecamatan*) and villages are the third and fourth tiers of government, respectively.

Shortly after democratizing, Indonesia implemented the "Big Bang" decentralization reforms, which expanded the authority of district governments (Shah, Qibthiyyah and Dita, 2012). From 2001 to 2014, districts were responsible for the provision of education, health, environmental, and infrastructure services (Law No. 22/1999), with 30 percent of consolidated government expenditure occurring at the district level (World Bank, 2003).[10] In practice, infrastructure projects involved collaboration between districts and villages: districts financed upgrades and procured engineers while villages were responsible for maintenance (World Bank, 2010). Overall, districts determined the vast majority of policies on local public services.

The Village Law (Law No. 6/2014) ushered in the second phase of decentralization starting in 2015, devolving some responsibilities from districts to villages. We therefore limit our sample to the years 2001–2014 to hold constant the responsibilities of subnational governments.[11]

Central transfers financed the vast majority of district expenditure over this period, as specified in Law No. 25/1999. The largest transfer is an unconditional, non-matching grant known as the General Allocation Grant (*Dana Alokasi Umum*), or "general grant" for short. On average over half of district revenue comes from this grant. Half of the grant funds the so-called "basic allocation," which covers the civil service wage bill. The rest is determined according to a "fiscal gap" formula that uses proxies for expenditure needs (e.g., population, land area, poverty) and fiscal capacity (e.g., predicted revenue from other sources) (World Bank, 2007; Cassidy, 2023). The central government regulates recruiting and employment contracts to prevent excessive spending on civil servant wages (Shah et al., 2012). In all other areas district spending out of the general grant is essentially unconstrained.

Some districts receive significant Shared Natural Resource Revenue (*Dana Bagi Hasil Sumber Daya*), which is distributed on the basis of local natural resource extraction and is unconditional. Finally, a small portion of the district budget (around 6 percent) comes from Special Allocation Grants (*Dana Alokasi Khusus*), which are conditional, matching grants awarded on a discretionary basis. Law No. 33/2004 modified the grant allocation formulas and increased the budget for the general grant (see Cassidy, 2023).

[10]See United Nations (1999) for an English translation of Law No. 22/1999.

[11]Technically, our sample includes years prior to 2001 to aid in the estimation of pretrends. These years do not enter into the estimates of treatment effects.

While Indonesia substantially decentralized expenditure responsibilities, it did not devolve tax authority to a similar degree. The central government sets tax rates on sales, individual income, and corporate income, and administers these taxes. The central government also set property tax rates until 2010. Between 2011 and 2014, districts were allowed to voluntarily adopt tax-setting authority over the property tax, with the vast majority of districts acquiring this authority in 2014. Because of the timing of this reform, and the fact that districts were reluctant to deviate from pre-decentralization property tax rates (von Haldenwang, 2017), the property tax was not an important local policy tool during the study period.

The central government returns a portion of the revenue to the district where the taxes were collected. The sharing rate is 9 percent for the property tax, 16 percent for the property transfer tax, and 12 percent for the income taxes. Minor local taxes and user fees, such as the motor vehicle tax and the hotel tax, are under the purview of districts but are subject to a rate ceiling set by the center. Almost all districts charge the maximum rate permitted (World Bank, 2003). Districts are also responsible for many business licenses, which formally may be obtained by paying a fee set by the district.

Prior to democratization, President Suharto appointed district heads. After the country transitioned to democracy, district voters directly elected members of the local parliaments, and these parliaments selected the district heads. Starting in 2005, districts introduced direct elections of district heads in a staggered fashion. The election timing was staggered because incumbents were allowed to finish their five-year terms, which for idiosyncratic reasons were not synchronized across districts.

2.2 District Fragmentation

Local elites typically spearhead the effort to create new districts. Formally, district parliamentarians petition to split a district, with the approval of the mayor of the original district, through a process known as *pemekaran*, or “blossoming.” The central government then decides whether to accept or reject the petition based on technical criteria. At the outset, the proposed parent and child districts all had to contain at least three subdistricts and possess sufficient economic capacity (Regulation No. 129/2000).[12] The new district boundaries follow the borders of existing subdistricts. Upon approval, the Ministry of Home Affairs appoints an interim head of the child district; the interim legislature features the same party composition as that of the original district. One to two years later, the first elections take place in the child district (Fitrani, Hofman and Kaiser, 2005).

During the sample period, the number of districts increased from 341 to 514, an increase of 50 percent. Panel (a) of Figure 1 shows that the growth of district governments over this period was interrupted twice—from 2004 to 2006 and from 2009 to 2012—by national moratoria, creating exogenous variation in the timing of districts splits. Over a hundred districts had applied to split before the first moratorium was announced and did not know when their application might be approved. After the first moratorium, the central government strengthened the splitting regulations to require that (1) the proposed districts all contain at least four subdistricts (in the case of *kota*) or five subdistricts (in the case of *kabupaten*); and (2) the original district has existed for at least seven years (Regulation No. 78/2007). We further discuss the moratoria in the context of our identification strategy in Section 4.

Panel (b) of Figure 1 provides a map of district borders in 2000 (thick black lines) and 2012 (thin gray lines), with districts that split over this period shaded in purple.[13] About one third of the original districts split at least once between 2001 and 2014. The map shows that the island of Java, the historical center of economic and political power, has relatively few districts that split. By contrast, district splitting was widespread in the "outer islands" of Sumatra, Kalimantan, Sulawesi, Maluku, Papua, and Nusa Tenggara.

There are several motivations for district creation. The first is purely political. The creation of a new government, with no entrenched incumbents, provides an opportunity for local elites to gain political power. Second, redistricting may satisfy local desires for more ethnically homogeneous jurisdictions (Fitrani et al., 2005; Pierskalla, 2016). Finally, there is a strong fiscal incentive to split. The general grant essentially has a large fixed component, which does not even depend on population. This virtually guarantees that transfers increase in per

[12]Districts are also allowed to amalgamate, but this has not occurred in practice.

[13]As discussed below, we omit districts that split for the first time during the period 2013–2014.

capita terms in both the child and parent districts following a split.

Bazzi and Gudgeon (2021) document that splitting causes total fiscal transfers to increase by 20 percent on average, while Cassidy (2023) shows that district expenditure increases roughly one-for-one with the general grant. Therefore, in the absence of other effects from fragmentation, splits should increase local GDP via a fiscal multiplier effect. In the empirical analysis, we compare the actual GDP response to splitting to a benchmark implied by the increase in transfers and local fiscal multipliers estimated in the literature.

3 Data

This section describes key features of our data and outcome variables. Further details on data sources and variable construction can be found in Online Appendix A. All outcome variables are aggregated to the level of district borders in 2000, unless specified otherwise. We identify splits using administrative data on district establishment dates, and we aggregate variables to original borders using a crosswalk provided by the World Bank.

District GDP. Our main outcome variable is district-level GDP. The Central Bureau of Statistics (*Badan Pusat Statistik*, or BPS) produces estimates of district GDP, which are disseminated by the World Bank's Indonesia Database for Policy and Economic Research (INDO-DAPOER). The GDP series spans 2000–2013. BPS often reports separate GDP estimates for parent and child districts in selected years prior to a split. This allows us to estimate the growth impacts of splitting separately for parent and child districts. For districts that experienced multiple splits, we aggregate parent and child GDP up to district borders as of the year of the first split, because this is the level at which GDP is usually reported. (See Online Appendix A for details.)

Bureaucratic capacity. We measure bureaucratic capacity using data on civil servants from multiple sources. The National Civil Service Agency (*Badan Kepegawaian Nasional*, or BKN) provides data on the number of civil servants in each district from 2005 to 2015.[14] We also use data on education and age from the 2005 Intercensal Population Survey (*Survei*

[14]We are grateful to Jan Pierskalla for sharing these data with us.

Penduduk Antar Sensus, or SUPAS) and the Indonesian National Labor Force Surveys (*Survei Angkatan Kerja Nasional*, or SAKERNAS) of 2007–2009 and 2011–2015. We augment the data on civil servants with information on the education level of the first democratically appointed mayor from Mukherjee (2023).[15]

Firm outcomes. We analyze firm-level outcomes using data from the Indonesian manufacturing survey of large and medium-sized firms (*Survei Industri Besar/Sedang*, or IBS), which covers the universe of manufacturing establishments with at least 20 workers. We combine the surveys to construct an unbalanced panel of establishments over the period 1995 to 2014. For simplicity, we use the terms "firm," "establishment," and "plant" interchangeably, even though we cannot link establishments belonging to the same firm.

IBS contains information on outcomes such as the total value of production, number of employees, and industry of operation. Establishments also report their total indirect tax payments, which combines taxes and fees administered by districts (establishment license fees, building and land taxes, compulsory donations to finance local infrastructure) and taxes administered by the central government (sales taxes, import duties, excise taxes). Another outcome of interest is "gifts" paid by the firm to external parties. We interpret this variable as including bribe payments to officials, following Henderson and Kuncoro (2006, 2011).

To support our interpretation of "gifts" as bribes, in the appendix we show that the within-firm incidence of bribery over time is positively correlated with firms' activities that require permits or licenses from the local government, such as electricity connection from the government, exports, land contracts, and building construction. By contrast, the incidence of bribery is negatively correlated with own generation of electricity or purchase of electricity from non-governmental sources (Appendix Table B.1).

Sample selection. We drop all five districts in the province of Jakarta, as these districts are managed by the province. We also omit the five districts that split for the first time during 2013–2014, because we cannot measure four-year growth post-split for these districts, as the GDP series ends in 2013. The resulting sample contains 331 districts at the level of 2000 borders.

[15]We are grateful to Priya Mukherjee for sharing these data with us.

Summary statistics. Appendix Table B.2 summarizes the baseline characteristics of splitting and non-splitting districts. Of the 331 districts in the sample, 98 experienced at least one split during the sample period, while 233 never split. Initial GDP was 8 log points lower in splitting districts than in non-splitting districts, however this difference is statistically insignificant. Splitting districts received more general grant revenue, were more ethnically fractionalized, and were less urbanized at baseline. While the ethnic fractionalization distributions exhibit excellent overlap for splitting and non-splitting districts, the urbanization rate distributions do not (Appendix Figure B.1). Many non-splitting districts have urbanization rates in excess of 75 percent, whereas the urbanization rate is below 75 percent for all splitting districts. Splitting districts also tend to have younger and less educated populations, though these differences are smaller than those for ethnic fractionalization and urbanization.[16]

4 Empirical Strategy

We use a difference-in-differences (DiD) strategy to estimate the effect of government fragmentation on economic growth. District creation is a political process. The decision of whether to split is correlated with several district-level characteristics, such as ethnic fractionalization. (See Pierskalla (2016) and Appendix Table B.2.) Rather than relying on cross-sectional variation in whether a district ever split, our identification strategy exploits idiosyncratic variation in the timing of splits. This variation comes from two sources.

First, there is generally a multi-year lag between when a district applies for a split and when the central government approves the split, and there is considerable uncertainty over whether the split will be approved. Therefore, prospective leaders of a new district lack precise control over the timing of its creation.

Second, the national government imposed moratoria on district splitting from 2004 to 2006 and from 2009 to 2012, generating additional idiosyncratic variation in the timing of splits. A district that applied to split in 2002 likely would have been approved in 2007

[16]Appendix Table B.3 provides additional summary statistics, and Appendix Table B.4 provides details on sample coverage.

rather than in 2004 as a result of the first moratorium. In fact, 114 district applications awaited consideration by the end of the first moratorium (BAPPENAS, 2007, p. 1). The central government justified the moratoria by citing the fiscal costs of district creation—total intergovernmental grants increase whenever a new district is formed—and the lack of qualified personnel needed to run the new governments (Bazzi and Gudgeon, 2021). To the best of our knowledge, the central government never cited the local economic impacts of splitting as a reason for the moratoria.

After the split is approved, an interim government is appointed. One to two years later, a democratically elected government takes over and the district starts receiving fiscal transfers from the central government. We define the split year as the first full calendar year following the passage of the legislation creating the new district(s). In this way we include the transition process as part of the "treated" period. Districts are defined according to the original district boundaries in 2000.

Because the timing of the first split varies across districts, a restrictive two-way fixed effects model that includes a single "post split" dummy may not recover a reasonably weighted average treatment effect in the presence of treatment effect heterogeneity.[17] To avoid this problem, we construct separate DiD estimators for each splitting cohort, defined according to the year of the district's first split.[18] We then take a weighted average of the cohort-specific estimates. The control group is always the set of districts that never split during the analysis period.

We nonparametrically control for region effects, so that identification relies on a conditional parallel trends assumption: splitting and non-splitting districts *located in the same region* would have experienced the same GDP growth on average in the absence of the split.

[17]See, e.g., de Chaisemartin and D'Haultfœuille (2020), Goodman-Bacon (2021), Sun and Abraham (2021), Callaway and Sant'Anna (2021), and Borusyak, Jaravel and Spiess (2021).

[18]Only one district (Kabupaten Bengkalis) split for the first time in 2010. To avoid estimating a cohort-specific treatment effect based on a single treated unit, we code this district as having split in 2009. Bengkalis's legislation creating a new district was passed on January 16, 2009, so it is basically accurate to code 2009 as the first full calendar year following passage of the legislation.

This assumption is weaker than the usual (unconditional) parallel trends assumption. In particular, it allows for regional heterogeneity in growth trajectories that may be correlated with the incidence of splitting.

On a practical note, we scale changes in GDP by initial GDP, rather than using changes in log GDP, to remain consistent with the fiscal multiplier literature. This allows us to relate percentage changes in GDP to changes in grants as a percentage of GDP. The results are very similar if we instead use changes in log GDP.

Define the set of splitting cohorts $\mathscr{E} \equiv \{2002, 2003, 2004, 2008, 2009\}$. Let $Y_{d,t}$ be real GDP in district d in year t, let E_d be the cohort of district d (with $E_d = \infty$ for never-splitters), and let $\boldsymbol{X}_d$ be a vector of region dummies.[19] Define the cohort indicators $D_d(e) \equiv 1(E_d = e)$. We implement our estimator in two steps. First, we estimate the regression

$$\frac{Y_{d,e+h} - Y_{d,e-1}}{Y_{d,e-1}} = \alpha_{e,h} + \beta_{e,h} D_d(e) + \boldsymbol{X}_d' \boldsymbol{\lambda}_{e,h} + \left(D_d(e) \cdot \dot{\boldsymbol{X}}_d(e)\right)' \gamma_{e,h} + \varepsilon_{d,e,h} \tag{1}$$

for each cohort e and time horizon h, using the subsample of districts that either split for the first time in year e or never split. The variable $\dot{\boldsymbol{X}}_d(e)$ is defined as $\boldsymbol{X}_d - \mathrm{E}(\boldsymbol{X}_d \mid E_d = e)$. The presence of the interaction term allows the covariates to have a different effect on potential outcomes in the treated and untreated states. Centering the covariates in the interaction term about their cohort-specific means gives $\beta_{e,h}$ an interpretation as the average treatment effect for cohort e. Under the conditional parallel trends assumption and a "no anticipation" assumption, the OLS estimator $\hat{\beta}_{e,h}$ is consistent for the cohort-specific average treatment effect on the treated (CATT) (Sun and Abraham, 2021; Wooldridge, 2021).[20]

[19] Following the Central Bureau of Statistics, we code seven regions: Sumatra, Java, Nusa Tenggara, Kalimantan, Sulawesi, Maluku, and Papua.

[20] Formally, let $Y_t(e)$ denote the potential outcome in year t if the district split for the first time in year e, with $e = \infty$ if the district never split, and let X be a vector of region dummies. The conditional parallel trends assumption states that

$$\mathrm{E}\left(\frac{Y_{e+h}(\infty) - Y_{e-1}(\infty)}{Y_{e-1}(\infty)} \;\middle|\; X, E = e\right) = \mathrm{E}\left(\frac{Y_{e+h}(\infty) - Y_{e-1}(\infty)}{Y_{e-1}(\infty)} \;\middle|\; X, E = \infty\right)$$

Note that because the covariates are binary and the regression is fully saturated, the model does not impose any functional form assumption on how potential outcomes depend on covariates; the baseline model thus controls for region-by-year effects nonparametrically. In robustness checks we will add non-binary covariates to Equation (1), thereby imposing a linear relationship between the covariates and potential outcomes, while allowing this relationship to differ in the treated and untreated states and over time.

Our approach to estimating each $\beta_{e,h}$ is similar to the one proposed by Wooldridge (2021), with a few differences. Wooldridge (2021) uses the average outcome across all pre-treatment years as the baseline. To remain consistent with the fiscal multiplier literature, we use the outcome in the year prior to the split as the baseline. Measuring the change in GDP relative to this baseline necessitates the use of cohort-specific equations as in (1). We stack these equations and jointly estimate the parameters. The other main difference is that Wooldridge (2021) forms counterfactual outcomes by averaging across all units that have not yet adopted the treatment as of year $e+h$. We instead use never-treated units as controls to ensure that the composition of the control group does not change over time.

In the second step, we combine the DiD estimates to form an estimate of the cohort-size-weighted average treatment effect on the treated,

$$\hat{\beta}_h = \sum_{e\in\mathscr{E}} \omega_e \hat{\beta}_{e,h}, \tag{2}$$

where ω_e is the share of splitting districts that belong to cohort e. We calculate standard errors for $\hat{\beta}_h$ that are robust to heteroskedasticity and clustering within original district boundaries.[21] While the conditional parallel trends assumption is not testable, it would be

for all e and $h > 0$. Under this assumption and the "no anticipation" assumption, $Y_t(e) = Y_t(\infty)$ for $t < e$, each DiD estimator identifies a cohort-specific average treatment effect on the treated (Sun and Abraham, 2021; Wooldridge, 2021). That is,

$$\beta_{e,h} = \mathrm{E}\left(\frac{Y_{e+h}(e) - Y_{e+h}(\infty)}{Y_{e-1}(\infty)} \;\middle|\; E = e\right).$$

[21]Let $\widehat{\mathrm{V}}_h$ be the cluster-robust estimator of the asymptotic variance-covariance matrix of the vector of

more plausible if splitting and non-splitting districts in the same region experienced similar GDP growth prior to the split. In the next section we test the hypothesis of common pretrends: $\beta_h = 0$ for $h < -1$.

Our estimation strategy has two advantages over the dynamic panel models commonly employed in the empirical growth literature. First, our approach is robust to treatment-effect heterogeneity across districts, whereas dynamic panel models assume homogeneous effects. Second, while dynamic panel models impose a specific functional form for dynamic treatment effects, our model makes no assumptions about dynamics.[22]

5 Results

5.1 Impact of Fragmentation on Growth

Figure 2 shows that district splitting reduces district GDP, and the effect becomes more pronounced over time. This is noteworthy because central transfers increase within the original district boundaries after a split, which should positively impact GDP through increased government spending. The figure shows that general grant revenue eventually rises by 10 percent of pre-split GDP three years after the split. This increase in transfers resulted in a commensurate increase in district expenditure, as we show in Section 6.1 ahead. We multiply this grant increase by the lowest estimate and a standard estimate of the local fiscal multipliers found in the literature (0.6 and 1.8) to generate predicted GDP responses to splitting.[23]

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estimators $\{\hat{\beta}_{e,h}\}_{e\in\mathscr{E}}$. Our estimator for the asymptotic variance of $\hat{\beta}_h$ is $\boldsymbol{\omega}'\widehat{\mathrm{V}}_h\boldsymbol{\omega}$, where $\boldsymbol{\omega}$ is the vector of cohort shares $\{\omega_e\}_{e\in\mathscr{E}}$.

[22]For example, consider the equation $y_{d,t} = \rho y_{d,t-1} + \beta D_{d,t} + \alpha_d + \gamma_t + \varepsilon_{d,t}$, where $y_{d,t}$ is log GDP, $D_{d,t}$ is a "post split" dummy, and $\rho \in (0,1)$. Iterating, we have $y_{d,t} = \rho^{h+1} y_{d,t-h-1} + \sum_{k=0}^{h} \rho^k \big(\beta D_{d,t-k} + \alpha_d + \gamma_{t-k} + \varepsilon_{d,t-k}\big)$. Now this equation, combined with a sequential exogeneity assumption, implies that the causal effect of splitting on (log) GDP h years later is $\beta \sum_{k=0}^{h} \rho^k$. Hence, the treatment effect is monotonic in h and approaches $\beta/(1-\rho)$ as $h \to \infty$.

[23]Chodorow-Reich (2019) reviews the literature on local multipliers and finds a cross-study mean of 1.8.

If district splits only affected GDP through the fiscal multiplier channel, then GDP should have increased by 6 to 18 percent four years after the split. However, in reality, GDP begins to decline two years after the split and eventually falls by 7 percent after four years. This implies that splits adversely impact economic activity through non-fiscal channels. The general grant and GDP both exhibit parallel trends for splitting and non-splitting districts prior to the split.

To summarize the impact of splitting in a single number, we estimate the cumulative effects on GDP. The first row of estimates in Table 1 reports the cumulative impacts from the year of the split to four years after the split, $\sum_{h=0}^{4} \hat{\beta}_h$. Districts experience a mechanical increase in general grant revenue as a share of pre-split GDP adding up to 29.0 percent (S.E. = 2.3 percent) at the end of four years after the split (column 1). This implies a cumulative GDP increase of around 17 percent for a multiplier of 0.6 (column 2) and an increase of over 50 percent for a multiplier of 1.8 (column 3). In contrast, actual GDP experiences a cumulative decline of 18.5 percent (S.E. = 7.0 percent) (column 4). While the point estimate is substantial, the degree of uncertainty surrounding the estimate is not trivial; the 95 percent confidence interval includes cumulative declines as small as 5 percent and as large as 32 percent. Nevertheless, we can confidently reject positive impacts.

Given that the increase in the general grant should have resulted in positive growth, it is unlikely that the adverse impact on growth occurred through fiscal channels alone. Evidence on local fiscal multipliers in developing countries is scarce, but the multiplier is never negative. At the national level, Kraay (2012) and Kraay (2014) estimate a fiscal multiplier of around 0.4 in developing countries in the short-term in response to a "windfall" increase in expenditure similar to these central government transfers.[24] Subnational fiscal multipliers are typically higher than this in developed countries (Chodorow-Reich, 2019). For example, Serrato and Wingender (2016) estimate a local income multiplier of 1.7 to 2 in the United States. Estimates vary widely in developing country contexts. On the high end, Corbi, Papaioannou and Surico (2019) find that grants to Brazilian municipalities have a local income multiplier of around 2. On the low end, Guo, Liu and Ma (2016) estimate a multiplier of 0.6 in China.

[24]Crucially, the increase in spending that results from these transfers do not signal an increase in future taxes, which could lead to an increase in GDP through wealth effects.

Even with the most conservative estimate of the fiscal multiplier at 0.6, the increase in the general grant should have significantly boosted GDP. However, our findings show that splits have a significant and negative effect on growth. This suggests that other factors may be contributing to the negative impact on growth, such as bureaucratic capacity and economies of scale in the provision of public goods. We explore mechanisms in Section 6.

5.2 Robustness Checks

Baseline imbalance. A potential threat to our strategy is that several baseline covariates are unbalanced for splitting and non-splitting districts (Appendix Table B.2). Covariate imbalance could lead to a violation of the conditional parallel trends assumption if these covariates are related to trends in potential outcomes. For instance, ethnic fractionalization and urbanization have both been linked to long-run economic growth, and both exhibit imbalance in our sample. As a first robustness check, Panel A of Appendix Table B.5 reports estimates controlling for baseline ethnic fractionalization, urbanization, age structure, and education in Equation (1).[25] The estimated decline in GDP due to splitting falls somewhat to 17.0 percent (S.E. = 7.6 percent). As a second robustness check, Panel B presents estimates based on the trimmed sample of districts with an urbanization rate that lies within the common support (0 to 71 percent). The estimate is virtually unchanged, showing a GDP decline of 18.9 percent (S.E. = 8.0 percent). Finally, Panel C presents estimates that use the trimmed sample and control for the aforementioned covariates. The estimate becomes larger in absolute value, indicating a GDP decline of 20.3 percent (7.9 percent). The conclusion that district splits reduce growth is quite robust to adjusting for covariate imbalance or imposing common support in the urbanization rate. (See Appendix Figure B.2 for horizon-specific estimates.)

Alternative measures of economic activity. It is unusual to find a measure of GDP at the level of granularity available in Indonesia, especially over such a long period. As a result, much of the literature looking at sub-national growth in developing countries has tended to rely on

[25]Note that the model allows the impact of baseline covariates to vary by year and treatment cohort, but it does impose that the conditional means of potential outcomes are linear in the covariates (Wooldridge, 2021).

alternative measures like household expenditure and nighttime luminosity. We investigate the impact of splits on these measures of economic activity in detail in Appendix B and find that they corroborate the impact on GDP. Splits result in a decline in household expenditure that is similar in magnitude to the decline in GDP (Appendix Figure B.3). Nighttime luminosity also falls after a split, though the estimates are imprecise (Appendix Figure B.3). We are reassured that our findings are not driven by measurement error in district GDP.

5.3 Early vs. Late Splits

The baseline estimates are a weighted average of cohort-specific treatment effects, which could in principle vary across cohort. To examine this possibility while maintaining reasonable sample sizes, we estimate separate effects for early (2002–2004) and late (2008–2009) splits, which are reported in Table 1.

Early and late splits could have different impacts on growth for a few reasons. First, in the early period district officials and voters had less experience with decentralized governance. All district governments, whether or not they split, had to contend with new expenditure and administration responsibilities, yet initially there was confusion over which level of government was responsible for specific functions. In this context, it would take time to establish chains of accountability between civil society, bureaucrats, and elected officials. Without these systems in place, the beneficial effects of district proliferation through increased accountability may have been impeded.

Second, because direct mayoral elections were rolled out over 2005–2008, all early splits occurred under indirectly elected mayors, and all late splits occurred under directly elected mayors. Consequently, any increase in yardstick competition due to splitting should be stronger for late splits. Late splits should therefore have more beneficial (or less harmful) effects than early splits, if yardstick competition is an important mechanism.

Finally, the central government tightened the regulations on splitting after the first moratorium, increasing the minimum number of subdistricts that each parent and child district needed to have. Late splits may have resulted in less geographically fragmented service provision as a result.

Despite these differences, there are important shared features between early and late splits that could result in similar impacts. For instance, the challenge of recruiting competent civil servants to staff the newly formed districts was equally significant for both early and late splitters. (We discuss government personnel in Section 6.2 ahead.) If the fall in GDP is primarily driven by a deterioration in bureaucratic capacity, then early and late splits should have similar impacts.

Surprisingly, we find that late splits actually caused a larger GDP decline than early splits, though the difference between the two estimates is not statistically significant (Table 1). Appendix Figure B.5 plots the dynamic effects of early and late splits. Due to the timing of these splits, we are able to estimate the impact of early splits on growth over a longer time horizon, and we can test for common trends for late splitters over a longer pre-period. The GDP dynamics in the first five years following a split are similar for early and late splitters. (For early splitters, GDP continues to fall in years five and six following the split, after which time the estimates become quite imprecise.) Late splitters do exhibit a downward—albeit imprecisely estimated—pretrend relative to non-splitters; however, the differential pretrend is absent when we condition on baseline district covariates (Appendix Figure B.6).

Both early and late splits appear to reduce GDP, but the relative magnitudes vary depending on the covariates and sample that we use, as shown in Appendix Table B.5. In the trimmed sample (Panel B), the estimates for early and late splits are quite similar. However, in the full sample with added controls (Panel A), late splits have a more pronounced negative impact on GDP, while early splits show a stronger negative effect in the trimmed sample with additional controls (Panel C). This sensitivity could be attributed to the smaller sample sizes for each subgroup of splitting districts. As a consequence, we cannot draw firm conclusions about the nature of treatment effect heterogeneity across splitting cohorts. (See Appendix Figures B.6, B.7, and B.8 for the horizon-specific estimates.)

5.4 Spillover Effects and Aggregate Growth Impact

To what extent are the results informative for the aggregate effects of district splitting? The baseline estimates could underestimate the aggregate impacts if district splits have negative spillover effects on nearby districts. Alternatively, the estimates could overstate the negative effects of splitting if splits cause firms to simply relocate their activity from splitting districts to non-splitting districts, leading to positive spillover effects.

One way to evaluate spillover effects is to estimate the impact of fragmentation at increasingly higher levels of geographic aggregation. We do this by estimating the equation

$$\frac{Y_{j,t+h} - Y_{j,t-1}}{Y_{j,t-1}} = \beta_h (\ln N_{j,t} - \ln N_{j,t-1}) + \lambda_{r(j),t,h} + \varepsilon_{j,t,h}, \tag{3}$$

where $N_{j,t}$ is the number of districts contained in geographic unit j in year t, and the $\lambda_{r(j),t,h}$ are region-by-year effects. The geographic units considered are original district (2000 borders), the intersection of province and island, and province. The advantage of considering province-by-island units is that they are, by construction, more numerous than provinces, increasing statistical precision. Furthermore, spillovers across islands may be limited.

The functional form of Equation (3) assumes constant effects of proportionate changes in the number of districts. For example, increasing the number of districts from one to two is assumed to have the same impact on growth as increasing the number of districts from two to four. Hatfield and Kosec (2013) make the same functional form assumption in studying the effect of the number of county governments on local income growth in the United States. It is unfortunately necessary to make parametric assumptions in order to compare effects at different levels of aggregation. The effect of doubling the number of districts in year t on the outcome in year $t+h$ is $\ln(2) \cdot \beta_h$.

Table 2 reports the cumulative effect of doubling the number of districts, $\ln(2) \cdot \sum_{h=0}^{4} \beta_h$.[26] Doubling the number of districts within the original 2000 borders leads to a cumulative decline in GDP by 13.4 percent (S.E. = 7.1 percent). This is somewhat smaller than the weighted CATT estimate of 18.5 percent. One potential reason is the functional form assumption, as the CATT estimator is nonparametric. Another potential explanation is that the first split slightly more than doubles the number of districts within the original borders, yielding an

[26] Appendix Figure B.9 plots the dynamic effects, and Appendix Table B.6 presents analogous results for nighttime luminosity.

average of 2.28 districts.[27] Scaling the estimate by ln(2.28) rather than ln(2) produces a cumulative decline of 15.9 percent, which is closer to the CATT estimate. The impact of doubling the number of districts at the province-by-island level is larger, showing a cumulative GDP decline of 17.7 percent (S.E. = 9.4 percent). The province-level estimate is larger still, showing a decline of 41.9 percent (S.E. = 21.7 percent).

The larger negative effects at higher levels of geographic aggregation suggest that splits have negative spillover effects. However, this conclusion is only tentative, as the estimates in Table 2 are imprecise—especially those at the province level. We can, however, rule out the possibility that splits simply result in a reshuffling of economic activity across space without having an aggregate impact.

We next investigate channels through which fragmentation might have had this negative growth impact. We find little evidence that the increase in number of jurisdictions led to productive competition among districts to increase accountability, reduce corruption, or improve the business environment in other ways. Instead, we find that the lower quality of bureaucrats staffing the new district contributed to the negative effects of fragmentation. We also find some evidence of economies of scale in the provision of public goods. Following a split, district expenditure skews heavily towards administrative expenditure due to the requirements of setting up a new government. Despite the large increase in transfers, productivity-enhancing infrastructure does not improve.

6 Mechanisms

In this section, we discuss evidence for key mechanisms through which fragmentation could theoretically impact economic growth. In Appendix B, we use machine learning methods to further explore sources of treatment effect heterogeneity that could shed light on mechanisms.

[27] Of the 98 districts that split for the first time in our sample period (excluding splits between 2012 and 2014), 76 divided into two districts, 17 divided into three districts, and five divided into four districts.

6.1 Economies of Scale

The creation of new governments entails greater spending on government administration, especially in the short-run, as buildings are constructed and personnel hired. Large fixed costs of government administration, in turn, imply economies of scale in the overall provision of public services. This is true even if changing the size of districts does not change the scale of public service providers, such as schools and health clinics. In contrast to the bureaucratic capacity channel, economies of scale is relevant for both parent and child districts. We find that this loss of benefits from scale can be substantial.

Figure 3 shows that total expenditure steadily increases after a split, rising by 16 percent of pre-split GDP four years later. For context, district spending was about 12 percent of GDP in 2001. The figure also shows the dynamic impact of splits on the district expenditure shares in five functional categories: administration, human capital, physical capital, economic development, and social services. The share of expenditure devoted to administration sharply increases in the year of the split and remains at an elevated level four years later. The average increase over four years is 3.8 percentage points, which is an 12 percent increase relative to the baseline share of 32.0 percent (Appendix Table B.7). This is achieved through a decrease in the share of spending devoted to human capital (education and health).

Some of these effects may change in the long run. For example, the share of expenditure on investment in physical capital initially falls but then starts to increase two years after the split. Overall, though, the spending composition results suggest a shift in spending toward public administration and away from productivity-enhancing areas.

Note that we cannot separate wages and salaries from other administrative expenditures. Part of the increase in administrative expenses could reflect rent-seeking and patronage through civil service employment. Since half of the general grant allocation is used to finance wages, the central government regulates recruitment and employment contracts (Shah et al., 2012), making this less likely. Nevertheless, informal payments for entry and promotion within the civil service are common (World Bank, 2007). The increase in administrative expenses could therefore also suggest a worsening of accountability.

Despite the increased emphasis on administrative expenditure following splits, public services might still improve thanks to the large increase in fiscal transfers. Figure 4 displays the average effects of splitting on 10 public services in the areas of education, health, public safety, and infrastructure. These data come from the Village Potential Statistics (*Pendataan Potensi Desa*, or PODES). [28] We report separate estimates for original districts, parent districts, and child districts. For each public service, the estimates indicate either no change or a decline following splits; the confidence intervals generally allow us to rule out moderate increases. These results are consistent with recent quasi-experimental evidence showing that decentralization worsens public service delivery in Vietnam (Malesky, Nguyen and Tran, 2014) and India (Chaudhary and Iyer, 2024). However, they contrast with the recent finding that reducing the size of local polities in Uttar Pradesh, India, improves the quality of public services (Narasimhan and Weaver, 2024).

The massive increase in spending induced by the general grant appears to have funded government administration without improving public service delivery. Economies of scale in governance may therefore help to explain the disappointing effects of splitting.

6.2 Bureaucratic Capacity

Another reason why fragmentation might harm GDP is the lack of bureaucratic capacity in newly created districts. There is no regulatory mechanism in place to reallocate civil servants from the parent district to the child district when a split occurs. Instead, civil servants transfer voluntarily, often due to geographical preferences or aspirations for career advancement. Most civil servants prefer to remain in the parent district with the established government, making it difficult for child districts to attract qualified bureaucrats. Civil servants in child districts often have less experience but are promoted prematurely to meet regulatory requirements (BAPPENAS, 2007). Moreover, setting up a new government is a cumbersome process that disrupts the core activities of public administration.

Appendix Table B.8 shows that bureaucracies in child districts are initially understaffed

[28] Appendix Figure B.10 plots the dynamic effects for the PODES outcomes, while Appendix Figure B.11 plots estimates for household outcomes taken from the National Socioeconomic Survey (*Survei Sosial Ekonomi Nasional*, or SUSENAS).

compared to parent districts, although they catch up 2–3 years after the split (Panel A). However, there are persistent differences in education and experience. Civil servants in child districts are less likely to hold a bachelor's degree than their counterparts in parent districts, potentially reflecting the challenge of recruiting high-quality candidates.[29] Child districts also tend to employ younger civil servants, which suggests they have less on-the-job experience (Panels B and C).

Given these problems—and the importance of bureaucracy for development (Besley, Burgess, Khan and Xu, 2022)—we expect the negative growth effects of splitting to be more pronounced for child districts. One challenge is to impute the increase in the general grant in parent and child districts separately due to the split, since the grant goes to the combined district prior to the split. We make the conservative assumption that the original district government would have spread expenditure across the district in proportion to the local population. A plausible alternative is that per capita expenditure before the split was higher in what becomes the parent district, as unequal treatment across regions may have motivated the split. In this case, we would be overestimating the increase in expenditure in parent districts and underestimating the increase in child districts.

Table 3 reports the cumulative effects of splitting separately for parent and child districts, while Appendix Figure B.12 plots the dynamic effects. The general grant increases in both parent and child districts after the split, but the increase is three times as large in the child district. Nevertheless, parent and child districts experience similar cumulative declines in GDP.[30] Relative to predicted GDP based on fiscal multipliers, the GDP decline is much larger for child districts. For instance, a fiscal multiplier of 1.8 predicts a cumulative GDP increase of 107.0 percent in child districts and 35.4 percent in parent districts.

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[29]By contrast, child districts have more educated mayors than parent districts, on average (Appendix Table B.9). Attracting qualified mayoral candidates in the child district is not a problem, given the prominence of the position.

[30]The sample size is smaller compared to Table 1 because the GDP data are not always backcasted to the year prior to the split for the parent and child districts. The cohort-size weights are adjusted to reflect the relative frequencies of each splitting cohort conditional on having non-missing GDP growth rates.

As a final piece of evidence, we show in Appendix B that the short-run reduction in physical capital expenditure following a split is due to delays in the execution of spending plans, not changes in spending priorities. These delays are concentrated in child districts, suggesting that the inexperience of civil servants in these districts played a role.

Together, the results suggest that bureaucratic capacity is an important mechanism behind the negative growth impacts of splitting. However, bureaucracy cannot explain why GDP also declines in parent districts, so other mechanisms must also play a role.

6.3 Accountability

An often-cited rationale in favor of more localized governance is that it can increase accountability either through competition between jurisdictions or through decreased distance between decision-makers and citizens. Because Indonesian districts mainly control expenditure decisions and not tax policy, we might expect competition for the tax base to matter less in this context. However, districts rely on firms for revenue through licenses and permits administered by the district, and through business tax revenue collected and shared by the central government. We examine the impact of government fragmentation on measures of accountability that are particularly relevant for firms—indirect taxes and fees and informal taxation or "gifts." We then test for downstream impacts on regulated activities, productivity, and competition.

To study firm-level outcomes, we modify Equation (1) by adding industry-by-year effects to control for industry-specific shocks. Let $j(i)$ be the four-digit industry classification of firm i. The regression is

$$Y_{i,d,e+h} - Y_{i,d,e-1} = \alpha_{j(i),e,h} + \beta_{e,h} D_d(e) + \boldsymbol{X}'_d \boldsymbol{\lambda}_{e,h} + \left(D_d(e) \cdot \dot{\boldsymbol{X}}_d(e)\right)' \gamma_{e,h} + \varepsilon_{i,d,e,h}, \quad (4)$$

where Y is a firm outcome measured in either logs or levels. We aggregate the estimates of $\beta_{e,h}$ using Equation (2). To examine competition, we use Equation (4) with outcomes that are measured at the "market" (i.e., industry-by-district) level.

Table 4 shows that increasing the number of local governments does not have a disciplining impact on bribery.[31] In fact, we find some evidence of the opposite—the probability that a manufacturing firm reports a "gift" payment rises by around 3 percentage points following a split. On the other hand, the average bribe rate—gift payments as a share of firm revenue—does not change. By contrast, the probability that the firm pays any indirect taxes or fees does not change, but the average tax rate falls by 0.11 percentage points. The small effect on formal taxes is unsurprising given that most taxes are administered centrally. (Firms do not separately report local and central taxes and fees in the IBS survey).

As Burgess et al. (2012) showed, Indonesia's district proliferation increased corruption in illegal logging, allowing cheaper access to timber, consistent with a model of bribes as a fee for service and district governments as monopolistically competitive entities. When the number of competitors increases, the service price (bribes) falls and the quantity sold increases. Turning to the latter, we find some evidence that exporting and land contracts, which both require licenses, increase following splits (columns 5 and 6 of Table 4).

The evidence presented so far suggests that intergovernmental competition modestly increases service access for firms while increasing the incidence of bribery. Overall, we do not find dramatic improvements in the business environment for manufacturing firms. We provide further evidence on the lack of improvement in government accountability and services using data from the Economic Governance Survey conducted by KPPOD (Regional Autonomy Watch) and the Asia Foundation. The survey was designed to measure how local governance affects the business environment across all parts of Indonesia. It contains a rich array of variables measuring the formal and informal costs of business licensing and corruption, as well as property rights enforcement. Two waves of the survey, in 2007 and 2011, collected data from two essentially non-overlapping sets of districts, which together

[31]Appendix Figure B.13 plots the dynamic effects. There is a sharp drop in 2000 in the probability that the firm pays any gifts and the probability that the firm pays any indirect taxes or fees (Appendix Figure B.14). The drop is not due to changes in the questionnaire or sample but may be linked to nation-wide changes at the time. By contrast, there is no trend break in firm revenue. Because we cannot determine whether the trend breaks in bribes and taxes represent a change in measurement or a change in the real variable, we exclude data on gifts and taxes prior to the year 2000.

represented nearly all districts at the time.

Appendix Figure B.15 summarizes the difference-in-differences estimates for 48 outcome variables from the Economic Governance Survey.[32] Each outcome is standardized to have a mean of zero and a standard deviation of one. The cost and wait variables are also winsorized at 99 percent level to reduce the influence of a few extreme outliers. Most of the estimates are close to zero and statistically insignificant. Six estimates are significant at the 5-percent level, though we would expect two or three estimates to be significant by random chance under the null of no effect, given the large number of outcomes considered. Overall, the results suggest that splitting has a small impact on the business environment. If anything, splitting leads to a reduction in business formality and an increase in the licensing burden. One caveat is that, due to the timing of the surveys, these results represent short-term effects of late splits.

The Economic Governance Survey contains large and small firms, while our manufacturing survey only covers firms with at least 20 employees. In the appendix, we present separate results from the Economic Governance Survey for firms with 20 or more employees (Appendix Figure B.16) and for firms with fewer than 20 employees (Appendix Figure B.17). The results suggest that the licensing burden increased for both large and small firms, but the reduction in formality was concentrated among small firms.

While splits do not generate significant *measured* improvements in the business environment, they could still generate unmeasured improvements, which should increase firm productivity or market competition. However, we find that splits do not significantly affect labor productivity, total factor productivity (TFP), or different measures of market compe-

[32]The regression specification is

$$Y_{f,d,t} = \beta_0 + \beta_1 Splitter_d + \beta_2 Post_t + \beta_3 Splitter_d \times Post_t + \lambda_{r(d),t} + \varepsilon_{f,d,t},$$

where $Y_{f,d,t}$ is a firm outcome, $Splitter_d$ is an indicator variable that equals one for districts that split between 2008 and 2010, $Post_t$ is an indicator variable that equals one if $t = 2011$, and $\lambda_{r(d),t}$ is an region-by-year effect. Districts that never split over the analysis period are used as controls.

tition (Appendix Table B.12).[33] This is not to suggest that political economy factors do not matter for productivity in Indonesia. Abeberese, Barnwal, Chaurey and Mukherjee (2023) find that the introduction of democratically elected district mayors increased the productivity of these same manufacturing firms. District splits apparently do not generate enough intergovernmental competition to produce a similar effect.

7 Discussion

The empirical literature on fragmentation and growth has found divergent results depending on the context. Even limiting our attention to seemingly similar contexts of large, developing economies in recent decades, we find differing experiences. Dahis and Szerman (2023) find that splits of Brazilian municipalities increase nighttime luminosity in child districts and leaves parent districts unaffected. Cohen (2024) finds that local government fragmentation in Uganda resulted in a decrease in nighttime luminosity, particularly in child districts.

Evaluating the source of heterogeneity in results is challenging given the complexity of relevant differences. For example, The empirical strategies that these papers pursue mean that the results are necessarily local to the size of the jurisdictions that split. As Narasimhan and Weaver (2024) show, fixing all other characteristics of administration such as the level of fiscal autonomy, experience of bureaucrats, and economies of scale, population size in the local administrative unit can impact economic outcomes.

In Brazil, the authors attribute the result to the expansion of the public sector, as they find

[33] Appendix Figure B.18 plots the corresponding dynamic effects. We estimate (log) TFP following the approach of Ackerberg, Caves and Frazer (2015), using total intermediate input costs (electricity, fuel, and raw materials) as a proxy for firm productivity. We implement their estimator using the `acfest` package in Stata (Manjón and Mañez, 2016). The firm's capital stock is not reported in IBS in 1996 and 2006, and it is missing for many firms in other years. We linearly interpolate missing capital values using values in adjacent years. Furthermore, we drop a few implausibly large values of capital and drop firms that exhibit large spikes in the capital variable. Specifically, we drop all values for firms that ever experienced a one- or two-year change in capital (in percentage terms) that was below the 1st percentile or above the 99th percentile.

no impacts on the private sector. Since annual GDP data are not available for Brazilian municipalities, it is not possible to directly compare their estimates with ours. This is especially important because one possible explanation for the difference in results is that the increase in transfers due to splitting is larger relative to the local economy in Brazil than in Indonesia—something we cannot evaluate without knowing local GDP. In Uganda, central government transfers to districts do not change very much but there are substantial reductions in foreign aid per capita, particularly in newly created districts, mirroring the decline in growth.

A common theme in all settings seems to be that government capacity matters. New district governments with new officials fare worse. In Indonesia, even parent districts were inexperienced with expenditure responsibilities; prior to decentralization in 2001, administration was heavily centralized. This is unlike both the Brazilian and Ugandan contexts where local governments in the parent district retained an electoral setting and administrative responsibilities that had been in place for many years. Further work in this area could more directly explore the importance of the experience of local government officials, especially in the administrative duties with which they are tasked.

An open question is under what conditions decentralization can improve accountability. In Brazil and Indonesia, there is evidence of greater rent-seeking and little increase in accountability (Lima and Neto, 2018). This is surprising because greater accountability is an often-cited rationale for decentralization—and, as Narasimhan and Weaver (2024) show, reducing the polity size can result in greater accountability through electoral mechanisms. A notable contrast here again is the amount of experience both citizens and elected officials have with existing institutional structures. In the Indian context, a village joins a smaller or larger existing district, leaving electoral structures and administrative responsibilities unchanged.

Our results also appear to be at odds with evidence from the United States and China. Stansel (2005) and Hatfield and Kosec (2013) find that greater local government fragmentation correlates with faster economic growth in the cross section of U.S. metropolitan areas. Their context differs from ours: the United States is a mature democracy with greater decentralization of tax-setting authority. Beyond differences in context, these findings capture longer-term impacts and relate to governments that have existed for a long time. Our estimates, by contrast, capture short-run effects for very young governments. China presents a unique context in which the central government provides explicit incentives for subnational governments to compete with one another and promote growth. In this context, Zhang et al. (2019) find that the fragmentation of municipal governments promotes growth to an extent. Increasing the number of districts from one to two increases growth, but further increases reduce growth on the margin. Similar to the studies on the United States, Zhang et al. (2019) exploit cross-sectional variation and study governments that have existed for a while.

Several factors could contribute to the positive impacts of fragmentation found in the United States and China. First, the negative impact of reduced bureaucratic capacity may not be present, given that the local governments in these studies had existed for a long time. Hatfield and Kosec (2013) note that "out of over 3000 counties in the continental United States, only six new functional counties have formed since 1970, and only six functional county governments have dissolved since 1970." Second, incentives for intergovernmental competition may have been stronger, given differences in the institutional environment. Nonetheless, the non-monotonic effect of fragmentation in Zhang et al. (2019) supports the idea that fragmentation can harm growth due to economies of scale.

8 Conclusion

The fragmentation of districts in Indonesia represents a major exercise in decentralization in a populous country. Exploiting idiosyncratic variation in the timing of district splits, we find that fragmentation reduces GDP in the short run. The results are robust to flexibly controlling for region effects and baseline levels of ethnic fractionalization, urbanization, age structure, and education. Bureaucratic capacity and economies of scale are important mechanisms that explain the negative impact of fragmentation. While splitting could increase accountability via stiffer intergovernmental competition, this benefit does not materialize in practice—at least in the short run.

An open question is how fragmentation will impact long-run growth. Because the district

GDP series ends in 2013, we focus on the GDP impact up to four years after a split. For early splitters, we show that GDP continues to fall in years five and six. Beyond that, the estimates are too imprecise to draw firm conclusions. Given that bureaucratic capacity appears to be an important mechanism, fragmentation should be less harmful for GDP in the long run as the new civil servants gain experience and recruiting improves as the government becomes more established. The long-run effects should also depend on the extent to which the increase in administrative expenditure reflects one-time spending on new government infrastructure rather than permanent increases in government payroll.

Government fragmentation remains an active process throughout the world.[34] Future research should examine the long-run effects of fragmentation in other settings. An important question is how to limit the disruption caused by the creation of new local governments, so that localities can better enjoy the potential benefits of decentralization.

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9 Tables

Table 1: Government Fragmentation and Economic Growth

	General Grant	Predicted GDP		Actual GDP
	(1)	(2) Multiplier = 0.6	(3) Multiplier = 1.8	(4)
Cumulative Effect of Split:				
All Splits	0.290^{***}	0.174^{***}	0.523^{***}	-0.185^{***}
	(0.023)	(0.014)	(0.041)	(0.070)
Early Splits	0.246^{***}	0.148^{***}	0.444^{***}	-0.130
	(0.020)	(0.012)	(0.035)	(0.086)
Late Splits	0.412^{***}	0.247^{***}	0.742^{***}	-0.336^{***}
	(0.066)	(0.040)	(0.120)	(0.116)
p-value, H_0: Early = Late	0.017	0.017	0.017	0.157
Observations	1,263	1,263	1,263	1,263
Districts	331	331	331	331

Notes: The first row reports estimates of the cumulative effect of the first district split, $\sum_{h=0}^{4} \beta_h$, based on the cohort-size-weighted CATT estimator (Equation (2)). The second and third rows report separate cohort-size-weighted CATT estimates for early (2002–2004) and late (2008–2009) splits. The outcome is measured as the cumulative growth from the year prior to the split to four years after the split, relative to GDP in the year prior to the split. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Government Fragmentation and Economic Growth at Different Levels of Aggregation

	Level of Aggregation		
	(1) Original District	(2) Province by Island	(3) Province
Cumulative Effect of Doubling Number of Districts	-0.134^{*} (0.071)	-0.177^{*} (0.094)	-0.419^{*} (0.217)
Observations	2,979	504	297
Clusters	331	56	33

Notes: This table reports estimates of the cumulative effect of doubling the number of districts, $\ln(2) \cdot \sum_{h=0}^{4} \beta_h$, obtained by replacing the outcome in Equation (3) with $\sum_{h=0}^{4}(Y_{d,t+h} - Y_{d,t-1})/Y_{d,t-1}$ and scaling the coefficient by ln(2). The model is estimated at three different levels of aggregation, as indicated. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by geographic unit. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Government Fragmentation and Economic Growth: Parent vs. Child Districts

	General Grant	Predicted GDP		Actual GDP
	(1)	(2) Multiplier = 0.6	(3) Multiplier = 1.8	(4)
	Panel A: Parent Districts			
Cumulative Effect of Split	0.196^{***}	0.118^{***}	0.354^{***}	-0.205^{**}
	(0.021)	(0.013)	(0.038)	(0.085)
Observations	1,258	1,258	1,258	1,258
Districts	326	326	326	326
	Panel B: Child Districts			
Cumulative Effect of Split	0.594^{***}	0.357^{***}	1.070^{***}	-0.138^{*}
	(0.049)	(0.029)	(0.088)	(0.074)
Observations	1,258	1,258	1,258	1,258
Districts	326	326	326	326

Notes: This table reports estimates of the cumulative effect of the first district split, $\sum_{h=0}^{4}\beta_h$, based on the cohort-size-weighted CATT estimator (Equation (2)). The outcome is measured as the cumulative growth from the year prior to the split to four years after the split, relative to GDP in the year prior to the split, $\sum_{h=0}^{4}(Y_{d,e+h} - Y_{d,e-1})/Y_{d,e-1}$ Separate estimates are reported for parent and child districts. The sample is restricted to districts for which the GDP data is reliably backcasted for parent and child districts at least one year prior to the split. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Impact of Fragmentation on Gifts, Taxes, and Regulated Activities

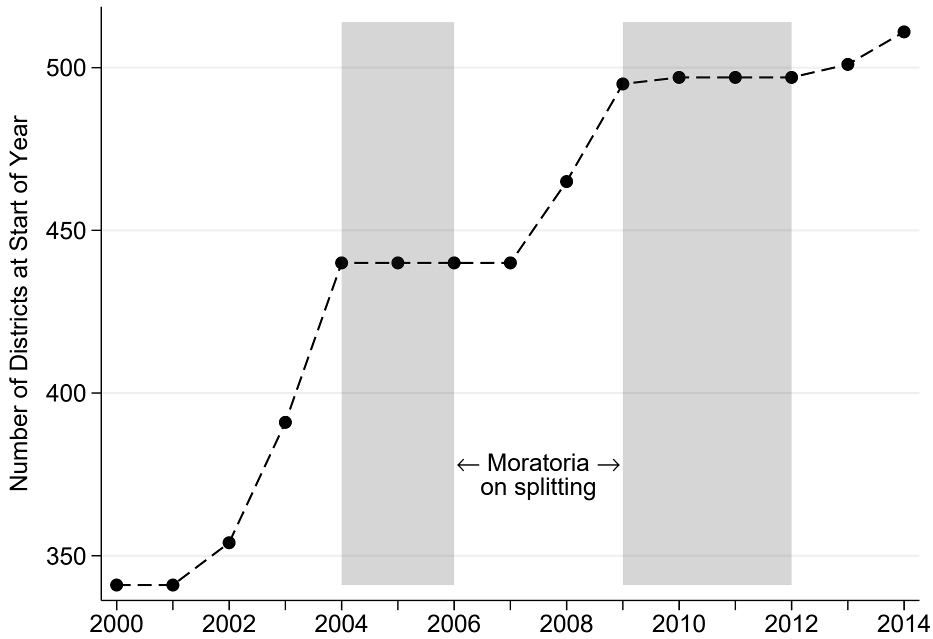
	Gifts		Indirect Taxes and Fees		Regulated Activities	
	(1)	(2)	(3)	(4)	(5)	(6)
	Paid Any	% Revenue	Paid Any	% Revenue	Any Exports	Any Land Contracts
Avg. Effect of Split	0.032^{***}	-0.005	0.001	-0.112^{**}	0.018^{*}	0.010^{*}
	(0.010)	(0.016)	(0.010)	(0.057)	(0.010)	(0.006)
Baseline Mean	0.623	0.303	0.720	0.974	0.181	0.056
Observations	76,324	75,774	76,756	76,151	54,064	76,324
Districts	307	307	308	308	294	307

Notes: This table reports estimates of the average effect of the first district split, based on the cohort-size-weighted CATT estimator (Equations (4) and (2)). The estimates are based on plant-level data. The outcomes are measured as the average change over the first five years following the split relative to the year prior to the split, $(1/6)\sum_{h=0}^{5}(Y_{i,d,e+h} - Y_{i,d,e-1})$. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

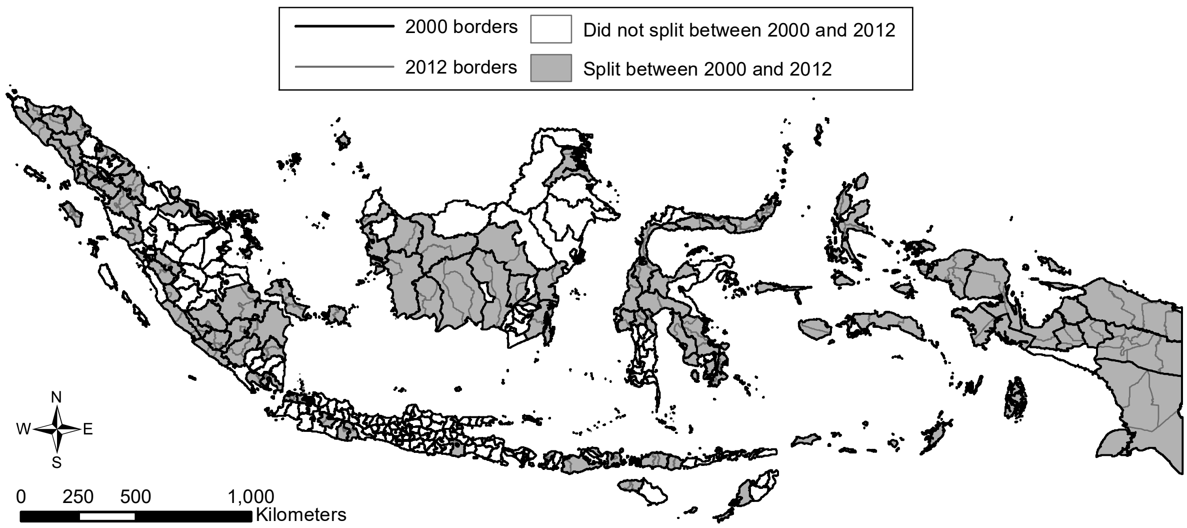
10 Figures

Figure 1: Indonesia's District Proliferation Across Time and Space

(a) Two Moratoria Generated Idiosyncratic Variation in the Timing of District Creation

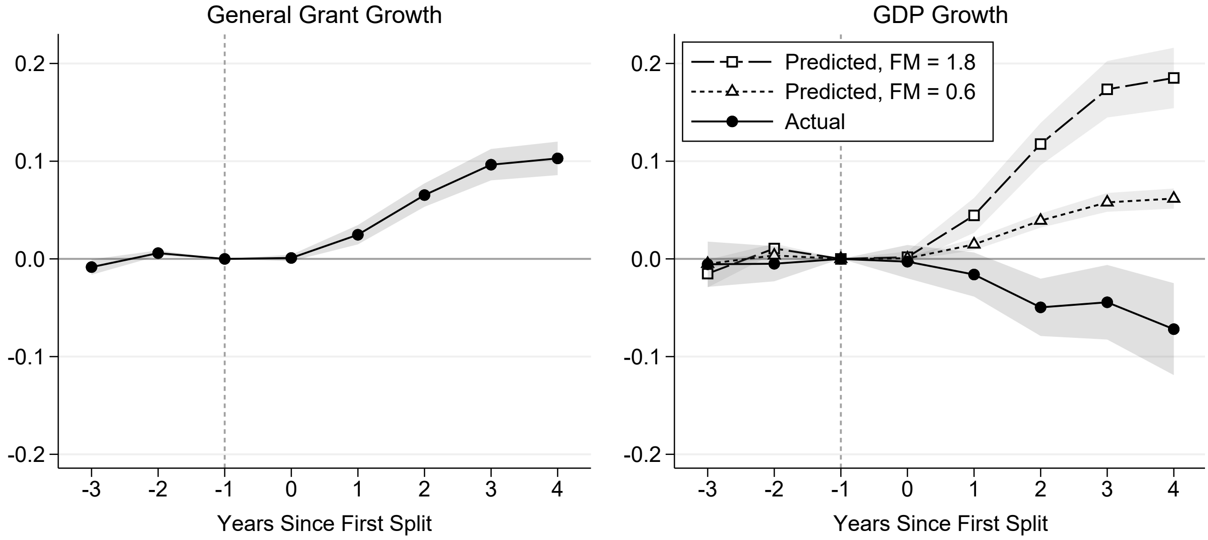


(b) District Splitting Was Geographically Widespread



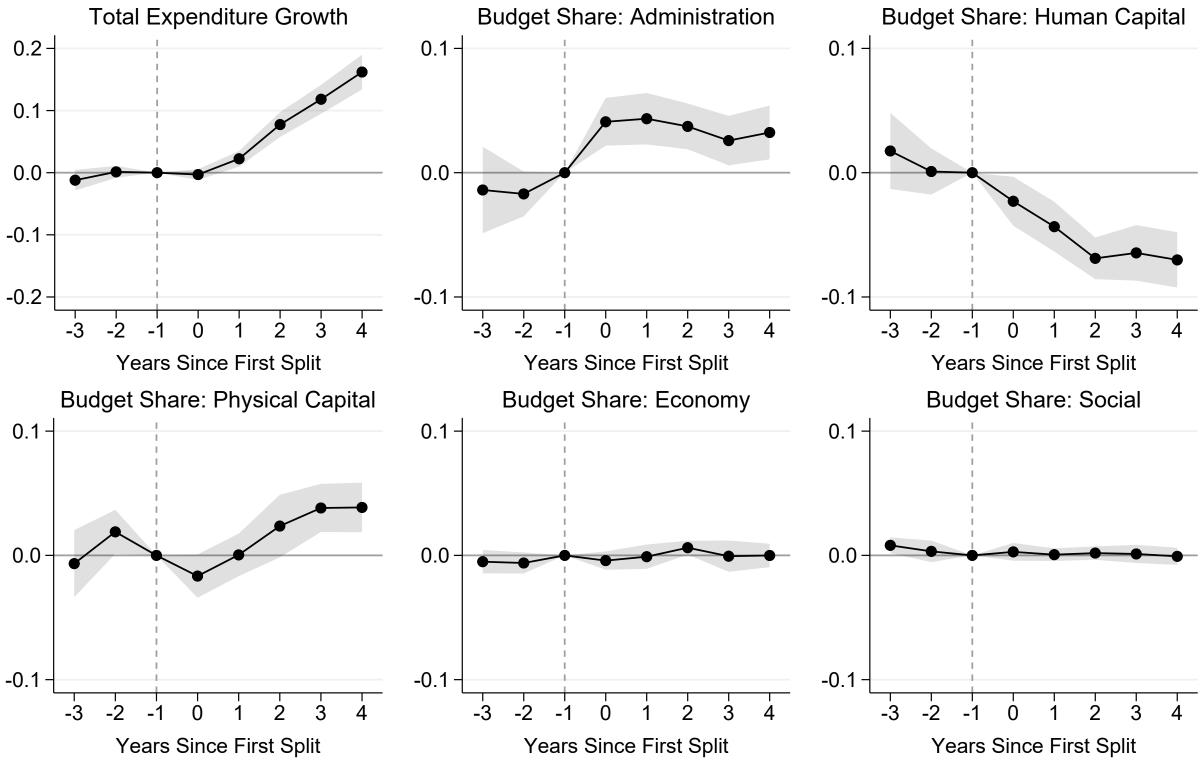
Notes: Panel (a) plots the number of districts at the start of the year. The two flat portions of the graph are due to moratoria on district creation imposed by the central government during the periods 2004–2006 and 2009–2012. Panel (b) displays district borders in 2000 and 2012 based on the 2012 district shapefile provided by the Central Bureau of Statistics and the district crosswalk provided by the World Bank's Indonesia Database for Policy and Economic Research (INDO-DAPOER).

Figure 2: The Effect of District Splits on General Grant and GDP



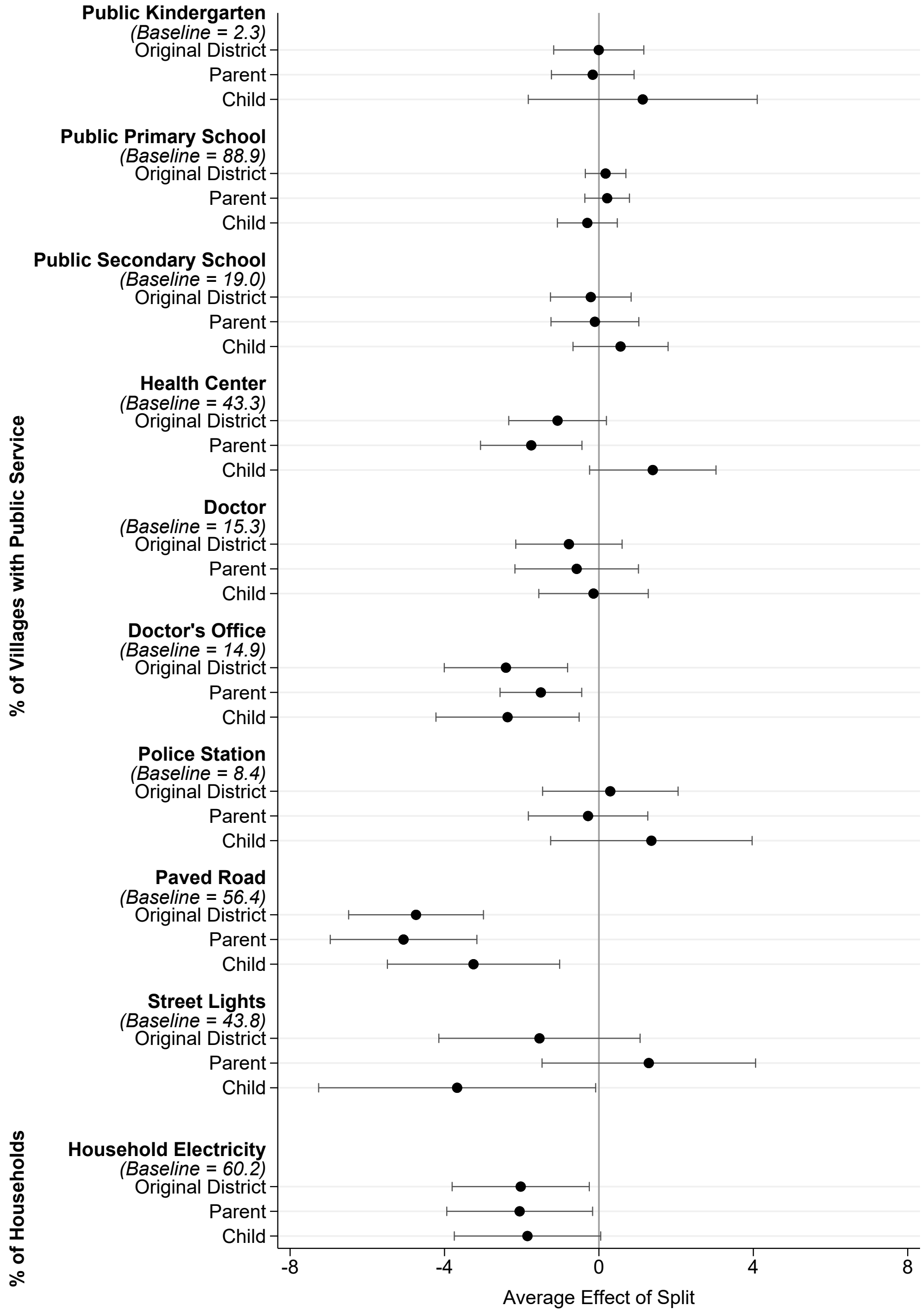
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. (The estimates for $h = -3$ omit the 2002 cohort and adjust the weights accordingly, because the data start in 2000.) The left panel shows the impact of the first district split on growth in general grant revenue relative to the year before the split, scaled by GDP in that year. The right panel shows the impact on GDP growth relative to year before the split as predicted by fiscal multiplier values of 0.6 and 1.8 given the one-for-one increase in expenditure due to the increase in general grants. It also plots the impact on actual GDP growth. The confidence intervals are robust to heteroskedasticity and clustering by district.

Figure 3: The Effect of District Splits on District Expenditure



Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. Total expenditure growth is measured as the change in total expenditure from year $t-1$ to year $t+h$ relative to district GDP in year $t-1$. The remaining outcomes are measured as the change in the expenditure share from year $t-1$ to year $t+h$. The estimate for total expenditure at $h=-3$ is based on the 2003, 2004, 2008, and 2009 cohorts, because the data start in 2000. The estimates for $h \geq -2$ use all cohorts. Because the functional expenditures are only available for 2001–2012, the estimates for the budget share outcomes at $h=-3$ are based on the 2004, 2008, and 2009 cohorts; the estimates at $h=-2$ use the 2003, 2004, 2008, and 2009 cohorts; the estimates at $h \in \{0,1,2,3\}$ use all cohorts; and the estimates at $h=4$ use the 2002, 2003, 2004, and 2008 cohorts. All cohort weights are adjusted for the omission of specific cohorts. The confidence intervals are robust to heteroskedasticity and clustering by district.

Figure 4: The Effect of District Splits on Access to Public Services (PODES)



Notes: This figure plots estimates of the average effect of the first district split and their 95-percent confidence intervals, based on the cohort-size-weighted CATT estimator (Equation (2)). All outcome variables come from PODES. The first nine outcomes are the percentage of villages that have the public service, and the last outcome is the percentage of households with electricity. Each outcome is measured as the average change in the public service from the (three-year) period prior to the split to the two (three-year) periods after the split, $(1/2)\sum_{h=0}^{1}(S_{d,e+h} - S_{d,e-1})$. The estimates use a balanced panel of 329 districts and control for region dummies, ethnic fractionalization, urbanization rate, share of population aged 15–64, share of population with a primary education, and share of population with a secondary education, all measured in 2000. The confidence intervals are robust to heteroskedasticity and clustering by district.

Online Appendix for
"Government Fragmentation and Economic Growth"

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Appendix A Data

District GDP

The Central Bureau of Statistics (*Badan Pusat Statistik*, or BPS) produces estimates of district GDP, which are disseminated by the World Bank's Indonesia Database for Policy and Economic Research (INDO-DAPOER). The GDP series starts in 2000 and ends in 2013 due to a change in BPS's methodology. BPS switched from using the United Nation's System of National Accounts 1993 (SNA 1993) to using SNA 2008. The GDP series based on SNA 2008 is only available for 2010 and beyond. The SNA 1993 GDP series that we use is missing for 12 district-years (one in 2010, two in 2012, and nine in 2013), mostly due to splits in 2013–2014 for which some backcasted GDP is not available. For these 12 cases we impute GDP using the SNA 2008 series growth rates and the SNA 1993 series levels in other years. We aggregate GDP to the level of district borders in 2000, using a crosswalk provided by the World Bank.

In certain analyses, we examine the outcomes of parent and child districts separately. To accomplish this, we need to measure GDP at the level of the newly formed districts prior to their creation. Fortunately, BPS provides retrospective GDP data, derived from census data, survey data, and administrative records. These data are then aggregated across the subdistricts that will form part of the new district. However, around 25 districts in INDO-DAPOER lack this backcasted GDP data, so we supplement it with information from BPS reports. In the end, we managed to obtain backcasted GDP data, at least for the year prior to the split, for 322 out of 331 districts in the analysis sample. In doing so, we always verified that the GDP numbers in the report matched the INDO-DAPOER numbers for years in which the data were non-missing in both sources.

For districts that experienced multiple splits, we aggregate parent and child GDP up to district borders as of the year of the first split, because this is the level at which GDP is usually reported. There are three districts (Aceh Timur, Kepulauan Riau, and Buton) that experienced a second split within two years of the first split and have backcasted GDP data available at the level of district borders established by the second split. For these districts, we aggregate parent and child GDP up to borders as of the year of the second split.

District Population

INDO-DAPOER provides annual data on district population, which are sourced from unpublished data from BPS. We aggregate population to the level of district borders in 2000.

District Budget

The Ministry of Finance (*Kementerian Keuangan*) and INDO-DAPOER provide revenue and expenditure data covering the period from 2001 to 2014. We aggregate these variables to the level of district borders in 2000. When analyzing parent and child districts separately, we apportion general grant revenue in years prior to splits according to average population shares in the district as reported in census and intercensal years, when the population data are most reliable. That is, we assume that prior to splits, fiscal resources were allocated to parent and child districts on an equal per-capita basis.

We also study district spending priorities, measured as the share of different expenditure categories in total expenditure. We aggregate 12 categories of "functional" expenditures, as defined by the World Bank in collaboration with local officials, into five broader categories: human capital, administration, physical capital, economy, and social expenditure. The data on expenditure disaggregated by function cover 2001–2012.

Household Expenditure

In addition to GDP, we also examine two alternative measures of economic activity. The first is household expenditure, which is collected by the National Socioeconomic Survey (*Survei Sosial Ekonomi Nasional*, or SUSENAS) and provided by INDO-DAPOER at the district level. Household expenditure is measured in per capita terms in INDO-DAPOER, so we multiply this variable by district population to obtain total household expenditure. INDO-DAPOER also provides data on household per capita expenditure for the poorest 20 percent of households in the district. The data on household expenditure are available for 2000-2007 and 2009-2014.

Nighttime Luminosity

The second alternative measure of economic activity is nighttime luminosity (Chen and Nordhaus, 2011; Henderson, Storeygard and Weil, 2012). We use the nighttime lights series from the Defense Meteorological Satellite Program's (DMSP) Operational Linescan System, which were processed by scientists at the National Oceanic and Atmospheric Administration's (NOAA) National Geophysical Data Center (NGDC). These data are available annually from 1992 to 2013 at the level of 30 arc-second pixels, which corresponds to 0.86 square kilometers at the equator. Each pixel is assigned an integer ranging from 0 (no light) to 63. We average the values across satellites for years with multiple satellite readings, and then sum the lights values across all pixels within each district.

For our analysis, we use nighttime lights data from 1997 to 2013. We use 1997 as the starting year in order to estimate pretrends over a longer time frame than what the GDP data allows. We omit earlier years because in 1995 and 1996 only one satellite is available. This results in noisier lights measurements and more unstable pretrend estimates, which are still centered around zero. From 1997 to 2007, measurements from two satellites are consistently available.

The DMSP nighttime lights suffer from several limitations, including lack of calibration, blurring, and topcoding (Elvidge, Baugh, Zhizhin and Hsu, 2013; Abrahams, Oram and

Lozano-Gracia, 2018; Bluhm and Krause, 2022). (Unfortunately, the superior Visible Infrared Imaging Radiometer Suite (VIIRS) series is not an option here as it begins in 2012.) The lack of calibration stems from undocumented changes in sensor amplification that lead to differences in measurements across years and satellites that are not related to human activity. While it is not possible to fully correct for this problem, we control for region-by-year effects, which absorbs some of the noise attributable to lack of calibration.

To address blurring, we use the algorithm developed by Abrahams et al. (2018). The algorithm detects and corrects illuminated pixels that do not actually contain a light source by redistributing the light to the pixel that does contain the source. As a result, the deblurred lights distribution includes a greater number of values that are extremely low or high (Appendix Figure B.4). In addition, the deblurring algorithm indirectly addresses the small number of topcoded observations in our sample. Jakarta contains the vast majority of topcoded observations, but this province is excluded from the analysis because its districts are managed by the provincial government. Outside of Jakarta, only 0.2 percent of village-years in the raw lights data are topcoded.

Bureaucratic Capacity

We measure bureaucratic capacity using data on civil servants from multiple sources. The National Civil Service Agency (*Badan Kepegawaian Nasional*, or BKN) provides data on the number of civil servants in each district from 2005 to 2015. These data were compiled by the World Bank and used in Pierskalla, Lauretig, Rosenberg and Sacks (2021).[A.1]

We also use data on education and age from the 2005 Intercensal Population Survey (*Survei Penduduk Antar Sensus*, or SUPAS). These data consist of a 0.51-percent random sample provided by IPUMS International (Minnesota Population Center, 2020). We use SUPAS because the census lacks industry and occupation codes that are detailed enough to identify civil servants. Our sample consists of all individuals with an industry code (`IND`) of 751 ("government administration") in SUPAS 2005. While these data include bureaucrats from all levels of government, they mostly correspond to district bureaucrats, as 70 percent of civil servants work for a district government (BAPPENAS, 2007).

In addition, we have data on education and age for the period 2007–2015 from the Indonesian National Labor Force Surveys (*Survei Angkatan Kerja Nasional*, or SAKERNAS), except for 2010 because district identifiers are not available for that year. Because new district codes are often introduced with lag in these surveys, we restrict the sample to district-years in which both the parent and the child appear in the sample and the child district has existed for at least three years. The SAKERNAS sample starts in 2007 because that is the first year the surveys are representative at the district level. As with IPUMS, we restrict our sample to households with an industry code of 751 ("government administration").

[A.1] We are grateful to Jan Pierskalla for sharing these data with us.

Because new district codes are introduced with a 2-3 year lag in these surveys, we first observe the earliest districts that split only 3 years after the split. The sample size and frequency of the surveys vary across years. We use the August round of the survey for all years because it was conducted every year and contains the largest sample. In 2006 the sample size was about 70,000 households, and it was increased to about 200,000 households starting in 2007.

We augment the data on civil servants with information on the education level of the first democratically appointed mayor from Mukherjee (2023).[A.2]

Baseline Covariates

Data on baseline urbanization, age structure, and education come from the 2000 Population Census (*Sensus Penduduk*), and data on ethnicity come from the 2010 Population Census. Both datasets consist of a 10-percent random sample provided by IPUMS. We calculate ethnic fractionalization at the level of 2000 district borders using the ethnicity data. We use the 2010 data for ethnicity because ethnic fractionalization cannot be calculated for three districts using the 2000 data. (The data are missing for Pidie, and Aceh Utara and Bireuen are combined in the 2000 data.) This decision is immaterial, as the correlation between ethnic fractionalization in the two census years is 0.993.

We examine heterogeneity in treatment effects by the level of ethnic polarization at the original district borders in 2000 using data from (Bazzi and Gudgeon, 2021).[A.3]

Public Service Delivery

Data on local public services come from the Village Potential Statistics (*Pendataan Potensi Desa*, or PODES) village censuses of 1996, 1999, 2002, 2005, 2008, 2011, and 2014.[A.4] PODES covers the universe of villages in Indonesia. Many villages split into multiple villages during the study period, leading to an increase in the number of villages from about 66,000 in 1996 to around 82,000 in 2014. In splitting districts, village splits were more common, with 19 percent of villages splitting, compared to only 6 percent in non-splitting districts. We aggregate outcomes to the level of village borders in 1996, resulting in a balanced panel of around 63,000 villages. The number of villages falls to around 55,000 after dropping a small number of amalgamating villages, all villages in Jakarta, and villages with data that appear to be unreliable due to either misreporting or an incorrect merge. See Cassidy (2023) for more details on the construction of the public goods dataset. We use data from 1996 to aid in

[A.2] We are grateful to Priya Mukherjee for sharing these data with us.

[A.3] We are grateful to Sam Bazzi for sharing this data with us, including for an expanded set of districts compared to their original paper.

[A.4] The 1999 and 2002 waves are titled PODES 2000 and PODES 2003, but they were enumerated in September–October of 1999 and August of 2002, respectively. Subsequent PODES waves were enumerated in April or May of the year in the title. We code the year of each observation using the enumeration year.

the estimation of pretrends for early splitting districts, but the results are similar when we exclude 1996 and aggregate to the level of village borders in 2000.

In all, we use 10 public service variables that are available across all waves of PODES. For many of the public services, PODES reports the presence of the service in the village but not the quantity in some or all years of the analysis period. We therefore define all outcomes as the percentage of villages with the service to remain consistent across outcomes. The only exception is household electricity, which is defined as the percentage of households in the district with electricity.

We supplement these data with three public service outcomes collected by SUSENAS and provided by INDO-DAPOER. We focus on the PODES data for two reasons. First, PODES provides a wider array of measures of public service access. Second, the PODES variables are available for a balanced panel of 329 districts, while the SUSENAS variables are only available for an unbalanced panel of 318 districts, 251 of which have non-missing data in all years.

Firm Outcomes

We analyze firm-level outcomes using data from the Indonesian manufacturing survey of large and medium-sized firms (*Survei Industri Besar/Sedang*, or IBS), which covers the universe of manufacturing establishments with at least 20 workers. We combine the surveys to construct an unbalanced panel of establishments over the period 1995 to 2014. Establishments in our sample are observed for an average of 9.2 years.

IBS contains information on outcomes such as the total value of production, number of employees, and industry of operation. Establishments also report their total indirect tax payments, which combines taxes and fees administered by districts (establishment license fees, building and land taxes, compulsory donations to finance local infrastructure) and taxes administered by the central government (sales taxes, import duties, excise taxes). Payroll taxes, corporate income taxes, and personal income taxes are excluded. Another outcome of interest is "gifts" paid by the firm to external parties. We interpret this variable as including bribe payments to officials, following Henderson and Kuncoro (2006, 2011). The use of the term "gifts" is common in surveys, such as the World Bank enterprise surveys, to elicit truthful information on informal payments. We therefore interpret this variable as referring to gifts that are part of the cost of doing business.[A.5]

To determine the treatment status of each establishment, we use their earliest recorded location since 2000. Establishments that were observed in 2000 are assigned to their recorded

[A.5] In our sample two thirds of firm-years feature positive "gift" payments (Appendix Table B.3), which is higher than the probability of any gift payments by companies in Indonesia as reported in the World Bank enterprise surveys in 2015 (30 percent) but lower than the probability of any bribe payment in Vietnam as reported in Bai, Jayachandran, Malesky and Olken (2019) of around 80 percent. The discrepancy between the World Bank estimates and our estimates is partly due to differences in the sample. When we restrict the World Bank sample to firms with over 20 employees in the manufacturing industry, the incidence of bribery rises from 25 percent to 40 percent in 2009.

district in 2000, while establishments first observed after 2000 are assigned to the district whose 2000 borders contain their first observed district.

IBS reports a five-digit industry code (`disic5`) starting in 1999. The classification system was overhauled between 2009 and 2010. From 1999 to 2009, the first four digits of `disic5` are similar to the four-digit ISIC Rev. 3.1, while from 2010 onward they are similar to ISIC Rev. 4. In two thirds of cases, the 2009 codes map uniquely to 2010 codes. For these cases we use the UN correspondence tables to harmonize the codes over time. For the remaining cases, we construct our own correspondence based on establishment data in 2009 and 2010. We first calculate the aggregate 2010 output of all firms that report each pair of codes in 2009 and 2010 observed in our dataset. Then, for each ISIC Rev. 4 code, we assign the ISIC Rev. 3.1 code corresponding to the greatest value of production among all ISIC Rev. 4-ISIC Rev. 3.1 pairs containing that ISIC Rev. 4 code. We observe 134 four-digit industries in our sample.

Sample Selection

We drop all five districts in the province of Jakarta, as these districts are managed at the province level. Dropping Jakarta reduces the number of firm-year observations by 25,868, or just under 8 percent of the original sample. We also omit the five districts that split for the first time during 2013–2014, because we cannot measure four-year growth post-split for these districts, as the GDP series ends in 2013. The resulting sample contains 331 districts.

Appendix B Additional Results

B.1 Alternative Measures of Economic Activity

As with any economic aggregate, district GDP is susceptible to measurement error. If this measurement error is correlated with splitting, our estimates could be biased. To investigate whether the main results are an artifact of measurement error, we examine two alternative measures of economic activity: household expenditure and nighttime luminosity.

B.1.1 Household Expenditure

To start, we estimate the parameters of Equation (1) using the percentage change in household expenditure as the dependent variable. As noted in Section 3, the household expenditure data for 2008 are missing, meaning that we cannot estimate $\beta_{2004,4}$, $\beta_{2008,0}$, or $\beta_{2009,h}$ for any h. As a solution, we redefine the base year as two years before the split, and estimate the effects for 1 to 3 years after the split. This approach enables us to include all five splitting cohorts in our reported estimates.

Figure B.3 reveals that district splitting led to a reduction in household expenditure—7.4 percent after three years—that was similar in magnitude, percentage-wise, to the decline in GDP. Nevertheless, the total expenditure of the poorest 20 percent of households remained unchanged, and even increased on a per capita basis. This discrepancy can be attributed to a slight reduction in district population following the split, as seen in Figure B.3. Overall, these findings are consistent with the main results for GDP, and further suggest that the poor were spared from the deleterious effects of splitting.

B.1.2 Nighttime Luminosity

Next, we estimate the impact of fragmentation on nighttime luminosity. In almost one percent of district-years, no light is detected, preventing us from measuring the percentage change in the outcome as described in Equation (1). To address this issue, we specify an exponential mean model to estimate the proportionate effects of splitting while accommodating the zero values for lights.

We define the set of treatment indicators,

$$W_{d,t}(e,s) \equiv 1(E_d = e) \cdot 1(t = s), \quad e \in \mathscr{E},\ s \in \{1997, \ldots, 2013\},$$

where $W_{d,t}(e,s)$ equals one in year s for districts belonging to cohort e. The model is

$$\begin{aligned}\mathrm{E}\left(Y_{d,t} \mid \boldsymbol{W}_{d,t}, \boldsymbol{X}_d, \alpha_d\right) = \exp\Bigg(&\alpha_d + \gamma_t + \sum_{e\in\mathscr{E}}\sum_{s\neq e-1} \beta_{e,s} W_{d,t}(e,s) + \sum_s \left(1(t=s)\cdot \boldsymbol{X}_d\right)' \boldsymbol{\lambda}_s \\ &+ \sum_{e\in\mathscr{E}}\sum_{s\neq e-1} \left(W_{d,t}(e,s)\cdot \dot{\boldsymbol{X}}_d(e)\right)' \boldsymbol{\delta}_{e,s}\Bigg),\end{aligned} \tag{B.1}$$

which controls for district fixed effects, year effects, and time-varying effects of the baseline covariates (i.e., region dummies). Additionally, the treatment effects are allowed to vary based on the year, splitting cohort, and covariate values.

Under a conditional parallel trends assumption for the log of the mean of Y, $\beta_{e,s}$ identifies the average treatment effect for cohort e in year s in proportionate terms (Wooldridge, 2023). Mechanically, $\beta_{e,s}$ equals the change in the log mean of Y from year $e-1$ to year s for districts in cohort e, minus the corresponding change for never-splitting districts, evaluated at the average of $\boldsymbol{X}_d$ in cohort e. Therefore, the interpretation of $\beta_{e,e+h}$ in Equation (B.1) is similar to that of $\beta_{e,h}$ in Equation (1).

We estimate the parameters of Equation (B.1) by fixed effects Poisson quasi-maximum likelihood.[B.1] Our estimate of the weighted average treatment effect on the treated h years after the first split is

$$\hat{\beta}_h = \sum_{e\in\mathscr{E}} \omega_e \hat{\beta}_{e,e+h}, \tag{B.2}$$

where ω_e is the share of splitting districts that belong to cohort e.

The estimates displayed in Figure B.3 suggest that government fragmentation reduced nighttime luminosity. Similar to GDP, luminosity falls with a delay, declining by 4.5 percent (S.E. = 5.0 percent) four years after the split. The cumulative decrease is 14.6 percent (S.E. = 19.5 percent). Prior to the split, luminosity exhibits similar trends in both splitting and non-splitting districts. Although the estimates are imprecise, they suggest a decline in economic activity following district splitting, consistent with the GDP results.

B.2 Execution of Spending Plans

The expenditure responses to splitting could reflect changes in district priorities or difficulties in executing desired spending plans. To disentangle these two effects, we compare realized and planned expenditure using data from the Ministry of Finance. At the level of original district borders, the realized and planned expenditure responses are similar for most categories (Appendix Table B.10). However, a notable exception is physical capital. Despite an increase in the planned expenditure share by 0.8 percentage points, the realized share devoted to

[B.1] The estimators are consistent as long as the conditional mean is correctly specified; the outcome need not have a Poisson distribution (Wooldridge, 1999). We estimate the parameters using the `ppmlhdfe` package for Stata (Correia, Guimarães and Zylkin, 2020, 2021).

physical capital investment actually decreases by 2.7 percentage points.[B.2]

A plausible explanation for the failure to implement capital spending plans is the lower bureaucratic capacity of child districts. While it is not possible to estimate separate effects of splitting for parent and child districts—their spending policies before the split are not observable—we can compare their realized and planned expenditure shares in the years following the split. Although realized and planned spending often differ, these discrepancies tend to occur in the same direction and to a similar extent for both parent and child districts, except in the case of physical capital expenditure (Panel C of Appendix Table B.11). For parent districts, the realized expenditure share for physical capital exceeds the planned share by 0.5 percentage points. By contrast, the realized share for child districts is 2.4 percentage points *lower* than the planned share. The difference between these two estimates is statistically significant. Together, these results suggest that splitting led to delays in the execution of physical capital spending plans due to the inferior quality of civil servants in child districts.

B.3 Treatment-Effect Heterogeneity

Contextual factors matter for the impact of government fragmentation. While we have investigated the influence of factors motivated by theory, there could be other considerations that remain unexplored. We therefore take advantage of recent advancements in applying machine learning tools to discipline heterogeneity analysis, and analyze the variation in treatment effects by district characteristics. We first implement the causal forest algorithm developed by Wager and Athey (2018) to predict treatment effects for all districts in our main estimation sample. We then analyze the features of the resulting estimated conditional average treatment effect function. We group districts based on the quintile of their predicted treatment effect, and we take the average of district characteristics in each quintile. Chernozhukov, Demirer, Duflo and Fernández-Val (2023) show that although the conditional average treatment effect function itself is biased, we can perform inference on the characteristics of this function.

To apply the causal forest algorithm, we create a “stacked” dataset as in our main specification. The dataset contains one observation for each splitting district in the year that they split. For each district that never splits, the data set contains five observations, one for each year corresponding to the five splitting cohorts. The outcome of interest is cumulative GDP growth between the year before the split and four years after the split—the same outcome used in our main results (Table 1). “Treatment” is defined as whether a district splits. The covariates we include are urbanization rate, ethnic fractionalization, ethnic polarization, population shares by age group (15–64 and 65+), population shares by education (completed primary and completed secondary), year dummies, and region dummies.

[B.2] We can only estimate the response for physical capital in the first three years following a split, because the Ministry of Finance stopped reporting the amount of infrastructure expenditure after 2006. This estimate thus corresponds to the initial decline in the share of spending on physical capital discussed above.

Defining the outcome (Y_i), treatment indicator (W_i), and covariates (X_i) in this way, the causal forest algorithm builds decision trees following an analogous procedure to standard random forest algorithms, except that the predicted variable is a treatment effect ($\tau(x)$), and not an observable feature of the data. Let B denote the set of decision trees trained using random sub-samples of the data. (We train a total of 10,000 trees.) For each tree b, every observation will fall into a particular leaf $L_b(x)$ based on its covariate values x. To predict the treatment effect for a given observation, each observation i in the training data is assigned a weight, $\alpha_i(x)$, capturing the frequency with which it falls into the same leaf as the prediction point, x. The predicted treatment effect is then calculated as (Athey and Wager, 2019)

$$\hat{\tau}(x) = \frac{\sum_{i=1}^{N} \alpha_i(x)(Y_i - \hat{m}(X_i))(W_i - \hat{e}(X_i))}{\sum_{i=1}^{N} \alpha_i(x)(W_i - \hat{e}(X_i))^2},$$

where $\hat{m}$ and $\hat{e}$ are predicted outcomes and propensity scores, respectively, estimated using a regression forest.

Table B.13 shows the averages of each included covariate within predicted treatment effect quintiles, for the full sample and the trimmed sample of districts, respectively. Only a few characteristics stand out. The difference in characteristics among the most- and least-affected districts is greatest for ethnic fractionalization and urbanization rate. The greatest predicted declines in GDP occur in districts with the greatest pre-split levels of fractionalization in 2000. In the full sample, areas with the greatest declines in GDP have substantially higher levels of urbanization. However, when we trim the sample to impose overlap in urbanization rates between the treated and control districts, this difference disappears (Appendix Table B.14). All other characteristics are similar across treatment effect quintiles. This suggests that ethnic differences may have exacerbated the negative impacts of fragmentation—for example, by limiting improvements in accountability.

B.4 Tables

Table B.1: Correlation between "Gifts" and Activities Requiring Permits

	Firm Paid Any Gifts					
	(1)	(2)	(3)	(4)	(5)	(6)
Any Exports	0.011**					
	(0.005)					
Any Expenditure On:						
Land Contract		0.040***				
		(0.008)				
Building Additions			0.034***			
			(0.008)			
Any Electricity Purchased:						
From Government				0.052***		
				(0.008)		
From Non-Government					−0.206***	
					(0.037)	
Any Electricity Generated						−0.024***
						(0.008)
Mean Indep. Var.	0.185	0.069	0.122	0.868	0.049	0.174
Observations	211,147	312,990	278,951	312,990	312,990	303,145
Districts	322	325	324	325	325	325

Notes: The outcome is an indicator variable that equals 1 if the firm reported paying any gifts. All regressions control for log firm revenue as a measure of firm size. Each regression controls for firm fixed effects, industry-by-year effects, and region-by-year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: Baseline District Characteristics

	(1) Splitters	(2) Non-Splitters	(3) Difference
Log GDP, 2000	15.159	15.242	−0.083
			(0.129)
Log General Grant, 2001	12.738	12.555	0.182***
			(0.058)
Log Population	12.903	12.905	−0.002
			(0.109)
Log Land Area, 2000	9.028	6.833	2.195***
			(0.173)
Ethnic Fractionalization, 2000	0.594	0.342	0.252***
			(0.037)
Urbanization Rate, 2000	0.194	0.453	−0.259***
			(0.036)
Share of Population Aged 0–14, 2000	0.355	0.303	0.052***
			(0.005)
Share of Population Aged 15–64, 2000	0.612	0.651	−0.039***
			(0.004)
Share of Population Aged 65+, 2000	0.033	0.046	−0.013***
			(0.002)
Share of Population with Primary Education, 2000	0.590	0.649	−0.059***
			(0.013)
Share of Population with Secondary Education, 2000	0.125	0.185	−0.060***
			(0.012)
Observations	98	233	

Notes: This table reports average baseline characteristics for splitting and non-splitting districts, and the difference of the averages. Standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: Summary Statistics

	(1) Mean	(2) Std. Dev.	(3) Min.	(4) Max.	(5) Obs.	(6) Districts
District Outcomes: INDO-DAPOER						
GDP (IDR Billions)	10,609	17,105	238	261,483	4,634	331
General Grant (IDR Billions)	479	317	0	3,926	4,961	331
Total Expenditure (IDR Billions)	835	632	11	7,752	4,842	331
Administration (% Expenditure)	31	10	1	93	3,576	331
Human Capital (% Expenditure)	43	13	0	82	3,576	331
Physical Capital (% Expenditure)	16	7	1	59	3,576	331
Economy (% Expenditure)	6	3	1	57	3,576	331
Social (% Expenditure)	4	3	0	40	3,576	331
Population (Thousands)	656	631	23	5,659	4,965	331
District Outcomes: PODES (Triennial)						
% of Villages w/ Public Kindergarten	5.8	7.9	0.0	100	2,309	330
% of Villages w/ Public Primary School	91.2	14.0	19.4	100	2,310	330
% of Villages w/ Public Secondary School	31.8	17.7	4.0	100	2,310	330
% of Villages w/ Health Center	58.2	23.0	6.0	100	2,310	330
% of Villages w/ Doctor	28.5	25.0	0.0	100	2,308	330
% of Villages w/ Doctor's Office	26.0	25.6	0.0	100	2,310	330
% of Villages w/ Police Station	16.6	15.7	0.0	100	2,302	330
% of Villages w/ Paved Road	69.7	25.3	0.0	100	2,308	330
% of Villages w/ Street Lights	63.0	32.5	0.0	100	2,308	330
% of Households w/ Electricity	71.9	24.1	0.0	100	2,310	330
Firm Outcomes: IBS						
Paid Any Gifts	0.68	0.47	0	1	392,097	325
Gifts as % of Revenue	0.39	2.34	0	100	388,248	325
Paid Any Indirect Taxes or Fees	0.77	0.42	0	1	393,424	325
Indirect Taxes and Fees as % of Revenue	1.18	4.49	0	100	389,253	325
Any Exports	0.19	0.39	0	1	227,574	324
Any Land Contracts	0.07	0.26	0	1	392,097	325
Total Output (IDR Millions)	70.60	531.07	0	38,594	386,623	325
Number of Employees	200.99	737.86	13	56,139	395,028	325
Output per Worker (IDR Millions)	0.25	1.16	0	99	386,623	325
Log TFP	0.19	0.45	-1	8	226,950	317
Market Outcomes: IBS						
Number of Establishments	4.54	12.62	0	545	70,516	325
Number of Entries	0.36	2.52	0	338	70,516	325
Number of Exits	0.28	1.61	0	106	58,064	323
Herfindahl-Hirschman Index	0.72	0.31	0	1	67,699	324

Notes: Variables with monetary values are measured in constant 2010 IDR millions or billions, as indicated. Column 6 displays the number of districts that have non-missing observations in at least one year. The one district missing from the PODES panel is Kota Banjar Baru, which is missing from the 1999 wave of PODES.

Table B.4: Sample Coverage

	(1) Years	(2) Districts per Year	(3) Years per Firm
District Outcomes: INDO-DAPOER			
GDP	2000–2013	331.0	
General Grant	2000–2014	330.7	
Total Expenditure	2000–2014	322.8	
Functional Expenditure Shares (All)	2001–2012	298.0	
Population	2000–2014	331.0	
District Outcomes: PODES			
Public Services (All)	1996, 1999, 2002, 2005, 2008, 2011, 2014	330.0	
Firm Outcomes: IBS			
Paid Any Gifts	1995–2014	308.6	9.2
Gifts as % of Revenue	1995–2014	308.2	9.1
Paid Any Indirect Taxes or Fees	1995–2014	309.0	9.2
Indirect Taxes and Fees as % of Revenue	1995–2014	308.5	9.1
Any Exports	1999–2014	297.4	6.1
Any Land Contracts	1995–2014	308.6	9.2
Total Output	1995–2014	308.5	9.0
Number of Employees	1995–2014	309.3	9.2
Output per Worker	1995–2014	308.5	9.0
Log TFP	1995–2014	285.9	7.5
Market Outcomes: IBS			
Number of Establishments	1999–2014	308.9	
Number of Entries	1999–2014	308.9	
Number of Exits	2000–2014	302.5	
Herfindahl-Hirschman Index	1999–2014	307.5	

Notes: Column 1 lists the years observed in the analysis sample, column 2 reports the average number of districts that are observed in a given year, and column 3 reports the average number of years that a given firm is observed. Districts per year is calculated over the period 2000–2014 for firm and market outcomes to reflect the sample used by the main estimates. The one district missing from the PODES panel is Kota Banjar Baru, which is missing from the 1999 wave of PODES.

Table B.5: Government Fragmentation and Economic Growth (Robustness)

	General Grant	Predicted GDP		Actual GDP
	(1)	(2) Multiplier = 0.6	(3) Multiplier = 1.8	(4)
	Panel A: Additional Controls			
Cumulative Effect of Split:				
All Splits	0.269***	0.161***	0.484***	−0.170**
	(0.017)	(0.010)	(0.030)	(0.076)
Early Splits	0.229***	0.137***	0.412***	−0.140
	(0.022)	(0.013)	(0.039)	(0.095)
Late Splits	0.378***	0.227***	0.681***	−0.253**
	(0.017)	(0.010)	(0.030)	(0.105)
p-value, H_0: Early = Late	0.000	0.000	0.000	0.423
Observations	1,262	1,262	1,262	1,262
Districts	330	330	330	330
	Panel B: Trimmed Sample			
Cumulative Effect of Split:				
All Splits	0.242***	0.145***	0.436***	−0.189**
	(0.024)	(0.014)	(0.042)	(0.080)
Early Splits	0.202***	0.121***	0.364***	−0.175*
	(0.021)	(0.012)	(0.037)	(0.095)
Late Splits	0.352***	0.211***	0.634***	−0.226*
	(0.066)	(0.040)	(0.120)	(0.117)
p-value, H_0: Early = Late	0.031	0.031	0.031	0.717
Observations	927	927	927	927
Districts	263	263	263	263
	Panel C: Additional Controls, Trimmed Sample			
Cumulative Effect of Split:				
All Splits	0.222***	0.133***	0.400***	−0.203***
	(0.018)	(0.011)	(0.032)	(0.079)
Early Splits	0.189***	0.113***	0.340***	−0.250**
	(0.022)	(0.013)	(0.039)	(0.098)
Late Splits	0.314***	0.188***	0.565***	−0.073
	(0.025)	(0.015)	(0.046)	(0.076)
p-value, H_0: Early = Late	0.000	0.000	0.000	0.112
Observations	927	927	927	927
Districts	263	263	263	263

Notes: The first row of each panel reports estimates of the cumulative effect of the first district split, $\sum_{h=0}^{4} \beta_h$, based on the cohort-size-weighted CATT estimator (Equation (2)). The second and third rows of each panel report separate cohort-size-weighted CATT estimates for early (2002–2004) and late (2008–2009) splits. The estimates in Panels A and C control for ethnic fractionalization, urbanization rate, share of population aged 15–64, share of population with a primary education, and share of population with a secondary education. All control variables are measured in 2000. In Panels B and C the sample consists of districts with urban population share falling within the common support (0 to 0.71). The outcome is measured as the cumulative growth from the year prior to the split to four years after the split, relative to GDP in the year prior to the split. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.6: Government Fragmentation and Nighttime Luminosity at Different Levels of Aggregation

	Level of Aggregation		
	(1) Original District	(2) Province by Island	(3) Province
	Panel A: Dynamic Specification		
Cumulative Effect of Doubling Number of Districts	−0.073	−0.292	−0.225
	(0.205)	(0.291)	(0.332)
Observations	4,290	728	429
Clusters	330	56	33
	Panel B: Static Specification		
Effect of Doubling Number of Districts	−0.050	−0.170*	−0.160
	(0.046)	(0.101)	(0.101)
Observations	4,620	784	462
Clusters	330	56	33

Notes: This table reports estimates of the effect of doubling the number of districts on nighttime luminosity at three different levels of aggregation, as indicated. The sample is limited to years 2000–2013 to remain consistent with Table 2. All estimates are obtained via fixed effects Poisson quasi-maximum likelihood. Panel A presents estimates based on the dynamic model $\mathrm{E}\left(Y_{j,t} \mid \ln N_{j,t}, \alpha_j, \lambda_{r(j),t}\right) = \exp\left(\sum_{k=0}^{4} \beta_k \ln N_{j,t-k} + \alpha_j + \lambda_{r(j),t}\right)$, where $Y_{j,t}$ is total nighttime luminosity in geographic unit j in year t, $N_{j,t}$ is the number of districts contained in geographic unit j in year t, and the $\lambda_{r(j),t}$ are region-by-year effects. In this model the effect of permanently doubling the number of districts on luminosity h years later (in proportionate terms) is $\ln(2) \cdot \sum_{k=0}^{h} \beta_k$. The table reports estimates of the cumulative effect over four years, $\ln(2) \cdot \sum_{h=0}^{4} \sum_{k=0}^{h} \beta_k$. Panel B presents estimates of $\ln(2) \cdot \beta$ from the static model $\mathrm{E}\left(Y_{j,t} \mid \ln N_{j,t}, \alpha_j, \lambda_{r(j),t}\right) = \exp\left(\beta \ln N_{j,t} + \alpha_j + \lambda_{r(j),t}\right)$. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by geographic unit. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table B.7: Impact of Fragmentation on District Expenditure

	Share of GDP	Budget Share of Functional Expenditure Category				
	(1) Total Expenditure	(2) Administration	(3) Human Capital	(4) Physical Capital	(5) Economy	(6) Social
Avg. Effect of Split	0.078***	0.038***	−0.060***	0.017**	0.001	0.004*
	(0.008)	(0.008)	(0.008)	(0.007)	(0.004)	(0.003)
Baseline Mean	0.115	0.320	0.430	0.161	0.059	0.030
Observations	1,239	1,207	1,207	1,207	1,207	1,207
Districts	325	321	321	321	321	321

Notes: This table reports estimates of the average effect of the first district split, based on the cohort-size-weighted CATT estimator (Equation (2)). The outcome in column 1 is the average change in total expenditure from the year prior to the split to years 0–4 after the split as a share of pre-split GDP, $(1/5)\sum_{h=0}^{4}(E_{d,e+h} - E_{d,e-1})/Y_{d,e-1}$. The outcomes in columns 2–6 are measured as the average change in the expenditure share over the first five years following the split relative to the expenditure share in the year prior to the split, $(1/5)\sum_{h=0}^{4}(ES_{d,e+h} - ES_{d,e-1})$. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.8: Civil Servants in Parent and Child Districts

	(1) Parent Districts	(2) Child Districts	(3) Difference
	Panel A: Civil Servants per 1,000 People		
0–1 Years after Split	18.2	14.0	4.2
	(1.5)	(0.9)	(1.7)
2–3 Years after Split	19.4	19.1	0.3
	(1.0)	(0.9)	(1.4)
4–5 Years after Split	20.9	22.4	−1.5
	(1.0)	(1.0)	(1.5)
	Panel B: Civil Servant Characteristics (SUPAS)		
High School Diploma (%)	89.8	88.4	1.4
	(1.5)	(1.4)	(2.1)
Bachelor's Degree (%)	21.5	18.3	3.3
	(1.7)	(1.3)	(2.2)
Age	40.0	38.3	1.7
	(0.3)	(0.4)	(0.5)
	Panel C: Civil Servant Characteristics (SAKERNAS)		
High School Diploma (%)	91.3	90.7	0.6
	(0.6)	(0.6)	(0.8)
Bachelor's Degree (%)	33.4	32.0	1.4
	(0.9)	(0.9)	(1.2)
Age	38.8	36.7	2.1
	(0.2)	(0.2)	(0.3)

Notes: This table reports the average characteristics of civil servants in splitting districts, separately for parent and child districts. Panel A uses pooled annual data from BKN for the years 2005–2014, Panel B uses cross-sectional data from SUPAS 2005, and Panel C uses pooled annual data from SAKERNAS 2007–2015. Panels A and C include all splitting districts, while Panel B includes districts that split during 2002–2004. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district.

Table B.9: Mayor Education in Parent and Child Districts

	(1) Parent Districts	(2) Child Districts	(3) Difference
Bachelor's Degree (%)	90.0	94.7	−4.7
	(3.9)	(2.3)	(4.5)
Graduate Degree (%)	25.0	36.2	−11.2
	(5.6)	(5.0)	(7.5)

Notes: This table reports the average education of the first democratically appointed mayor in splitting districts, separately for parent and child districts. The sample includes all districts that split during 2002–2004. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district.

Table B.10: Impact of Fragmentation on District Expenditure Priorities: Ministry of Finance Data

	(1) Administration	(2) Human Capital	(3) Physical Capital	(4) Economy	(5) Social
	Panel A: Realized Expenditure Share				
Average Effect of Split	0.034^{***}	-0.050^{***}	-0.027^{*}	-0.014	0.008
	(0.012)	(0.009)	(0.016)	(0.009)	(0.005)
Baseline Mean	0.160	0.170	0.368	0.228	0.074
Observations	766	766	374	374	766
Districts	292	292	251	251	292
	Panel B: Planned Expenditure Share				
Average Effect of Split	0.041^{***}	-0.045^{***}	0.008	-0.022^{*}	0.001
	(0.011)	(0.008)	(0.014)	(0.013)	(0.006)
Baseline Mean	0.146	0.167	0.343	0.260	0.085
Observations	766	766	374	374	766
Districts	292	292	251	251	292
	Panel C: Difference Between Realized and Planned Expenditure Shares				
Average Effect of Split	-0.007	-0.005	-0.035^{***}	0.008	0.007^{*}
	(0.010)	(0.007)	(0.011)	(0.007)	(0.004)
Observations	766	766	374	374	766
Districts	292	292	251	251	292

Notes: This table reports estimates of the average effect of the first district split, based on the cohort-size-weighted CATT estimator (Equation (2)). The data on realized (Panel A) and planned (Panel B) expenditure come from the Ministry of Finance. In columns 1, 2, and 5, the outcome is measured as the average change in the expenditure share over the first five years following the split relative to the expenditure share in the year prior to the split, $\sum_{h=0}^{4}(E_{d,e+h} - E_{d,e-1})/5$. In columns 3 and 4, the outcome is measured as the average change over the first three years due to the data being unavailable after 2006. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.11: Post-Split Expenditure Priorities by Parent and Child: Ministry of Finance Data

	(1) Administration	(2) Human Capital	(3) Physical Capital	(4) Economy	(5) Social
	Panel A: Average Post-Split Realized Expenditure Share				
Parent	0.335	0.362	0.176	0.076	0.043
	(0.013)	(0.012)	(0.014)	(0.005)	(0.002)
Child	0.376	0.304	0.173	0.053	0.043
	(0.015)	(0.011)	(0.011)	(0.005)	(0.003)
Difference	−0.041	0.058	0.003	0.023	0.000
	(0.015)	(0.011)	(0.015)	(0.005)	(0.003)
Observations	484	484	196	196	484
Districts	87	87	50	50	87
	Panel B: Average Post-Split Planned Expenditure Share				
Parent	0.323	0.382	0.171	0.087	0.036
	(0.010)	(0.012)	(0.015)	(0.005)	(0.002)
Child	0.364	0.319	0.197	0.061	0.039
	(0.011)	(0.012)	(0.012)	(0.005)	(0.003)
Difference	−0.041	0.063	−0.026	0.027	−0.003
	(0.010)	(0.010)	(0.015)	(0.005)	(0.003)
Observations	484	484	196	196	484
Districts	87	87	50	50	87
	Panel C: Average Post-Split Difference Between Realized and Planned Expenditure Shares				
Parent	0.012	−0.020	0.005	−0.011	0.007
	(0.009)	(0.008)	(0.011)	(0.003)	(0.001)
Child	0.012	−0.015	−0.024	−0.007	0.004
	(0.011)	(0.008)	(0.009)	(0.003)	(0.002)
Difference	0.000	−0.005	0.029	−0.004	0.003
	(0.013)	(0.010)	(0.015)	(0.003)	(0.002)
Observations	484	484	196	196	484
Districts	87	87	50	50	87

Notes: This table reports average expenditure shares over the first five years following a split separately for parent and child districts. The data on realized (Panel A) and planned (Panel B) expenditure come from the Ministry of Finance. Columns 3 and 4 use a smaller sample because the relevant data are unavailable after 2006. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district.

Table B.12: Impact of Fragmentation on Manufacturing Productivity and Competition

	(1)	(2)	(3)	(4)
	Panel A: Firm-Level Productivity			
	Log Output	Log Number of Workers	Log Output per Worker	Log TFP
Avg. Effect of Split	0.055**	0.024***	0.030	0.002
	(0.026)	(0.009)	(0.025)	(0.014)
Observations	70,992	77,636	70,992	44,551
Districts	308	308	308	289
	Panel B: Market-Level Competition			
	Establishments	Entries	Exits	Herfindahl-Hirschman Index
Avg. Effect of Split	0.027	0.094	−0.005	−0.001
	(0.266)	(0.068)	(0.068)	(0.008)
Observations	16,796	16,796	15,194	16,144
Districts	312	312	310	312

Notes: This table reports estimates of the average effect of the first district split, based on the cohort-size-weighted CATT estimator (Equations (2) and (4)). The estimates in Panel A are based on plant-level data, and the estimates in Panel B are based on market-level (district-by-industry) data. The outcomes are measured as the average change over the first five years following the split relative to the year prior to the split, $(1/6)\sum_{h=0}^{5}(Y_{i,d,e+h} - Y_{i,d,e-1})$. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.13: Characteristics of Districts by Predicted Treatment Effect Quintiles

	Average Value by Predicted Treatment Effect Quintile				
	(1)	(2)	(3)	(4)	(5)
Predicted Treatment Effect	−0.33	−0.18	−0.13	−0.09	−0.02
	(0.07)	(0.03)	(0.01)	(0.01)	(0.04)
Covariates:					
Urbanization Rate	0.63	0.38	0.50	0.41	0.25
	(0.34)	(0.31)	(0.32)	(0.33)	(0.26)
Ethnic Fractionalization	0.74	0.38	0.16	0.23	0.29
	(0.18)	(0.32)	(0.20)	(0.21)	(0.26)
Ethnic Polarization	0.20	0.16	0.13	0.14	0.15
	(0.13)	(0.16)	(0.16)	(0.11)	(0.21)
Pop. Share Aged 15–64	0.66	0.65	0.66	0.65	0.63
	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)
Pop. Share Aged 65+	0.02	0.05	0.05	0.05	0.05
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Pop. Share w/ Primary Edu.	0.71	0.67	0.67	0.61	0.56
	(0.08)	(0.07)	(0.08)	(0.11)	(0.11)
Pop. Share w/ Secondary Edu.	0.26	0.17	0.18	0.16	0.12
	(0.12)	(0.10)	(0.10)	(0.10)	(0.08)
Year = 2002	0.19	0.19	0.22	0.18	0.19
	(0.40)	(0.39)	(0.42)	(0.38)	(0.39)
Year = 2003	0.22	0.18	0.19	0.23	0.21
	(0.41)	(0.39)	(0.39)	(0.42)	(0.41)
Year = 2004	0.20	0.22	0.22	0.20	0.22
	(0.40)	(0.41)	(0.42)	(0.40)	(0.41)
Year = 2008	0.20	0.19	0.18	0.21	0.19
	(0.40)	(0.39)	(0.39)	(0.41)	(0.39)
Year = 2009	0.19	0.23	0.19	0.18	0.19
	(0.39)	(0.42)	(0.39)	(0.39)	(0.39)
Observations	253	252	253	252	252

Notes: This table reports average characteristics by quintile of predicted treatment effects, predicted using the causal forest algorithm developed by Wager and Athey (2018). We follow the procedure described in Section B.3. All covariates listed in the table are included in the set of covariates used to train the model, in addition to region dummies. Covariates are measured in 2000. Ethnic polarization has been multiplied by 10 to increase readability. In this table, we use the full sample of treatment and control districts.

Table B.14: Characteristics of Districts by Predicted Treatment Effect Quintiles (Trimmed Sample)

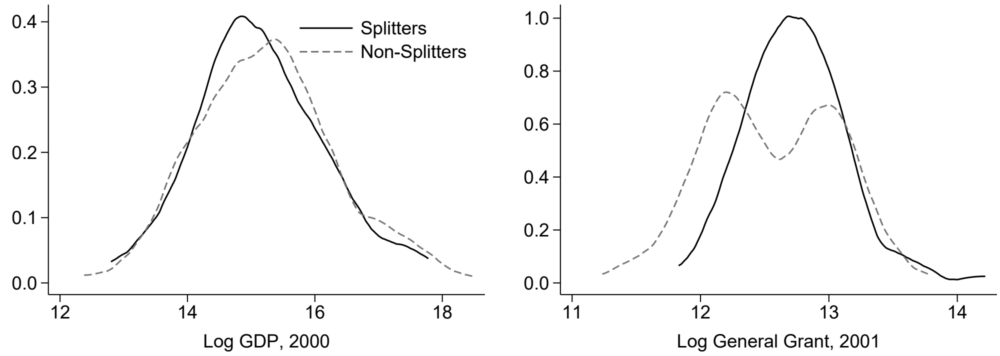
	Average Value by Predicted Treatment Effect Quintile				
	(1)	(2)	(3)	(4)	(5)
Predicted Treatment Effect	−0.36	−0.15	−0.11	−0.07	0.01
	(0.10)	(0.02)	(0.01)	(0.01)	(0.04)
Covariates:					
Urbanization Rate	0.24	0.24	0.28	0.30	0.19
	(0.17)	(0.15)	(0.17)	(0.16)	(0.11)
Ethnic Fractionalization	0.73	0.29	0.18	0.19	0.16
	(0.19)	(0.26)	(0.22)	(0.21)	(0.18)
Ethnic Polarization	0.19	0.12	0.09	0.12	0.06
	(0.15)	(0.13)	(0.11)	(0.18)	(0.04)
Pop. Share Aged 15–64	0.63	0.64	0.64	0.65	0.63
	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)
Pop. Share Aged 65+	0.02	0.05	0.06	0.06	0.06
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Pop. Share w/ Primary Edu.	0.62	0.62	0.63	0.60	0.53
	(0.07)	(0.08)	(0.07)	(0.08)	(0.08)
Pop. Share w/ Secondary Edu.	0.14	0.13	0.12	0.12	0.10
	(0.06)	(0.05)	(0.05)	(0.06)	(0.03)
Year = 2002	0.18	0.17	0.20	0.21	0.20
	(0.38)	(0.37)	(0.40)	(0.41)	(0.40)
Year = 2003	0.24	0.18	0.23	0.18	0.20
	(0.43)	(0.39)	(0.42)	(0.39)	(0.40)
Year = 2004	0.22	0.19	0.17	0.24	0.26
	(0.41)	(0.39)	(0.38)	(0.43)	(0.44)
Year = 2008	0.18	0.19	0.20	0.18	0.19
	(0.39)	(0.40)	(0.40)	(0.39)	(0.39)
Year = 2009	0.19	0.26	0.19	0.18	0.15
	(0.39)	(0.44)	(0.40)	(0.38)	(0.35)
Observations	186	185	186	185	185

Notes: This table reports average characteristics by quintile of predicted treatment effects, predicted using the causal forest algorithm developed by Wager and Athey (2018). We follow the procedure described in Section B.3. All covariates listed in the table are included in the set of covariates used to train the model, in addition to region dummies. Covariates are measured in 2000. Ethnic polarization has been multiplied by 10 to increase readability. In this table, we trim the sample to increase overlap in covariates between splitters and non-splitters.

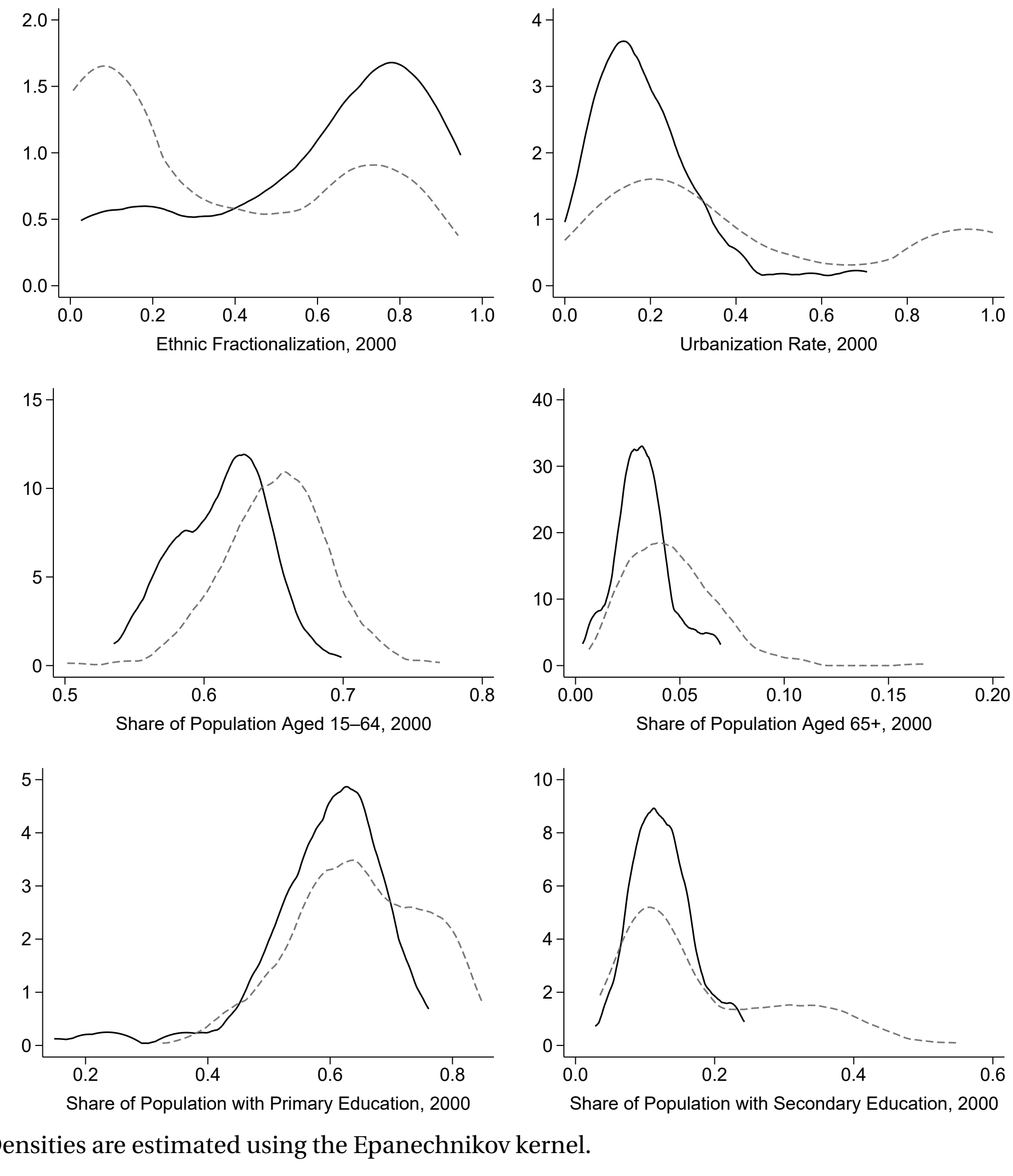
B.5 Figures

Figure B.1: Density of Baseline District Outcomes and Covariates

(a) Baseline Outcomes



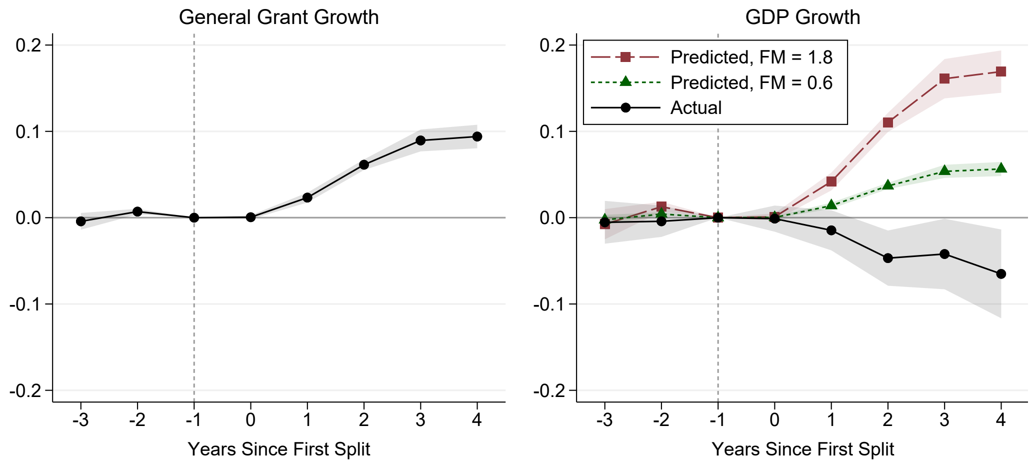
(b) Baseline Covariates



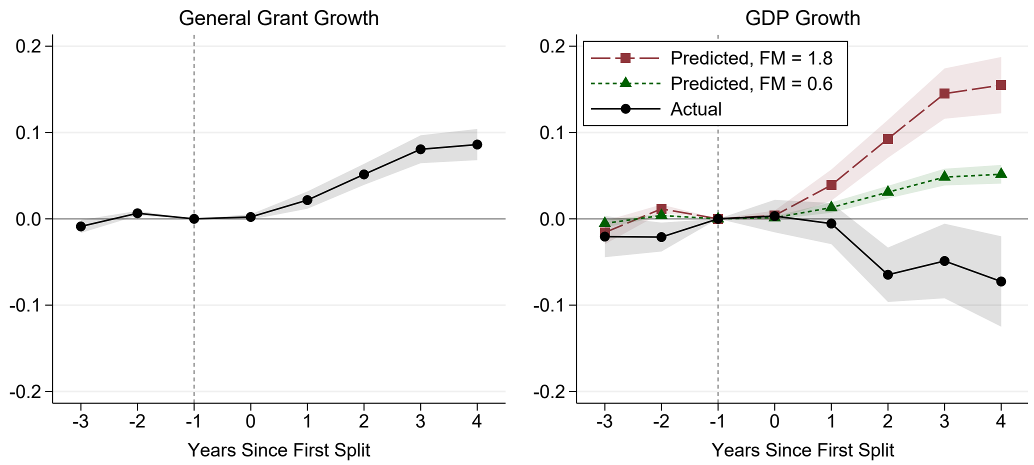
Notes: Densities are estimated using the Epanechnikov kernel.

Figure B.2: The Effect of District Splits on General Grant and GDP (Robustness)

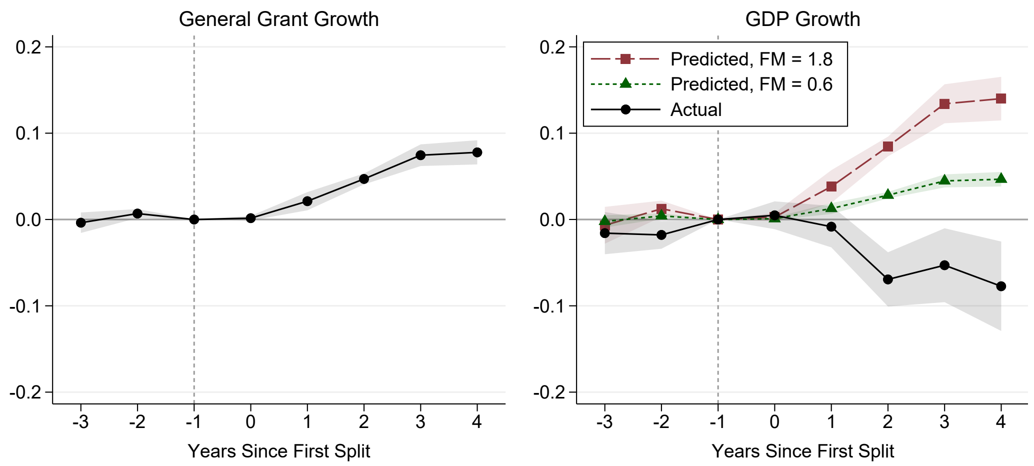
(a) Additional Controls



(b) Trimmed Sample

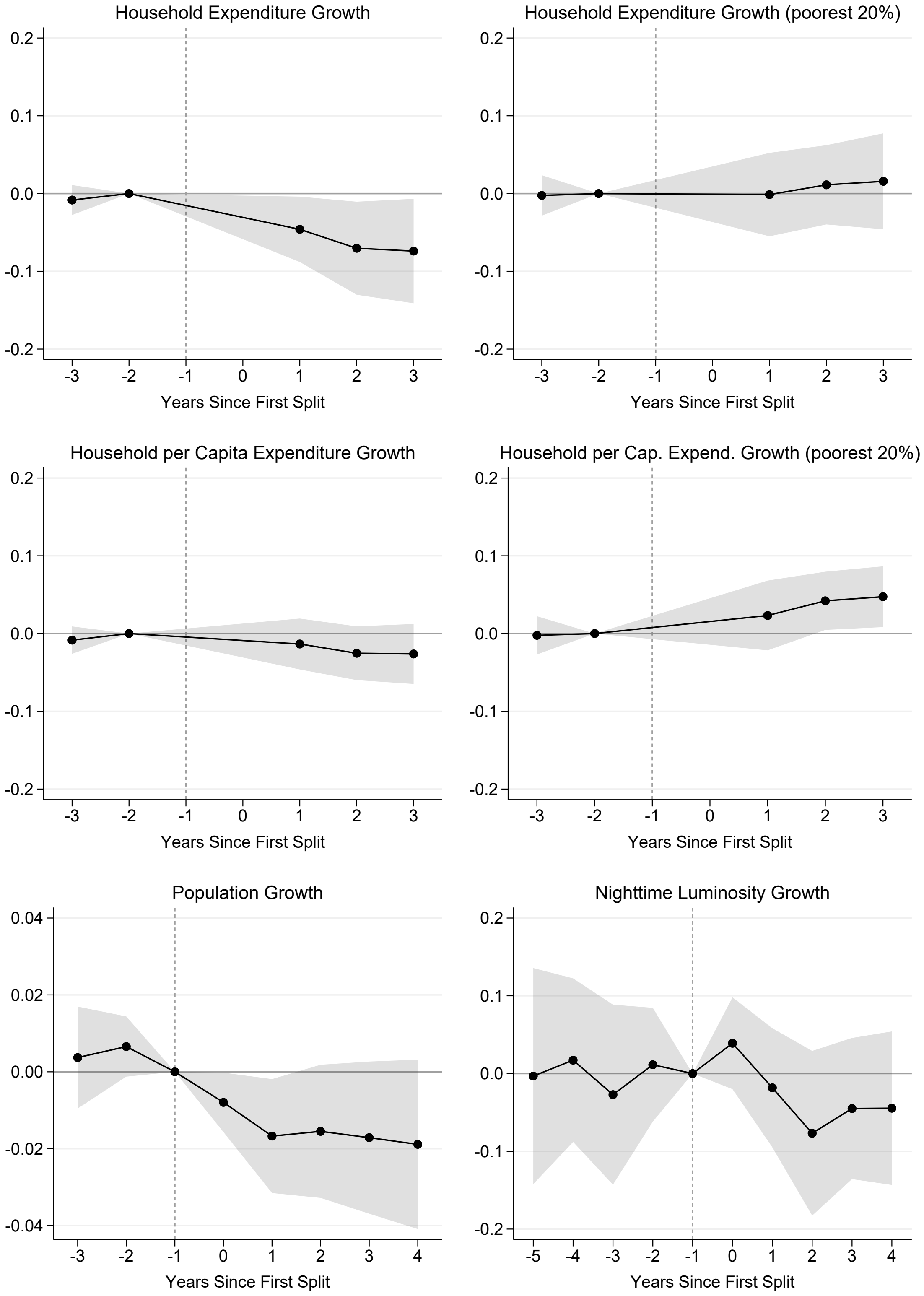


(c) Additional Controls, Trimmed Sample



Notes: This figure plots robustness checks for Figure 2. The estimates in Panels (a) and (c) control for ethnic fractionalization, urbanization rate, share of population aged 15–64, share of population with a primary education, and share of population with a secondary education, all measured in 2000. In Panels (b) and (c) the sample includes districts with urban population share falling within the common support (0 to 0.71).

Figure B.3: The Effect of District Splits on Alternative Measures of Economic Activity



Notes: This figure plots estimates of the cohort-size-weighted CATT and their 95-percent confidence intervals. The estimates for household expenditure and population are based on Equation (1) and are averaged according to Equation (2). The base year is defined as two years before the split for the household expenditure results due to missing data in 2008. The estimates for nighttime luminosity are based on Equation (B.1) and are averaged according to Equation (B.2). The confidence intervals are robust to heteroskedasticity and clustering by district.

Figure B.4: Density of Nighttime Luminosity

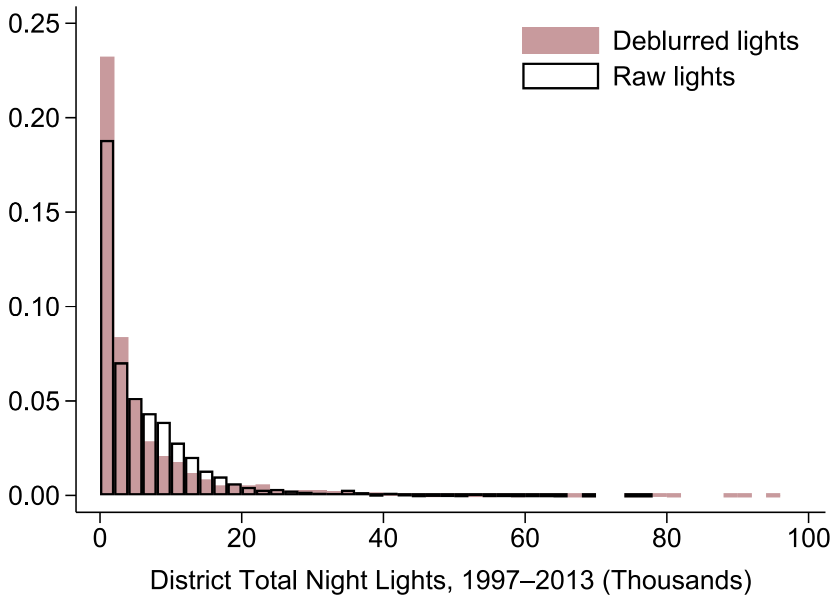
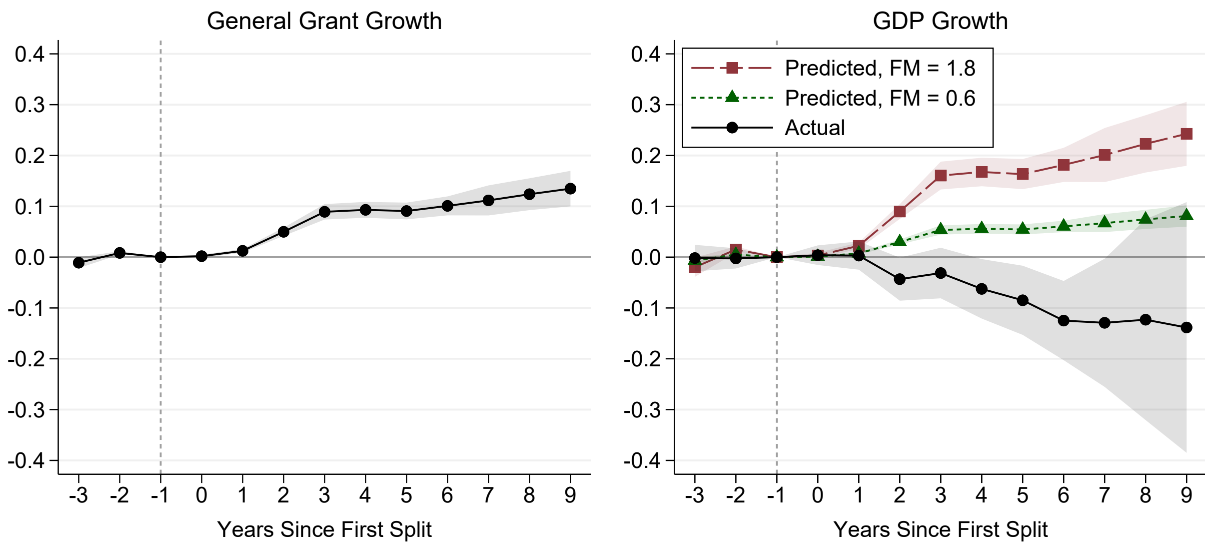
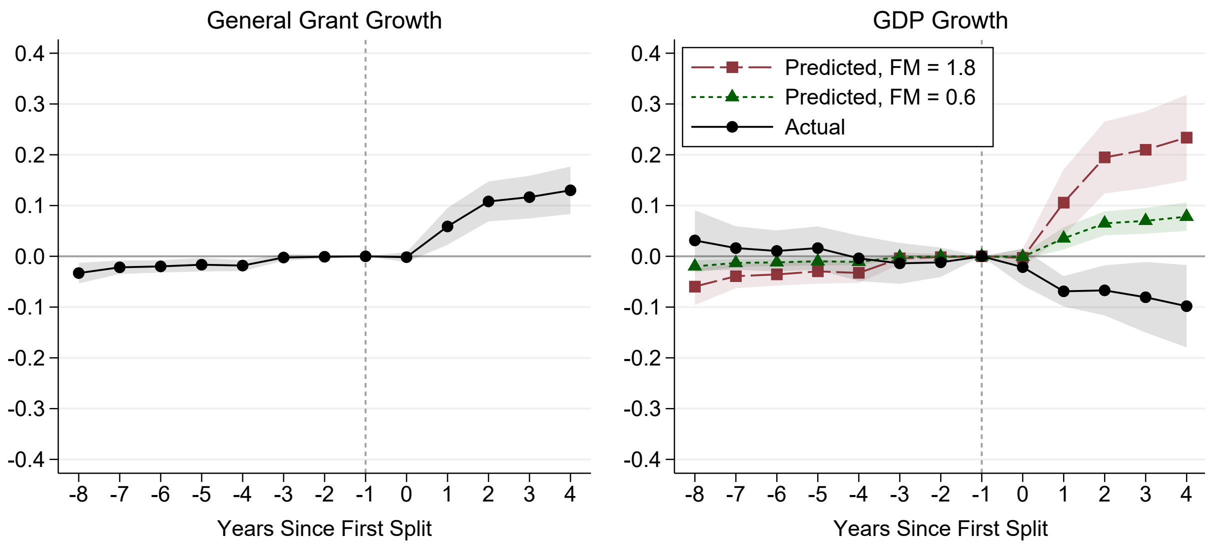


Figure B.5: The Effect of District Splits on General Grant and GDP: Early vs. Late Splits (Baseline Specification)

(a) Early Splits (2002–2004)



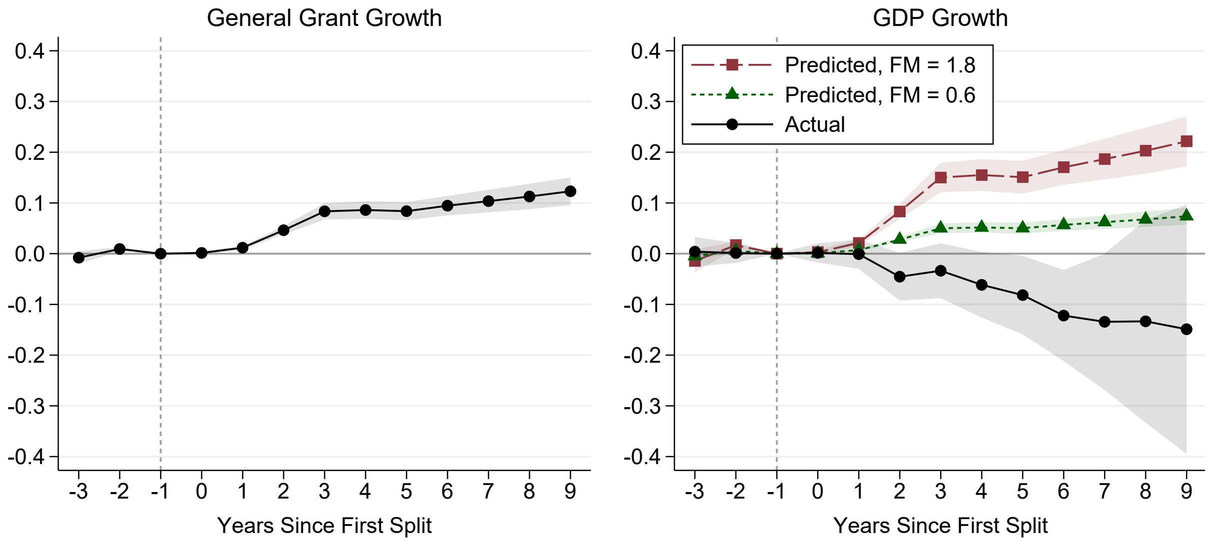
(b) Late Splits (2008–2009)



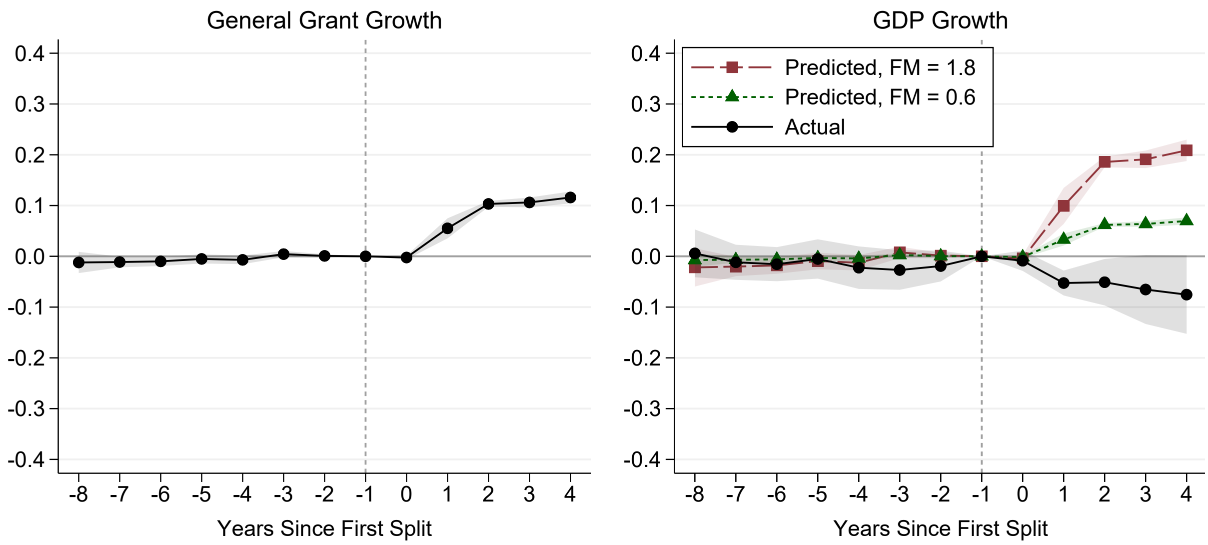
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. (The estimates for $h = -3$ omit the 2002 cohort and adjust the weights accordingly, because the data start in 2000.) The left panel shows the impact of the first district split on growth in general grant revenue relative to the year before the split, scaled by GDP in that year. The right panel shows the impact on GDP growth relative to year before the split as predicted by fiscal multiplier values of 0.6 and 1.8 given the one-for-one increase in expenditure due to the increase in general grants. It also plots the impact on actual GDP growth. The confidence intervals are robust to heteroskedasticity and clustering by district.

Figure B.6: The Effect of District Splits on General Grant and GDP: Early vs. Late Splits (Additional Controls)

(a) Early Splits (2002–2004)



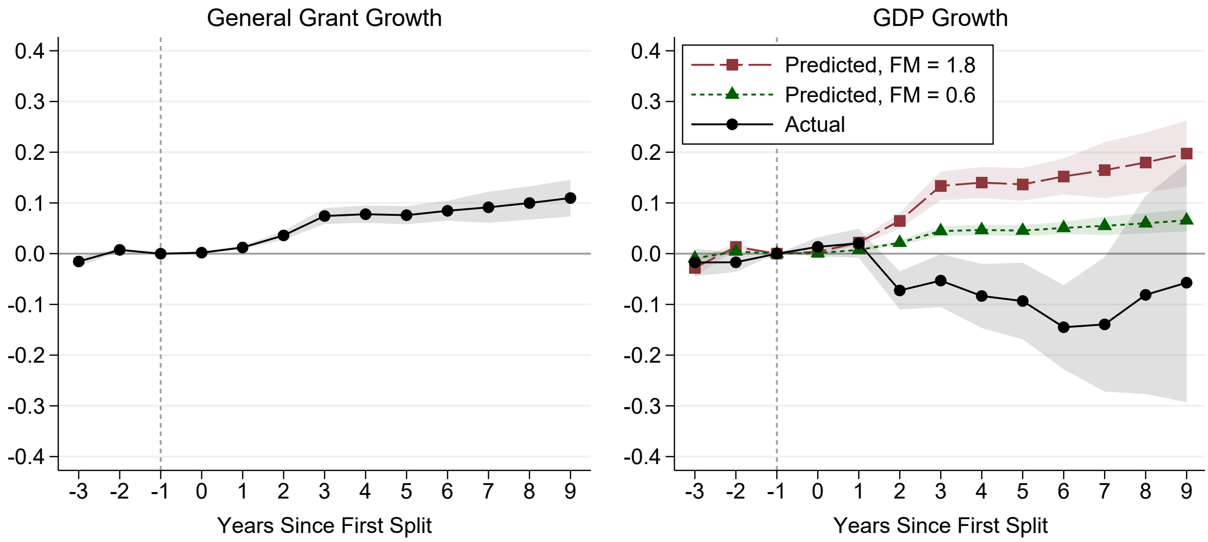
(b) Late Splits (2008–2009)



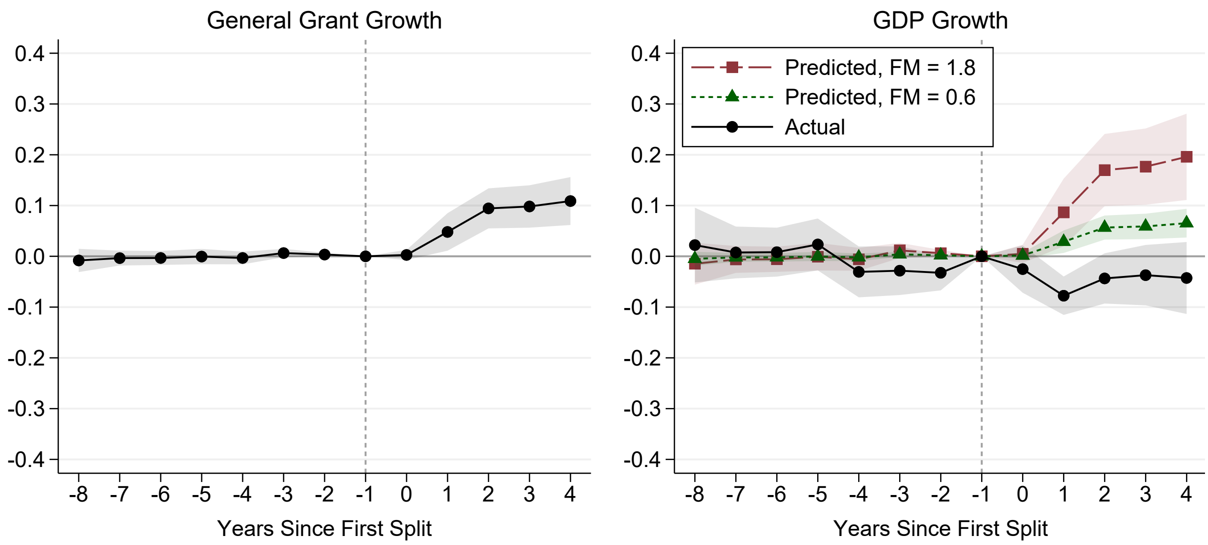
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. (The estimates for $h = -3$ omit the 2002 cohort and adjust the weights accordingly, because the data start in 2000.) The left panel shows the impact of the first district split on growth in general grant revenue relative to the year before the split, scaled by GDP in that year. The right panel shows the impact on GDP growth relative to year before the split as predicted by fiscal multiplier values of 0.6 and 1.8 given the one-for-one increase in expenditure due to the increase in general grants. It also plots the impact on actual GDP growth. The confidence intervals are robust to heteroskedasticity and clustering by district.

Figure B.7: The Effect of District Splits on General Grant and GDP: Early vs. Late Splits (Trimmed Sample)

(a) Early Splits (2002–2004)



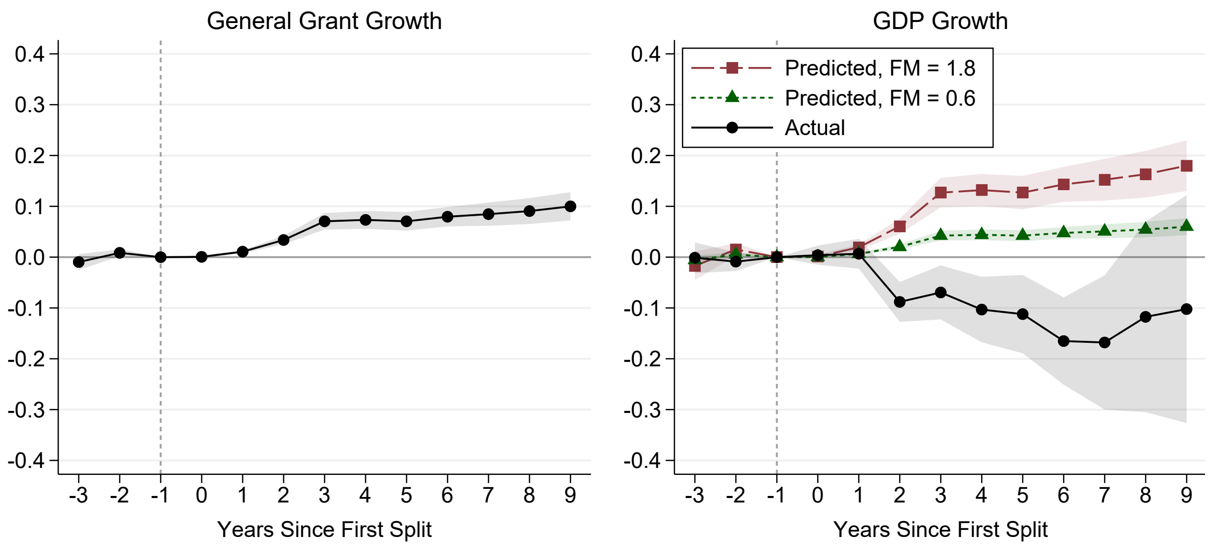
(b) Late Splits (2008–2009)



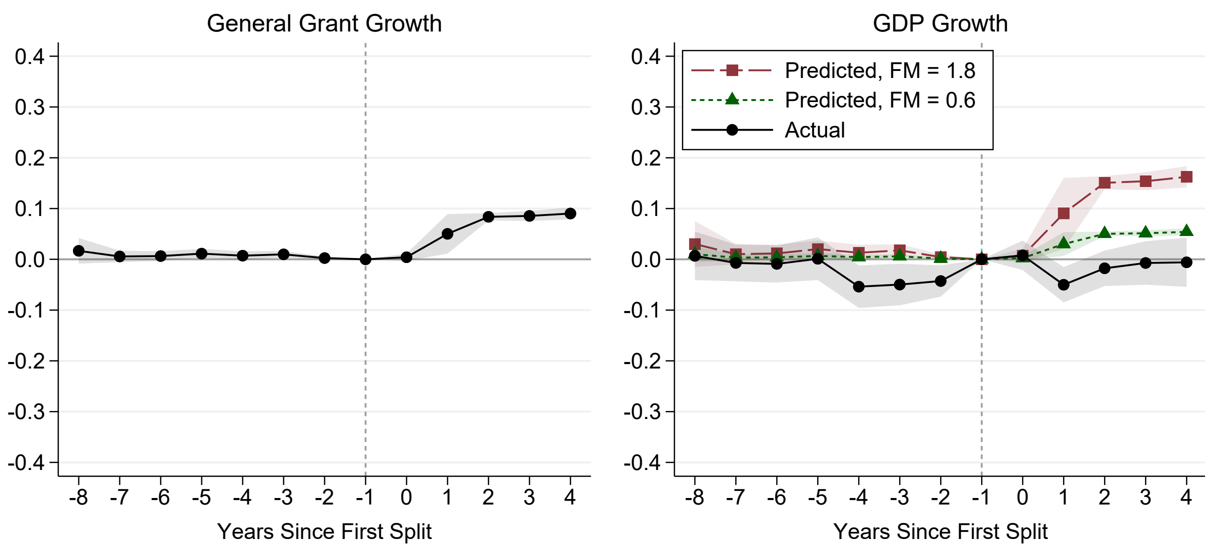
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. (The estimates for $h = -3$ omit the 2002 cohort and adjust the weights accordingly, because the data start in 2000.) The left panel shows the impact of the first district split on growth in general grant revenue relative to the year before the split, scaled by GDP in that year. The right panel shows the impact on GDP growth relative to year before the split as predicted by fiscal multiplier values of 0.6 and 1.8 given the one-for-one increase in expenditure due to the increase in general grants. It also plots the impact on actual GDP growth. The confidence intervals are robust to heteroskedasticity and clustering by district.

Figure B.8: The Effect of District Splits on General Grant and GDP: Early vs. Late Splits (Additional Controls, Trimmed Sample)

(a) Early Splits (2002–2004)

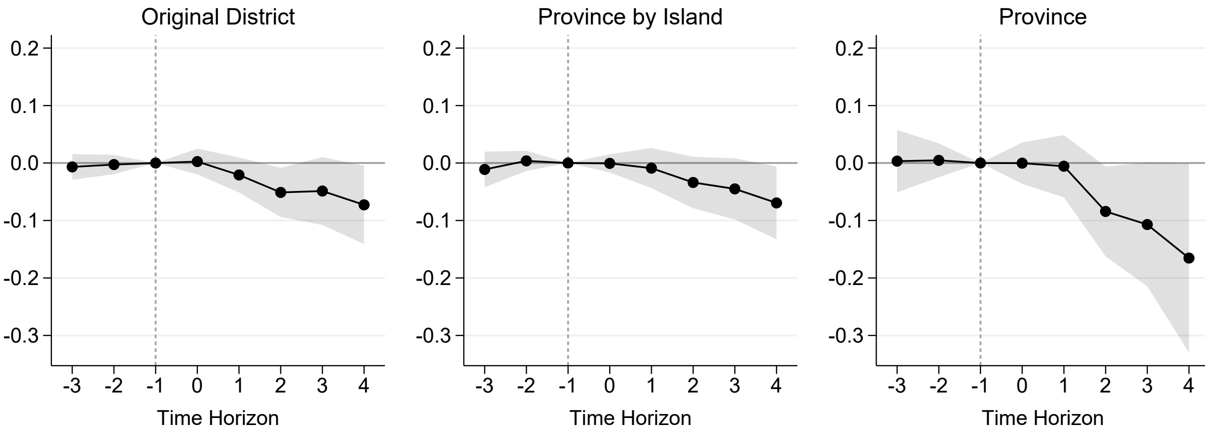


(b) Late Splits (2008–2009)



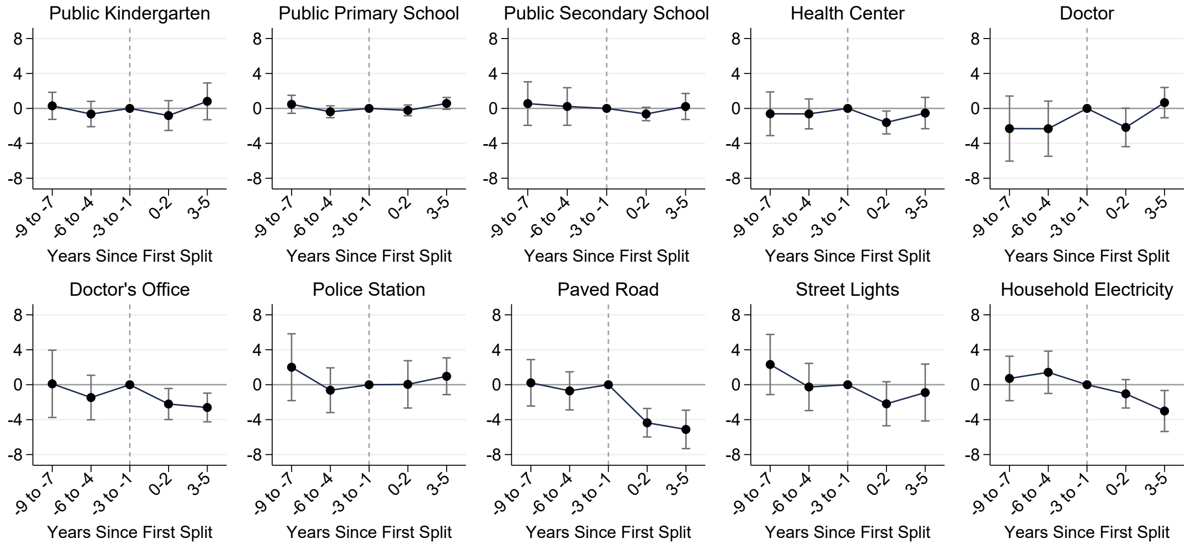
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. (The estimates for $h = -3$ omit the 2002 cohort and adjust the weights accordingly, because the data start in 2000.) The left panel shows the impact of the first district split on growth in general grant revenue relative to the year before the split, scaled by GDP in that year. The right panel shows the impact on GDP growth relative to year before the split as predicted by fiscal multiplier values of 0.6 and 1.8 given the one-for-one increase in expenditure due to the increase in general grants. It also plots the impact on actual GDP growth. The confidence intervals are robust to heteroskedasticity and clustering by district.

Figure B.9: Growth Effects of Doubling the Number of Districts at Different Levels of Aggregation



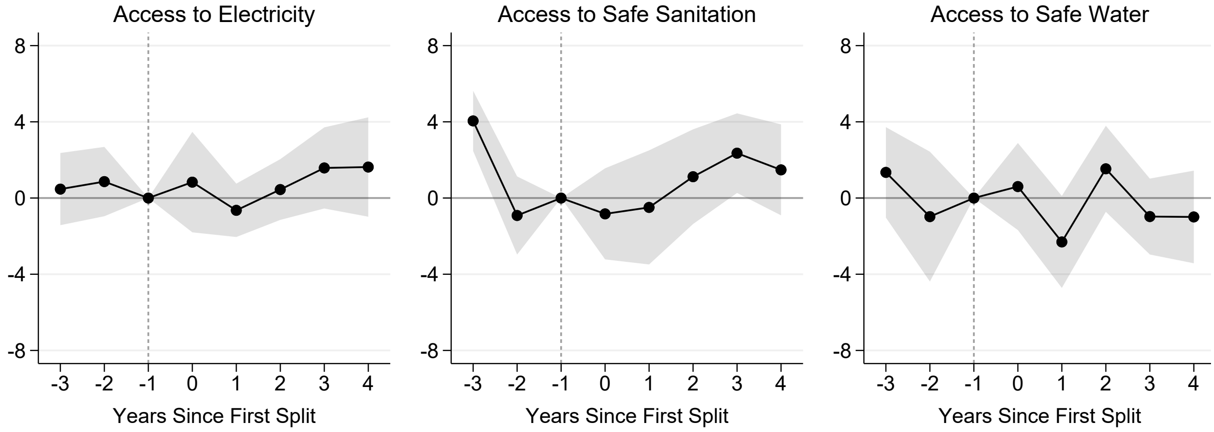
Notes: This figure plots point estimates and 95-percent confidence intervals of $\ln(2) \cdot \beta_h$ from Equation (3), the effect of doubling the number of districts in year t on GDP in year $t+h$. The model is estimated at three different levels of aggregation, as indicated. The confidence intervals are robust to heteroskedasticity and clustering by geographic unit.

Figure B.10: The Effect of District Splits on Access to Public Services (PODES)



Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. (The estimates for $h \in \{-9, -8, -7\}$ omit the 2002 cohort and adjust the weights accordingly, because the data start in 1996.) All outcome variables come from PODES. The first nine outcomes are the percentage of villages that have the public service, and the last outcome is the percentage of households with electricity. Each outcome is measured in terms of its change from year $t-1$ to year $t+h$. The estimates use a balanced panel of 329 districts and control for region dummies, ethnic fractionalization, urbanization rate, share of population aged 15–64, share of population with a primary education, and share of population with a secondary education, all measured in 2000. The confidence intervals are robust to heteroskedasticity and clustering by district.

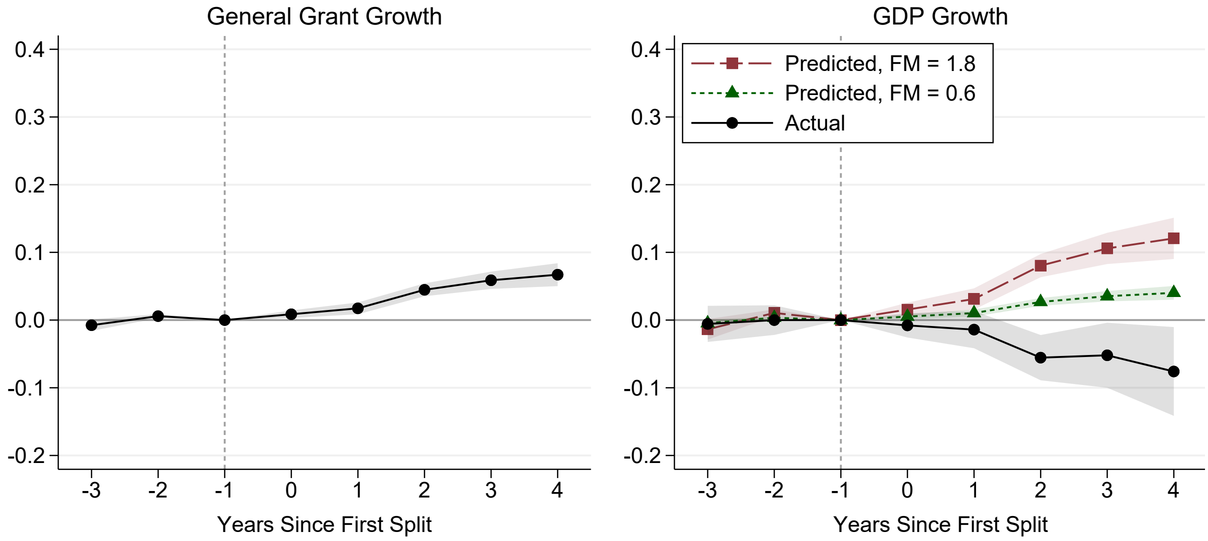
Figure B.11: The Effect of District Splits on Household Access to Infrastructure (SUSENAS)



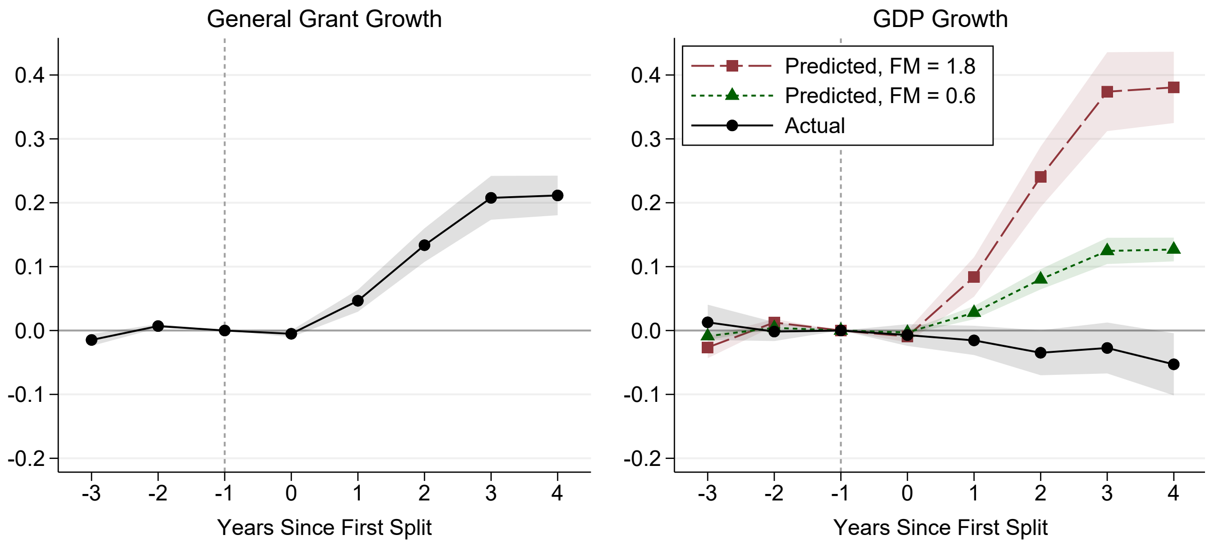
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. All outcome variables come from SUSENAS and are aggregated and disseminated by INDO-DAPOER. Household electricity access is missing in 2005 for all districts. We fill in these missing values with the average of the district values in 2004 and 2006. The estimates use an unbalanced panel of 318 districts and control for region dummies, ethnic fractionalization, urbanization rate, share of population aged 15–64, share of population with a primary education, and share of population with a secondary education, all measured in 2000. The confidence intervals are robust to heteroskedasticity and clustering by district.

Figure B.12: The Effect of District Splits on General Grant and GDP: Parent vs. Child Districts

(a) Parent Districts

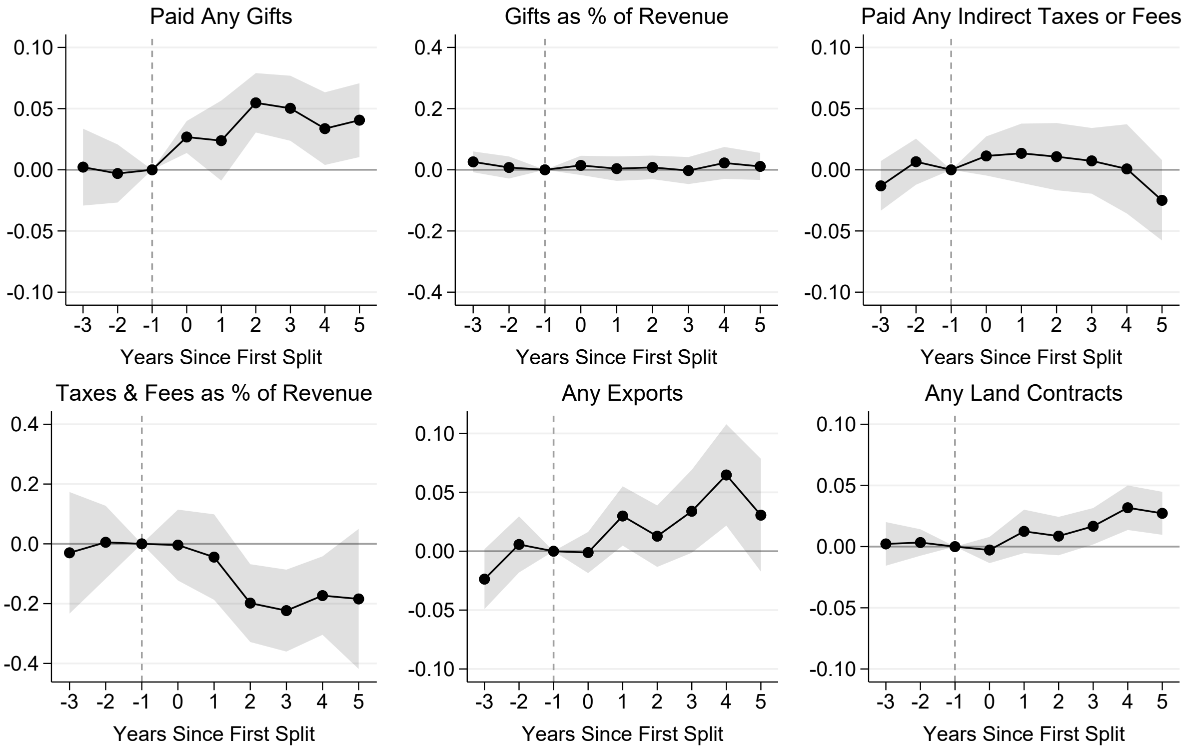


(b) Child Districts



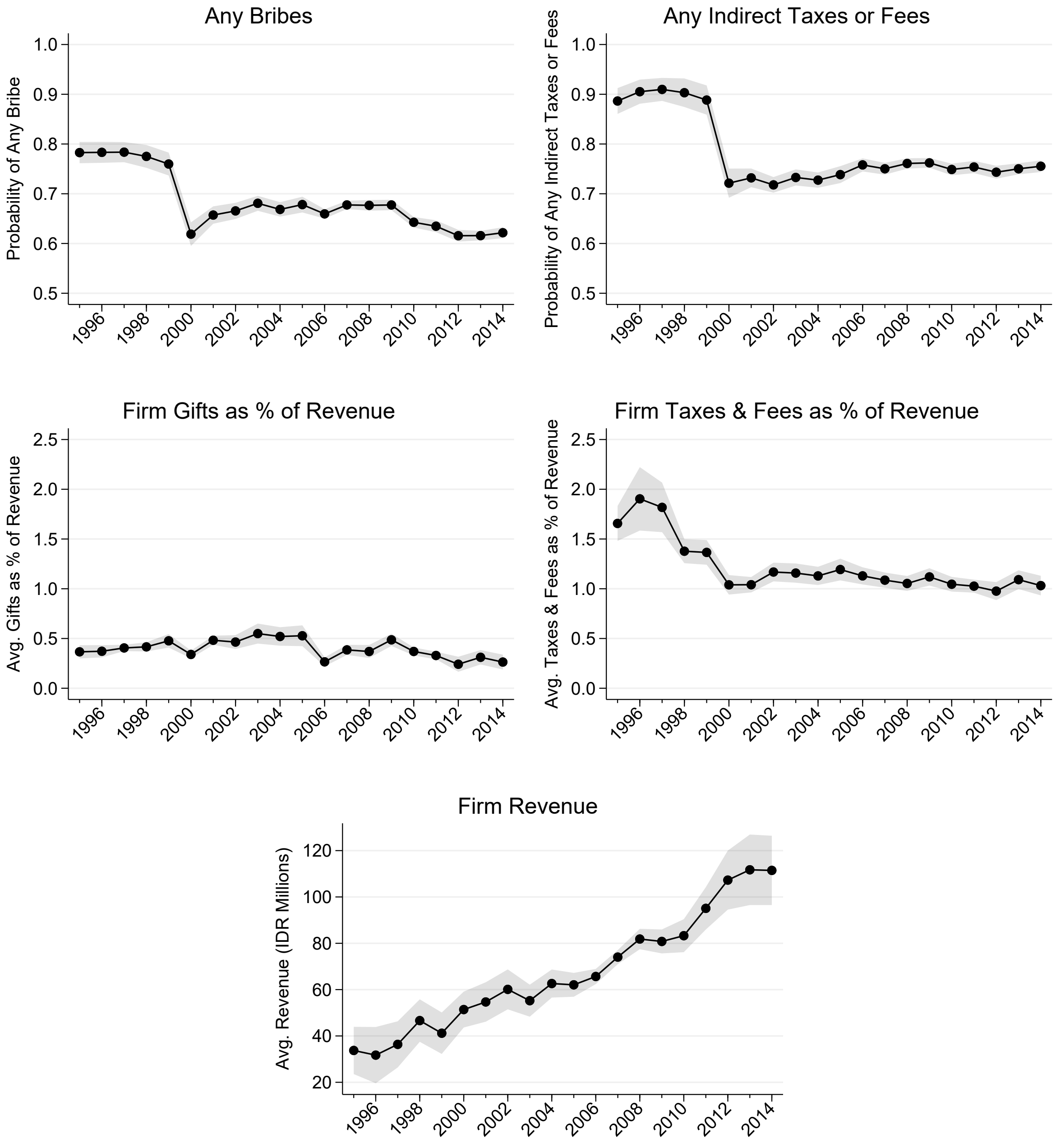
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. (The estimates for $h = -3$ omit the 2002 cohort and adjust the weights accordingly, because the data start in 2000.) The left panel shows the impact of the first district split on growth in general grant revenue relative to the year before the split, scaled by GDP in that year. The right panel shows the impact on GDP growth relative to year before the split as predicted by fiscal multiplier values of 0.6 and 1.8 given the one-for-one increase in expenditure due to the increase in general grants. It also plots the impact on actual GDP growth. The confidence intervals are robust to heteroskedasticity and clustering by district.

Figure B.13: The Effect of District Splits on Gifts, Taxes, and Regulated Activities



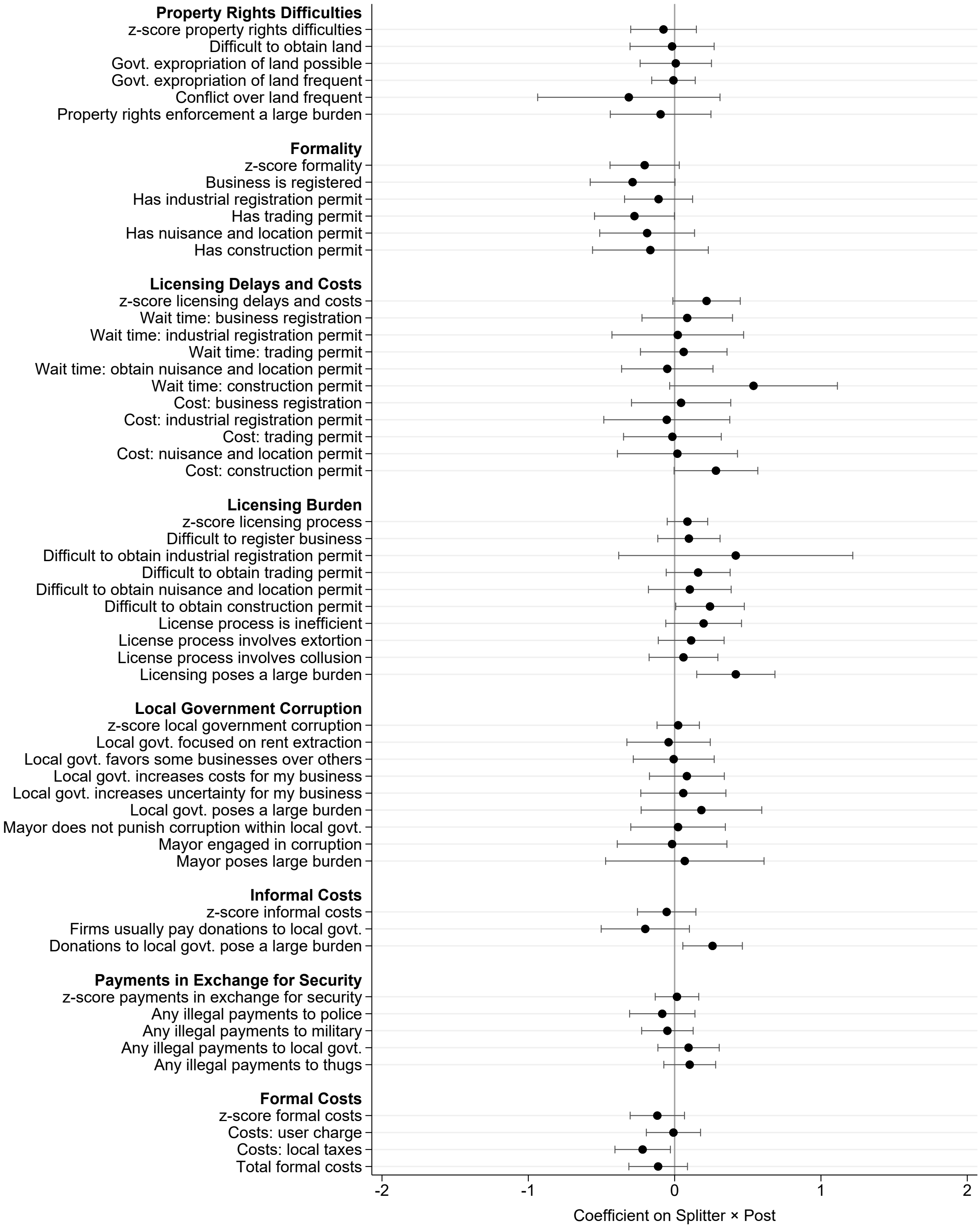
Notes: This figure plots estimates of the cohort-size-weighted CATT, using Equations (2) and (4), and their 95-percent confidence intervals. (The estimates for $h = -3$ omit the 2002 cohort and adjust the weights accordingly, because the data appear unreliable prior to 2000.) The confidence intervals are robust to heteroskedasticity and clustering by district.

Figure B.14: Trends in Firm-Level Variables from IBS, 1995–2014



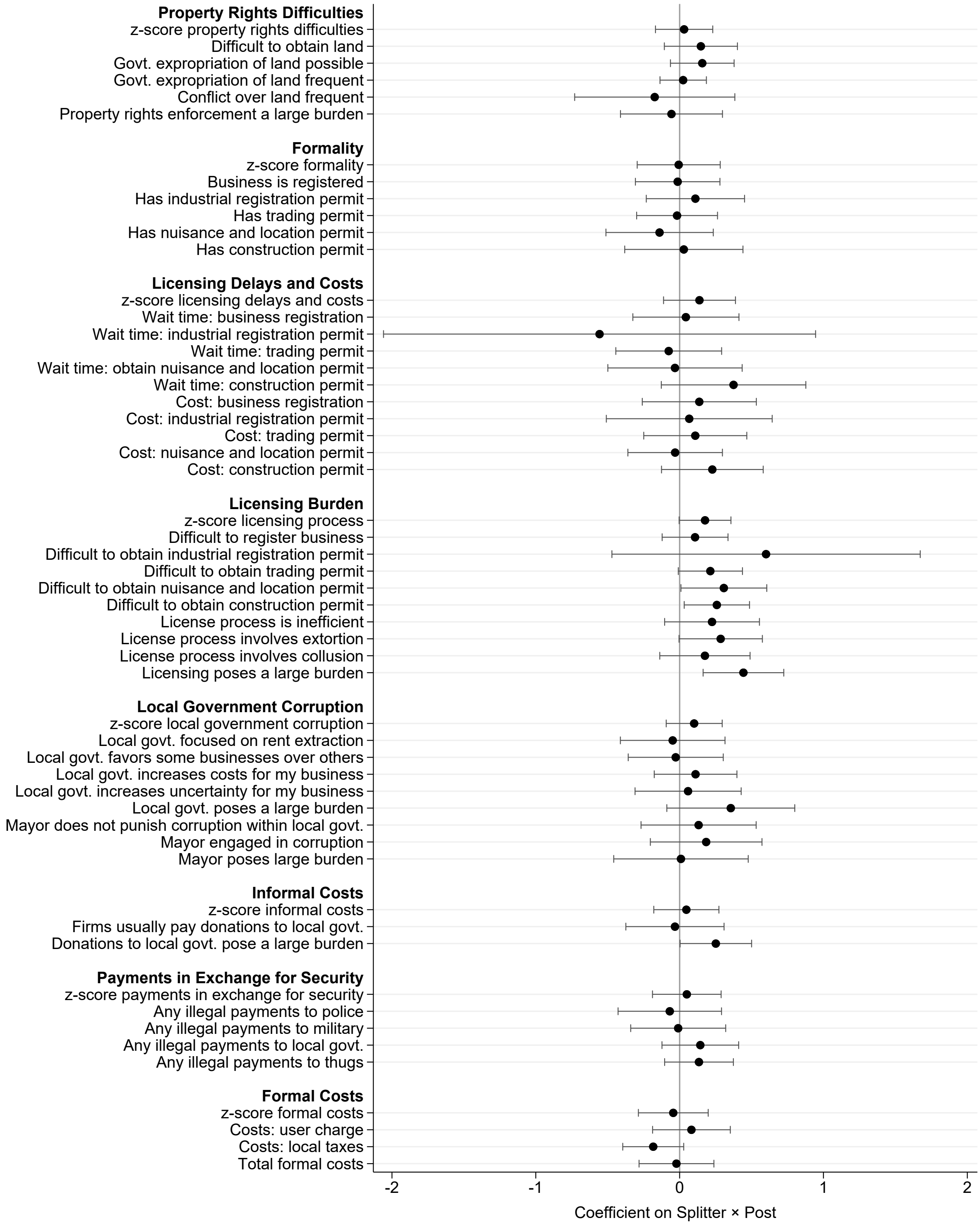
Notes: This figure plots the fitted values of a regression of the outcome on year dummies, controlling for firm fixed effects. Standard errors are clustered by district, and 95-percent confidence intervals are reported.

Figure B.15: The Effect of District Splits on Economic Governance



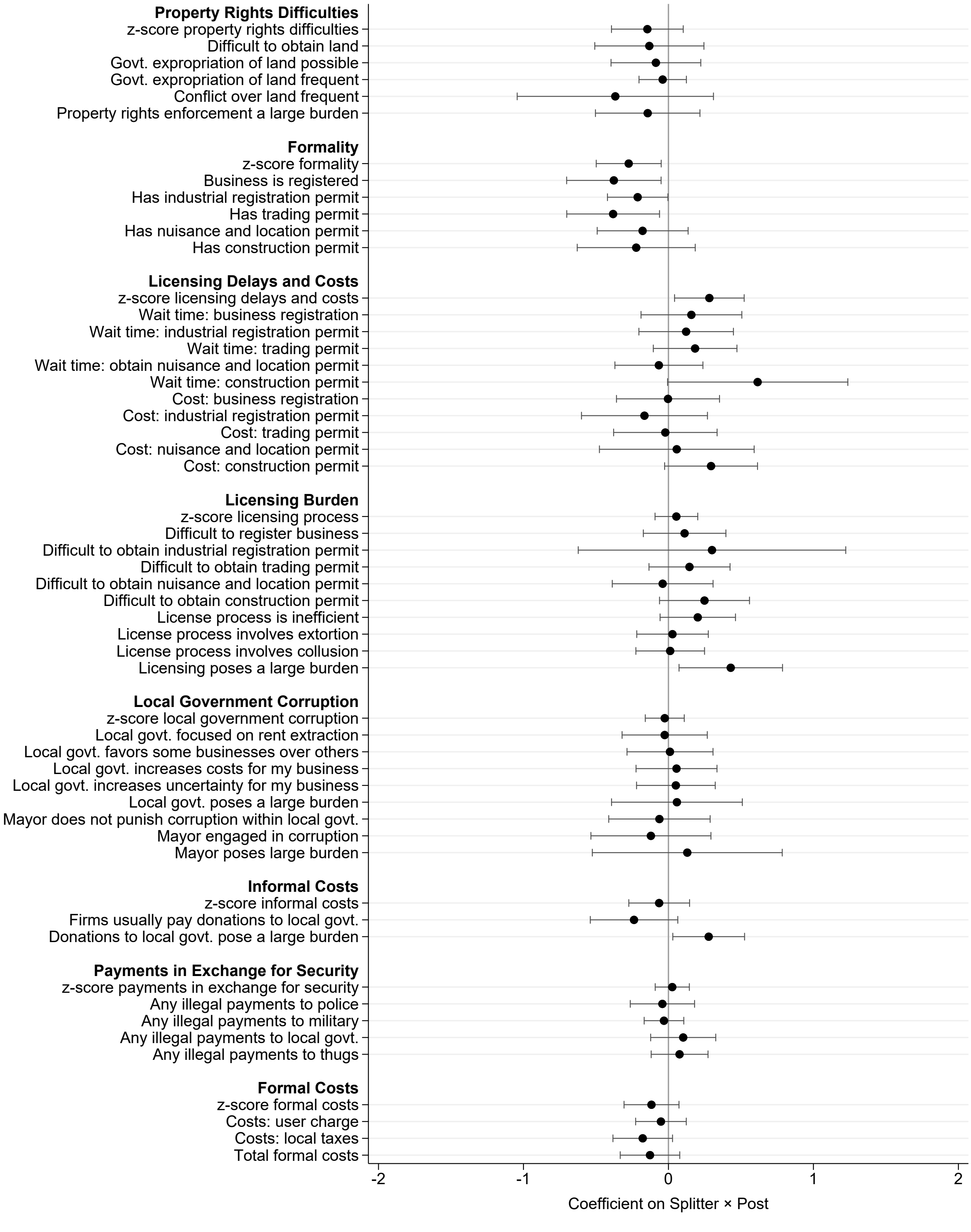
Notes: This figure plots estimates of β_3 in $Y_{f,d,t} = \beta_0 + \beta_1 Splitter_d + \beta_2 Post_t + \beta_3 Splitter_d \times Post_t + \lambda_{r(d),t} + \varepsilon_{f,d,t}$ and 95-percent confidence intervals, using data from the Economic Governance Survey. All outcomes are standardized to have a mean of zero and a standard deviation of one.

Figure B.16: The Effect of District Splits on the Business Environment (20+ Employees)



Notes: This figure plots estimates of β_3 in $Y_{f,d,t} = \beta_0 + \beta_1 Splitter_d + \beta_2 Post_t + \beta_3 Splitter_d \times Post_t + \lambda_{r(d),t} + \varepsilon_{f,d,t}$ and 95-percent confidence intervals, using the subsample of firms with 20 or more employees. All outcomes are standardized to have a mean of zero and a standard deviation of one.

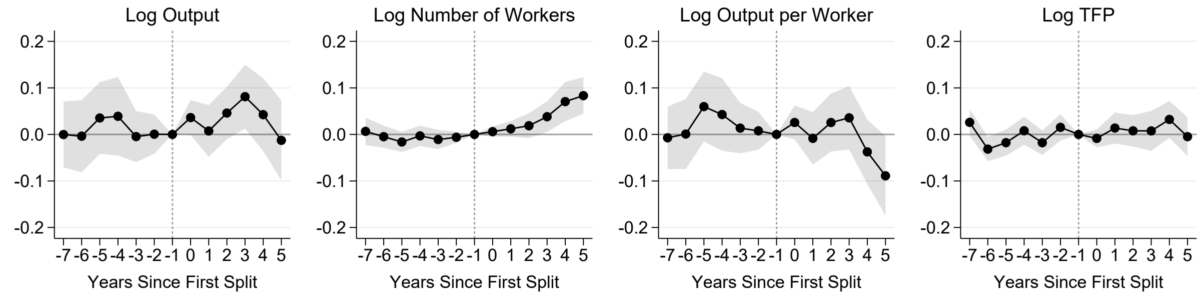
Figure B.17: The Effect of District Splits on the Business Environment (1–19 Employees)



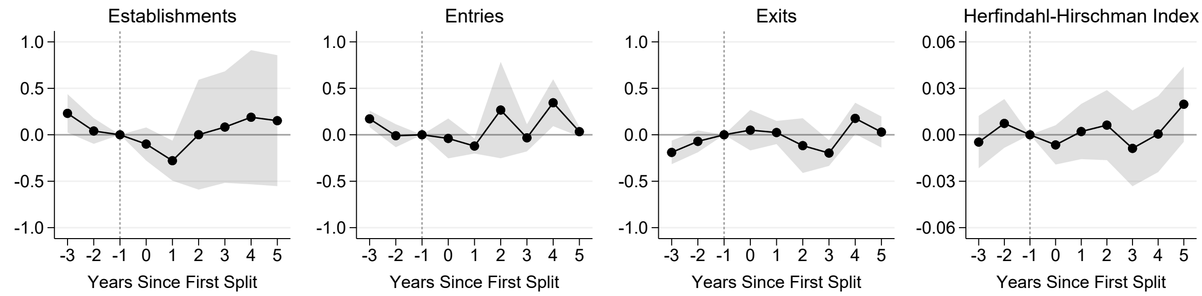
Notes: This figure plots estimates of β_3 in $Y_{f,d,t} = \beta_0 + \beta_1 Splitter_d + \beta_2 Post_t + \beta_3 Splitter_d \times Post_t + \lambda_{r(d),t} + \varepsilon_{f,d,t}$ and 95-percent confidence intervals, using the subsample of firms with fewer than 20 employees. All outcomes are standardized to have a mean of zero and a standard deviation of one.

Figure B.18: The Effect of District Splits on Manufacturing Productivity and Competition

(a) Firm-Level Outcomes



(b) Market-Level Outcomes



Notes: This figure plots estimates of the cohort-size-weighted CATT, using Equations (2) and (4), and their 95-percent confidence intervals. The confidence intervals are robust to heteroskedasticity and clustering by district.

Appendix C References

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