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

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

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)

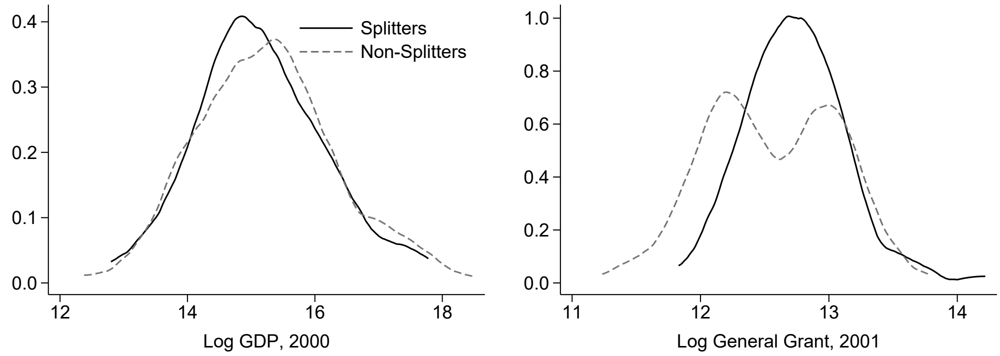
	Average Value by Predicted Treatment Effect Quintile				
	(1)	(2)	(3)	(4)	(5)
Predicted Treatment Effect	−0.36	−0.15	−0.11	−0.07	0.01
	(0.10)	(0.02)	(0.01)	(0.01)	(0.04)
Covariates:					
Urbanization Rate	0.24	0.24	0.28	0.30	0.19
	(0.17)	(0.15)	(0.17)	(0.16)	(0.11)
Ethnic Fractionalization	0.73	0.29	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.

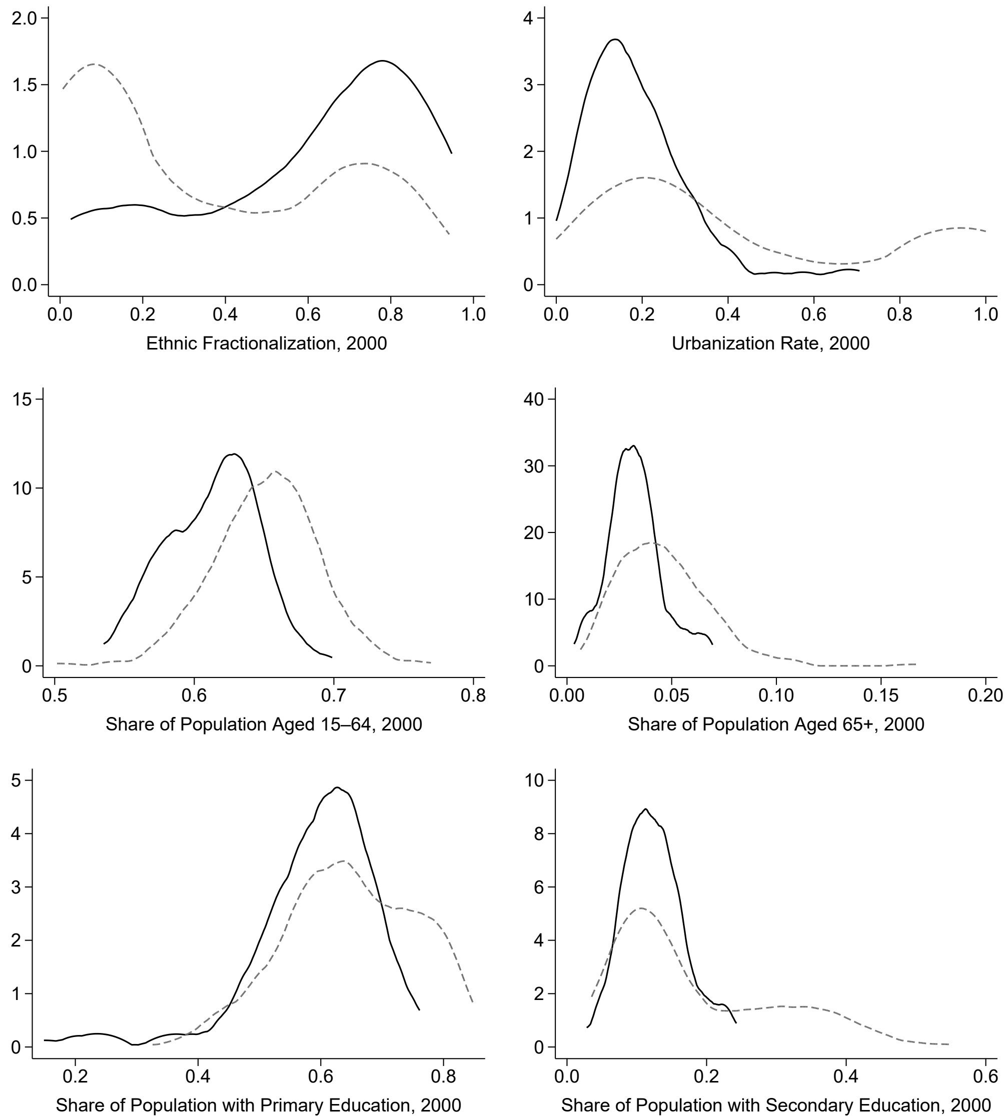
B.5 Figures

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

(a) Baseline Outcomes



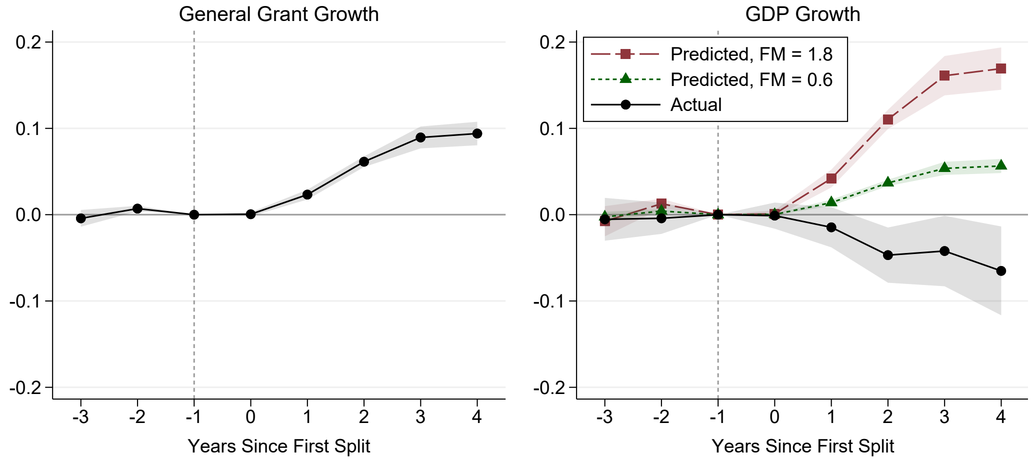
(b) Baseline Covariates



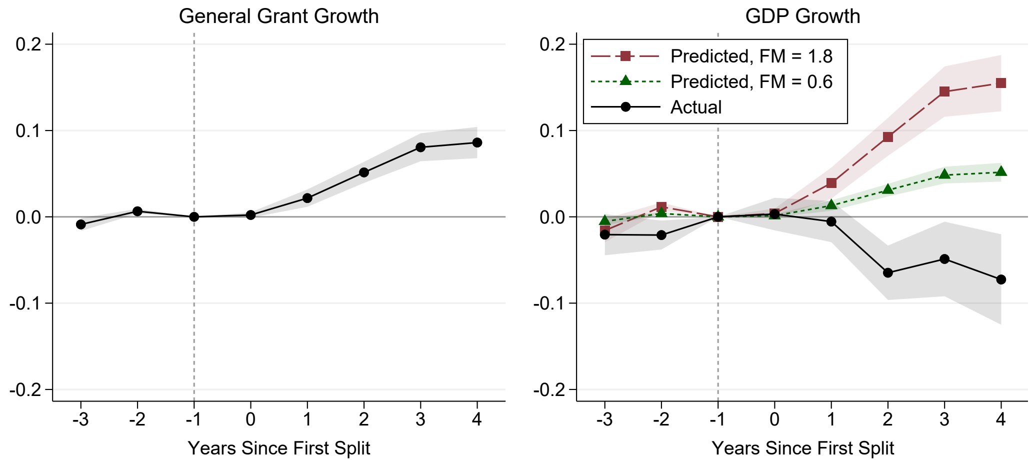
Notes: Densities are estimated using the Epanechnikov kernel.

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

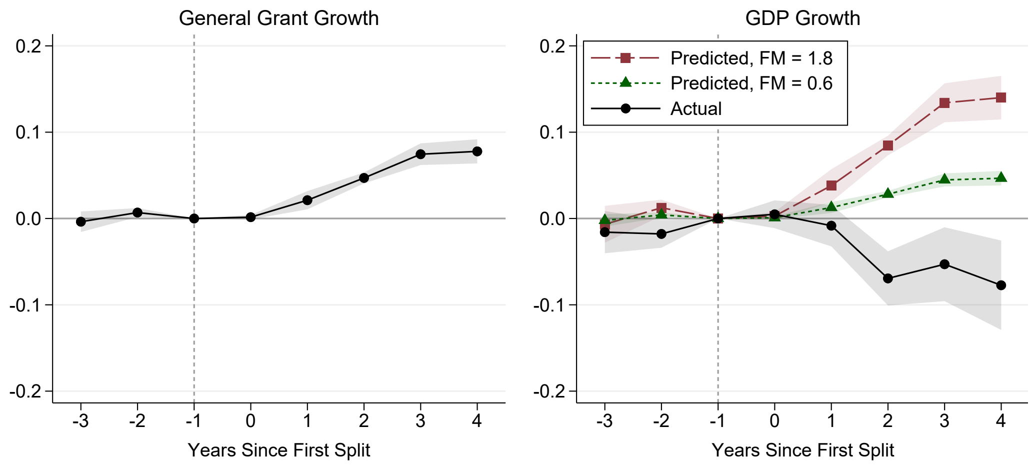
(a) Additional Controls



(b) Trimmed Sample

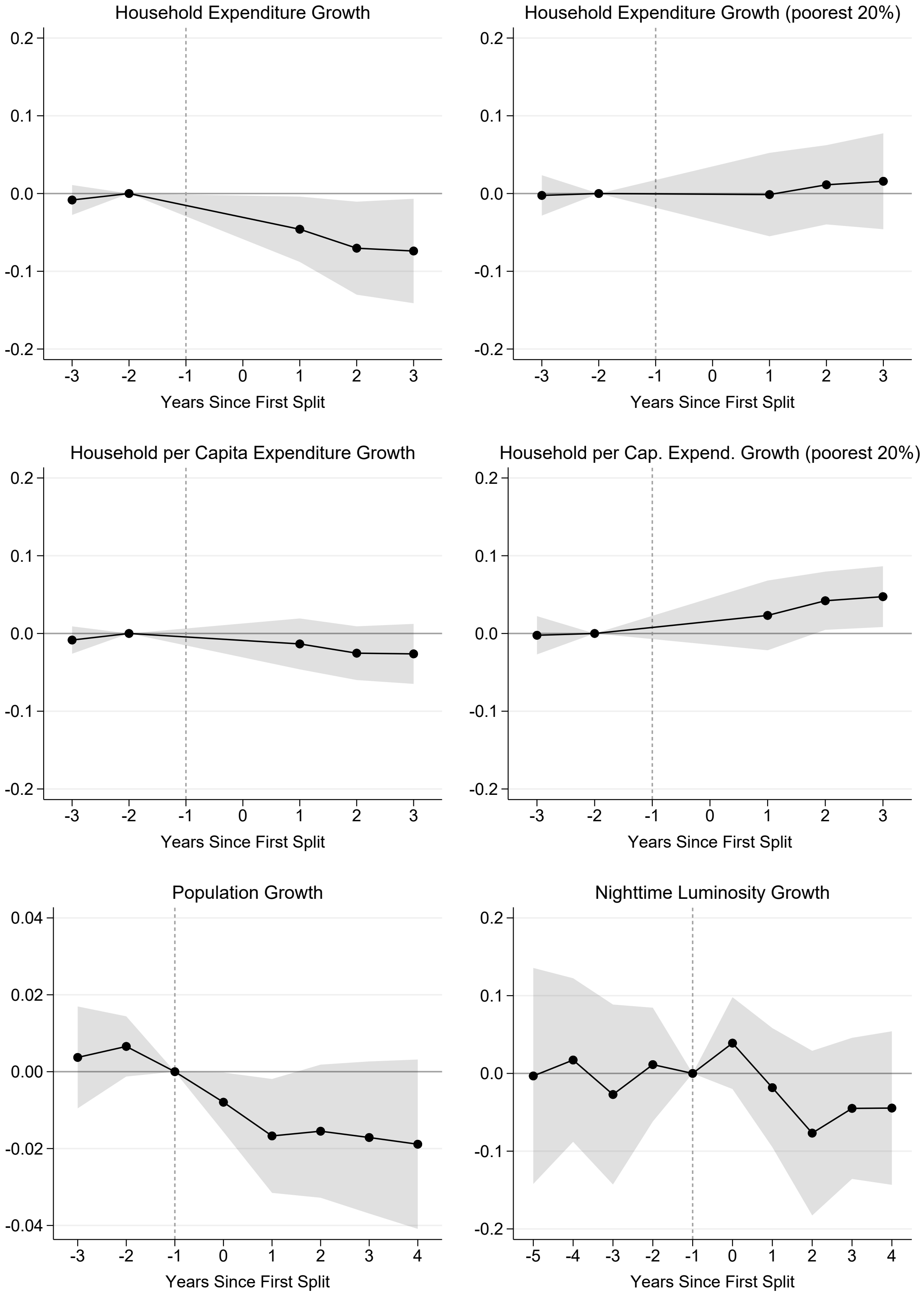


(c) Additional Controls, Trimmed Sample



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

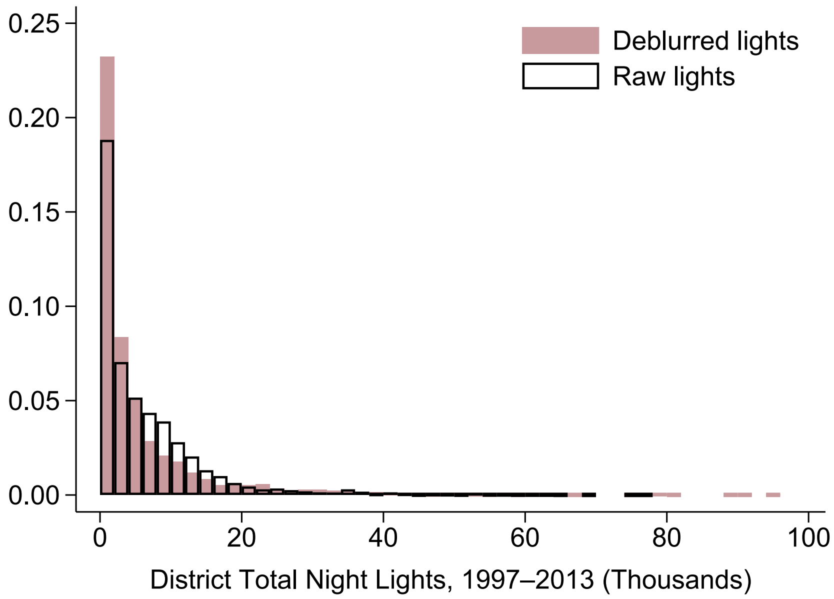
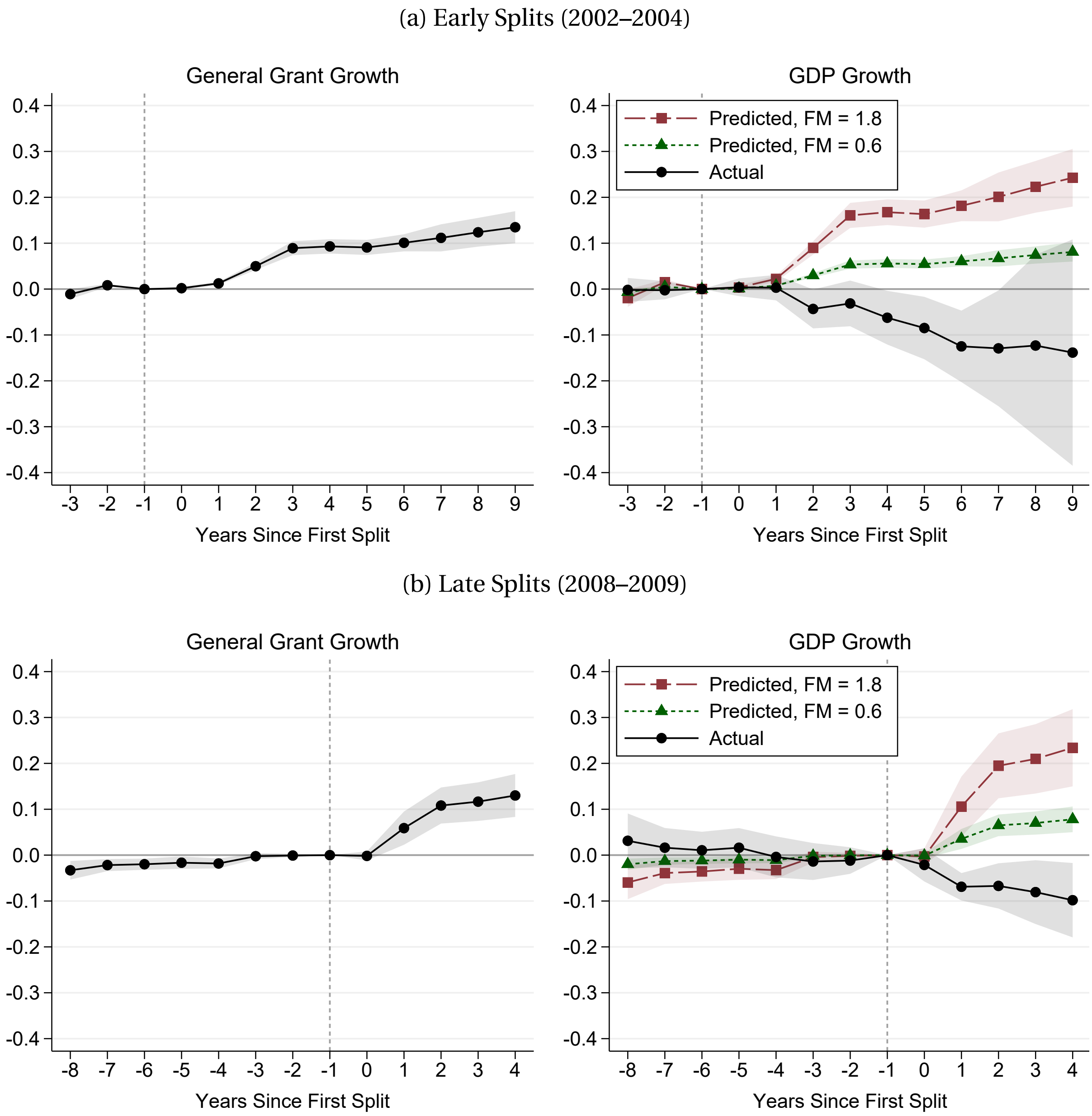


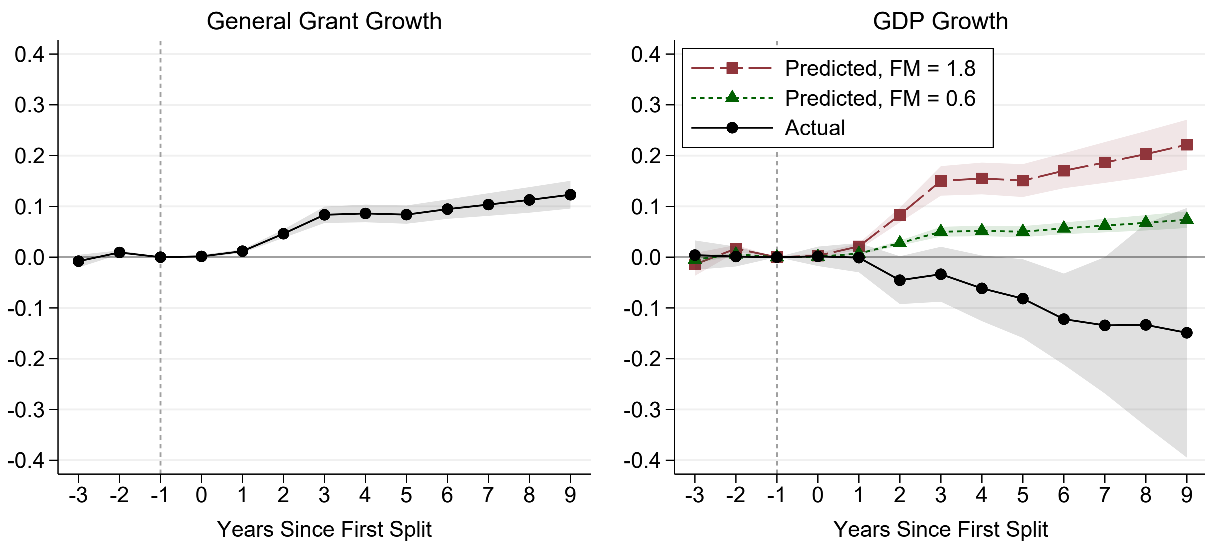
Figure B.5: The Effect of District Splits on General Grant and GDP: Early vs. Late Splits (Baseline Specification)



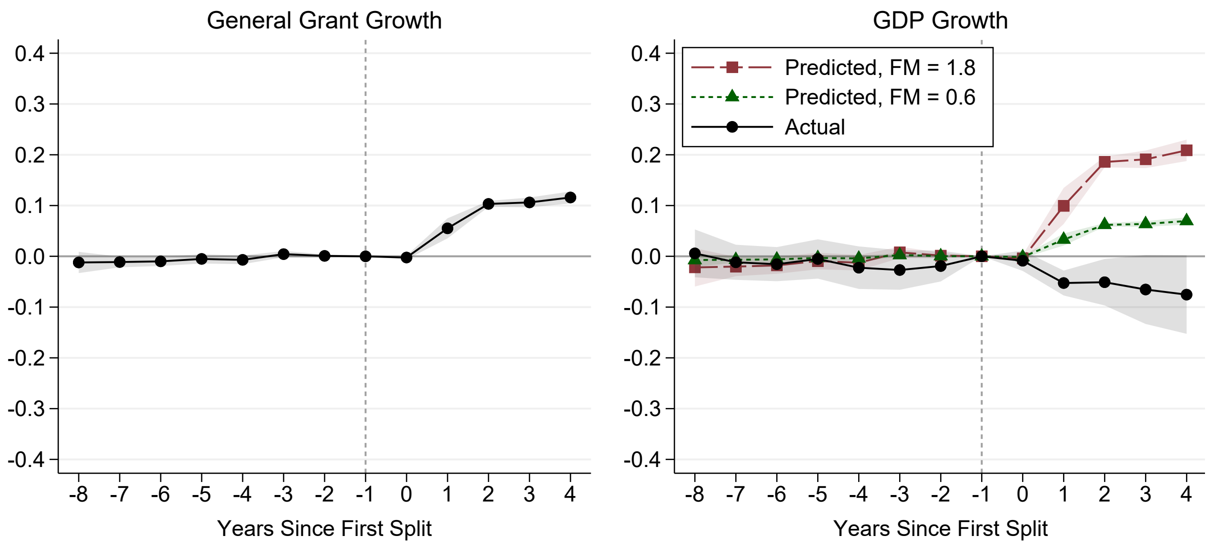
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. (The estimates for $h = -3$ omit the 2002 cohort and adjust the weights accordingly, because the data start in 2000.) The left panel shows the impact of the first district split 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)



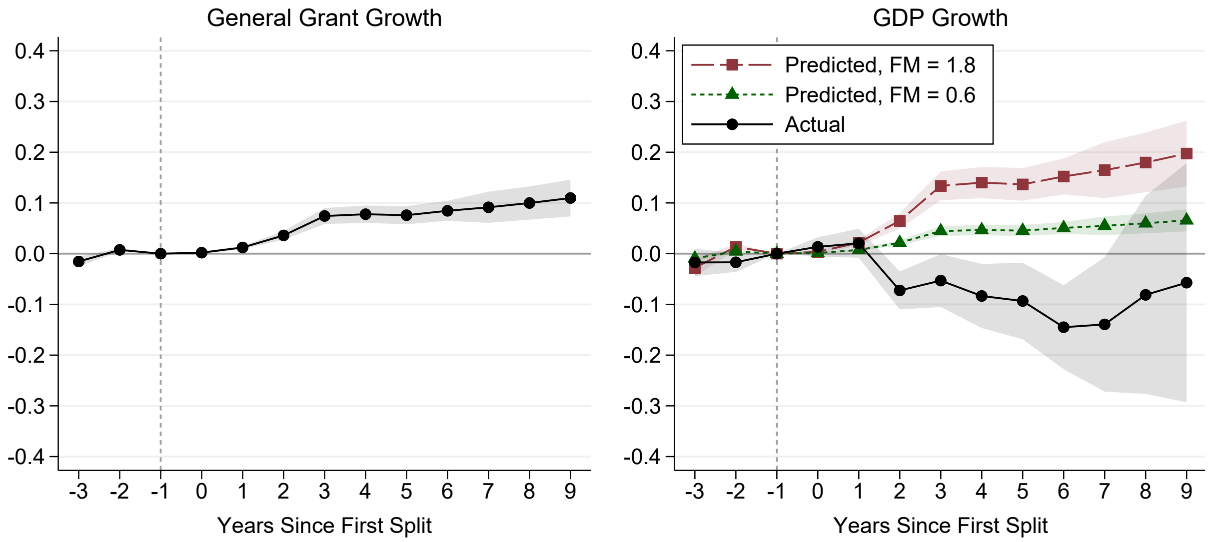
(b) Late Splits (2008–2009)



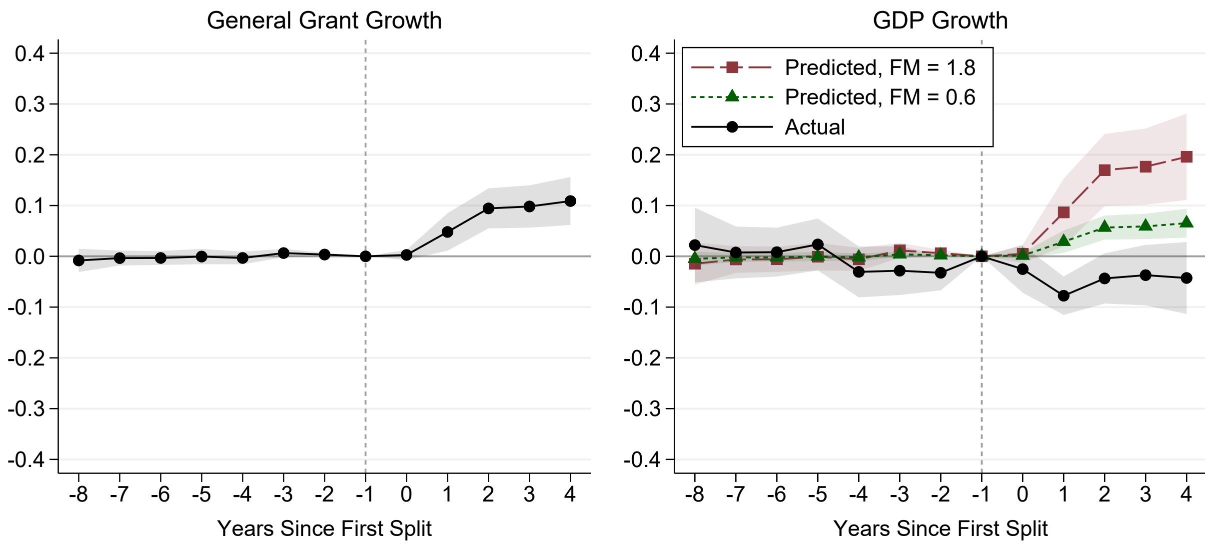
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. (The estimates for $h = -3$ omit the 2002 cohort and adjust the weights accordingly, because the data start in 2000.) The left panel shows the impact of the first district split 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)



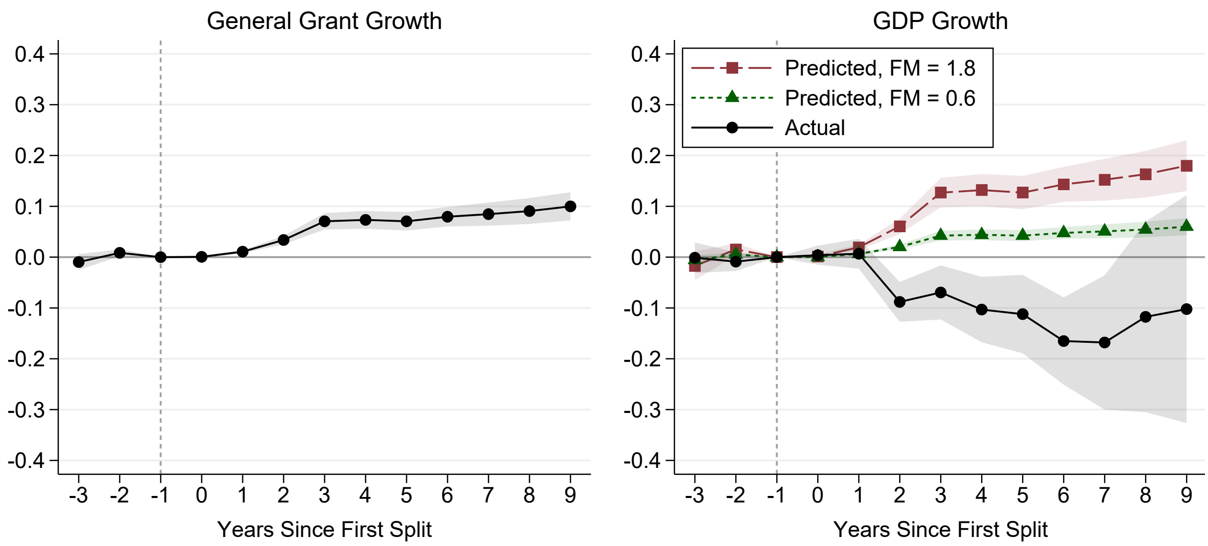
(b) Late Splits (2008–2009)



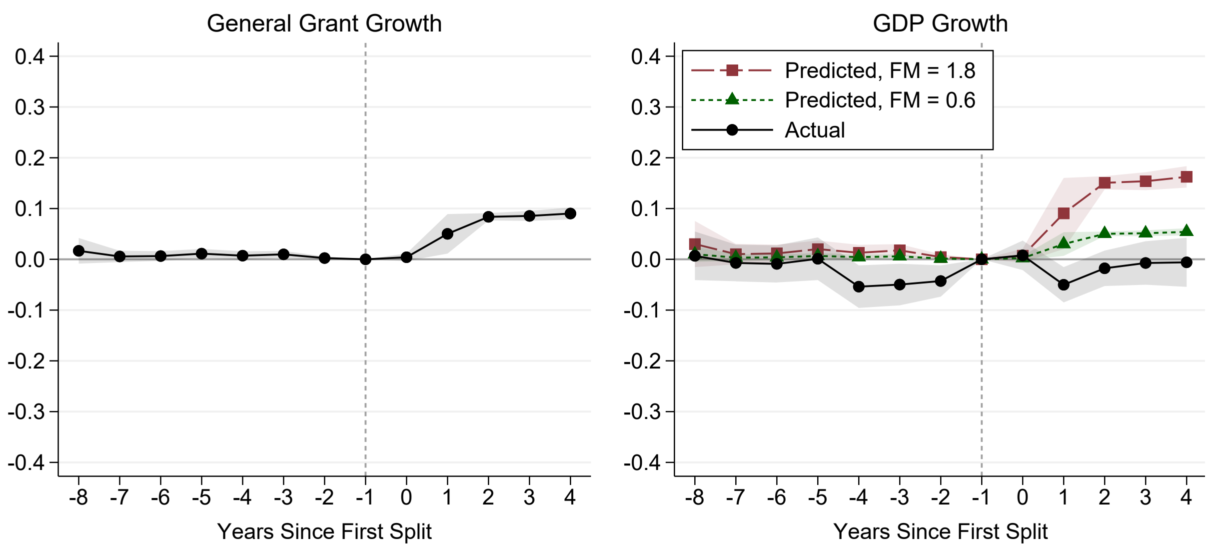
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. (The estimates for $h = -3$ omit the 2002 cohort and adjust the weights accordingly, because the data start in 2000.) The left panel shows the impact of the first district split 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)

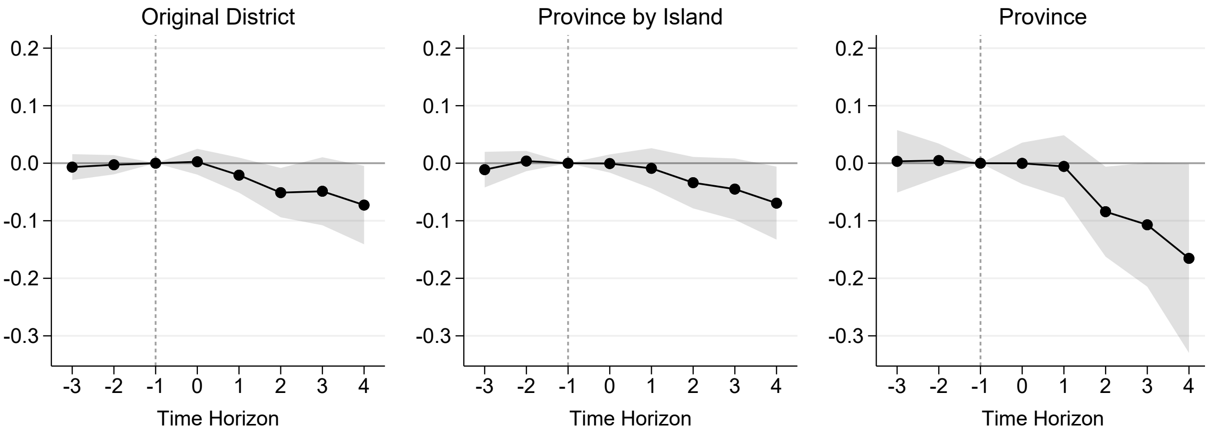


(b) Late Splits (2008–2009)



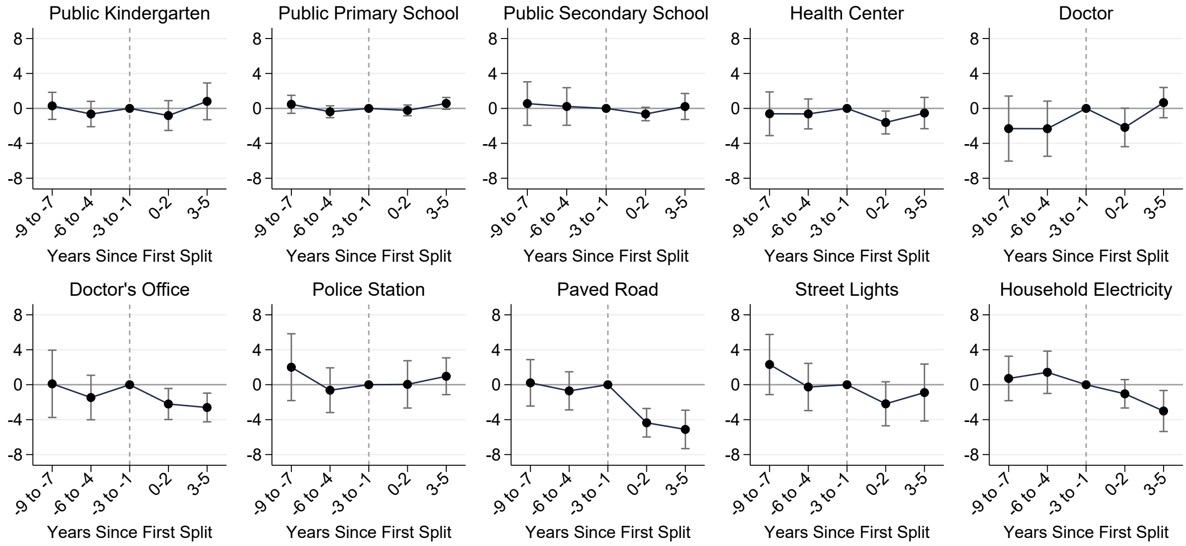
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. (The estimates for $h = -3$ omit the 2002 cohort and adjust the weights accordingly, because the data start in 2000.) The left panel shows the impact of the first district split 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



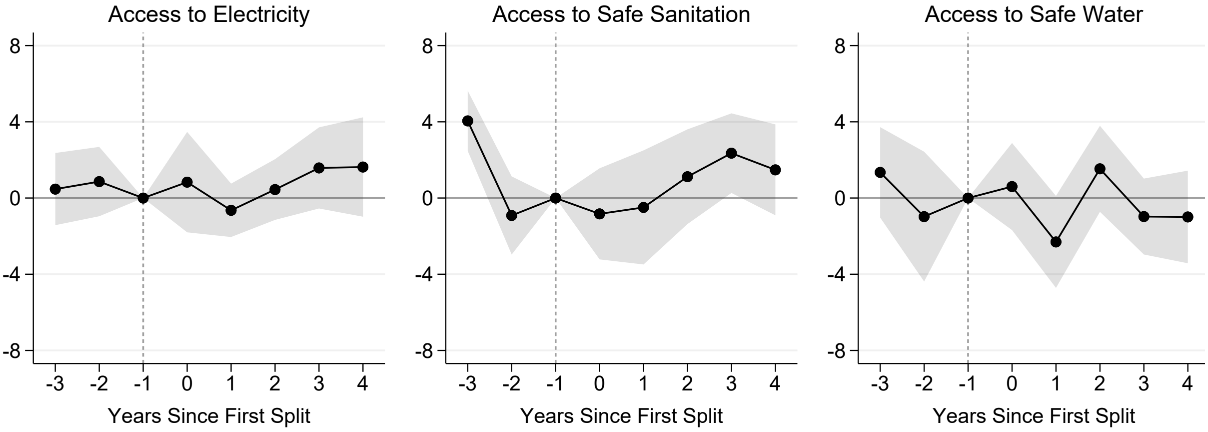
Notes: This figure plots point estimates and 95-percent confidence intervals of $\ln(2) \cdot \beta_h$ from Equation (3), the effect of doubling the number of districts in year t on GDP in year $t+h$. The model is estimated at three different levels of aggregation, as indicated. The confidence intervals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

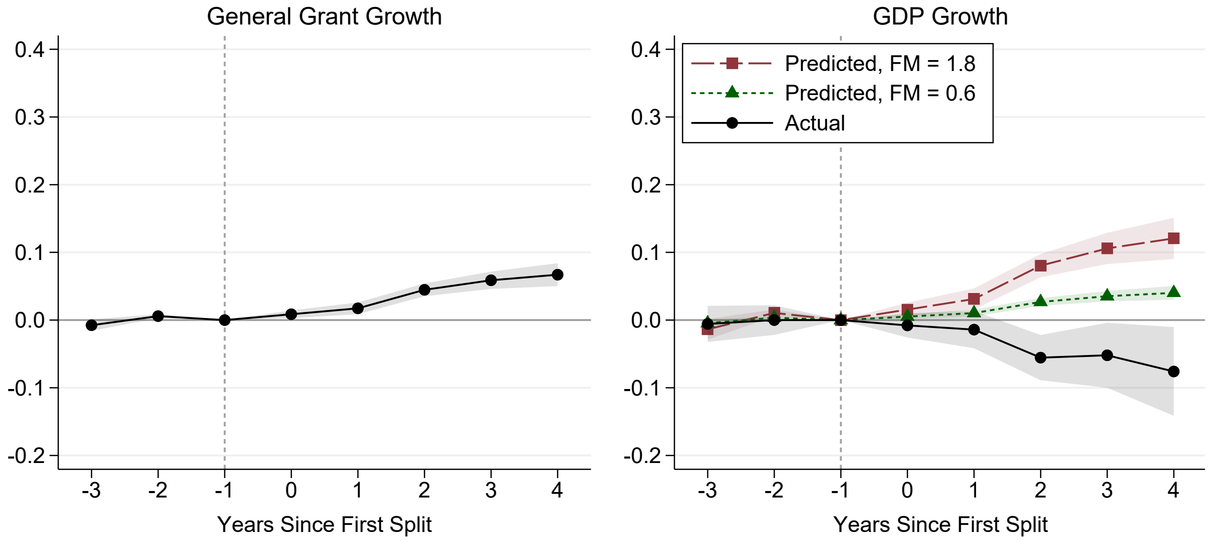
Figure B.11: The Effect of District Splits on Household Access to Infrastructure (SUSENAS)



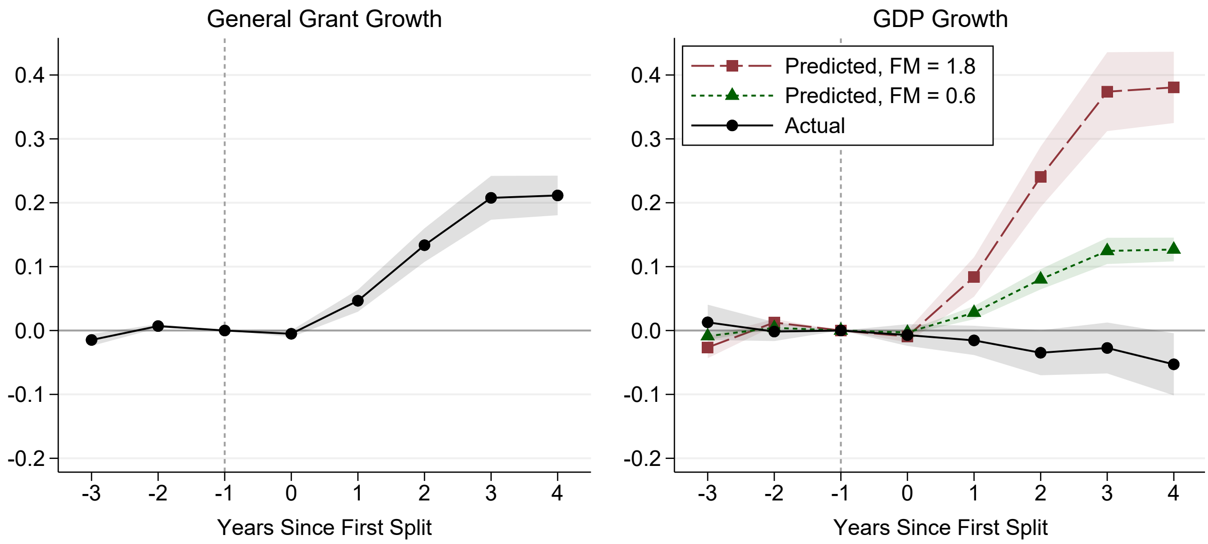
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. All outcome variables come from SUSENAS and are aggregated and disseminated by INDO-DAPOER. Household electricity access is missing in 2005 for all districts. We fill in these 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

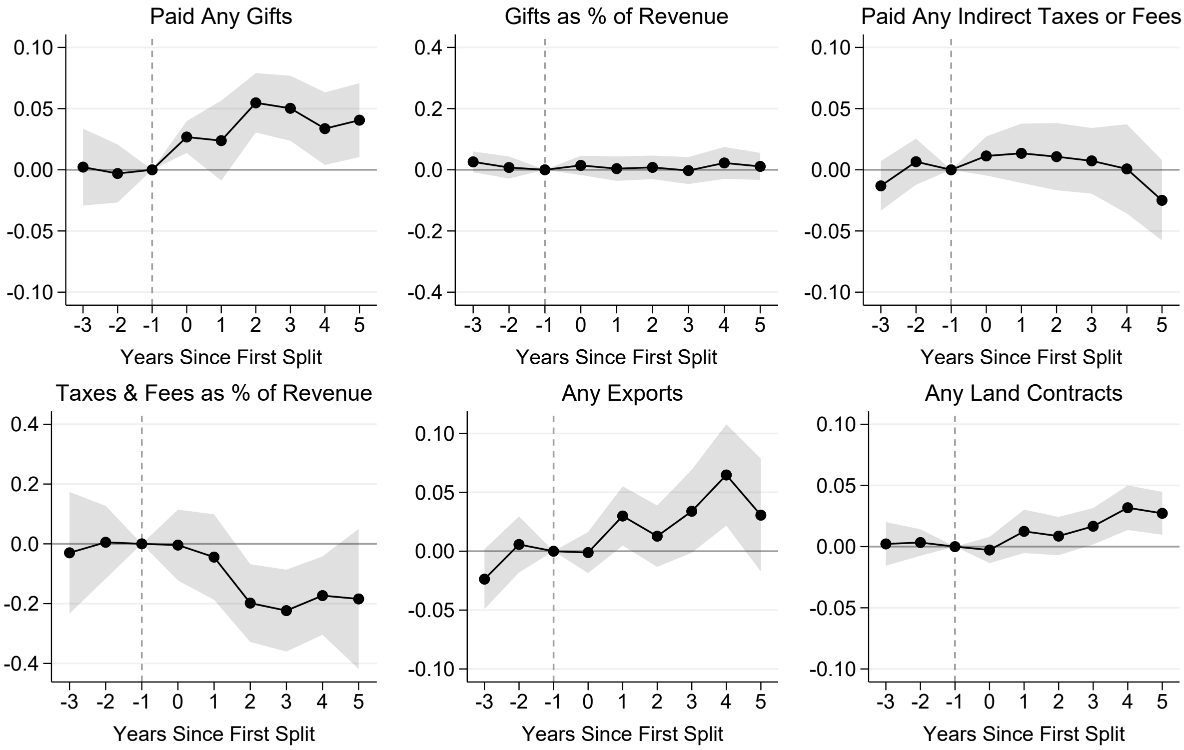


(b) Child Districts



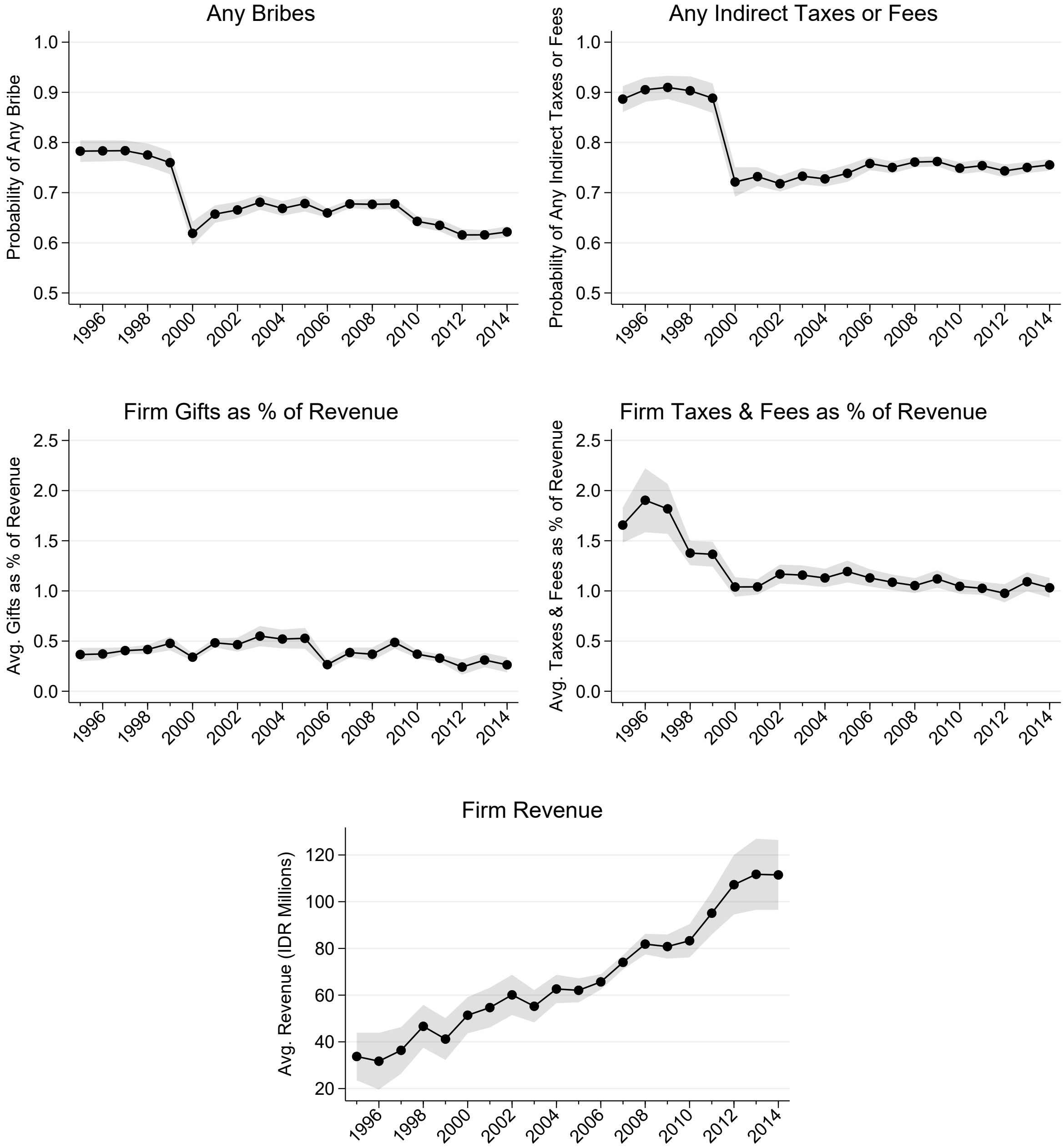
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. (The estimates for $h = -3$ omit the 2002 cohort and adjust the weights accordingly, because the data start in 2000.) The left panel shows the impact of the first district split 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



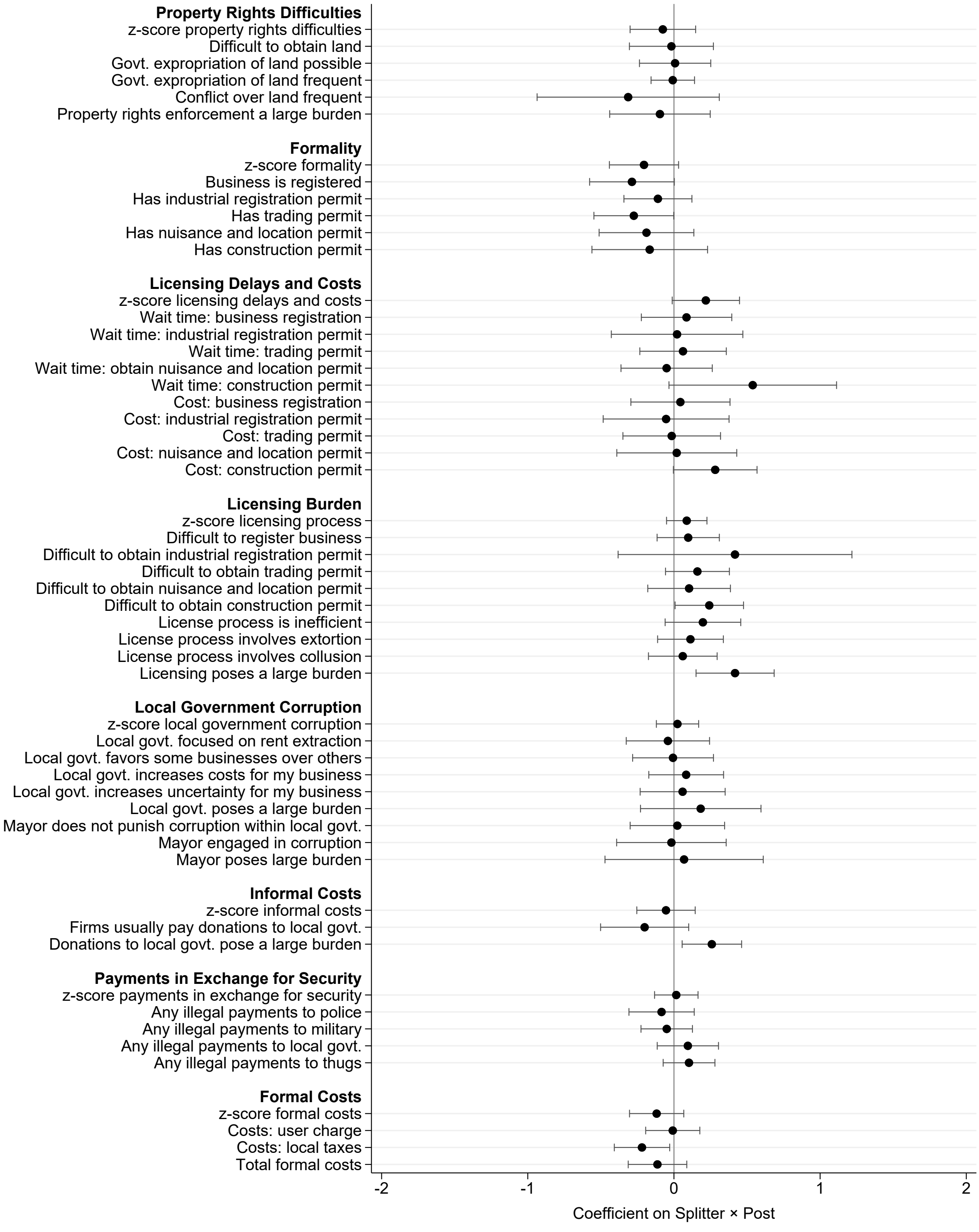
Notes: This figure plots estimates of the cohort-size-weighted CATT, using Equations (2) and (4), and their 95-percent confidence intervals. (The estimates for $h = -3$ omit the 2002 cohort and adjust the weights accordingly, because the data appear unreliable prior to 2000.) The confidence intervals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

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



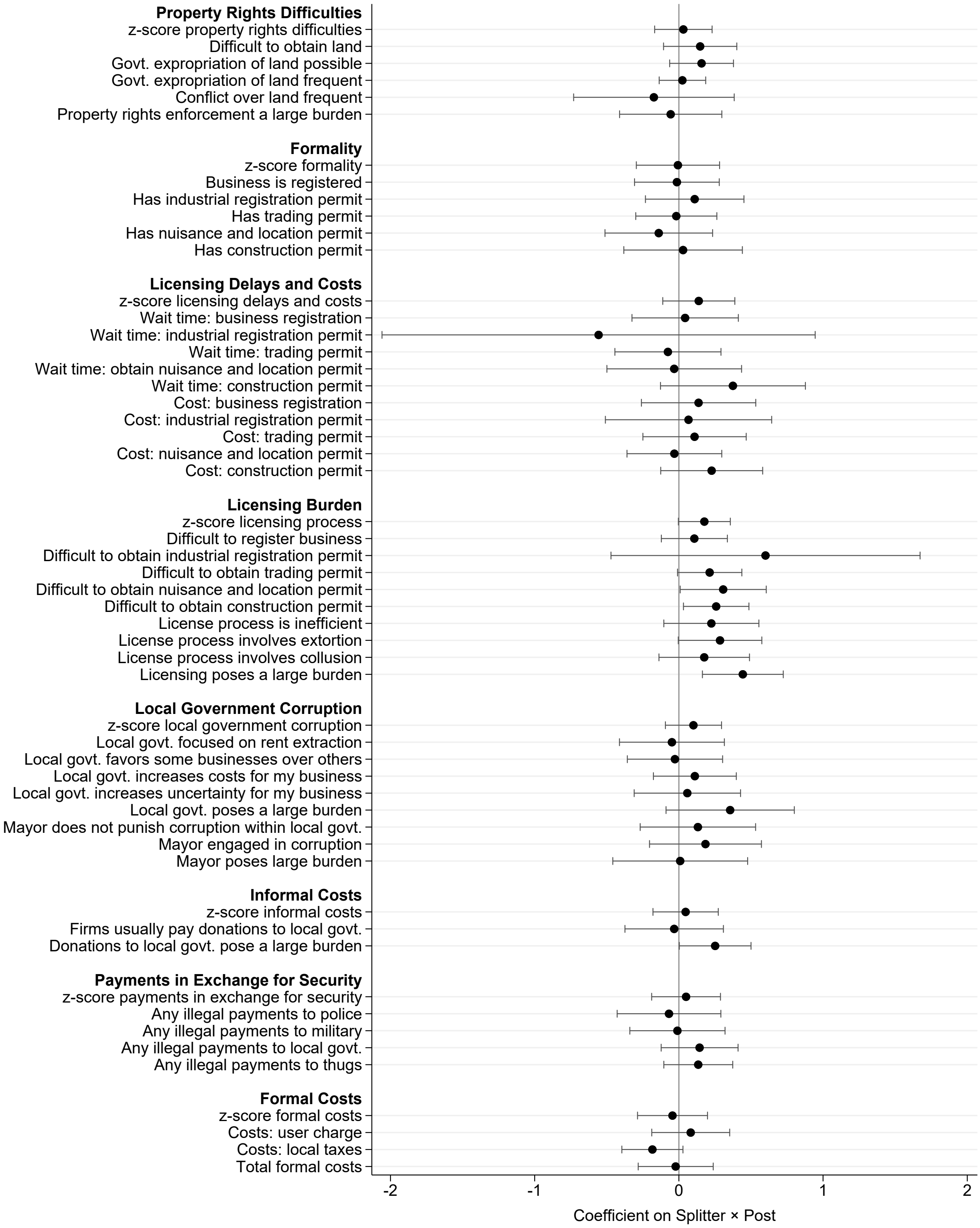
Notes: This figure plots the fitted values of a regression of the outcome on year dummies, controlling for firm fixed effects. Standard errors are clustered by district, and 95-percent confidence intervals are reported.

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



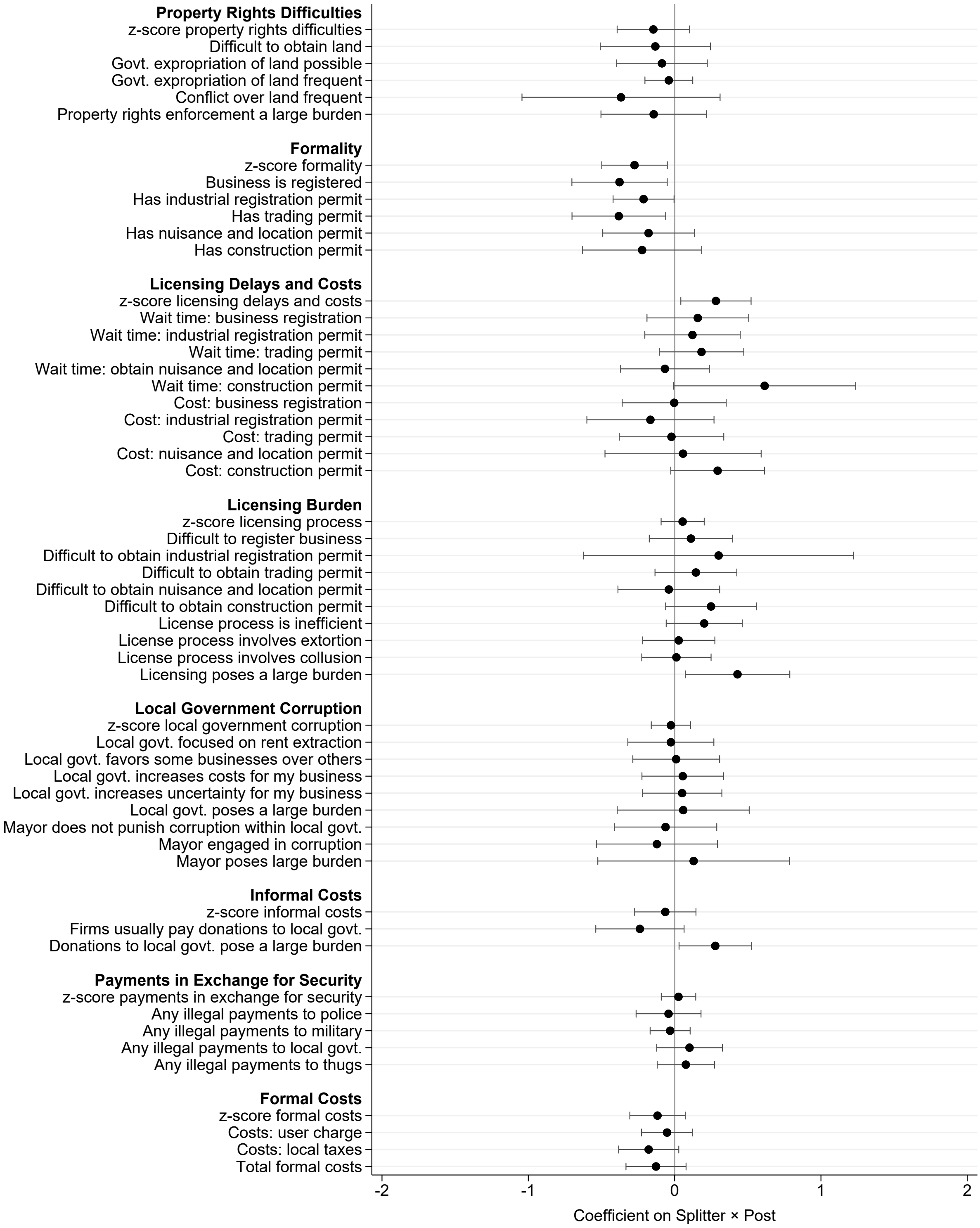
Notes: This figure plots estimates of β_3 in $Y_{f,d,t} = \beta_0 + \beta_1 Splitter_d + \beta_2 Post_t + \beta_3 Splitter_d \times Post_t + \lambda_{r(d),t} + \varepsilon_{f,d,t}$ and 95-percent confidence intervals, using data from the Economic Governance Survey. All outcomes are standardized to have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

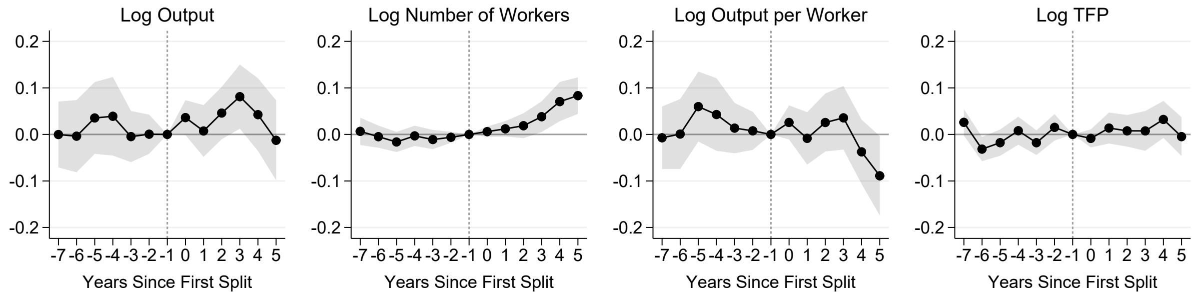
Figure B.17: The Effect of District Splits on the Business Environment (1–19 Employees)



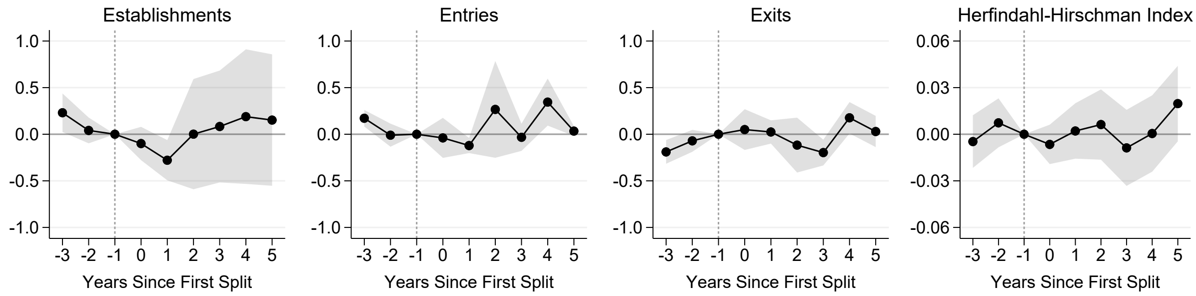
Notes: This figure plots estimates of β_3 in $Y_{f,d,t} = \beta_0 + \beta_1 Splitter_d + \beta_2 Post_t + \beta_3 Splitter_d \times Post_t + \lambda_{r(d),t} + \varepsilon_{f,d,t}$ and 95-percent confidence intervals, using the subsample of firms with fewer than 20 employees. All outcomes are standardized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