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28 February 2022

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February 28, 2022

Abstract

How does the fragmentation of local governments affect economic activity? We examine this question in the context of a major period of decentralization in Indonesia in which the number of local governments increased by 50 percent within a decade. Exploiting idiosyncratic variation in the timing of district splits, we find that fragmentation reduces district GDP in the short run—despite large increases in central transfers. The downsides of fragmentation due to economies of scale and the inexperience of new government personnel outweigh the potential upsides of increased accountability and competition. The GDP decline is larger in "child" districts that acquire a new capital and government. Furthermore, splitting districts spend more on administration and show no improvement in the areas of public good provision, red tape, and corruption.

JEL codes: H77, O43, O47, D73
Keywords: Economic growth, local governments, economies of scale, rent-seeking

*We thank David Agrawal, Pierre Bachas, Augustin Bergeron, Hoyt Bleakley, Charlie Brown, Jan Brueckner, Bob Hammond, Jim Hines, Lakshmi Iyer, Laura Kawano, Byung-Cheol Kim, Adam Looney, Byron Lutz, Eva Mörk, Swapnil Motghare, Angela Oh, Louise Sheiner, Joel Slemrod, Juan Carlos Suárez Serrato, Eleanor Wilking, Dean Yang, and seminar participants at the University of Alabama, University of Michigan, The Ohio State University, Middle Tennessee State University, the Midwest International Economic Development Conference, the National Tax Association Annual Conference, the European Meeting of the Urban Economics Association, the Virtual Meeting of the Urban Economics Association, and the International Institute of Public Finance Annual Congress for helpful comments. We are grateful to Evan Kresch and Erman Rahman for generously sharing data. We gratefully acknowledge financial support from the University of Michigan Library, the University of Michigan Rackham Graduate School, and the Michigan Institute for Teaching and Research in Economics (MITRE).

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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. More centralized systems exploit economies of scale in the provision of public goods, are better positioned to internalize policy spillovers, and feature fewer, more experienced politicians and bureaucrats. More 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 good provision can be goals in themselves, the stated aim of most decentralization policy is to achieve greater economic prosperity through these channels.[1] However, the net impact of decentralization on economic growth remains an open question.

This paper examines how an increase in the number of jurisdictions responsible for decision-making—a natural consequence of decentralization—impacts economic growth. We study an extraordinary period of decentralization in Indonesia from 2000 to 2014, which saw the number of district governments increase from 341 to 514 with the stated goal of accelerating regional economic development (Regulation 129/2000). Exploiting idiosyncratic variation in the timing of district splits generated by two national moratoria, we find that local government fragmentation reduces economic growth in the short run. Splitting districts experience a cumulative GDP loss of 19 percent over five years following a split relative to non-splitting districts. The decline is striking, as splitting induces a large increase in central transfers; the fiscal shock alone should produce a 17 percent cumulative *increase* in GDP under a conservative multiplier assumption.

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 allow us to rule out the possibility that splits simply cause a reshuffling of economic activity across space without having any aggregate impact. 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.[2] 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

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

[2] 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 (2020) for a review of the literature on decentralization and growth.

(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).[3] On the other 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 administrative capacity falls due to the inexperience of the politicians and bureaucrats running the new governments.

We find that experience 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.[4] Experience 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 the quality of growth-enhancing infrastructure—despite the increase in central transfers. By contrast, the hypothesized gains in accountability never materialize. Following a split, firms report paying bribes and license fees 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 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 a region. To address this concern, we show that the results are robust to controlling for 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

[3] 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).

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

the cohort-specific estimates.[5]

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. This is important because proxies of GDP, such as night lights, can be unreliable at the local level in developing countries.[6]

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 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.) An exception is Dahis and Szerman (2021), who find that municipal splits increase public good provision and nighttime luminosity in Brazil, but have no impact on the number of establishments or private-sector employment. Prior research on Indonesia examines the impact of district splitting on public services (Lewis, 2017), ethnic conflict (Bazzi and Gudgeon, 2021), and deforestation (Burgess, Hansen, Olken, Potapov and Sieber, 2012; Alesina, Gennaioli and Lovo, 2019). In contrast to these papers, we focus on growth and employ a different identification strategy. 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). Prior research in developed country settings focuses on the effect of amalgamations on public expenditure.[8]

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

[6] Gibson, Olivia, Boe-Gibson and Li (2021) show that GDP is *negatively* correlated with the popular DMSP night lights in the cross section of rural districts in Indonesia. The within-district correlation over time is essentially zero.

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

[8] 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.

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 has focused on within-country variation in a single aspect of decentralization—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) 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 experience and scale for realizing the potential gains from decentralization.

In Section 2 we describe the institutional context of Indonesia's decentralization reforms. Section 3 describes our dataset, which combines data on districts, villages, and manufacturing firms. Section 4 then explains the empirical strategy, and Section 5 presents the results. In Section 6 we compare our results to those of previous studies and discuss potential implications for long-run growth. Section 7 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 plays 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.

Starting in 2001, districts were empowered to make decisions on most local public expenditure in the areas of health, education, and infrastructure. Districts are responsible for about 30 percent of consolidated government expenditure (World Bank, 2003). Decentralization advanced further with the implementation of the Village Law starting in 2015. This law ex-

panded the fiscal autonomy of villages through a significant increase in central transfers and mandated district transfers to village governments. We therefore limit our sample to the years 2001–2014 to hold constant the authority and responsibilities of subnational governments.[9]

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. At the outset, the proposed parent and child districts all had to contain at least three subdistricts and possess sufficient economic capacity (Regulation 129/2000).[10] 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).

[9] Technically, our analysis period includes 2000 to aid in the estimation of pretrends. This year does not enter into the estimates of treatment effects.

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

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, which created exogenous variation in the timing of districts splits. Dozens of 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 five subdistricts, and (2) the original district has existed for at least seven years. 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.[11] 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 incumbent politicians, 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. Half of the general grant (*Dana Alokasi Umum*), the largest central transfer to districts, consists of an essentially fixed component that is meant to cover the civil service wage bill. The rest of the grant is apportioned according to a 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, 2021). The structure of the grant virtually guarantees that transfers increase in per 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. District expenditure increases roughly one-for-one with the general grant (Cassidy, 2021). 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.

[11] We were unable to find a shapefile of 2014 district borders. No districts became newly autonomous in 2013, and 14 districts because newly autonomous in 2014. At the level of 2000 borders, only four districts experienced their first split in 2014.

3 Data

District level. Data on district GDP, revenue, and expenditure come from the Ministry of Finance (*Kementerian Keuangan*) and the World Bank's Indonesia Database for Policy and Economic Research (INDO-DAPOER). The GDP data span 2000–2013, and the public finance data cover 2001–2014. We aggregate the variables to the level of district borders in 2000, using a crosswalk provided by the World Bank. The GDP series ends in 2013, because the Indonesian Central Bureau of Statistics (*Badan Pusat Statistik*) changed their methodology and only provides the new district GDP series for 2010 onward.

In some of our analysis, we examine the outcomes of parent and child districts separately. To do so, we use the backcasted GDP figures for the newly created districts provided in the World Bank data. 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 discuss the implications of this assumption and its failure in Section 5.

The availability of local GDP data is one of the advantages of studying decentralization in the Indonesian context; in many other developing countries, local economic growth has to be measured using alternatives like night-time luminosity. As Gibson et al. (2021) show, the popular DMSP night lights, which cover 1992–2013, are *negatively* correlated with GDP in the cross section of rural districts in Indonesia. The within-district correlation over time is essentially zero for both rural and urban districts.

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.

Village level. Data on local public goods come from the Village Potential Statistics (*Pendataan Potensi Desa*, or PODES) survey waves of 1996, 1999, 2002, 2005, 2008, 2011, and 2014.[12] PODES covers the universe of villages in Indonesia. Many villages split into multiple villages during the period of study: the number of villages increased from about 66,000 in 1996 to around 82,000 in 2014. We aggregate outcomes to the level of village borders in 1996, resulting in a balanced panel of around 63,000 villages.[13]

[12] The 1999 and 2002 surveys 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.

[13] 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 (2021) for more details on the construction of the public goods dataset. We use data from 1996 to aid in the estimation of pretrends for early splitting districts, but the results are similar when we exclude 1996.

Firm level. To study firm-level outcomes, we use 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 2000–2014. For ease of exposition, we will use the terms "firm," "establishment," and "plant" interchangeably, though we cannot link establishments belonging to the same firm. IBS contains information on outcomes such as total value of production, number of employees, and industry of operation. The treatment status of each establishment is determined by their earliest recorded location since 2000. In other words, establishments that are observed in 2000 are assigned to their recorded district in 2000. Establishments that are first observed after 2000 are assigned to the district whose 2000 borders contain their first observed district.

Establishments report their total tax payments, including land and building tax, and "company license fees," which are administered by local governments. Another outcome of interest is "gifts" paid by manufacturing firms to external parties. We interpret this variable as including bribe payments to officials, following Henderson and Kuncoro (2006, 2011). The terminology of "gifts" is often used in surveys such as the World Bank enterprise surveys to elicit truthful information on informal payments. We therefore interpret the response to this variable, which explicitly instructs the respondent to exclude gift payments by individuals and to consider only the firm, as referring to gifts that are part of the cost of doing business.[14]

To lend further credence to our interpretation of "gifts" as bribes, in the appendix we show that the incidence of bribery 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, bribe incidence is negatively correlated with own generation of electricity or purchase of electricity from non-governmental sources (Appendix Table A.2).

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.[15]

Summary statistics. Table 1 summarizes the baseline characteristics of splitting and non-splitting districts. Of the 331 districts in the sample, 98 experienced at least one split during

[14] In our sample 65 percent of firm-years featured positive "gift" payments (Appendix Table A.1), 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. Part of the discrepancy between the World Bank estimates and our estimates are 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.

[15] Although establishment data are available for earlier years, there is a sharp drop in two key variables starting in the year 2000—the probability of any positive reports of gift payments and taxes (Appendix Figure B.8) The drop is not due 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 prior to the year 2000.

the sample period, while 233 never split.[16] Initial GDP was 7 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 does not exhibit overlap (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. Table 1 also shows that splitting districts tend to have younger and less educated populations, though these differences are smaller than those for ethnic fractionalization and urbanization rate.[17]

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 Table 1.) 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 rather than in 2004 as a result of the first moratorium. In fact, more than 100 applications awaited consideration by the end of the first moratorium. 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

[16] We 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.

[17] Appendix Table A.1 provides additional summary statistics.

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.[18] To avoid this problem, we construct separate DiD estimators for each splitting cohort, defined according to the year of the district's first split.[19] 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. 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.

Let $Y_{d,t}$ be real GDP in district d in year t, let E_d be the cohort of district d, and let X_d be a vector of region dummies.[20] Define the cohort indicators $D_d^e = 1(E_d = e)$. We implement our estimator in two steps. First, we estimate the regression

$$\frac{Y_{d,e+h} - Y_{d,e-1}}{Y_{d,e-1}} = \alpha + \tau^{e,h} \cdot D_d^e + X_d'\delta + D_d^e \cdot \dot{X}_d^{e\prime}\phi + \varepsilon_{d,e}^h \tag{1}$$

for each cohort e, using the subsample of districts that either split for the first time in year e or never split. The variable $\dot{X}_d^e$ is defined as $X_d - \mathrm{E}(X_d \mid E_d = e)$. (We have omitted e and h superscripts for some of the parameters to simplify notation.) 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 merely shifts the intercept for treated districts, thus ensuring that $\tau^{e,h}$ is the desired average treatment effect. Under the conditional parallel trends assumption and a "no anticipation" assumption, the OLS estimator $\hat{\tau}^{e,h}$ is consistent for the cohort-specific average

[18] 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).

[19] 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.

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

treatment effect on the treated (CATT) (Sun and Abraham, 2021; Wooldridge, 2021).[21]

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 is fully nonparametric. 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.

Our approach to estimating each $\tau^{e,h}$ 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. In order 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 the cohort-specific 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,

$$\hat{\tau}^h = \sum_e \omega_e \hat{\tau}^{e,h}, \tag{2}$$

where ω_e is the share of splitting districts that belong to cohort e. We calculate standard errors for $\hat{\tau}^h$ that are robust to heteroskedasticity and clustering within original district boundaries.[22] While the conditional parallel trends assumption is not testable, it would be 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: $\tau^h = 0$ for $h < -1$.

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

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

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,

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

[22] Let $\widehat{\mathrm{V}}^h$ be the cluster-robust estimator of the asymptotic variance-covariance matrix of the vector of estimators $\{\hat{\tau}^{e,h}\}_e$. Our estimator for the asymptotic variance of $\hat{\tau}^h$ is $\boldsymbol{\omega}'\widehat{\mathrm{V}}^h\boldsymbol{\omega}$, where $\boldsymbol{\omega}$ is the vector of cohort shares $\{\omega_e\}_e$.

5 Results

5.1 Impact of Fragmentation on Growth

District splitting reduces district GDP, and the effect grows over time (Figure 2). This is despite the fact that central transfers mechanically increase within the original district borders after a split, which should have a positive impact on GDP through increased government expenditure, given reasonable estimates of local fiscal multipliers. The figure shows that after a split, general grant revenue eventually increases by 10 percent of GDP three years after the split. This increase in transfers resulted in a commensurate increase in district expenditure (Cassidy, 2021). We multiply this grant increase by lower and upper bounds of the local fiscal multipliers found in the literature (0.6 and 1.8) to generate predicted GDP responses to splitting. If districts splits affected GDP solely through the fiscal multiplier channel, then GDP should have eventually risen by 6 to 18 percent four years after the split. In reality, GDP begins to fall two years after the split, and eventually falls by 7 percent after four years. This suggests that splits negatively affect economic activity through non-fiscal channels. Both the general grant and GDP exhibit parallel pre-trends for splitting and non-splitting districts prior to the split.

To summarize the impact of splitting with a single number, we estimate the cumulative effects on GDP over the short run. Table 2 reports the cumulative impacts from the year of the split to four years after the split, $\sum_{h=0}^{4} \hat{\tau}^h$. Districts experienced a mechanical increase in general grant revenue as a share of pre-split GDP adding up to 28.9 percent (S.E. = 2.2 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). By contrast, actual GDP experienced a cumulative decline of 18.7 percent (S.E. = 6.8 percent) (column 4). While the point estimate is large, the degree of uncertainty surrounding the estimate is nontrivial; the 95 percent confidence interval includes cumulative declines as small as 5.4 percent and as large as 32 percent. We can, however, confidently rule out positive impacts. At least in the immediate term after fragmentation, increasing the number of local government seems to hinder rather than promote economic growth within the affected region.

It is unlikely that this adverse impact on growth occurs through fiscal channels alone. Evidence on local fiscal multipliers in developing countries is scarce, but 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. 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. 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. Even with the most conservative fiscal multiplier estimate of 0.6, the increase in the general grant should have substantially increased GDP, even absent other beneficial impacts of fragmentation such as increased intergovernmental competition. Instead we find that splits have a large, negative, and statistically significant impact on growth.

5.2 Robustness Checks

A potential threat to our strategy is that several baseline covariates are unbalanced for splitting and non-splitting districts (Table 1). 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 A.3 reports estimates controlling for baseline ethnic fractionalization, urbanization, age structure, and education in Equation (1).[23] The estimated decline in GDP due to splitting falls somewhat to 16.7 percent (S.E. = 7.2 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 slightly increases in absolute value, showing a GDP decline of 19.0 percent (S.E. = 7.7 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 20.2 percent (7.4 percent). The conclusion that district splits reduce growth is quite robust to adjusting for covariate imbalance or imposing common support in the urbanization rate. (Appendix Figure B.3 plots the corresponding horizon-specific estimates.)

5.3 Heterogeneity: Early vs. Late Splits

The baseline estimates are a weighted average of cohort-specific treatment effects, which in principle could 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. 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, potentially magnifying the harmful effects of splits. In a short period of time, the share of subnational expenditure on public services doubled, and two-thirds of the central bureaucracy was reassigned to local governments (World Bank, 2003). All district governments, whether

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

or not they split, had to contend with new expenditure and administration responsibilities. There continued to be confusion over which level of government was responsible for specific functions, which would make accountability challenging. In this context, it would take time to establish chains of accountability between civil society, bureaucrats, and elected officials. Without these systems in place, beneficial effects of district proliferation through increased accountability or choice of district 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.

Appendix Figure B.4 plots the estimates for 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 growth impacts in the first five years following a split are remarkably 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 (Figure B.5). Overall, early and late splits appear to have similar impacts on growth.

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

$$\frac{Y_{j,t+h} - Y_{j,t-1}}{Y_{j,t-1}} = \beta^h \cdot (\ln N_{j,t} - \ln N_{j,t-1}) + \lambda^h_{r(j),t} + \varepsilon^h_{j,t}, \tag{3}$$

where $N_{j,t}$ is the number of districts contained in geographic unit j in year t. 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 3 reports the cumulative effect of doubling the number of districts, $\ln(2) \cdot \sum_{h=0}^{4} \beta^h$. (Appendix Figure B.2 plots the dynamic effects.) Doubling the number of districts within the original 2000 borders leads to a cumulative decline in GDP by 13.2 percent (S.E. = 7.0 percent). This is somewhat smaller than the weighted CATT estimate of 18.7 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 average of 2.26 districts.[24] Scaling the estimate by $\ln(2.26)$ rather than $\ln(2)$ produces a cumulative decline of 15.5 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 17.2 percent (S.E. = 9.4 percent). The province-level estimate is larger still, showing a decline of 38.9 percent (S.E. = 21.3 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 3 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 lack of experience among the politicians and 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.

[24] Of the 103 districts that split for the first time in our sample period, 81 divided into two districts, 17 divided into three districts, and five divided into four districts.

5.5 Mechanism: Experience of Personnel

One reason why fragmentation might harm GDP is that newly created district governments lack experience and require large initial investments in both physical administrative infrastructure and personnel. In this case, we would expect the negative growth effects to be concentrated in the child district, since the parent district retained the original government and capital. To investigate this channel, we estimate separate effects for parent and child districts.

Figure 3 displays the grant and GDP responses to splitting for parent and 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. Under this assumption, general grant transfers increase in both parent and child districts after the split, but the increase is over twice as large in the child district. A plausible alternative is that per capita expenditure before the split was actually 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 4 shows that, even though the increase in transfers is much larger for child districts, parent and child districts experience similar cumulative declines in GDP of around 20 percent.[25] Relative to predicted GDP based on fiscal multipliers, the GDP decline is much larger for child districts. For instance, the consensus subnational fiscal multiplier of 1.8 predicts a cumulative GDP increase of 97.6 percent in child districts and 31.8 percent in parent districts. These results suggest that experience matters—child districts perform worse than parent districts on average, in light of their greater increase in transfers. However, experience cannot explain why GDP also declines in parent districts, so other mechanisms likely play a role.

5.6 Mechanism: Economies of Scale

The creation of a new government entails greater spending on government administration, especially in the short-run, as new buildings are constructed and new personnel hired. In addition, government fragmentation may sacrifice cost savings due to economies of scale in public good provision to potentially increase efficiency in other dimensions. Unlike the experience channel, the economies of scale channel is relevant for both parent and child districts. We find that this loss of benefits from scale can be substantial.

[25] The sample size is smaller compared to Table 2 because the GDP data are not always backcasted to the year prior to the split for the parent and child districts. In particular, GDP is not backcasted for several districts in 2008, to the extent that no district in the 2009 splitting cohort is included in these estimates. The cohort-size weights are adjusted to reflect the relative frequencies of each splitting cohort conditional on having non-missing GDP growth rates.

Figure 4 shows the dynamic impact of splits on the district expenditure shares in five functional categories: administration, human capital, physical capital investment, economic investments, and social service provision. The administrative expenditure share sharply increases in the year of the split and remains at a higher level four years later. The average increase over five years in 3.7 percentage points, which is an 11 percent increase relative to the baseline share of 32.4 percent (Table 5). This is achieved through a decrease in the share of spending devoted to human capital (education and health). These shifts in expenditure are consistent with large fixed costs of government administration and, hence, economies of scale in public good provision.

Some of these effects may change in the long run. For example, the relative share of expenditure on investment in physical capital starts to increase two years after the split. Overall, the spending composition results suggest a shift in spending toward public administration and away from productivity-enhancing areas. Consistent with this result, Figure 5 shows that splits cause a reduction in road quality—despite the large increase in transfers.

5.7 Mechanism: 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—formal taxes and fees and informal taxation or "gifts."

Figure 6 shows that increasing the number of local governments does not have a disciplining impact on bribery. 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. This increase is statistically significant starting two years after the split. On the other hand, the average bribe rate—gift payments as a share of firm revenue—is unchanged. Similarly, firms are slightly more likely to pay any formal taxes and fees, but the average tax rate does not increase. We would expect the effect of splits on formal taxes to be small given that most taxes are administered centrally. Unfortunately, firms do not separately report local and central tax payments 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, which requires licenses, increases following splits, but other essential licensed activities such as land contracts do not (Figure 6). Ultimately, the increase in competition may have increased output in certain sectors (such as timber) but not across the manufacturing sector in general.

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 represented nearly all districts at the time.

Figure 7 summarizes the difference-in-differences estimates for 48 outcome variables from the Economic Governance Survey.[26] 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 and for firms with fewer than 20 employees. 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 *measured* improvements in the business environment, they could still generate unmeasured improvements, which should increase firm output or productivity. However, we find that splits do not significantly increase output, firm size, or

[26] 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.

worker productivity (Figure 8). This is not to suggest that political economy factors do not matter for productivity in Indonesia. Abeberese, Barnwal, Chaurey and Mukherjee (2021) 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.

We cannot rule out the possibility that splits improved the business environment for *some* firms—for example, those that experienced the largest reduction moving costs as a result of the split. The IBS survey does not report the village or subdistrict where the firm is located, so we cannot measure the firm's geographic proximity to competing districts. However, we can measure public good provision at the village level. We find that splits reduce road quality both in villages that experience a reduction in the distance to the nearest competing district as a result of the split, and in villages that do not experience such a reduction. (Results available upon request.) Thus splits do not seem to induce districts to target productivity-enhancing infrastructure investments to areas where firm mobility increases.

6 Discussion

The adverse impact of splitting on growth in Indonesia contrasts with the findings of Dahis and Szerman (2021) for Brazil. They find that municipal splits increase nighttime luminosity in child districts and have no impact on luminosity in parent districts. The authors attribute the result to the expansion of the public sector, as they find no impacts on the private sector. Since GDP data are not available for Brazilian municipalities, it is not possible to directly compare their estimates with ours. Still, splitting appears to yield better results in Brazil than in Indonesia. One possible explanation is that the increase transfers due to splitting is larger relative to the local economy in Brazil than in Indonesia. Without GDP data, however, we cannot evaluate this explanation. Another possibility is that Brazil does a better job of managing the transition to a new government in the child district. Lima and Neto (2018) find that municipal splits increase public spending per capita in Brazil. They attribute the increase to economies of scale and rent-seeking, so some of the same mechanisms appear to be at work in Indonesia and Brazil.

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 experience 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.

7 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. Government experience 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 experience appears to be an important mechanism, fragmentation should be less harmful for GDP in the long run as the new politicians and bureaucrats gain experience. 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.[27] 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.

[27] As of July 2017, there were 246 pending applications to create new provinces and districts in Indonesia (Tempo, 2017).

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8 Tables

Table 1: Baseline District Characteristics

	Splitters	Non-Splitters	Difference
Log GDP, 2000	15.166	15.237	−0.071
			(0.129)
Log General Grant, 2001	12.744	12.552	0.191***
			(0.058)
Log Population, 2000	12.910	12.901	0.008
			(0.109)
Log Land Area, 2000	9.031	6.831	2.200***
			(0.173)
Ethnic Fractionalization, 2000	0.590	0.344	0.246***
			(0.037)
Share of Population in Urban Areas, 2000	0.192	0.454	−0.262***
			(0.036)
Share of Population Aged 0–14, 2000	0.355	0.303	0.052***
			(0.005)
Share of Population Aged 15–64, 2000	0.613	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	0.124	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 2: 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	0.289^{***}	0.173^{***}	0.520^{***}	-0.187^{***}
	(0.022)	(0.013)	(0.040)	(0.068)
Observations	1,261	1,261	1,261	1,261
District Clusters	331	331	331	331

Notes: This table reports estimates of the cumulative effect of the first district split, $\sum_{h=0}^{4} \tau^h$, 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. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: 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	-0.132^{*}	-0.172^{*}	-0.389^{*}
	(0.070)	(0.094)	(0.213)
Observations	2,969	503	297
Number of 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 in the table. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by geographic unit. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: 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	0.177***	0.106***	0.318***	−0.197*
	(0.022)	(0.013)	(0.040)	(0.107)
Observations	1,229	1,229	1,229	1,226
District Clusters	299	299	299	296
	Panel B: Child Districts			
Cumulative Effect of Split	0.542***	0.325***	0.976***	−0.205**
	(0.067)	(0.040)	(0.121)	(0.085)
Observations	1,229	1,229	1,229	1,226
District Clusters	299	299	299	296

Notes: This table reports estimates of the cumulative effect of the first district split, $\sum_{h=0}^{4} \tau^h$, 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. 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 5: Impact of Fragmentation on District Expenditure Priorities

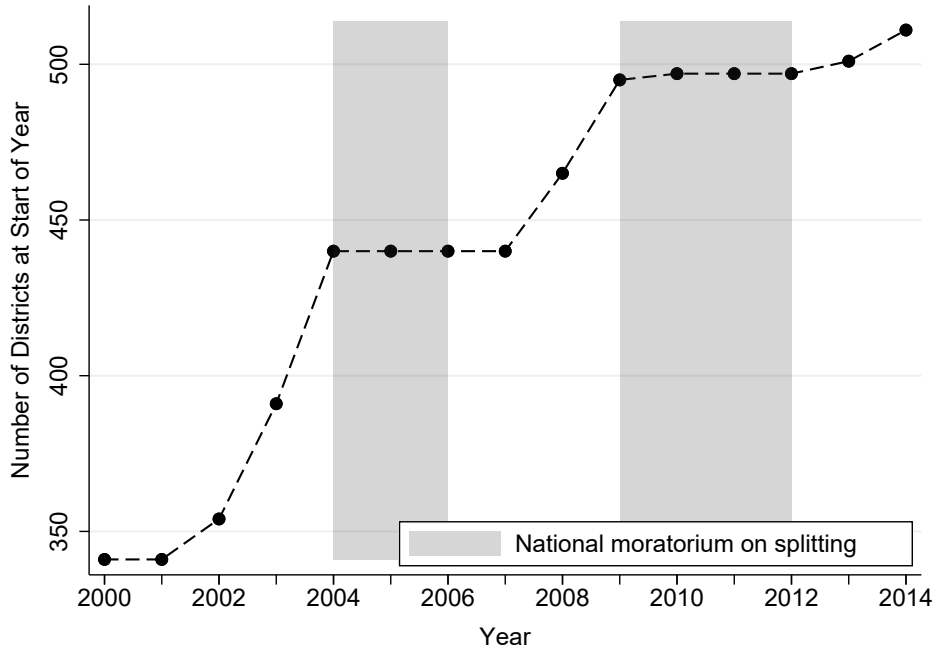
	Expenditure Share in Functional Category:				
	Administration	Human Capital	Physical Capital	Economy	Social
	(1)	(2)	(3)	(4)	(5)
Average Effect of Split	0.037***	−0.052***	0.016*	0.001	−0.002
	(0.008)	(0.009)	(0.008)	(0.003)	(0.002)
Baseline Mean	0.324	0.425	0.162	0.059	0.030
Observations	713	713	713	713	713
District Clusters	272	272	272	272	272

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 is measured as the average change in expenditure share over five years after the split relative to the expenditure share in the year prior to split, $\sum_{h=0}^{4}(E_{d,t+h} - E_{d,t-1})/5$. Because the functional expenditures are only available for 2001–2012, the estimates are based on the 2002, 2003, 2004, and 2008 cohorts. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

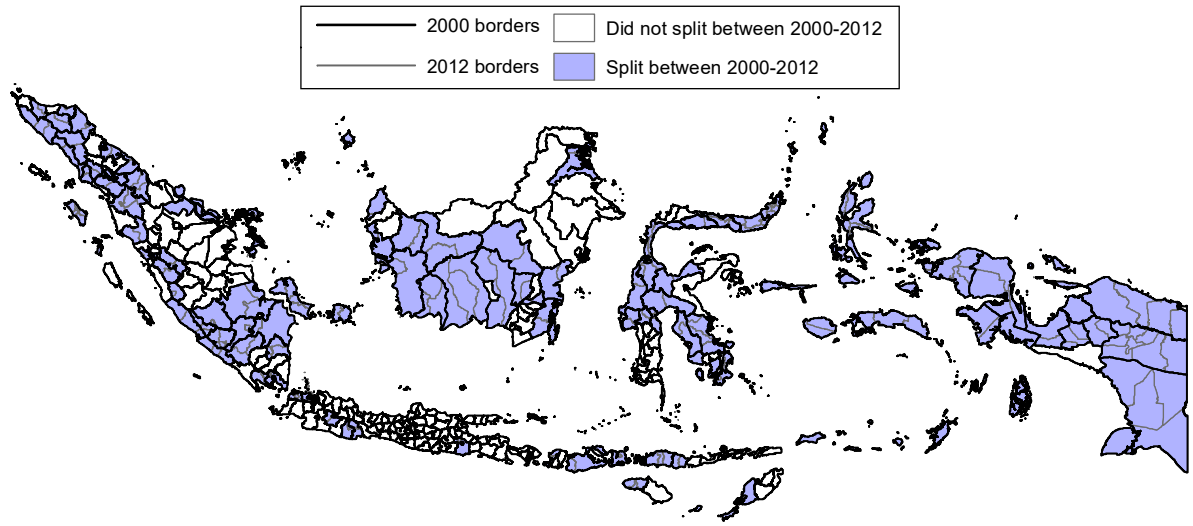
9 Figures

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

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

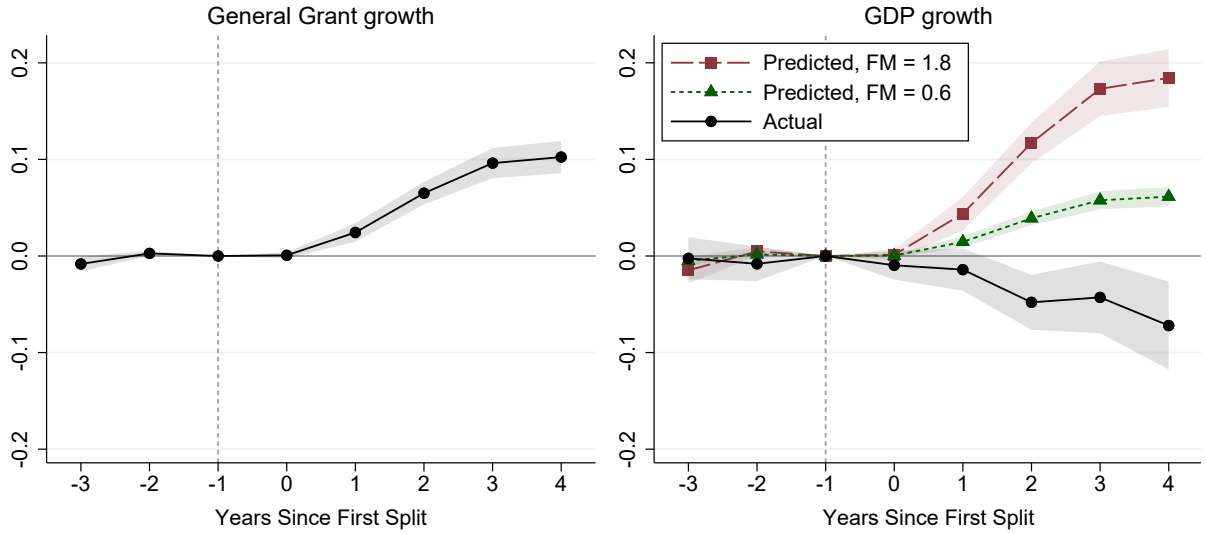


(b) District Splitting Was Geographically Widespread



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).

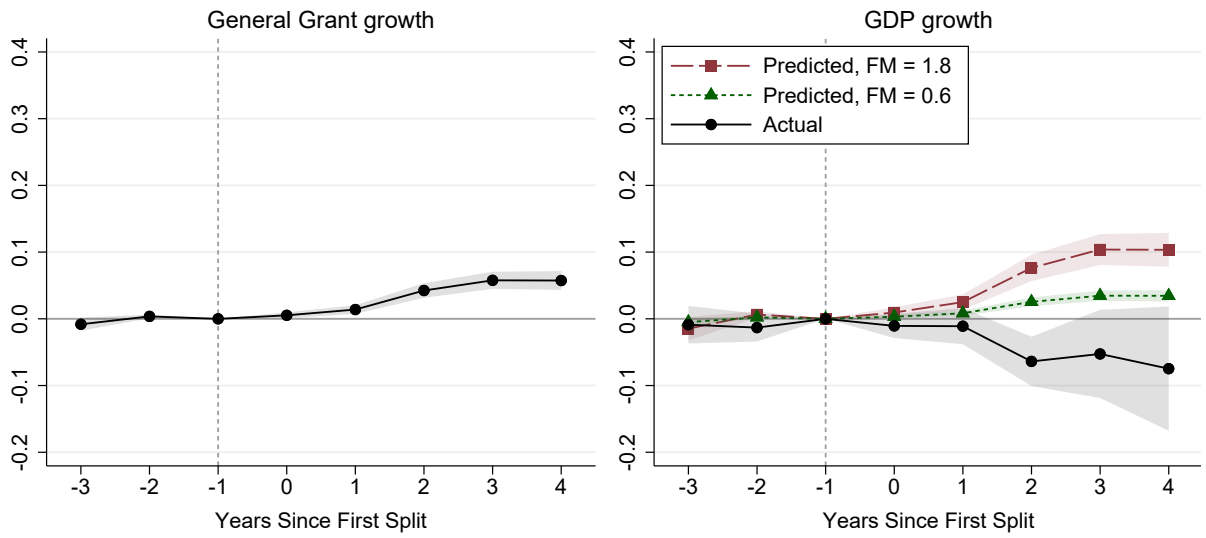
Figure 2: The Effect of District Splits on General Grant and GDP



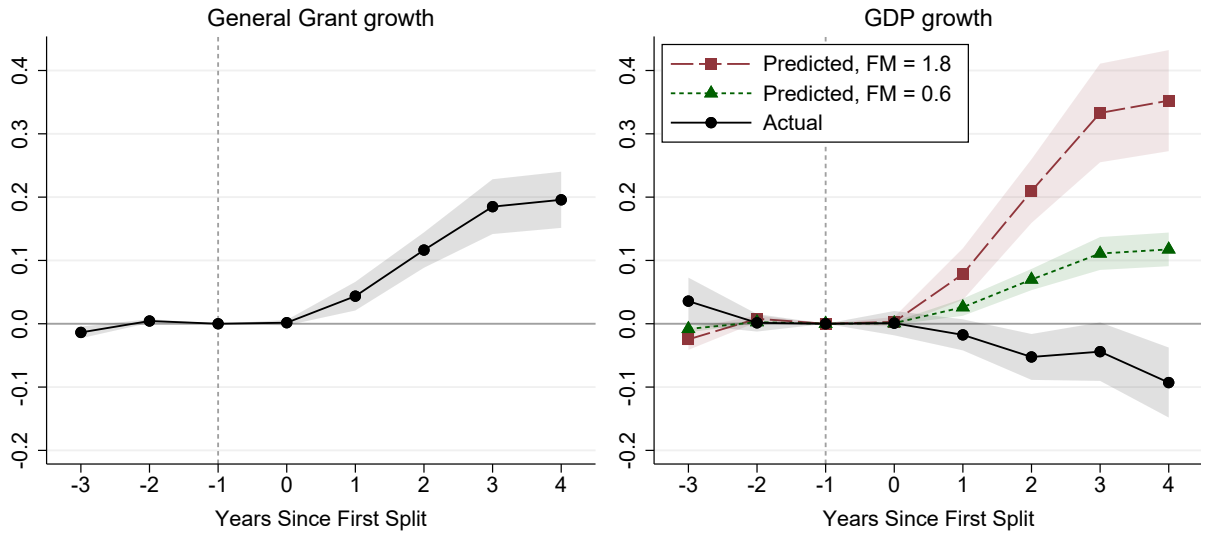
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. The left panel shows the impact of the first district split on growth in general grant revenue relative to the year before the split, scaled by GDP in that year. The right panel 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 General Grant and GDP: Parent vs. Child District

(a) Parent Districts

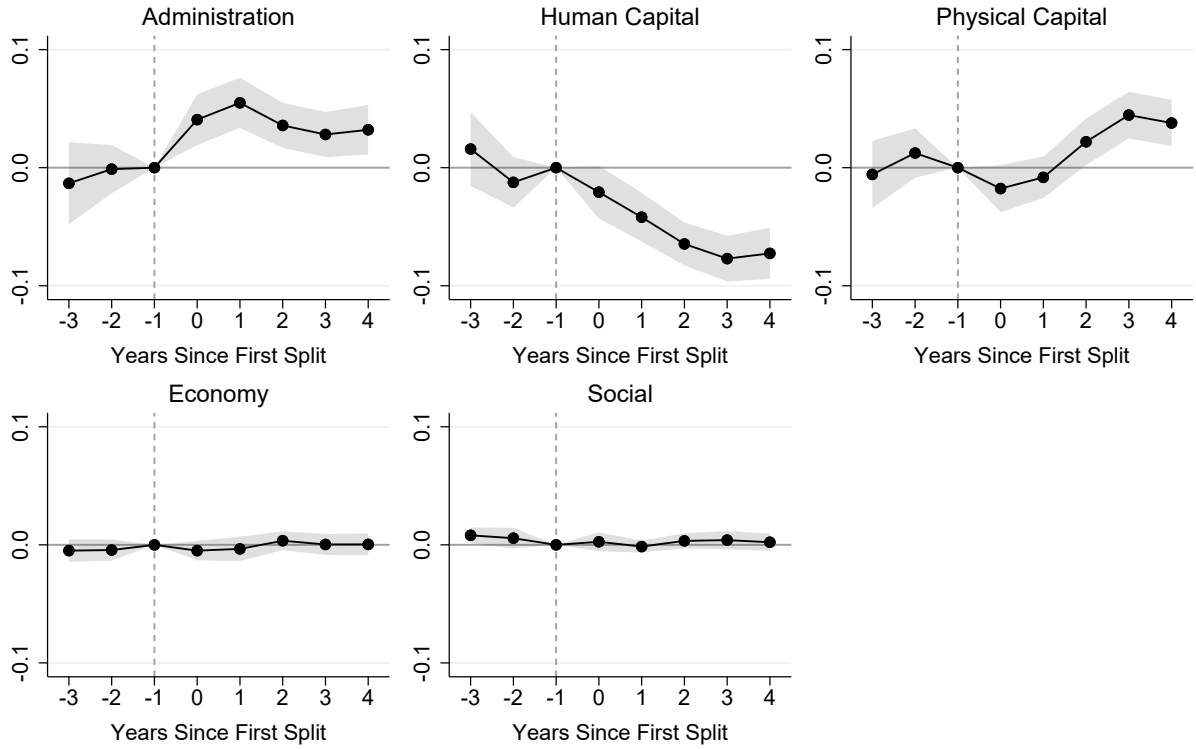


(b) Child Districts



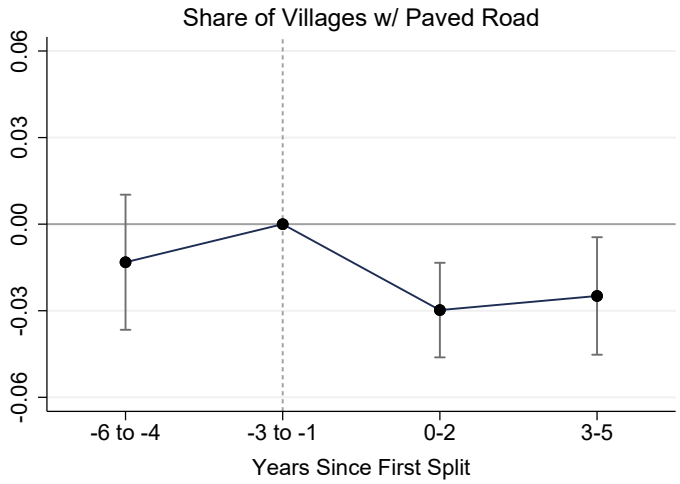
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. The left panel shows the impact of the first district split on growth in general grant revenue relative to the year before the split, scaled by GDP in that year. The right panel 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 4: The Effect of District Splits on District Expenditure Priorities



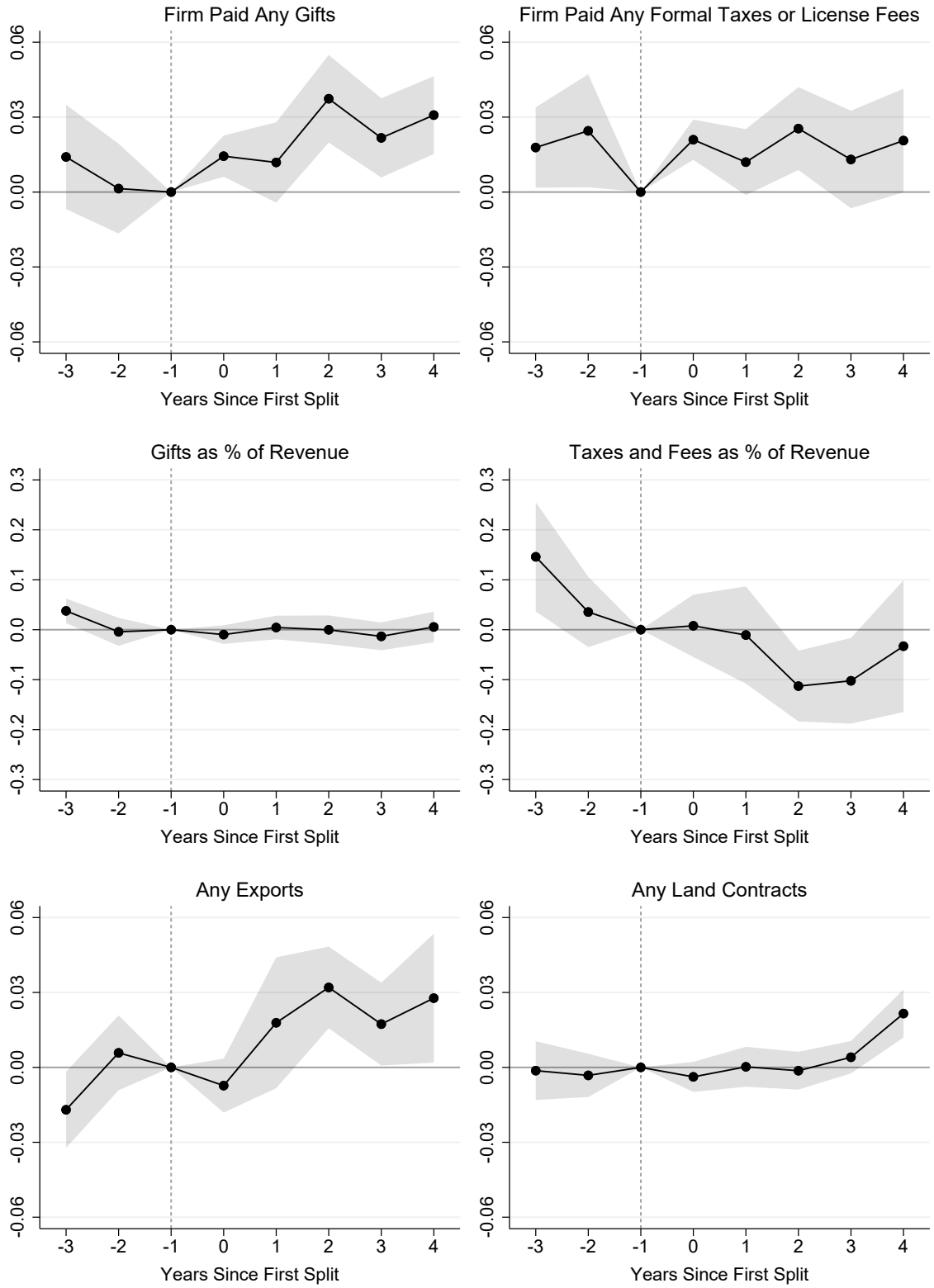
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. Each outcome is measured as the change in the expenditure share from year $t-1$ to year $t+h$. Because the functional expenditures are only available for 2001–2012, the pre-split estimates are based on the 2004, 2008, and 2009 cohorts; and the post-split estimates are based on the 2002, 2003, 2004 and 2008 cohorts. The confidence intervals are robust to heteroskedasticity and clustering by district.

Figure 5: The Effect of District Splits on Road Quality



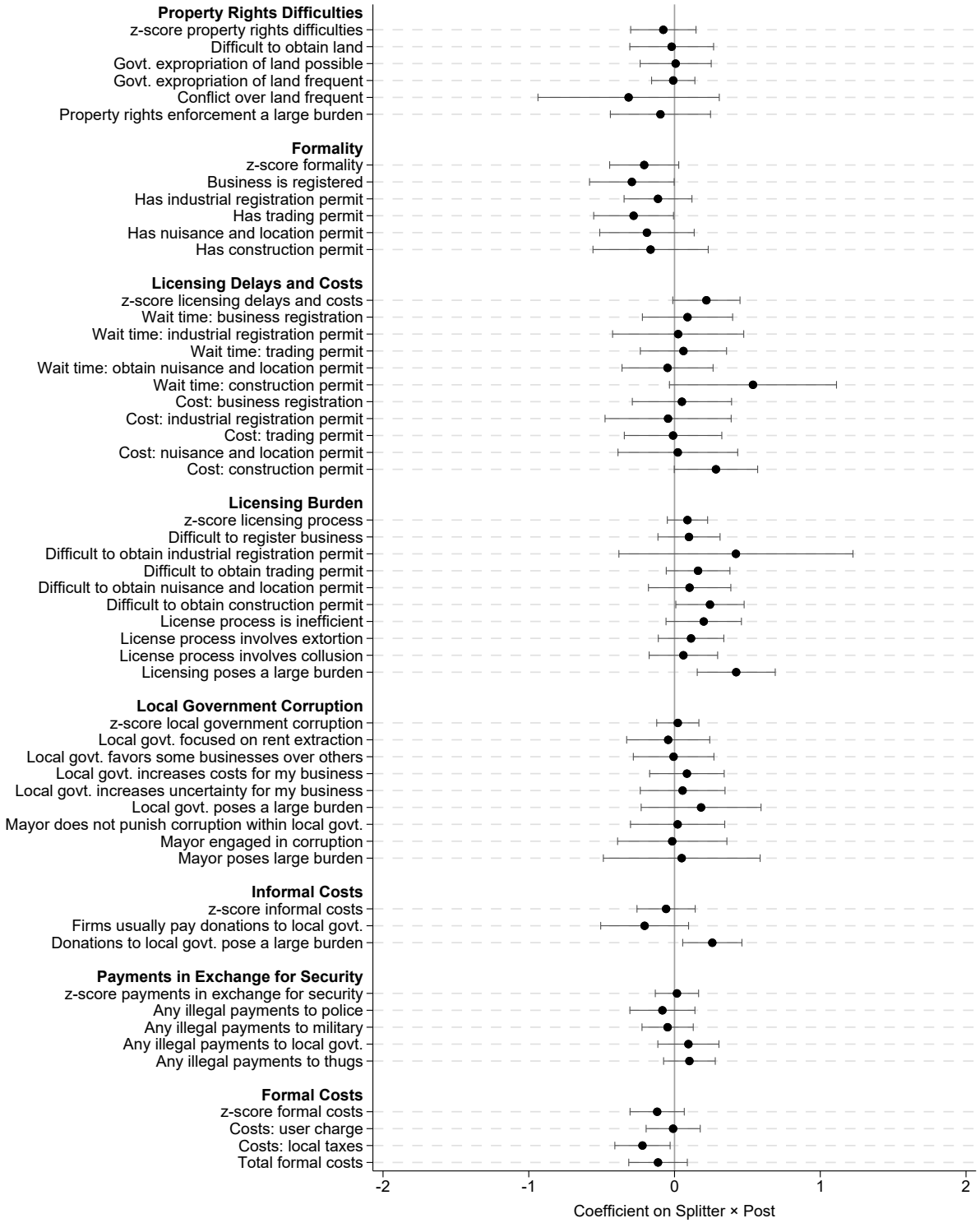
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. The outcome variable is measured as the change in the public good from period $t-1$ to period $t+h$. The confidence intervals are robust to heteroskedasticity and clustering by district.

Figure 6: The Effect of District Splits on Gifts, Taxes, and Regulated Activities



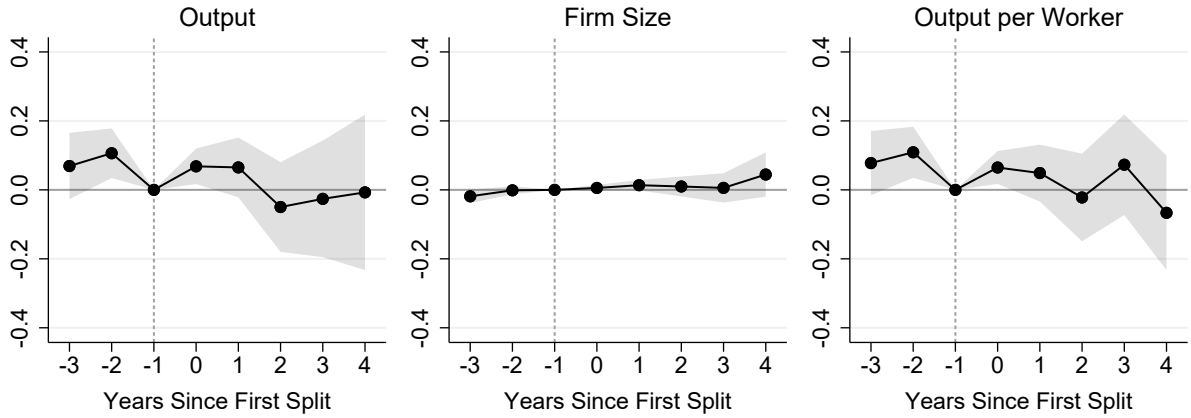
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. "Any Gifts," "Any Taxes or Fees," "Any Exports," and "Any Land Contracts" are each measured in terms of its change from year $t-1$ to year $t+h$. Taxes and gifts as a share of revenue are both measured in terms of their percentage change from $t-1$ to $t+h$. The confidence intervals are robust to heteroskedasticity and clustering by district.

Figure 7: 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 8: The Effect of District Splits on Firm Growth and Productivity



Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. Each outcome—real output, number of workers, and real output per worker—is measured in terms of its percentage change from year $t-1$ to year $t+h$. The confidence intervals are robust to heteroskedasticity and clustering by district.

A Appendix Tables

Table A.1: Summary Statistics

	Mean	Std. Dev.	Min.	Max.	Obs.
Panel A: Firm-Level Variables					
Firm Remitted Any Formal Taxes or Fees	0.74	0.44	0.00	1.00	320,383
Firm Recorded Some Gifts to Others	0.65	0.48	0.00	1.00	319,056
Formal Taxes and Fees as % of Revenue	1.07	4.38	0.00	100.00	316,727
Gifts as % of Revenue	0.39	2.45	0.00	100.00	315,663
Total Revenue (IDR 1 million)	77.41	737.20	0.00	142,003.66	314,328
Number of Employees	195.33	741.39	20.00	56,139.00	321,987
Any Export	0.19	0.39	0.00	1.00	214,694
Any Expenditure on Land Rent/Contract	0.07	0.26	0.00	1.00	319,056
Panel B: District-Level Variables					
Population (1000s)	650.45	629.27	22.73	5,658.92	5,030
Land Area (1000s km^2)	5.77	10.70	0.02	119.75	5,040
Number of Districts in Original District	1.29	0.70	1.00	8.00	5,040
Panel C: Village-Level Variables					
Main Road Made of Asphalt	0.64	0.48	0.00	1.00	369,348
Village Population (1000s)	3.39	4.91	0.02	912.91	383,750
Distance to Nearest District in 2000 (km)	11.64	24.18	0.00	405.85	384,657
Distance to Nearest District in 2014 (km)	7.58	12.43	0.00	176.24	384,657
Δ Distance to Nearest District (Splitters)	−8.90	23.66	−248.96	0.00	156,275
Became Closer to Competing District (Splitters)	0.44	0.50	0.00	1.00	156,275

Notes: The value of firm output is measured in constant 2010 IDR 1 million (≈ USD 100).

Table A.2: Correlation between "Gifts" and Activities Requiring Permits

	Firm Paid Any Gifts					
	(1)	(2)	(3)	(4)	(5)	(6)
Any Export	0.009*					
	(0.005)					
Any Expenditure On:						
Land Contract		0.041***				
		(0.009)				
Building Additions			0.036***			
			(0.008)			
Any Electricity Purchased:						
From Government				0.051***		
				(0.008)		
From Non-Government					−0.218***	
					(0.038)	
Any Electricity Generated						−0.032***
						(0.009)
Mean Indep. Var	0.187	0.069	0.124	0.872	0.052	0.173
Observations	188,739	288,679	254,542	288,679	288,679	278,793
District Clusters	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 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 A.3: Government Fragmentation and Economic Growth (Robustness)

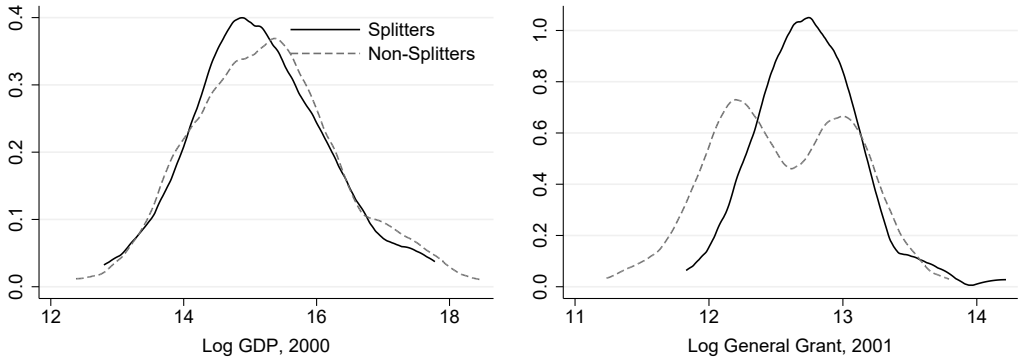
	General Grant	Predicted GDP		Actual GDP
	(1)	(2) Multiplier = 0.6	(3) Multiplier = 1.8	(4)
	Panel A: Additional Controls			
Cumulative Effect of Split	0.267***	0.160***	0.480***	−0.167**
	(0.016)	(0.010)	(0.029)	(0.072)
Observations	1,260	1,260	1,260	1,260
District Clusters	330	330	330	330
	Panel B: Trimmed Sample			
Cumulative Effect of Split	0.240***	0.144***	0.432***	−0.190**
	(0.023)	(0.014)	(0.041)	(0.077)
Observations	925	925	925	925
District Clusters	263	263	263	263
	Panel C: Additional Controls, Trimmed Sample			
Cumulative Effect of Split	0.220***	0.132***	0.396***	−0.202***
	(0.017)	(0.010)	(0.030)	(0.074)
Observations	925	925	925	925
District Clusters	263	263	263	263

Notes: This table reports estimates of the cumulative effect of the first district split, $\sum_{h=0}^{4} \tau^h$, based on the cohort-size-weighted CATT estimator (Equation (2)). The estimates in Panels A and C control for ethnic fractionalization, urban population share, 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

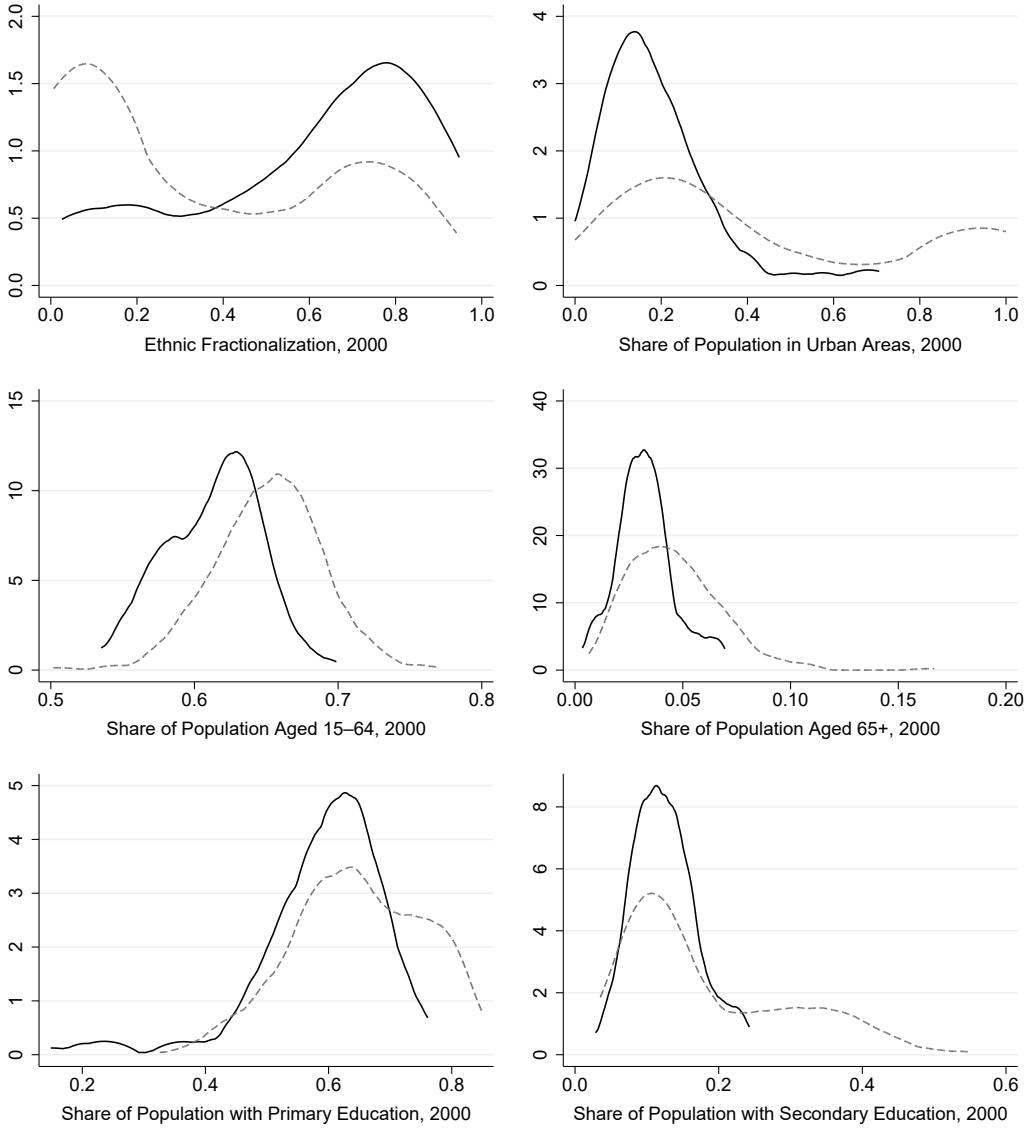
B Appendix Figures

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

(a) Baseline Outcomes

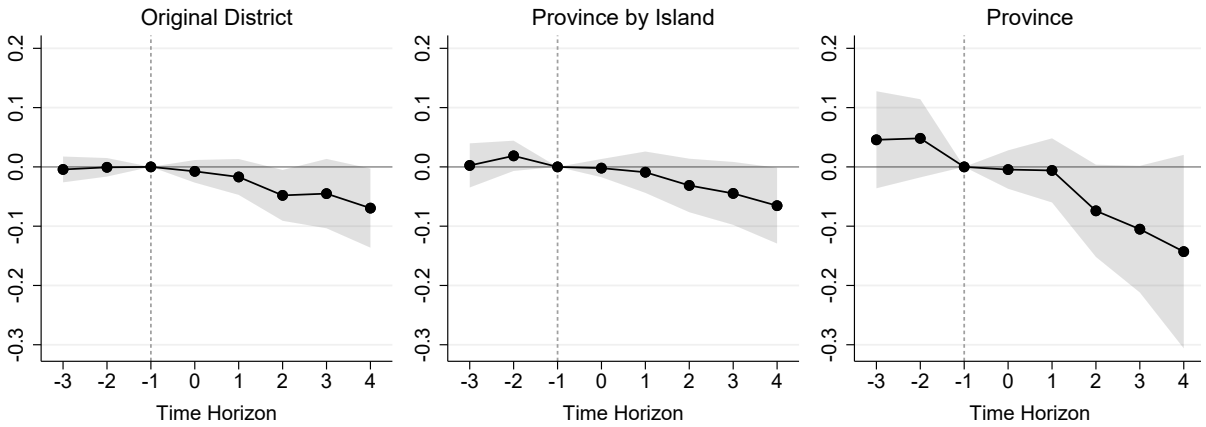


(b) Baseline Covariates



Notes: Densities are estimated using the Epanechnikov kernel.

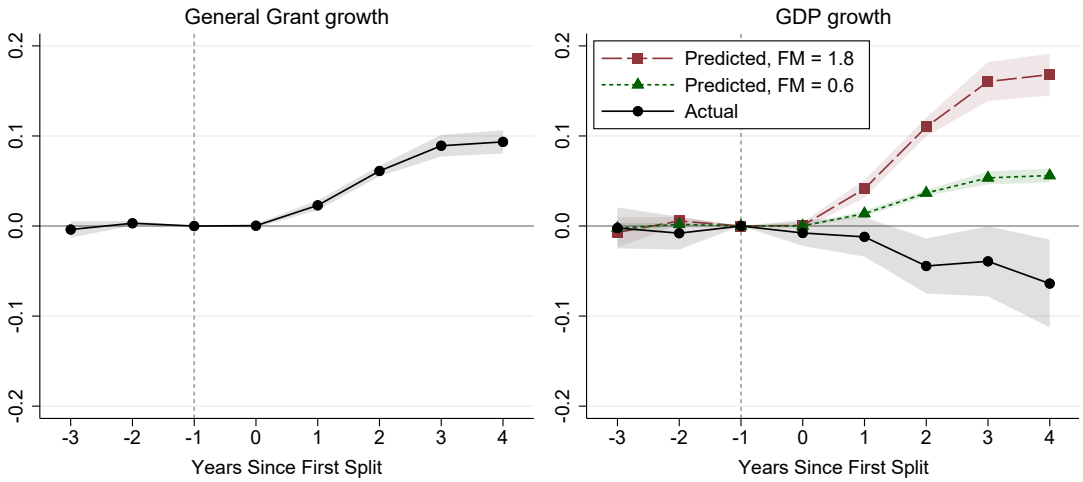
Figure B.2: Growth Effects of Doubling the Number of Districts at Different Levels of Aggregation



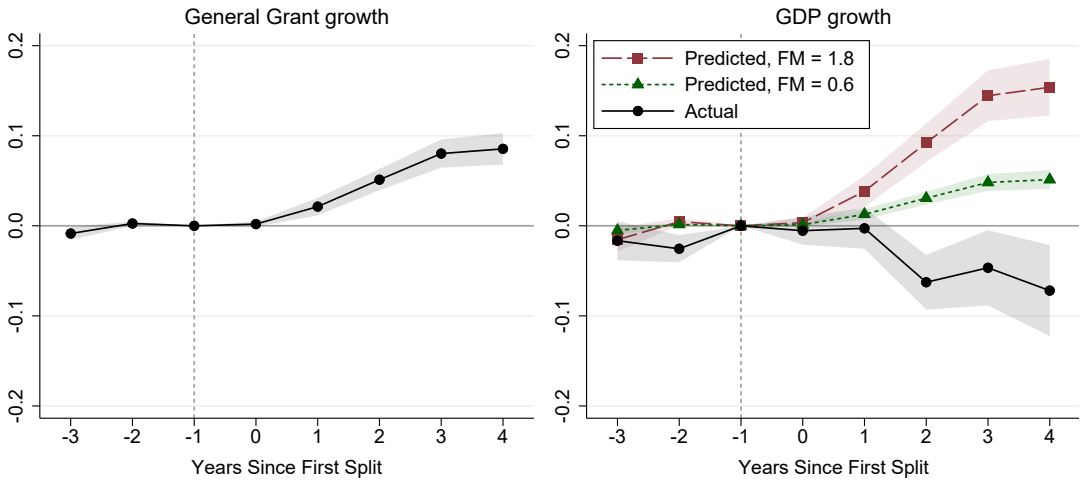
Notes: This figure plots point estimates and 95-percent confidence intervals of $\ln(2) \cdot \beta^h$ from Equation (3), the effect of doubling the number of districts in year t on GDP in year $t+h$. The model is estimated at three different levels of aggregation, as indicated in the figure. The confidence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

Figure B.3: The Effect of District Splits on General Grant and GDP (Robustness)

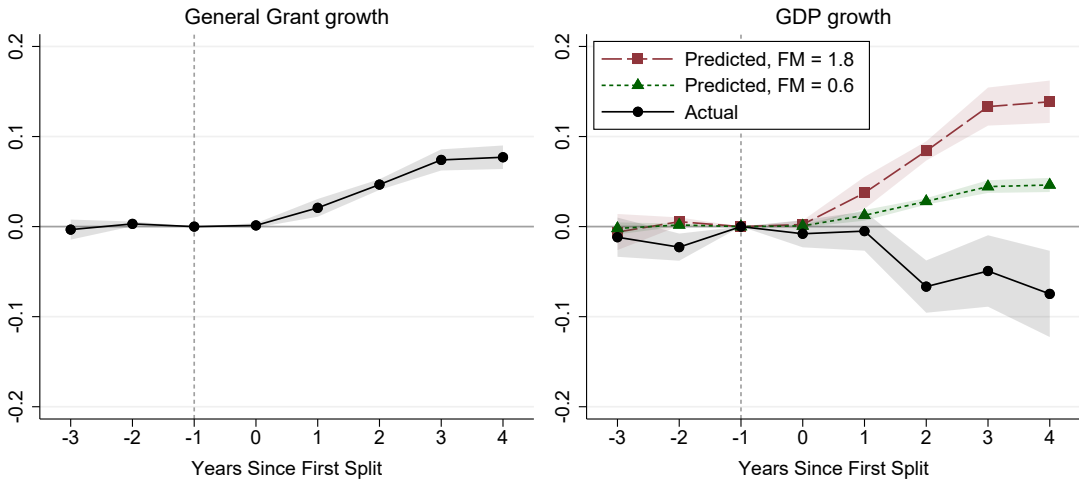
(a) Additional Controls



(b) Trimmed Sample



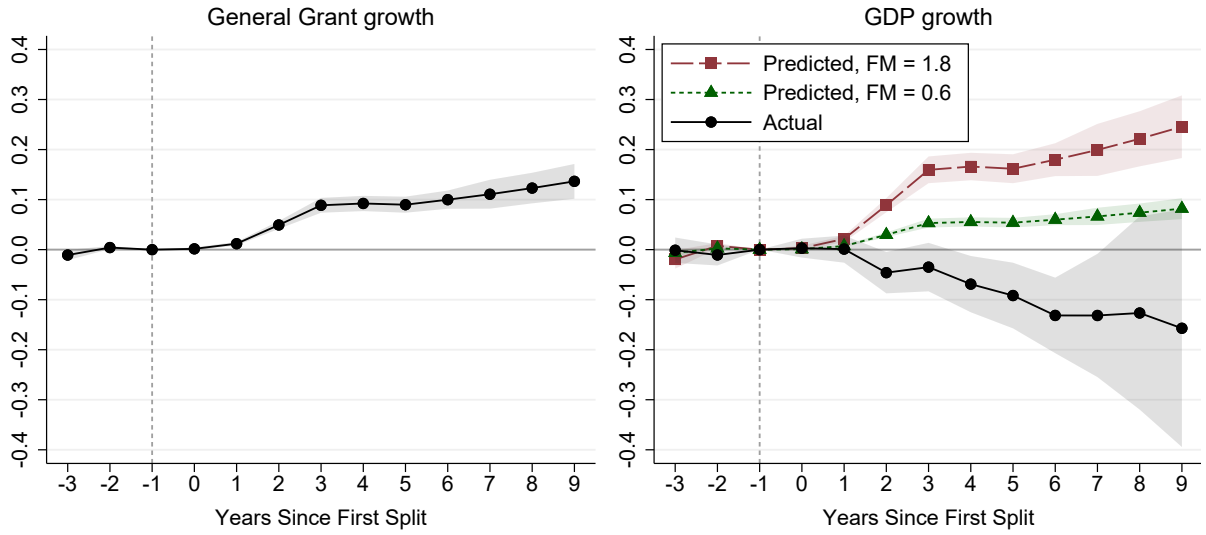
(c) Additional Controls, Trimmed Sample



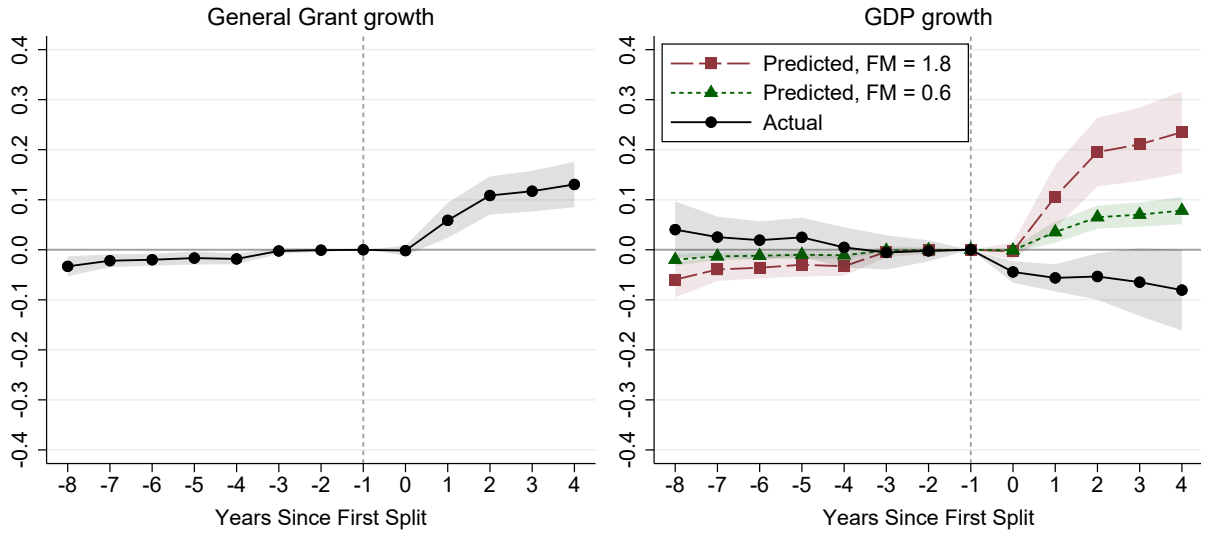
Notes: This figure plots robustness checks for Figure 2. The estimates in Panels (a) and (c) control for ethnic fractionalization, urban population share, share of population aged 15–64, share of population with a primary education, and share of population with a secondary education, all measured in 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.4: The Effect of District Splits on General Grant and GDP: Early vs. Late Splits (Baseline Specification)

(a) Early Splits (2002–2004)



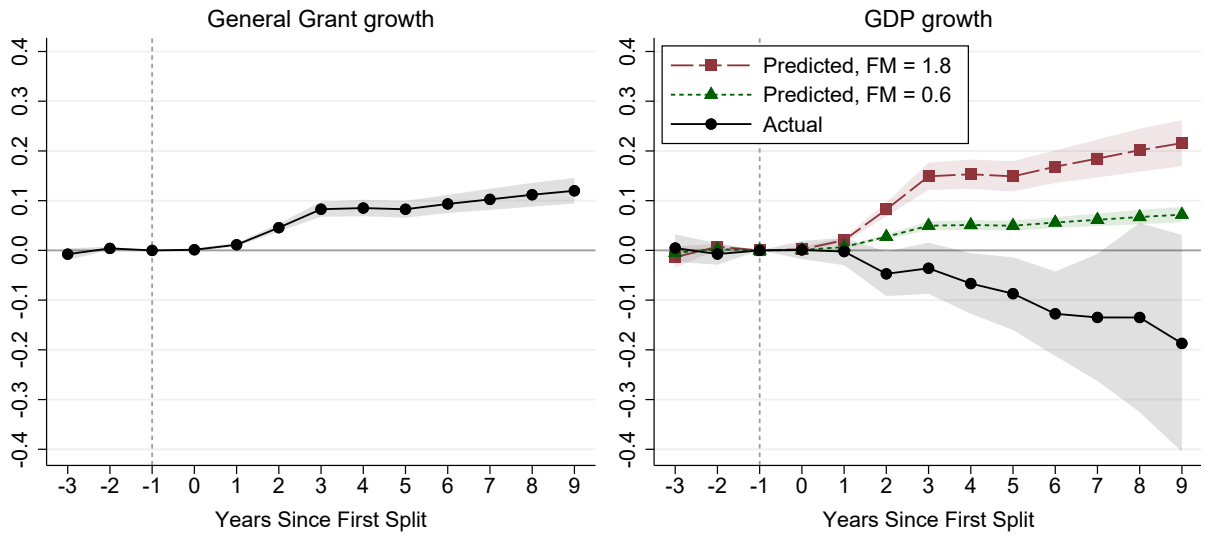
(b) Late Splits (2008–2009)



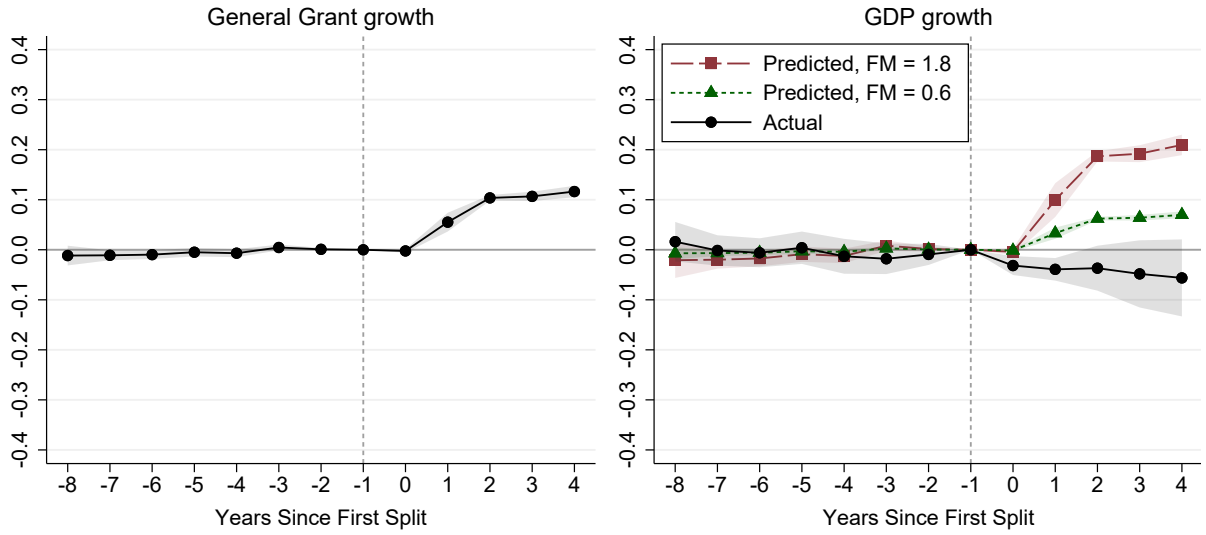
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. The left panel shows the impact of the first district split on growth in general grant revenue relative to the year before the split, scaled by GDP in that year. The right panel 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.5: The Effect of District Splits on General Grant and GDP: Early vs. Late Splits (Additional Controls)

(a) Early Splits (2002–2004)



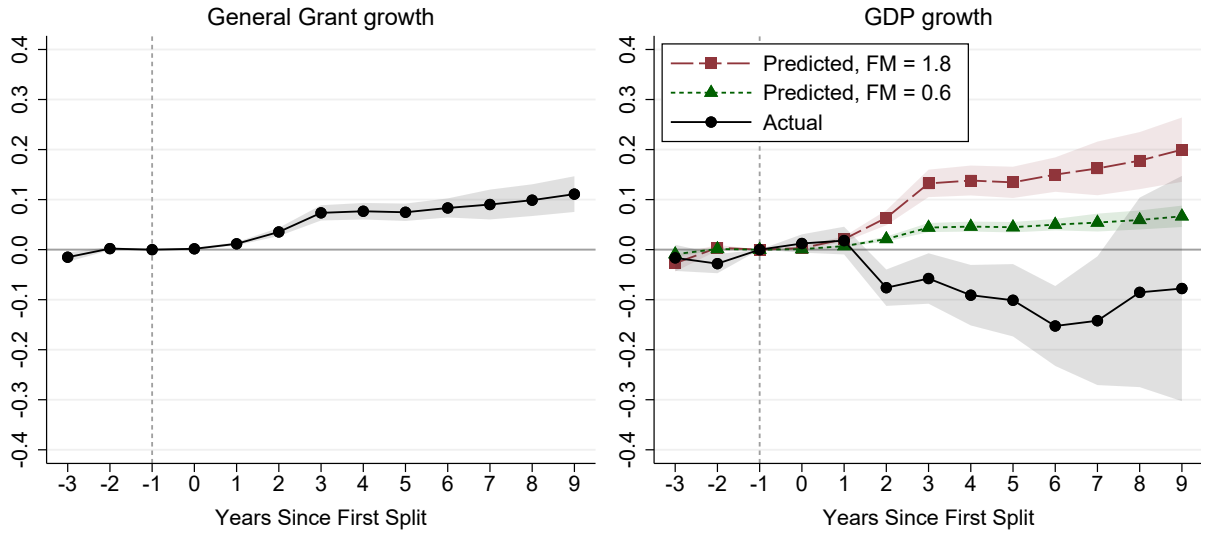
(b) Late Splits (2008–2009)



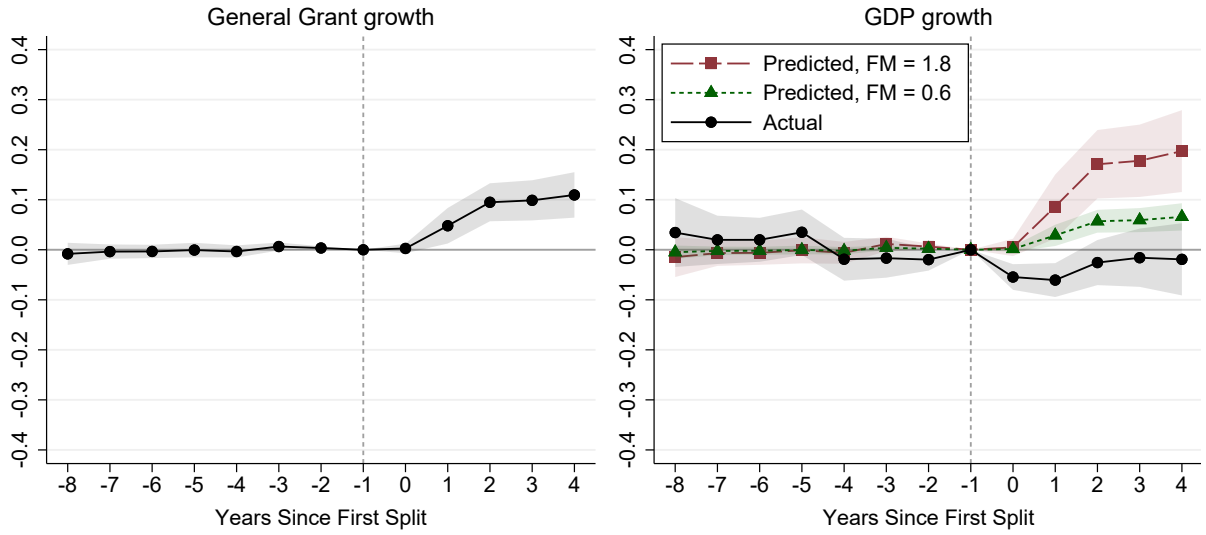
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. The left panel shows the impact of the first district split on growth in general grant revenue relative to the year before the split, scaled by GDP in that year. The right panel 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 (Trimmed Sample)

(a) Early Splits (2002–2004)



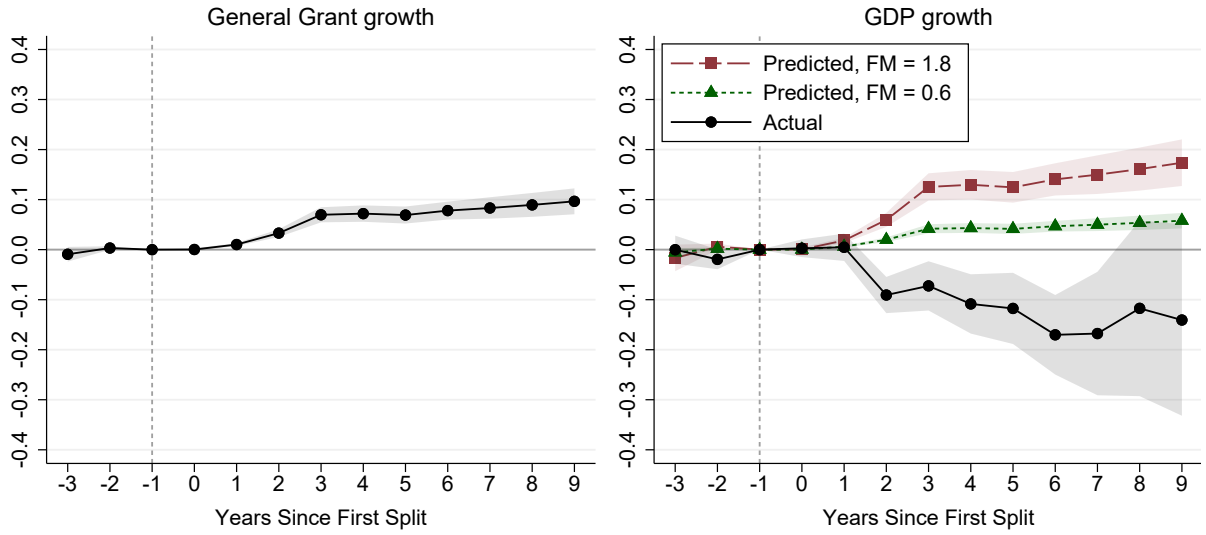
(b) Late Splits (2008–2009)



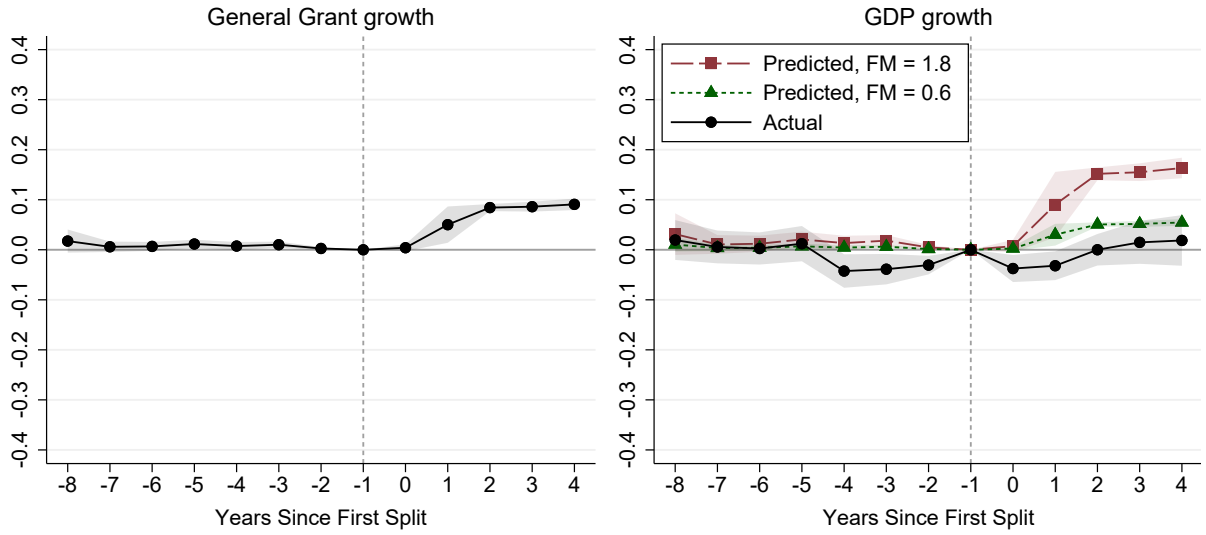
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. The left panel shows the impact of the first district split on growth in general grant revenue relative to the year before the split, scaled by GDP in that year. The right panel 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 (Additional Controls, Trimmed Sample)

(a) Early Splits (2002–2004)

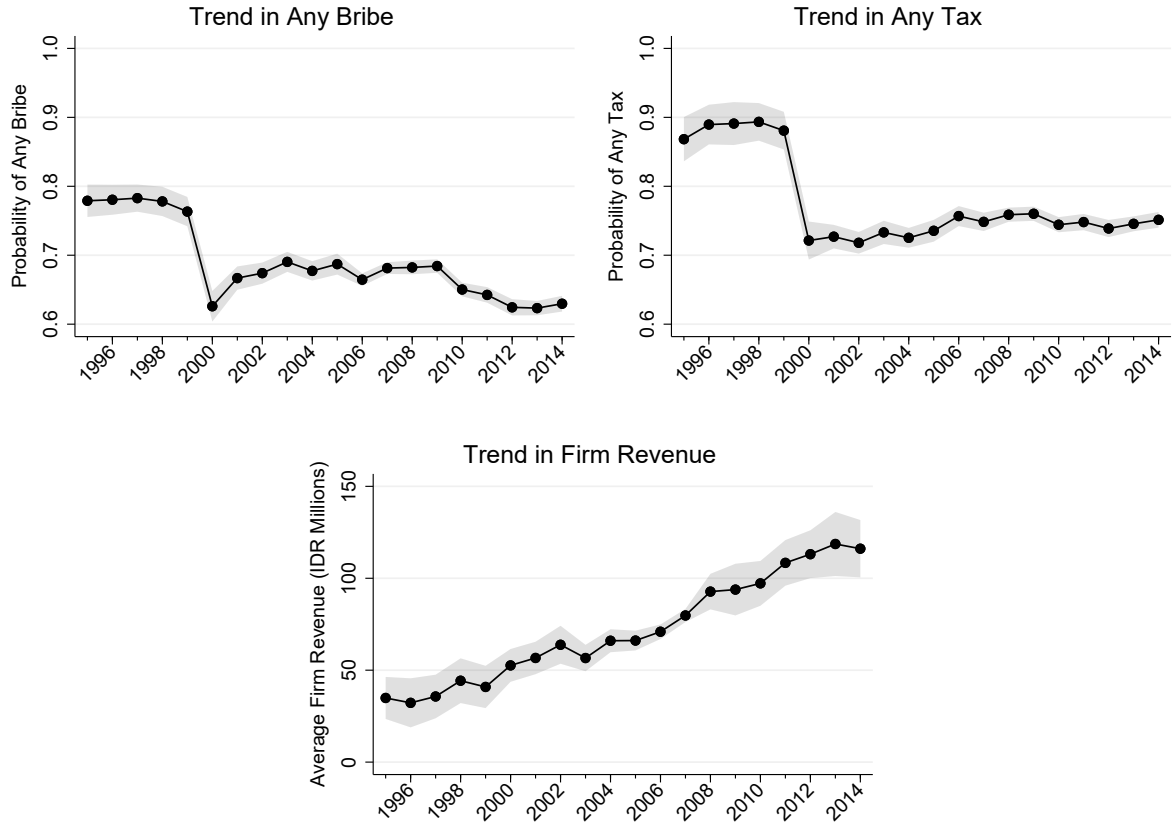


(b) Late Splits (2008–2009)



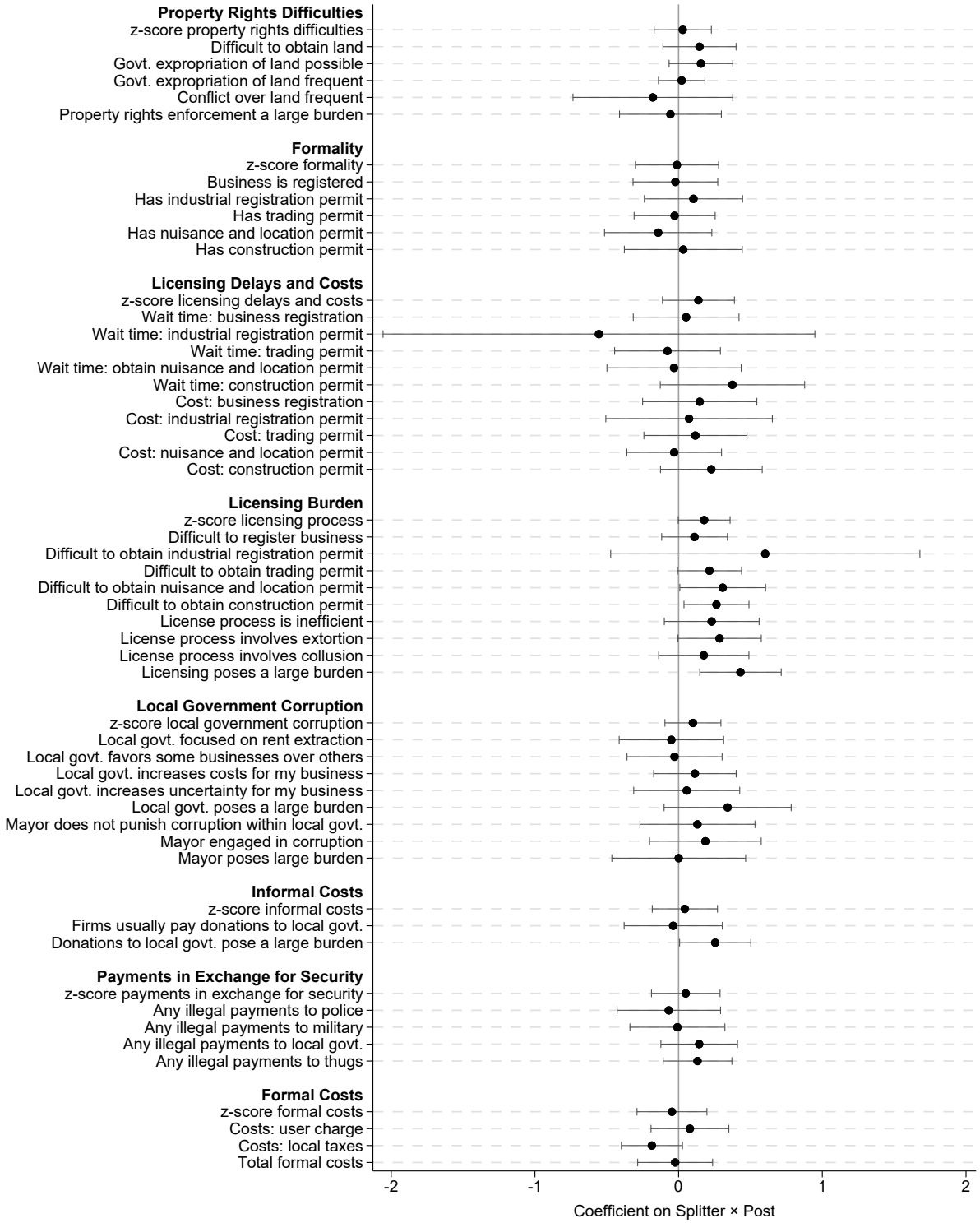
Notes: This figure plots estimates of the cohort-size-weighted CATT (Equation (2)) and their 95-percent confidence intervals. The left panel shows the impact of the first district split on growth in general grant revenue relative to the year before the split, scaled by GDP in that year. The right panel 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: Trends in Key Firm Variables in IBS Survey, 1995–2014



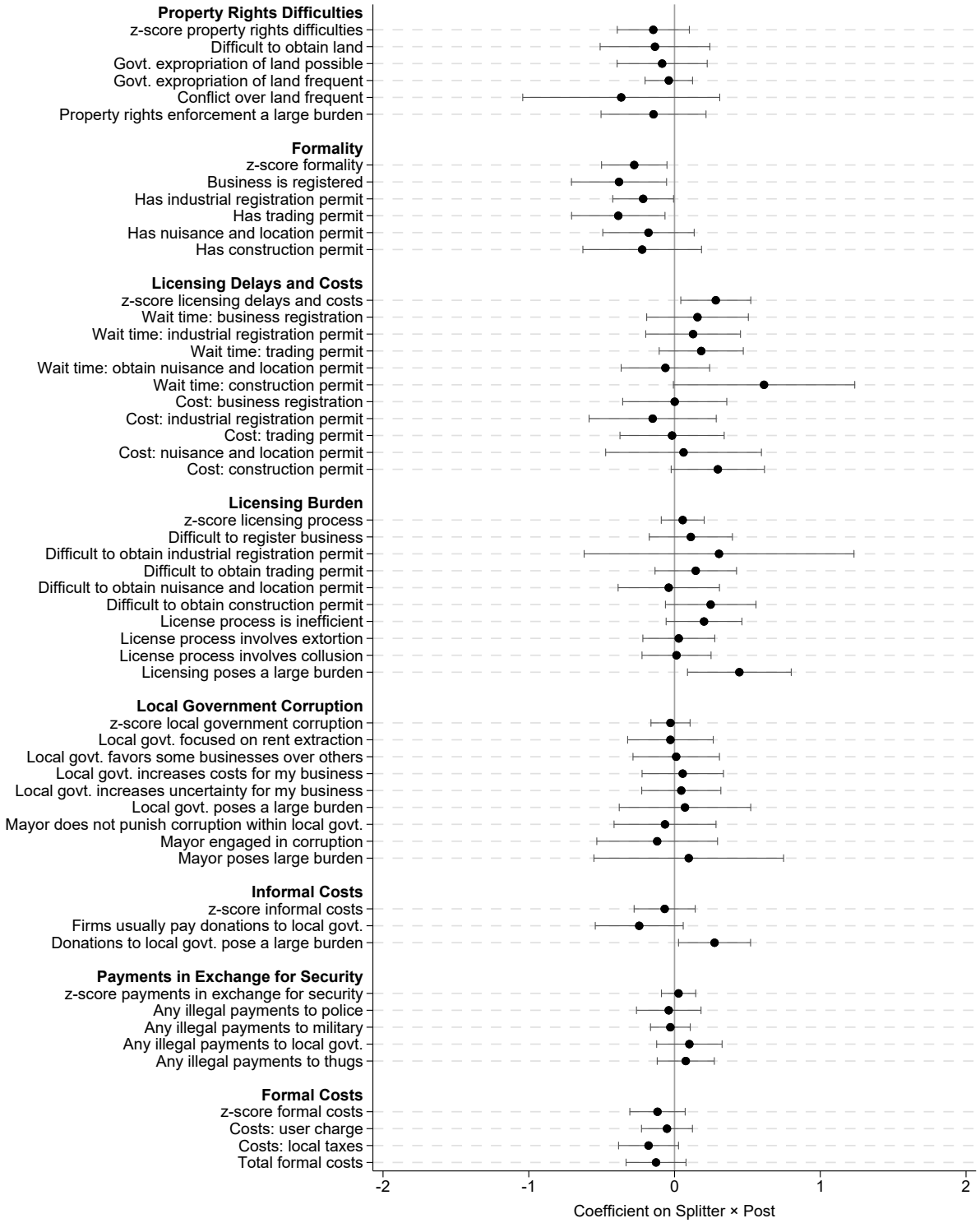
Notes: This figure plots the fitted values of a regression of the outcome on year dummies, controlling for firm fixed effects. Standard errors are clustered by district, and 95-percent confidence intervals are reported.

Figure B.9: 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.10: 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.