Does Intergovernmental Competition Improve the Business Environment?*

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Abstract

Corruption, red tape, and underprovision of public goods impose significant burdens on firms in developing countries. We examine whether intergovernmental competition improves the business environment in the context of a major period of decentralization in Indonesia that increased the number of local governments by 50 percent within a decade. District governments, which are responsible for the majority of local public expenditure and receive revenue from business licensing and fees, split into smaller districts, increasing the number of local governments within original district boundaries. We exploit idiosyncratic variation in the timing of splits generated by a national moratorium to estimate the causal effects of intergovernmental competition. We find that the fragmentation of local government leads to an increase in the prevalence of informal gift payments that is accompanied with some increase in services, which is consistent with imperfect mobility and monopolistically competitive district governments. However, competition does cause an increase in infrastructure expenditure, potentially benefiting firms. Districts target infrastructure improvements toward villages that become closer to competing districts as a result of the split.

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1 Introduction

The World Bank estimates that 18 percent of businesses around the world and 28 percent of businesses in low-income countries have been asked to pay a bribe at least once (World Bank, 2017). Often these bribe payments are made for routine business activities like registration or licensing, which are the purview of local government officials. Local governments may also rely on the support of local businesses for own tax revenue or for re-election. Thus local governments face a trade-off between rent extraction and attracting and retaining firms, which have the option of "voting with their feet" by relocating to a more business-friendly jurisdiction (Tiebout, 1956). Competition among local governments may thereby limit rent-seeking. This is one reason why international agencies have encouraged decentralization as a tool for promoting economic development (World Bank, 1999; United Nations, 2009; International Monetary Fund, 2009).

Decentralization, which is the assignment of expenditure or revenue responsibility to local governments, might be recommended to deliver the appropriate level of local public goods to match diverse citizen preferences (Oates, 1972). It is an active and important institutional choice that is made by both developed and developing country governments. Even if political accountability is not its primary goal, decentralization could have important effects on policy choices operating through the electoral or revenue concerns of elected officials. This paper examines the consequences of decentralization for firms—a decision-making unit that can potentially hold elected officials accountable through revenue rather than re-election incentives.

We examine an extraordinary period of decentralization in Indonesia from 2001 to 2014, which increased the number of district governments by 50 percent. Districts are responsible for the majority of local public expenditure and receive revenue from business licensing and fees. Districts also receive shared revenue from taxes paid by local businesses, which are designed and administered by the central government. The splitting of districts into smaller districts increased the number of local governments within original district boundaries. Our design exploits cross-district variation in splitting, combined with idiosyncratic variation in the timing of splits generated by a national moratorium, to estimate the effect of intergovernmental competition on business fees and bribes paid by manufacturing firms to local officials.

We combine this empirical setting with unusual data on"gifts" by manufacturing firms in Indonesia to external parties, which we interpret as including payments to government officials in the course of business.[1] These payments are positively correlated with business activities requiring licenses and negatively correlated with choices that avoid government involvement, supporting our interpretation. We also show that estimates of bribe payments in our data are comparable to other surveys that explicitly ask about bribe payments. In addition to such informal payments, we also consider formal taxes and fees, and public goods that are particularly important to businesses.

[1] In Indonesia the terminology of "gift" payments is a common euphemism in surveys to refer to bribe payments.

Existing theory and empirical evidence on the impact of decentralization on rent-seeking are often conflicting. Supporting the view that decentralization improves accountability through interjurisdictional competition are models by Brennan and Buchanan (1980) and Persson and Tabellini (2002). Crucially, these models rely on mobility of constituents as a disciplining mechanism. Empirical evidence suggests that mobility plays a crucial role in creating accountability in local governments. Diamond (2017) shows that local governments in the United States are able to extract greater rents when the housing supply is more inelastic, due to a reduction in the threat of out-migration. Bai, Jayachandran, Malesky and Olken (2019) show that firm growth is associated with lower bribes and suggest that mobility is the main driver of this correlation. With imperfect mobility, which is a more serious concern in a developing country context (Bardhan, 2002), the benefits of competition are lower and other factors play a larger role in determining the consequences. For example, there is a mechanical increase in the number of bureaucrats and potentially a decrease in their average quality, which could increase rent-seeking. Even with perfect mobility, governments may compete on other margins such as the provision of necessary infrastructure, formal taxes and fees, or even the processing times for permits and procedures to retain firms without reducing petty corruption.

Some models of interjurisdictional competition suggest that competition itself could increase rent-seeking. Bribes associated with licenses may even increase if we consider these goods as being monopolistically supplied by local governments; an increase in the number of competitors increases the amount of licenses supplied, decreases the average bribe "price" of the license but could raise the prevalence of bribe payments (Shleifer and Vishny, 1993).[2]

Empirical evidence on the subject is also mixed. In a cross-country study, Fisman and Gatti (2002) find that greater decentralization is associated with lower corruption. On the other hand, Burgess, Hansen, Olken, Potapov and Sieber (2012) study the same decentralization episode as this paper and find that illegal logging, which is controlled by local governments, increased as a result of decentralization. This paper contributes to the scarce direct empirical evidence on the impact of decentralization on bribery associated with interactions between firms and local governments. It also examines whether local governments become more accountable to businesses through other margins such as the provision of important public goods like roads and street lighting.

We find little evidence that competition among local governments reduces the business fees and bribes paid by firms on average, and in fact, find evidence of the opposite. The prevalence of bribe payments increase, particularly in districts that split under appointed mayors. Mobility matters to some extent—in districts that split under elected mayors, the probability of any gift payments increase for large firms, which are less mobile. In other margins, we see improvements. Formal taxes and fees decrease as a share of revenue, activities

[2] Shleifer and Vishny (1993) distinguish between corruption with and without "theft," i.e. whether the bureaucrat must remit the official cost of the license to the government. In the case without theft, competition reduces the bribe price to zero, reducing the prevalence of bribes. With theft, competition lowers the price but not to zero, raising the prevalence of bribes

associated with major licenses such as land rentals, purchases and building additions, increase.

We also find that competition influences the provision of public goods that are complementary to firm activities. District splits lead to an increase in infrastructure expenditure, even after controlling for changes in transfer revenue. Following a split, road quality and street lighting improve in villages that become closer to competing districts as a result of the change in borders. This suggests that district governments target infrastructure improvements toward areas where firms could relocate to another district by moving only a short distance.

This paper contributes to the literature on the impact of decentralization and the closely related literature on intergovernmental competition. One of the channels through which decentralization can improve welfare is by creating the opportunity to "vote with your feet." Because we focus on the fragmentation of a particular level of government rather than the transfer of power from a central to subnational government, our results isolate the contribution of competition in decentralization.

Previous literature on decentralization has mainly focused on public goods such as health and education—amenities that a household might consider when choosing their location. We add to this literature by studying the impact on outcomes that are particularly important for firms and consider firms as the unit holding governments accountable through the threat of exit rather than households.

The empirical setting of Indonesia's decentralization offers many advantages. First, an increase in the number of local governments is perhaps the most natural way to conceptualize an increase in horizontal competition among governments. Few papers on intergovernmental competition exploit this type of variation.[3] One exception is Lima and Neto (2018), who examine the effect of municipal secessions on local public expenditure in Brazil. On the other hand, a large literature examines situations in which municipal jurisdictions grow in size by annexing or merging with nearby areas (e.g., Reingewertz, 2012; Breuille and Zanaj, 2013).

Second, our panel dataset allows us to control for time-invariant regional characteristics. This is important, because areas with many local governments may differ from those with few local governments along many dimensions. Some prior work has addressed this problem by finding an instrument that explains cross-sectional variation in interjurisdictional competition. For example, Hoxby (2000) uses the number of streams in a metropolitan area as an instrument for the number of school districts, and Mast (2018) uses the location of a town within a county as an instrument for the number of nearby county governments. Valid instruments are often hard to come by in cross-sectional settings. Exploiting the creation of new governments over time therefore offers a promising alternative approach that will prove useful in countries besides Indonesia.

[3] In a cross-country study of the relationship between decentralization and corruption, Arikan (2004) measures decentralization as the number of local jurisdictions per capita. However, the cross-country literature often measures decentralization using the subnational share of total public expenditure (e.g., Fisman and Gatti, 2002).

2 Empirical Context

Following the ouster of autocratic ruler Suharto in 1998, Indonesia transitioned to democracy and instituted a series of political and fiscal reforms. Districts are the second tier of government, provinces are aggregations of districts, and sub-districts 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. Decentralization advanced further with the implementation of the Village Law starting in 2015. This law expanded 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 local governments.

District parliamentarians could petition to split a district into two or more districts, with the approval of the mayor of the original district. The central government would then decide whether to accept or reject the petition. During the sample period, the number of districts increased from 341 to 514, an increase of 50 percent.

Figure 2 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.[4] About one third of the original districts split at least once between 2001 and 2014. The map shows that 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.

The central government sets tax rates on property, sales, individual income, and corporate income, and administers these taxes. It then 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 (World Bank, 2003). District governments are directly responsible for many business licenses, which formally may be obtained by paying a fee set by the district.

District officials are held accountable through elections. Following decentralization, district heads were elected by members of the local parliaments (instead of appointed by the central government). Starting in 2005, districts introduced direct elections of district heads in a staggered fashion.[5]

The central government funds the operation of local government and promotes fiscal equalization though the General Grant (*Dana Alokasi Umum*). This grant includes a basic allocation consisting of a lump-sum transfer and portion that depends on the civil service wage bill. The rest of the grant is apportioned according to a formula that uses proxies for

[4] 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.

[5] The election timing was staggered because incumbents were allowed to finish their five-year terms, which for idiosyncratic reasons were not synchronized across districts.

expenditure needs (e.g., population, land area, poverty) and fiscal capacity (e.g., predicted revenue from other sources) (World Bank, 2007; Cassidy, 2019). The formula-based portion of the grant increases by less than one-for-one with population. This feature, together with the lump-sum component, virtually guarantee that grant revenue per capita increases in both the child and parent districts following a split. Bazzi and Gudgeon (2018) document that splits causes total fiscal transfers to increase by 20 percent on average Our empirical strategy will account for the mechanical change in district revenue due to these transfers, and examine the change in district revenue and expenditure above this mechanical increase.

3 Theoretical Framework

We hypothesize that the district splits exogenously raised competition between local governments within the boundaries of the original district, and potentially in neighboring districts. As a result, we expect that the formal and informal tax burdens of firms would decrease as districts compete to attract and retain businesses. We also expect competition to increase the provision of public goods, such as infrastructure, that are complementary to firm activities.

We conceive of bureaucrats in local governments as providing a good (licenses) whose quantity they can restrict, for example by delaying or denying permits, and for which they can charge firms a bribe on top of official fees. Bureaucrats in each local government act in their own interest, do not collude with other governments, and face a downward sloping demand curve for their product. This is the basic model in Shleifer and Vishny (1993), who use standard models of Bertrand or Cournot competition to show that an increase in the number of government agents (districts) supplying the same good (business licenses) would increase the quantity of goods supplied (more licenses) and decrease the price (lower average bribe). One counter-intuitive implication is that more firms might be able to obtain licenses, thereby increasing the probability that a given firm pays a bribe but decreasing the average bribe price.

Firms' choice of location (and therefore, government) may depend on factors other than the bribe price, supply of licenses, or local public goods. Such factors include the availability of natural resources, labor, or specific intermediate inputs. These factors constrain the competitive impact of new districts. Bureaucrats may also price discriminate according to firms' bargaining power or location-specific preferences. For example, districts might attract firms with lower moving costs, such as new firms, by offering to ease procedures without making such concessions for established firms with high moving costs. Districts may also focus improvements in public goods on areas near competing districts, where firms could change districts without losing access to their local suppliers and workers. We test for heterogeneity in the impact of district splits along these dimensions.

4 Data

Our dataset combines establishment-level survey data with institutional data describing the proliferation of districts over the period 2001–14. For ease of exposition, we will use the terms "firm," "establishment," and "plant" interchangeably, even though we cannot link establishments belonging to the same firm. We use data from the Indonesian Annual Manufacturing survey, which covers the universe of establishments with at least 20 workers. It captures information on establishment production such as total value of production, number of employees, and industry of operation.

Establishments report their total tax payments, including land and building tax, and "company license fees," which are administered by local governments. However, most tax rates and rules are set by the central government.

Another outcome of interest is "gifts" to others specifically by the firm and not by the owner or manager, which can include payments to government officials. 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. Therefore we 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.

In our sample 65 percent of firm-years featured positive "gift" payments (see Table 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 et al. (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.

To lend further credence to our interpretation of "gifts" as bribes, Table 2 shows 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 any own generation of electricity or purchase of electricity from non-governmental sources.

Data on district revenue and expenditure for 2001–2014 come from the Ministry of Finance (*Kementerian Keuangan*) and the World Bank's Indonesia Database for Policy and Economic Research (INDO-DAPOER). We aggregate the public finance variables to the level of district borders in 2000. Data on local public goods come from the Village Potential Statistics (*Pendataan Potensi Desa*, or PODES) survey waves of 2000, 2003, 2005, 2008, 2011, and 2014. The PODES survey is intended to cover the universe of villages in Indonesia, of which there were around 70,000 in 2014. Many villages split into multiple villages during the period of study, so we aggregate village outcomes to the level of village borders in 2000. See Cassidy (2019) for more

details on the construction of the public finance and public goods datasets.

We drop all five districts in the province of Jakarta. These districts are managed at the province level and hence do not compete with other districts in same province. Dropping Jakarta reduces the number of firm-year observations by 25,868, or just under 8 percent of the original sample.

4.1 Summary Statistics

Table 1 provides the summary statistics. Panel A summarizes the firm-level variables. The sample around 300,000 firm-year observations. Most firms make payments to local officials: firms paid formal taxes or business license fees in 74 percent of the observations, and they paid gifts in 65 percent of the observations. These payments represented a small fraction of total firm revenue on average, though there is considerable variation across firms. Formal taxes and fees were 1.07 percent of revenue on average with a standard deviation of 4.43. Gifts represented 0.39 percent of revenue on average with a standard deviation of 2.49. Both types of payments ranged from 0 percent to nearly 100 percent of revenue in a given year.[6] Firm size also varies considerably. Total revenue is 76 million IDR (in constant 2010 IDR), or roughly 7,600 USD, on average, with a standard deviation of 740 million IDR (74,000 USD). The maximum revenue observed in the sample is 142 billion IDR (14.2 million USD). The number of employees has a mean of 194, standard deviation of 740, a minimum of 20, and a maximum of over 56,000. Twenty-three percent of firms are "large" in the sense of having employed at least 200 workers at some point during the sample period.

Panel B of Table 1 summarizes the district-level variables. While just under one third of the districts that existed in 2000 eventually split, only 21 percent of the firm-year observations occur following the first split of a district. This is because many splits occurred later in the sample, and a majority of firms are located on the island of Java, where splitting was less common. The number of districts observed within the 2000 district borders ranges from one to eight. Fifty-six percent of observations occur under the leadership of a directly elected district mayor, and districts received a general grant of 1.21 million IDR (121 USD) per capita on average, calculated at the level of the borders in 2000.

5 Empirical Strategy

We use district splitting as an exogenous, local shock to intergovernmental competition. We first estimate the reduced-form effect of district splits on formal and informal tax payments by firms located within the boundaries of the original district. Because the splits may have spillover effects on neighboring districts, our baseline estimates are a lower bound on the

[6] In a small number of cases, taxes or gifts exceeded revenue, sometimes by a very large amount. We treat these observations as survey errors and drop them. This problem occurs in only 0.1 percent of the observations.

direct effect of the splits on the tax burden. We also test for spillover effects ahead.

A district's decision of whether to split is endogenous. Examining the first wave of splits following decentralization (2001–03), Fitrani, Hofman and Kaiser (2005) find that splits are more likely among districts with low population density, high ethnic diversity, and a bloated bureaucracy. 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. After the split has been approved, it takes another one to two years before the new district becomes autonomous, meaning that they have held their first elections and have started receiving transfers from the central government. Thus the 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. In fact, more than 100 applications awaited consideration by the end of the first moratorium. (See Bazzi and Gudgeon, 2018, for details.) We assume that the regulatory factors that influence the timing of splits are exogenous with respect to local economic and political conditions that may affect our outcomes of interest. Specifically, we assume that average firm outcomes in districts that split in a given year, and districts that did not split in that year, would have followed parallel paths in the absence of splitting.

When a new district is created, 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 year when these transfers first arrive, as this is when the new government is autonomous and can credibly compete with neighboring districts.

Districts are defined according to the original district boundaries in 2000. 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.

The baseline firm-level specification is

$$Y_{fdit} = \beta Split_{dit} + \alpha_f + \lambda_{it} + \varepsilon_{fdit}, \tag{1}$$

where Y_{fdit} is an outcome of establishment f, located in district d and island group i, in year t. $Split$ is an indicator variable that equals 1 in the year of the district's first split and following years.

The model includes establishment fixed effects, α_f, so that the effect of splitting is identified from changes over time within the same firm. This not only allows the level of firm outcomes to

systematically differ in splitting and non-splitting districts prior to the split; it also eliminates the influence of changes in the composition of firms over time within the district. Finally, the model includes island × year effects, λ_{it}, to allow for arbitrary differences in development trajectories across regions.[7]

The parameter of interest is β, the average effect of the first district split on firm outcomes. Two related assumptions are needed to identify β. First, the outcomes of firms in splitting districts and non-splitting districts would have experienced similar trends, on average, in the absence of splitting. Second, firms in districts that split early and districts that split late would have experienced similar trends, on average, had all splits occurred at the same time.

We focus on two main outcomes: taxes/fees and bribes paid by firms. Intergovernmental competition may affect these outcomes along both an extensive margin and an intensive margin. To examine extensive-margin responses, we define Y to be an indicator variable that equals 1 if the firm paid any taxes/fees (or bribes). To examine the intensive margin, we define Y to be the taxes/fees (or bribes) paid by the firm as a share of the firm's total revenue. We measure total revenue as the total value of goods produced, as reported in the census.

While equation (1) is attractive due to its parsimony, it assumes that treatment effects are constant across time and districts. In the presence of treatment-effect heterogeneity acrosss time or districts, the fixed-effects estimator may not recover a reasonably weighted average treatment effect (see, e.g., de Chaisemartin and D'Haultf??uille, 2019). We address this concern in two ways. First, we report estimates using a "balanced" panel of districts in which each splitting district is observed the same number of times pre- and post-split. Specifically, the subsample restricts the set of event periods to $\{-3, -2, -1, 0, 1, 2, 3, 4\}$ and the set of treated districts to those that are observed in every event period included in the sample. This guards against overweighting early splitters relative to late splitters in estimating β.

Second, we estimate the flexible model

$$Y_{fdit} = \sum_{s \in \mathcal{S}} \beta_s 1(t - T_d = s) + \alpha_f + \lambda_{it} + \varepsilon_{fdit}, \tag{2}$$

which allows for treatment effects to vary according to the amount of time since treatment occurred. The variable T_d is the year district d first split, and s indexes event-time periods.[8] The omitted reference period is $s = -1$, the year prior to the first split. The indicator variable $1(t - T_d = s)$ is zero for all periods in districts that never split. The parameter β_s thus represents the effect of the first split on outcomes s years after the split occurred, relative to the effect of splitting on outcomes one year prior to the split. In addition to estimating dynamic effects, the flexible model allows us to test for differential trends prior to the split among firms in splitting and non-splitting districts. The null hypothesis of no differential pre-trends is $\beta_s = 0$ for all

[7] Following the Indonesian Statistical Bureau, we code seven island groups: Sumatra, Java, Nusa Tenggara, Kalimantan, Sulawesi, Maluku, and Papua.

[8] The set of event-time periods is $\mathcal{S} = \{-4+, -3, -2, 0, 1, 2, 3, 4, 5, 6+\}$.

$s < 0$.

We also compare the dynamic effects in this full sample to effects in two more restrictive district sample that we observe over two balanced event-time panels. To eliminate the effect of changes in the composition of districts observed at each event-time, we consider a sample of districts that are observed in every period three years prior to and 4 years following the district split (66 % of districts that ever split). A more inclusive panel restriccts the event-time horizon to 1 year prior and 4 years following the district split, which covers 83 % of districts that split.

6 Results

6.1 Baseline Estimates

On average, we find that the increase in the number of local governments does not have a disciplining impact on bribery. In fact, we find some evidence of the opposite—bribes are more prevalent following splits although the average amount of bribes as a share of revenue does not change. Table 3 presents the estimates of β in equation (1).[9]Formal taxes and fees as a share of revenue declines by 13.1 percentage points following the first split without any change in the probability of formal tax/fee payments (columns 1 and 3). On the other hand, the probability that a firm reports any bribe payments increases by 2.7 percentage points following the first split, while there is no change in bribe payments as a share of revenue (columns 2 and 4). (Note that the effect on the probability of bribery in insignificant in the balanced panel.) Figure 4 plots the dynamic estimates from (2) and shows an absence of differential trends prior to the split.

One possible interpretation is that splits do not change the bribe *rate*—the informal price of services provided by the bureaucrat—but potentially increase access to these services with a payment of fee. In Shleifer and Vishny (1993), competition between local governments can increase the incidence of corruption in the same way that Cournot competition between firms increases total quantity supplied. More firms might now be able to get the licenses they require or construction approval, after payment of a fee.

We test whether activities associated with fees increase after splits and find that they do, at least temporarily. Table 4 shows how exports, land rentals/ contracts, land purchases, building additions and electricity connections are affected by district splits. Only the probability of land contracts show a statistically significant increase (by 1.3 percentage points) after a split. However, Figure 5 shows that there is a delayed increase in capital investment in land and buildings, as well as an increasing trend in the dynamic treatment effect on exports.

[9]The sample sizes are smaller than those reported in Table 1, because we drop singleton groups in order to ensure valid inference (Correia, 2015). Singleton groups are groups defined by the fixed-effects structure that contain only one observation. In our case, a singleton group is either a firm observed in only one year or an island-year pair in which only one firm is observed. We use the Stata package `reghdfe`, which identifies and drops singleton groups (Correia, 2016).

Although we are unable to directly measure changes in licensing following splits, we find some evidence that acitivities associated with important local business licenses for manufacturing firms increase following splits. This is consistent with an increase in the prevalence of bribes associated with more firms getting access to required licenses.

Because we use a staggered adoption design,that the dynamic treatment effects in the full samplevary because of changes in the composition of districts observed at each event-time. Appendix figures C.1 and C.2 show the dynamic treatment effects in two "balanced" samples, i.e., restricted to districts that are observed at all event-times within the displayed time horizon. Figure C.1 shows the results for a longer event-time horizon that is available for the more restricted sample of districts, which excludes early and late-splitters. Figure C.2 shows the results for a shorter event-time horizon that is available for a larger sample of districts. We see that there are parallel pre-trends in our main outcomes for the restricted balanced panel but that the impact on bribes is largely driven by districts that are excluded from this sample. This suggests that there is a difference in treatment effects across early and late splitters, which we hypothesize is due to the difference in electoral incentives over time.

Mayors, who must approve applications to form new districts, were only directly elected starting in 2005. The exact year in which direct elections were introduced varies because appointed mayors were allowed to complete their terms, which ended at different times. We compare the impact of splits across districts whose applications were likely approved by directly elected mayors ("Split Post-Election") with those likely approved by appointed mayors ("Split Pre-Election"). Figure 7 presents the results. We see that the increase in bribes following splits comes from districts whose splits were approved by appointed rather than elected mayors, which was more likely among earlier splits. This finding is in line with Martinez-Bravo, Mukherjee and Stegmann (2017) who showed that governance quality is lower in districts where Suharto-appointed mayors were replaced later. In the main tables, we present the results for the full sample of districts as well as the more restrictive balanced sample that is observed for an event-time horizon 3 years prior to and 4 years following the first split.

Another explanation might be that newly created districts have less experienced officials and that the increase in prevalence of bribes might be driven by this inexperience. To the contrary, we find that the increase in bribe prevalence is driven by parent districts, not child districts. The probability of any gift payments increase by 3.3 percentage points in the parent district following a split, while there is no change in the child district (Table 5). One caveat to these results is that we measure firms' location in the child or parent district based on where they are observed in the first year of the split. The assumption is that firms do not move within the first year. To the extent that this assumption fails and that there is some immediate movement of firms without a relationship with local officials out of the parent district, we are likely to find that those who remain are more likely to pay bribes.

The firm-level data does not provide finer geographic identifiers than the district. But as we will show in the section on public goods, we find evidence that district governments reallocate

resources towards villages within the district that are closer to a district border as a result of a split. We test whether the average treatment effects in firm outcomes measured at the district level are masking within-district heterogeneity by looking at variation in treatment effect by the share of villages within the district that are closer to a border following a split. We do not find strong evidence of such treatment heterogeneity. Figure 6 shows very similar patterns in the change in prevalence of bribes and formal taxes/fees across districts where a larger or smaller share of villages experienced a change in the distance to the nearest border after the first split.

6.2 Robustness Checks

Several confounding factors may cause the baseline estimates to be biased. First, in some cases district splits resulted in new districts that were more ethnically homogeneous than the original district. The level of ethnic fractionalizatoin could directly affect rent-seeking.

Second, prior to 2005, the mayor of each district was appointed by the district legislature. Districts then introduced direct mayoral elections in a staggered fashion over the period 2005–08. Direct elections were introduced in different years because the last appointed mayor was allowed to finish his or her term, and mayoral terms were not synchronized across districts. Among firm-year observations in districts that eventually split, 62 percent occur in districts that experienced a *de facto* split prior to 2005. If directly elected mayors matter for rent-seeking, the potential correlation between the timing of splits and the introduction of direct elections could bias our baseline estimates.

A third potential source of bias relates to the structure of fiscal transfers from the central government to district governments. The most important intergovernmental grant, the General Grant (*Dana Alokasi Umum*), is allocated according to a formula that includes a lump-sum component which does not depend on district population. As a result, when a district splits into multiple districts, grant revenue per capita mechanically increases as measured at the level of the original district borders. Henderson and Kuncoro (2006) argue that grant revenue can crowd out license fees and bribes by causing an increase in the salaries of local bureaucrats. Therefore our baseline estimates may partially reflect the influence of the General Grant, which increases after a split, on fees and gifts.

Figure 8 shows how the baseline results change when we control for potential confounders. The first control is a time-varying measure of ethnic factionalization calculated according to the distribution of ethnic groups in 2000. The variable was constructed by Alesina, Gennaioli and Lovo (2019). The second control is an indicator variable that equals 1 in the year following the first direct election of the mayor and subsequent years. The third control is log General Grant revenue per capita and its square. The figure shows that adding these controls to our regression, either individually or all at once, has little impact on the baseline firm-level results.

6.3 Spillover Effects

The baseline estimates represent the effect of a district split on the average difference in outcomes between splitting and non-splitting districts. The estimates thus capture *relative* effects, because a split could affect outcomes in both splitting and non-splitting districts through the channel of intergovernmental competition. Arguably, the policy-relevant parameter is an *absolute* effect, defined as average difference in district potential outcomes in the "split" and "no-split" scenarios.

To estimate the absolute effect, it is necessary to control for the geographic spillover effects of splitting. These spillover effects are also of independent interest, and are useful for quantifying the aggregate effects of creating a new district. It is not possible to identify spillover effects when the nature of spillovers is left fully unspecified. We therefore assume that district splits can affect "neighboring" districts—those districts that share a border—but have no impact on more distant districts.

To allow for spillover effects, we augment equation (1) with one of two variables. The first is *Post 1st Neighbor Split*, an indicator variable that equals 1 after the first time a neighboring district experiences a split. The second is *Frac. Neighbors Split*, which equals the fraction of neighboring districts that have split.[10]

Table 7 presents the results. Controlling for spillover effects has little impact on the estimated direct effect of splitting. This result holds across all outcomes, samples, and neighbor-split measures. This implies that the relative and absolute effects of splitting on outcomes in the district that split are very similar. Consistent with this interpretation, the estimated spillover effects of neighbor splits tend to be small and statistically insignificant. We conclude that there is no evidence that district splits have meaningful spillover effects across districts.

6.4 Local Public Goods

In addition to extracting bribes, district officials may personally benefit from directing expenditure toward bureaucrat wages instead of public goods. Appendix Table B.3 shows that district splits may temper this form of rent-seeking. After controlling for the potentially nonlinear effect of the General Grant, we find that personnel expenditure falls and infrastructure expenditure rises following splits. Of course, these results do not prove that rent-seeking falls; an increase in reported spending may not correspond to a real increase in public goods if officials pocket the additional funds or grant contracts to their cronies at inflated prices. We therefore turn our attention to survey-based measures of road quality and street lighting to understand whether intergovernmental competition improves public good provision. We focus on road quality because it is the public good for which we have data that is most clearly complementary to firm activities.

[10] This variable equals zero for districts with no neighbors.

The previous section establishes that districts increase infrastructure spending following a split. This result is consistent with competition causing an increase in the provision of public goods that are complementary to firm activities. However, the expenditure data suffer from two limitations. First, an increase in reported spending may not correspond to a real increase in public goods if public officials pocket the additional funds or grant contracts to their cronies at inflated prices. Second, district-level expenditure is not informative for how public goods are allocated *within* the district. When a district splits, only 44 percent of its villages experience a reduction in the distance to the nearest competing district on average. If competition is highly localized, splits may induce districts to invest only in those areas that become closer to a neighboring district. Figure 3 illustrates how splits increase the proximity to the nearest competitor for some villages but not others.

The PODES surveys are filled out by village heads, not district government officials. Districts contain 200 villages on average. The data thus are not susceptible to misreporting by district officials. The geographically disaggregated nature of the data also allows us to examine how intergovernmental competition affects the spatial distribution of public goods within a district. When a district splits, only 44 percent of its villages experience a reduction in the distance to the nearest competing district on average. If competition is highly localized, splits may induce districts to invest only in those areas that become closer to a neighboring district. Figure 3 illustrates how splits increase the proximity to the nearest competitor for some villages but not others.

Our baseline specification is

$$Y_{vdit} = \beta Split_{dit} + \delta Split_{dit} \times Closer_v + \alpha_d + \lambda_{it} + \varepsilon_{vdit}, \tag{3}$$

where Y_{vdit} is road quality in village v, located in district d and island group i, in year t. The variable $Closer_v$ is an indicator variable that equals one if the village experienced a reduction in the distance to the nearest competing district due to the first district split. Thus the average change in road quality due to a split is β for villages that did not become closer to the nearest competing district and $\beta + \delta$ for villages that did become closer to a competitor. Our hypothesis is that competition causes districts to focus resources on areas where firms can relocate to another district at low cost. Formally, we expect $\delta > 0$ and $\beta + \delta > 0$.[11]

Table 8 displays the results. In columns 1 and 2, the outcome is an indicator variable equal to one if the main village road is made of asphalt, and zero if it is made of gravel, dirt, or other materials. Column 1 shows the estimates excluding the interaction term. District splits have no impact on the prevalence of paved roads on average. Column 2 adds the interaction term to the regression. The first district split lowers the probability of having a paved road by around 3

[11]Distances are calculated using the borders of autonomous districts, i.e., districts that have started receiving central transfers. Replacing $Closer_v$ with the change in the distance to the nearest competing district yields qualitatively similar results. Controlling for log General Grant revenue per capita and village organizational type (*kelurahan* vs. *desa*) has little impact on the point estimates.

percentage points for villages that do not become closer to a competing district, and this effect is statistically significant at the 10-percent level. However, villages that become closer to the nearest competing district see a 4.5 percentage-point increase in the probability of having a paved road. This effect is significant at the one-percent level. The effect of splits on the two groups differs by 7.6 percentage points, and this difference is significant at the one-percent level.

Columns 3 and 4 measure road quality using an indicator variable equal to one if the main village road is passable by a four-wheeled motor vehicle at all times of the year, and zero otherwise. The estimates are consistent with those in columns 1 and 2. District splits have no impact on this road quality measure on average, but there is significant heterogeneity across villages. The probability that the main road is passable year-round falls by 1.7 percentage points following a split for villages that did not become closer to the nearest competing district. At the same time, villages that become closer to the nearest competitor see a 2.5 percentage-point increase in the probability of having a passable road. This effect is significant at the one-percent level. The two effects differ by 4.2 percentage points, and this difference is significant at the one-percent level.

In columns 5 and 6, the outcome is an indicator variable equal to one if the main village road has street lights, and zero otherwise. The results are very similar to those using the previous two measures of road quality. District splits do not impact street lighting on average, but they do raise the probability of street lighting in villages that became closer to the nearest competing district.

While the competition created by splits does not lead to district-wide improvements in road quality, it does cause a significant reallocation of public investment towards areas where firms' relocation costs fall the most.

To examine dynamic treatment effects and test for differential pre-trends, we estimate the flexible specification

$$Y_{vdit} = \sum_{s} \beta_s 1(t - T_d \in B_s) + \alpha_d + \lambda_{it} + \varepsilon_{vdit}, \tag{4}$$

where T_d is the year district d first split, and B_s is an event-time period.[12] The omitted reference period is $[-3, -1]$, or 1–3 years prior to the first split. The indicator variable $1(t - T_d \in B_s)$ is zero for all periods in districts that never split. The parameter β_s thus represents the average effect of the first split on outcomes in period B_s relative to when the split occurred, compared to the effect of splitting on outcomes 1–3 years prior to the split.

The left-hand-side graphs in Figure 9 plot the estimates of $\{\beta_s\}$ in (4) for the three measures of road quality. All graphs show that the average effect of the first district split on road quality

[12]The set of event-time periods is $\{-4+, [0, 2], [3, 5], 6+\}$, chosen to roughly equalize the number of treated districts observed in each bin. Multi-year bins are necessary, because public good provision is observed approximately every three years.

is close to zero in both the short run and long run. There is no evidence of differential trends prior to the first split.

We are also interested in the dynamic effects of splitting on the allocation of public goods within the district. We modify (4) to allow for heterogeneous effects according to whether the village became closer to the nearest competing district as a result of the split. We estimate the specification

$$Y_{vdit} = \sum_s \left(\beta_s^C 1(t - T_d \in B_s) \times Closer_v + \beta_s^N 1(t - T_d \in B_s) \times NotCloser_v \right) + \alpha_d + \lambda_{it} + \varepsilon_{vdit}, \tag{5}$$

where $Closer_v$ is an indicator variable that equals one if the village experienced a reduction in the distance to the nearest competing district due to the first district split, and $NotCloser_v = 1 - Closer_v$.

The right-hand-side graphs in Figure 9 plot the estimates of $\{\beta_s^C, \beta_s^N\}$ in (5) for the three measures of road quality. Villages that become closer to the nearest competing district see an increase in road quality following the first split, and the effect generally grows over time. By contrast, villages that see no change in proximity to the nearest neighboring district experience either a reduction or no change in road quality after the split.

Together, the graphs in Figure 9 confirm that intergovernmental competition causes districts to reallocate public goods towards areas where firms have the strongest threat of exit, without increasing the quality of public goods overall.

7 Conclusion

The fragmentation of districts in Indonesia represents a major exercise in decentralization in a populous country. Because the timing of district splits is exogenous to characteristics of the districts, this episode presents a unique natural experiment in which to study the effects of intergovernmental competition—an important channel through which the effects of decentralization operate—on the business environment. In particular, we examine the impact on formal and informal tax payments by manufacturing establishments.

Contrary to expectation, we find that the proliferation of districts did not significantly affect tax and bribe payments by the average establishment, and in fact, slightly increased the prevalence of gift (bribe) payments by 0.03 percentage points. This finding is consistent with a model where the licenses provided by bureaucrats are a desirable good for firms and the quantity of these license provided is determined in equilibrium to maximize bureaucrat rents. In this context, it might be desirable for bureaucrats to limit the quantity of these licenses provided in exchange for an informal fee, which would also limit the incidence of bribes. The increase in the number of districts raises competition and therefore increases the total quantity

supplied.

We find that activities associated with important licenses such as construction increase after splits but do not observe any change in average gift payments as a share of revenue and cannot directly measure the "bribe price". Increase in gift payments are not explained by low mobility or inexperienced bureaucrats as the increase occurs mainly in parent districts that retain the original government and among smaller firms that are more mobile. The effect of fragmentation depends on whether the split was likely to have been approved by an appointed rather than directly elected mayor - prevalence of gift payments increase in the former but not the latter.

District governments appear to compete by reallocating resources towards villages that become closer to a district border as a result of a split. Road quality increases in villages that become closer to the nearest competing district due to the split, and deteriorates elsewhere implying that moving costs are high and districts compete only for the most mobile of firms.

Our findings suggest that even a massive decentralization effort may not significantly reduce corruption or improve the ease of doing business for firms when moving costs are high. Nonetheless, this paper shows that decentralization can induce some level of competition on the margins of red tape, corruption, and public good provision in settings where local governments have few tax instruments at their disposal.

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8 Tables

Table 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	302,630
Firm Recorded Some Gifts to Others	0.65	0.48	0.00	1.00	301,303
Formal Taxes and Fees as % of Revenue	1.07	4.43	0.00	100.00	299,096
Gifts as % of Revenue	0.39	2.49	0.00	100.00	298,032
Total Revenue (IDR 1 million)	76.67	740.12	0.00	142,003.66	304,231
Number of Employees	194.25	740.29	20.00	56,139.00	304,234
Large Firm (Max # Employees $\geq$ 200)	0.23	0.42	0.00	1.00	304,552
Any Government Ownership	0.08	0.27	0.00	1.00	304,552
Industry Spatial Concentration	0.03	0.04	−0.06	0.62	304,164
Panel B: District-Level Variables					
Post 1st Split	0.21	0.41	0.00	1.00	4,704
Post 1st Neighbor Split	0.43	0.50	0.00	1.00	4,704
Population (1000s, 2000 Borders)	653.95	633.52	24.01	5,658.92	4,704
Land Area (1000s km^2, 2000 Borders)	5.77	10.70	0.02	119.75	4,704
Number of Districts in Original District	1.31	0.72	1.00	8.00	4,704
Number of Neighboring Districts (2000 Borders)	3.73	2.20	0.00	10.00	4,704
Number of Neighbors that Split	0.87	1.28	0.00	8.00	4,704
Fraction of Neighbors that Split	0.24	0.33	0.00	1.00	4,704
Ethnic Fractionalization (2000 Borders)	0.41	0.32	0.01	0.94	4,676
Directly Elected Mayor	0.56	0.50	0.00	1.00	4,704
General Grant Revenue per Capita	1.21	1.02	0.00	8.67	4,704
Panel C: Village-Level Variables					
Main Road Made of Asphalt	0.66	0.47	0.00	1.00	279,637
Main Road Passable Year Round	0.93	0.25	0.00	1.00	279,637
Main Road Has Street Lights	0.64	0.48	0.00	1.00	283,067
Village Population (1000s)	3.06	2.48	0.21	16.48	287,520
Distance to Nearest District in 2000 (10s km)	1.05	2.20	0.00	40.59	287,520
Distance to Nearest District in 2014 (10s km)	0.73	1.20	0.00	17.62	287,520
Δ Distance to Nearest District (10s km) (Splitters)	−0.78	2.18	−24.88	0.00	106,680
Became Closer to Competing District (Splitters)	0.44	0.50	0.00	1.00	106,680

Notes: The value of firm output is measured in constant 2010 IDR 1 million ($\approx$ USD 100). District grant revenue is measured in constant 2010 IDR 1 million per capita, aggregated to 2000 district borders. District and village variables are aggregated up to 2000 borders.

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

	Firm Paid Any Gifts					
	(1)	(2)	(3)	(4)	(5)	(6)
Any Export	0.007					
	(0.005)					
Any Expenditure On:						
Land Contract		0.038***				
		(0.009)				
Building Additions			0.030***			
			(0.008)			
Any Electricity Purchased:						
From Government				0.049***		
				(0.008)		
From Non-Government					−0.223***	
					(0.037)	
Any Electricity Generated						−0.035***
						(0.009)
Mean Indep. Var	0.186	0.070	0.126	0.874	0.051	0.173
Observations	187,006	284,683	250,542	284,683	284,683	274,799
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 includes a full set of firm fixed effects and island × 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 3: The Effect of the First District Split on Rent-Seeking

Panel A: All Districts

	Firm Paid Any:		Payments as % of Revenue:	
	(1) Taxes/Fees	(2) Gifts	(3) Taxes/Fees	(4) Gifts
Post 1st Split	0.000	0.027***	−0.131**	−0.017
	(0.014)	(0.009)	(0.060)	(0.039)
Observations	286,183	284,904	282,697	281,674
District Clusters	325	325	325	325

Panel B: Balanced Panel

	Firm Paid Any:		Payments as % of Revenue:	
	(1) Taxes/Fees	(2) Gifts	(3) Taxes/Fees	(4) Gifts
Post 1st Split	−0.019	0.011	−0.149***	−0.024
	(0.016)	(0.010)	(0.055)	(0.026)
Observations	250,638	249,476	247,562	246,653
District Clusters	290	290	290	290

Notes: Panel A reports results for the full sample, and Panel B reports results for the subsample that restricts the set of event periods to $\{-3, -2, -1, 0, 1, 2, 3, 4\}$ and the set of treated districts to those that are observed in every event period included in the sample. *Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. In columns 1 and 2, the outcome is an indicator variable that equals 1 if the firm paid any formal taxes or gifts, respectively. In columns 3 and 4, the outcome is the value of taxes or gifts, respectively, paid by the firm as a percentage of firm revenue. Each regression includes a full set of firm fixed effects and island × 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 4: The Effect of the First District Split on Permit-Requiring Activities

	Any Revenue From/Expenditure On:				
	(1) Any Export	(2) Land Contract	(3) Land Purchases	(4) Building Additions	(5) Elec. from Govt.
Post 1st Split	0.008	0.013***	0.004	0.003	−0.008
	(0.008)	(0.004)	(0.007)	(0.006)	(0.011)
Observations	187,091	284,904	250,632	250,744	287,787
District Clusters	322	325	324	324	325

Notes: Post 1st Split is an indicator variable that equals 1 after the first time the district splits into two or more districts. Each regression includes a full set of firm fixed effects and island × 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 5: Heterogeneous Effects of the First Split by Parent and Child District

Panel A: All Districts

	Firm Paid Any:		Payments as % of Revenue:	
	(1) Taxes/Fees	(2) Gifts	(3) Taxes/Fees	(4) Gifts
Post 1st Split	−0.003	0.033***	−0.134*	−0.001
	(0.017)	(0.011)	(0.080)	(0.042)
Post 1st Split × Child	0.023	−0.033	0.009	−0.081
	(0.026)	(0.025)	(0.230)	(0.059)
Observations	284,888	283,610	281,430	280,410
District Clusters	313	313	313	313

Panel B: Balanced Panel

	Firm Paid Any:		Payments as % of Revenue:	
	(1) Taxes/Fees	(2) Gifts	(3) Taxes/Fees	(4) Gifts
Post 1st Split	−0.019	0.015	−0.152***	−0.006
	(0.018)	(0.012)	(0.057)	(0.029)
Post 1st Split × Child	0.006	−0.032	0.078	−0.114
	(0.030)	(0.036)	(0.146)	(0.116)
Observations	250,410	249,248	247,342	246,433
District Clusters	280	280	280	280

Notes: Panel A reports results for the full sample, and Panel B reports results for the subsample that restricts the set of event periods to $\{-3, -2, -1, 0, 1, 2, 3, 4\}$ and the set of treated districts to those that are observed in every event period included in the sample. *Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. *Child District* is an indicator for whether a firm in a district that split was observed to be in a newly created district in the first year of the split. We assume that firms do not move in the very first year of the *de jure* split. In columns 1 and 2, the outcome is an indicator variable that equals 1 if the firm paid any formal taxes or gifts, respectively. In columns 3 and 4, the outcome is the value of taxes or gifts, respectively, paid by the firm as a percentage of firm revenue. Each regression includes a full set of firm fixed effects and island × 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 6: The Effect of the First District Split on Permit-Requiring Activities: Heterogeneity by Firm Size

	Any Revenue From/Expenditure On:				
	(1) Any Export	(2) Land Contract	(3) Land Purchases	(4) Building Additions	(5) Elec. from Govt.
Post 1st Split	0.011	0.011***	0.003	0.005	−0.009
	(0.007)	(0.004)	(0.005)	(0.006)	(0.010)
Post 1st Split × Large Firm	−0.017	0.007	0.004	−0.011	0.006
	(0.014)	(0.005)	(0.013)	(0.019)	(0.010)
Observations	187,091	284,904	250,632	250,744	287,787
District Clusters	322	325	324	324	325

Notes: Post 1st Split is an indicator variable that equals 1 after the first time the district splits into two or more districts. Each regression includes a full set of firm fixed effects and island × 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 7: Spillover Effects of District Splits on Rent-Seeking

Panel A: All Districts

	Firm Paid Any:				Payments as % of Revenue:			
	(1) Taxes/Fees	(2) Taxes/Fees	(3) Gifts	(4) Gifts	(5) Taxes/Fees	(6) Taxes/Fees	(7) Gifts	(8) Gifts
Post 1st Split	0.002	0.000	0.026**	0.027***	−0.121**	−0.131**	−0.022	−0.017
	(0.016)	(0.014)	(0.011)	(0.009)	(0.058)	(0.060)	(0.036)	(0.038)
Post 1st Neighbor Split	−0.004		0.004		−0.031		0.016	
	(0.015)		(0.014)		(0.058)		(0.033)	
Frac. Neigh. Split		−0.013		−0.002		0.032		0.052
		(0.023)		(0.024)		(0.157)		(0.046)
Observations	286,183	286,183	284,904	284,904	282,697	282,697	281,674	281,674
District Clusters	325	325	325	325	325	325	325	325

Panel B: Balanced Panel

	Firm Paid Any:				Payments as % of Revenue:			
	(1) Taxes/Fees	(2) Taxes/Fees	(3) Gifts	(4) Gifts	(5) Taxes/Fees	(6) Taxes/Fees	(7) Gifts	(8) Gifts
Post 1st Split	−0.019	−0.019	0.009	0.011	−0.137***	−0.149***	−0.022	−0.024
	(0.018)	(0.016)	(0.011)	(0.010)	(0.052)	(0.056)	(0.024)	(0.026)
Post 1st Neighbor Split	0.002		0.008		−0.039		−0.008	
	(0.016)		(0.016)		(0.065)		(0.036)	
Frac. Neigh. Split		−0.012		0.000		−0.008		0.032
		(0.027)		(0.026)		(0.168)		(0.049)
Observations	250,638	250,638	249,476	249,476	247,562	247,562	246,653	246,653
District Clusters	290	290	290	290	290	290	290	290

Notes: Panel A reports results for the full sample, and Panel B reports results for the subsample that restricts the set of event periods to $\{-3, -2, -1, 0, 1, 2, 3, 4\}$ and the set of treated districts to those that are observed in every event period included in the sample. *Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. *Post 1st Neighbor Split* is an indicator variable that equals 1 after the first time a neighboring district experiences a split, and *Frac. Neighbors Split* equals the fraction of neighboring districts that have split. In columns 1 and 2, the outcome is an indicator variable that equals 1 if the firm paid any formal taxes or gifts, respectively. In columns 3 and 4, the outcome is the value of taxes or gifts, respectively, paid by the firm as a percentage of firm revenue. Each regression includes a full set of firm fixed effects and island × 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 8: The Effect of the First District Split on Village Road Quality

Panel A: All Districts

	Main Village Road:					
	Made of Asphalt		Passable Year Round		Has Street Lights	
	(1)	(2)	(3)	(4)	(5)	(6)
Post 1st Split	0.001	−0.031*	0.001	−0.017***	0.010	−0.019
	(0.015)	(0.017)	(0.005)	(0.006)	(0.016)	(0.017)
Post 1st Split × Closer		0.076***		0.042***		0.069***
		(0.017)		(0.009)		(0.015)
Sum of Coefficients		0.045***		0.025***		0.050***
		(0.017)		(0.007)		(0.018)
Dep. Variable Mean	0.658	0.658	0.935	0.935	0.644	0.644
Observations	279,637	279,637	279,637	279,637	283,067	283,067
District Clusters	332	332	332	332	332	332

Panel B: Balanced Panel

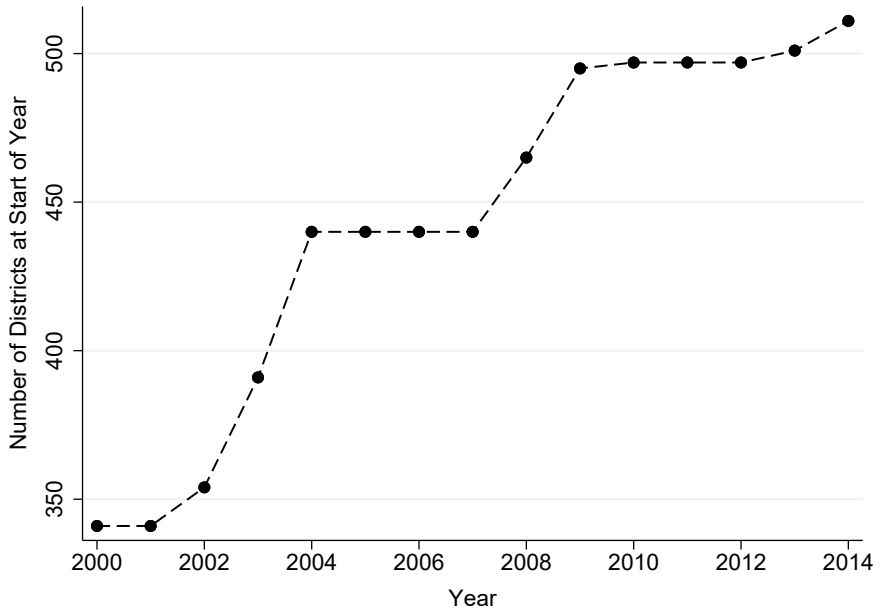
	Main Village Road:					
	Made of Asphalt		Passable Year Round		Has Street Lights	
	(1)	(2)	(3)	(4)	(5)	(6)
Post 1st Split	−0.003	−0.029	−0.006	−0.025***	−0.007	−0.032
	(0.017)	(0.021)	(0.006)	(0.008)	(0.018)	(0.020)
Post 1st Split × Closer		0.060***		0.043***		0.058***
		(0.021)		(0.011)		(0.015)
Sum of Coefficients		0.031*		0.018**		0.026
		(0.018)		(0.008)		(0.020)
Dep. Variable Mean	0.676	0.676	0.944	0.944	0.689	0.689
Observations	221,674	221,674	221,674	221,674	223,543	223,543
District Clusters	298	298	298	298	298	298

Notes: Panel A reports results for the full sample, and Panel B reports results for the subsample that restricts the set of event periods to $\{[-6,-4],[-3,-1],[0,2],[3,5]\}$ and the set of treated districts to those that are observed in every event period included in the sample. *Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. *Closer* is an indicator variable that equals 1 if the village experienced a reduction in the distance to the nearest competing district due to the first district split. In columns 1 and 2, the outcome is an indicator variable that equals 1 if the village main road is paved with asphalt In columns 3 and 4, the outcome is an indicator variable that equals 1 if the village main road is passable year round. In columns 5 and 6, the outcome is an indicator variable that equals 1 if the village main road has street lights. Each regression includes a full set of district fixed effects and island × year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

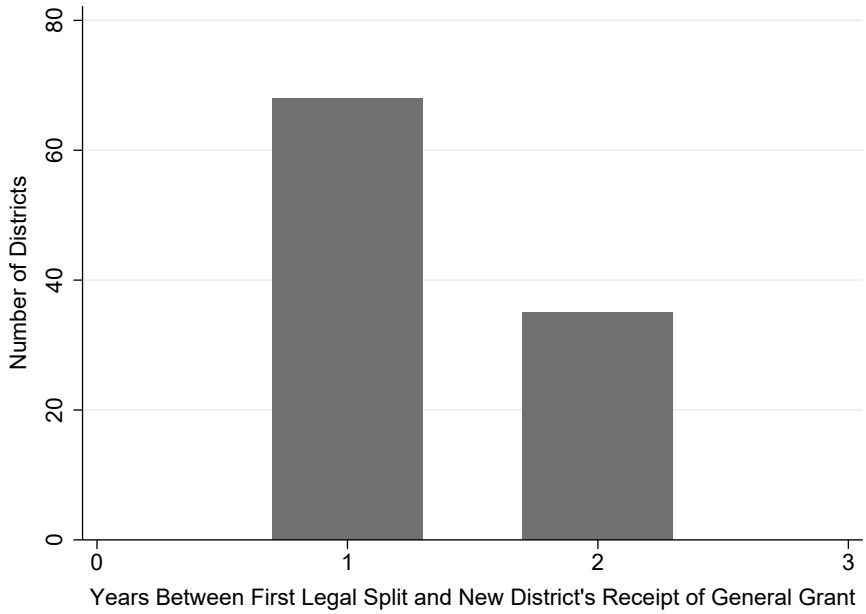
9 Figures

Figure 1: The Timing of *De Jure* and *De Facto* District Creation

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

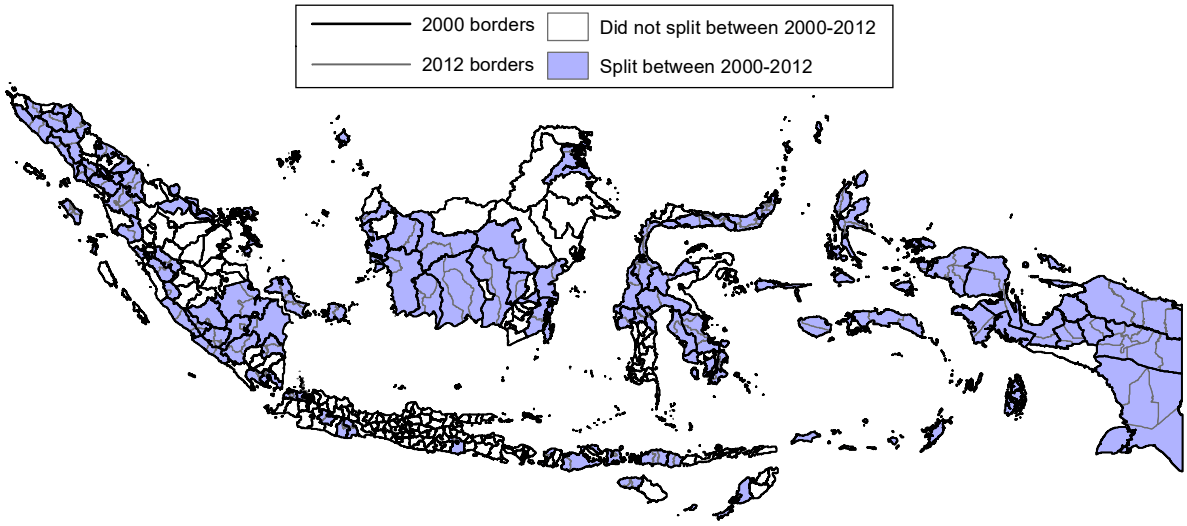


(b) Lag Time Between *De Jure* and *De Facto* District Creation



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) shows the frequency distribution of the difference between the year of legal creation of the first new district (within 2000 borders) and the year the new district first receives the general grant.

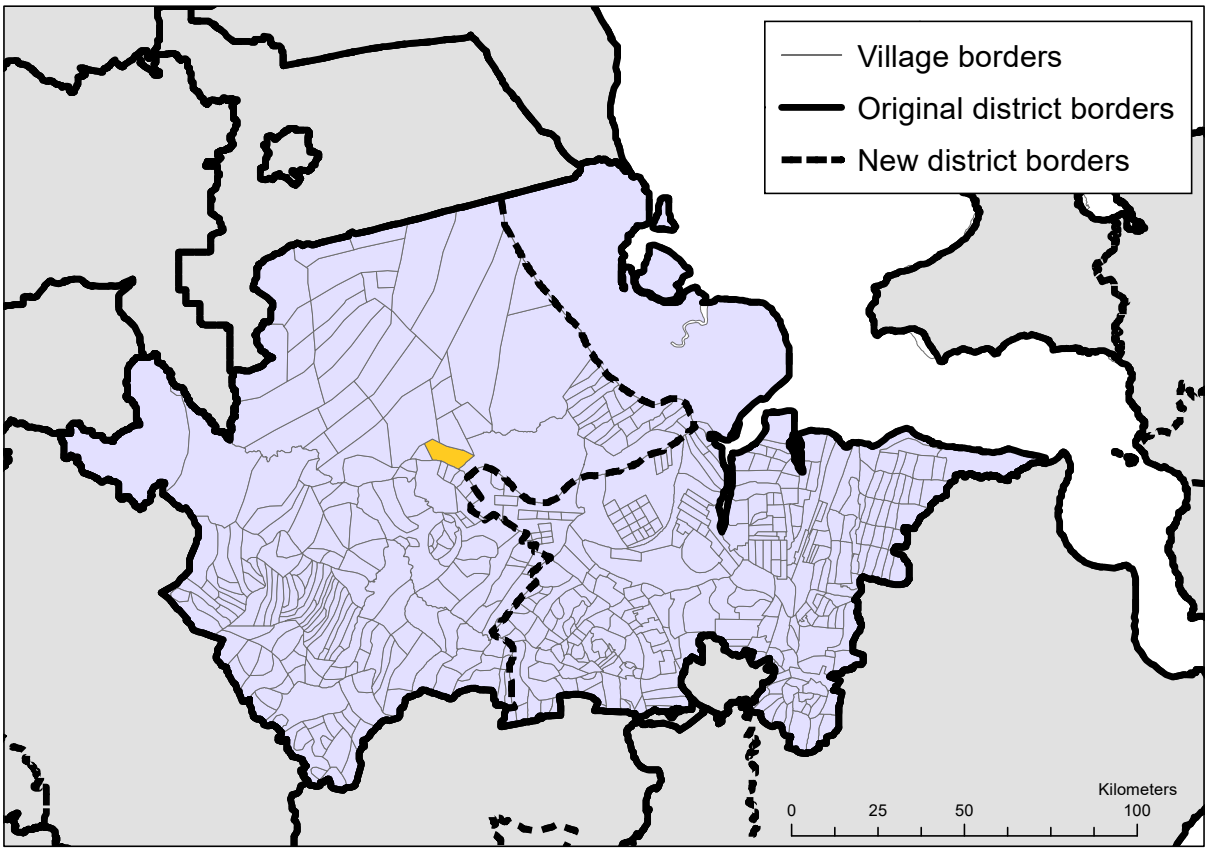
Figure 2: Indonesia's Widespread District Proliferation, 2000–2012



Notes: This map displays district borders in 2000 and 2012 based on the 2012 district shapefile provided by the Indonesian Statistical Bureau and the district crosswalk provided by the World Bank's Indonesia Database for Policy and Economic Research (INDO-DAPOER).

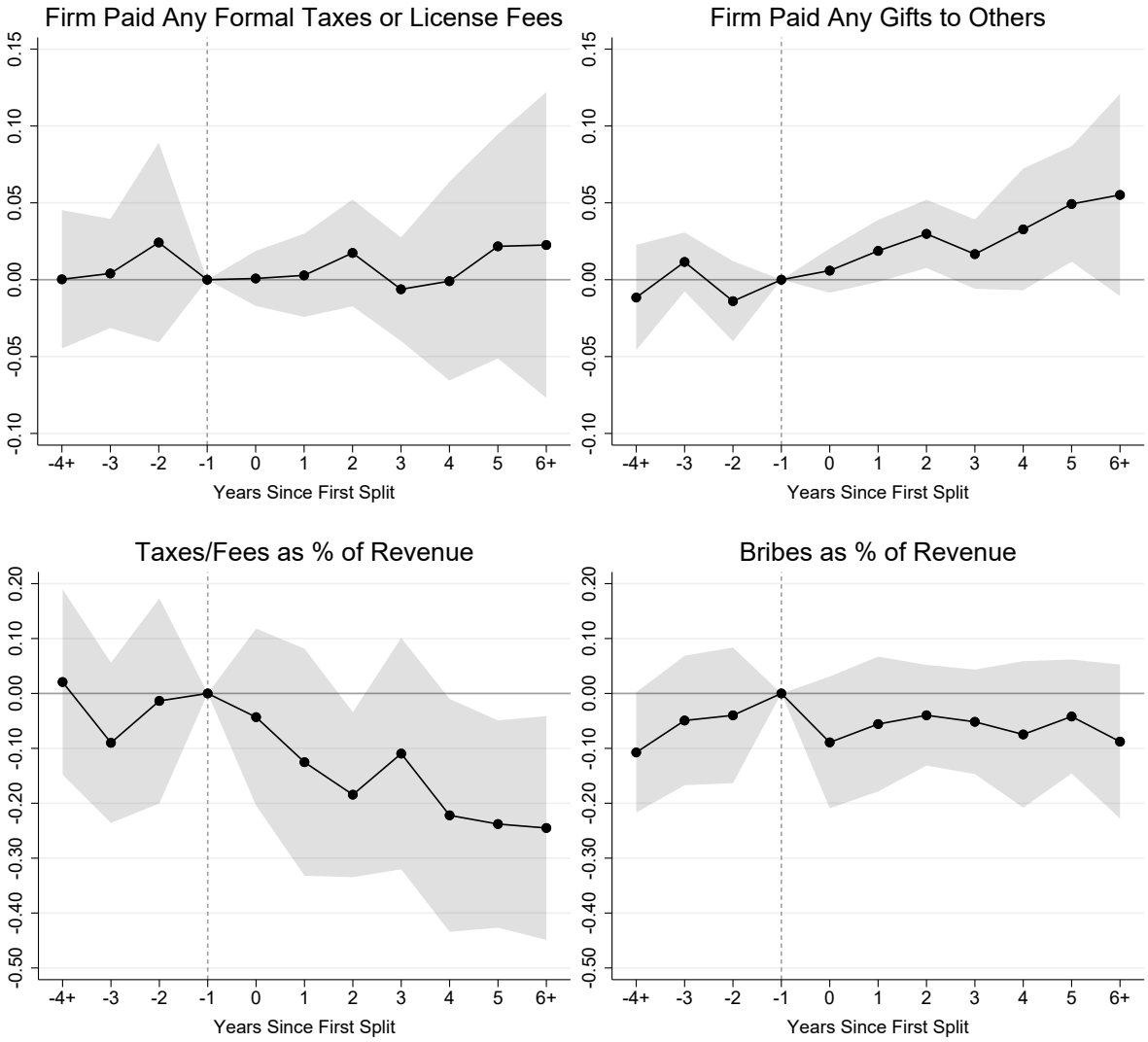
Figure 3: The Effect of District Splits on Village Proximity to Competing Districts

Musi Banyuasin District, South Sumatra Province



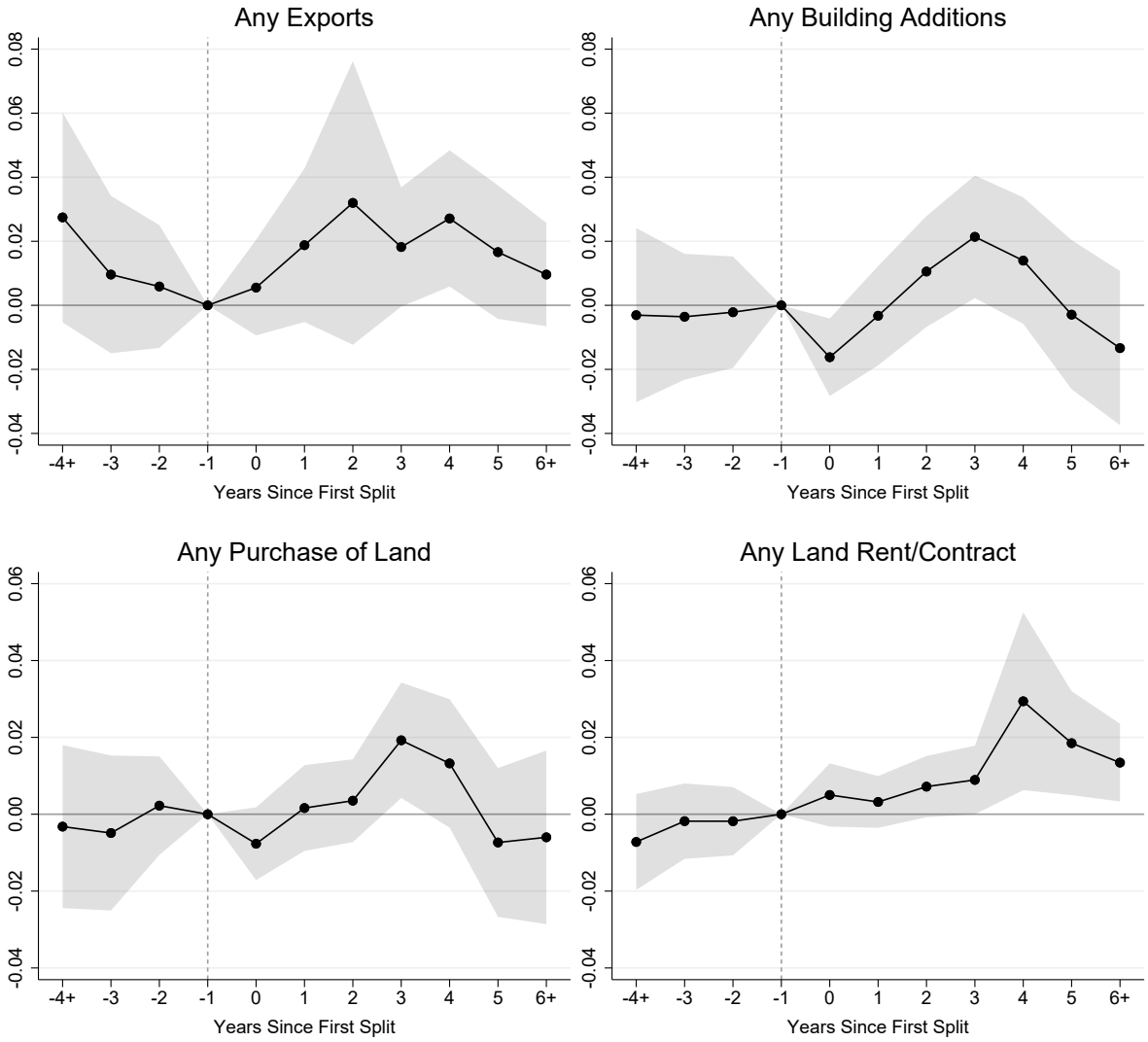
Notes: This map displays the original district borders (thick solid lines) of Musi Banyausin prior to its (de jure) split in 2002. The thick dashed lines represent the new district borders following the split. The child district, Banyuasin, is located to the east of the new borders. The example village, highlighted in orange, becomes significantly closer to a competing district as a result of the split. For many non-highlighted villages, the distance to the nearest competing district does not change.

Figure 4: The Effect of the First District Split on Firm-Level Outcomes



Notes: This figure plots the estimates of $\{\beta_s\}_{s \in \mathcal{S}}$ from (2) and their 95-percent confidence intervals.

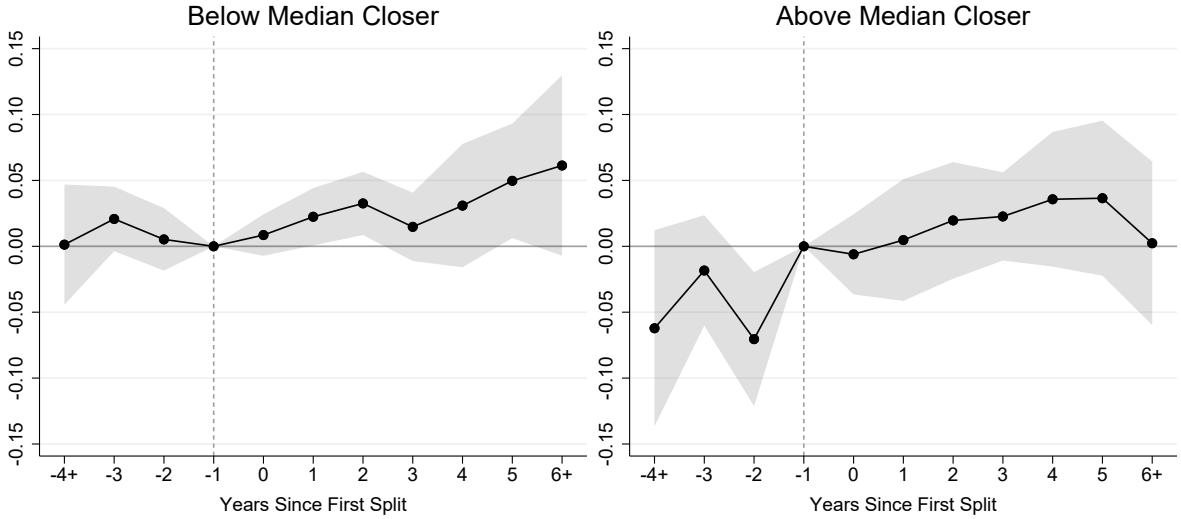
Figure 5: The Effect of the First District Split on Regulated Activities



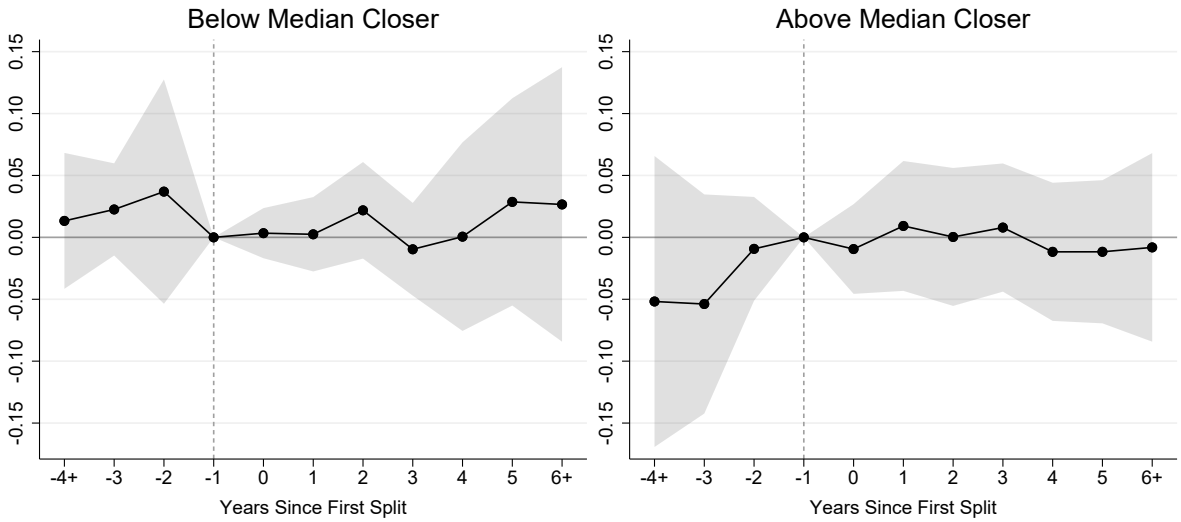
Notes: This figure plots the estimates of $\{\beta_s\}_{s \in \mathcal{S}}$ from (2) and their 95-percent confidence intervals.

Figure 6: The Effect of the First District Split By Share of Villages Closer to a District Border

(a) Outcome: Firm Recorded Any Gifts to Others



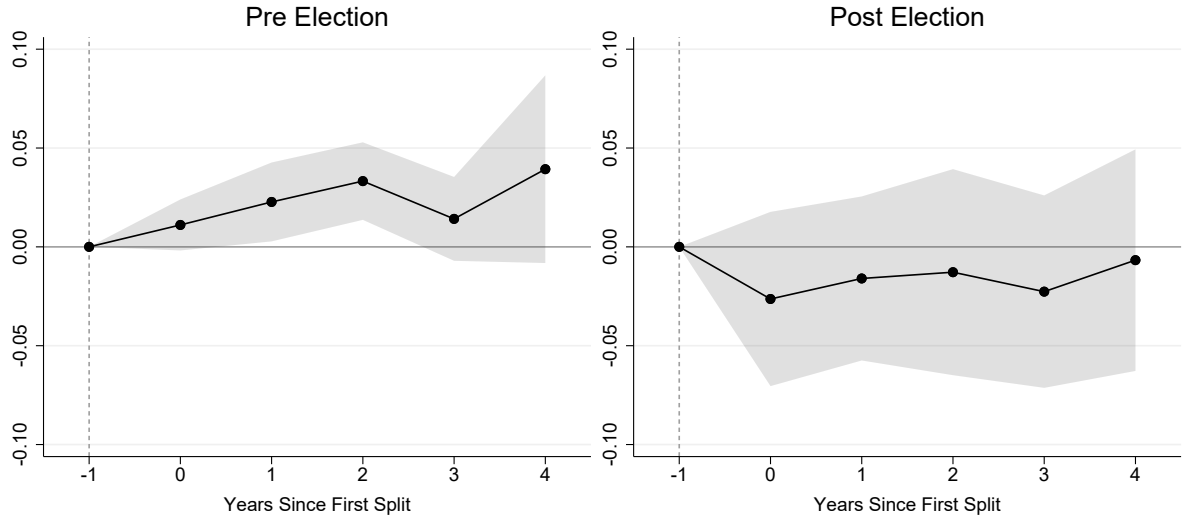
(b) Outcome: Firm Remitted Any Formal Taxes or License Fees



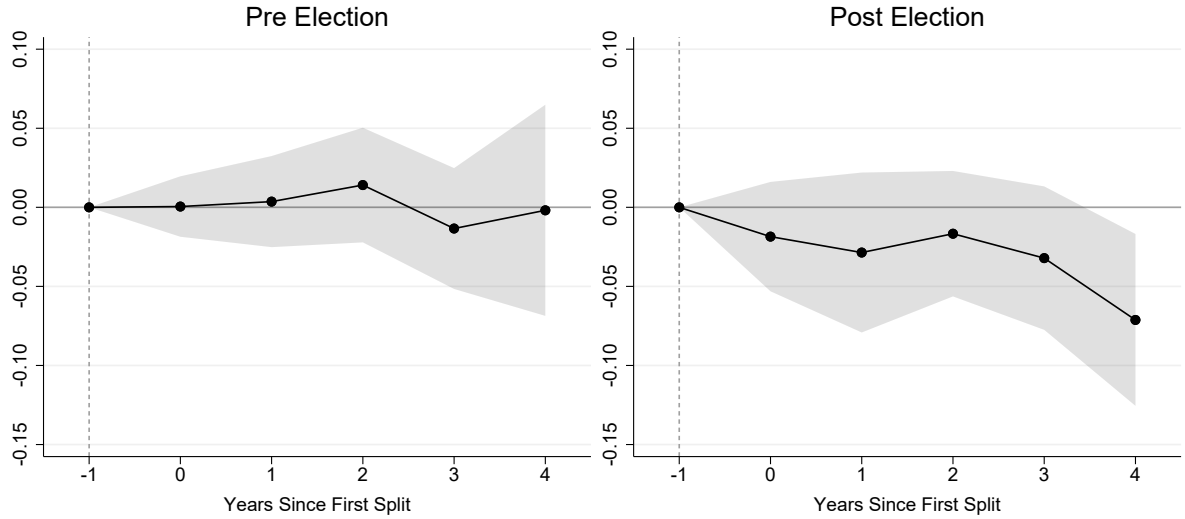
Notes: This figure plots the estimates of $\{\beta_s\}_{s \in \mathcal{S}}$ from (2) and their 95-percent confidence intervals.

Figure 7: The Effect of the First District Split By Whether Split Approved by Elected Mayor, Balanced Panel

(a) Outcome: Firm Recorded Any Gifts to Others

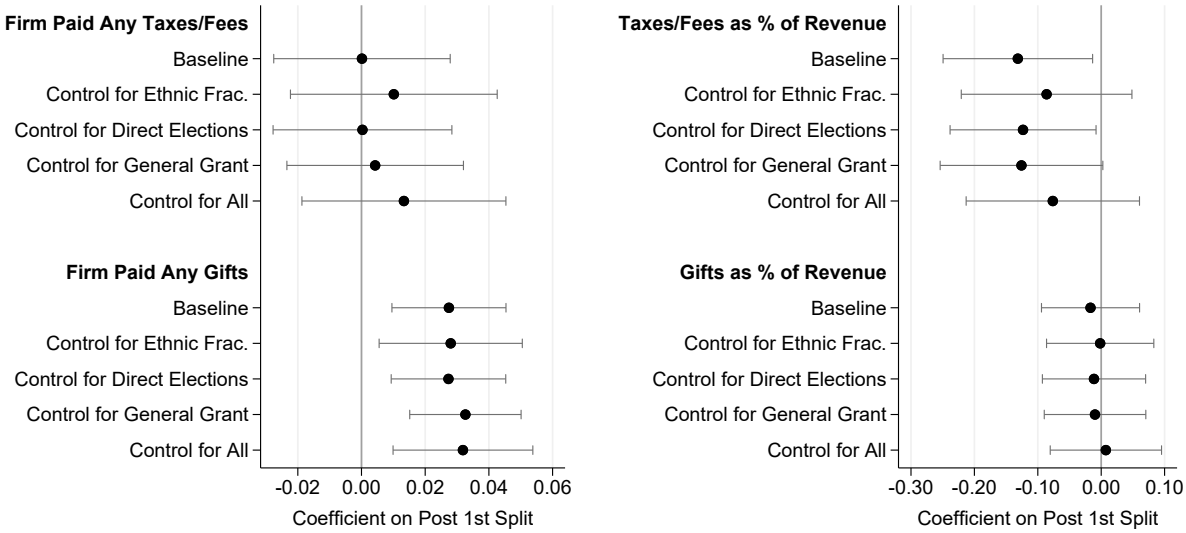


(b) Outcome: Firm Remitted Any Formal Taxes or License Fees



Notes: This figure plots the estimates of $\{\beta_s\}_{s \in \mathcal{S}}$ from (2) and their 95-percent confidence intervals.

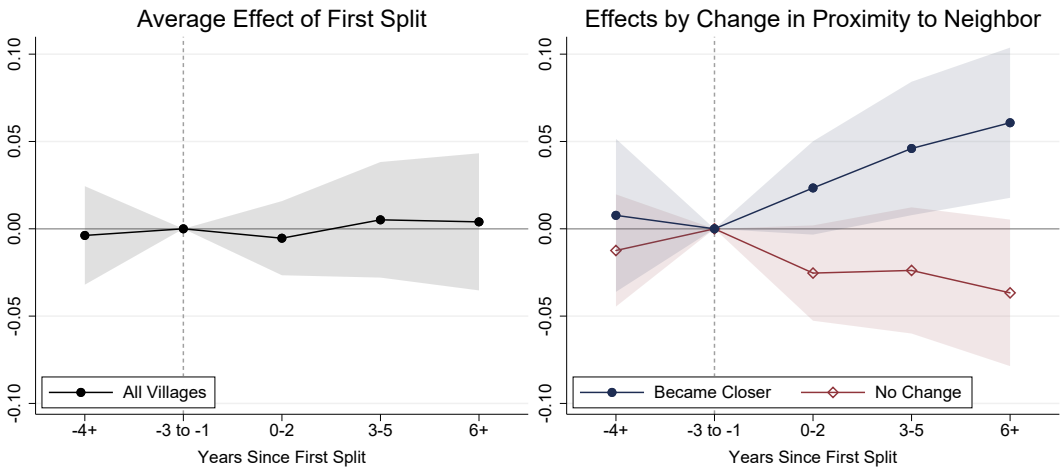
Figure 8: Robustness of Firm-Level Results to Adding Controls



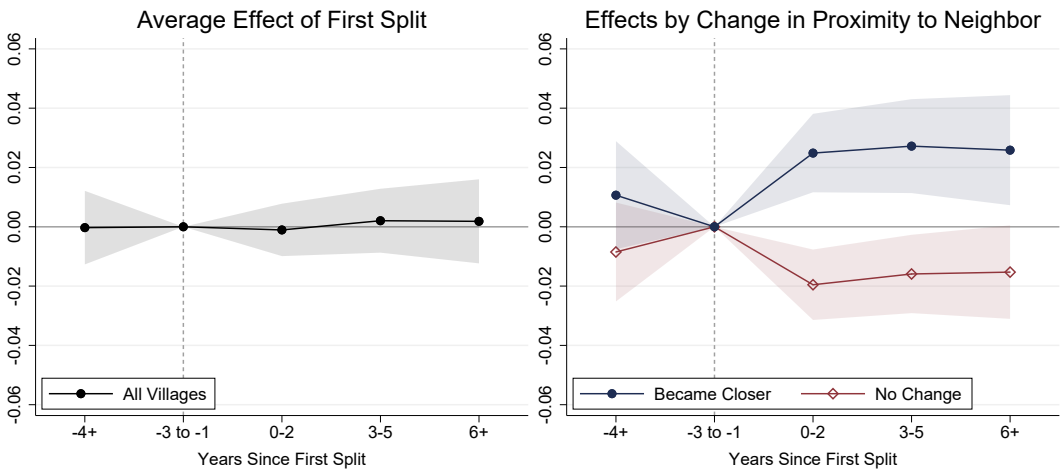
Notes: This figure plots the estimates of β from (1) and their 95-percent confidence intervals after controlling for various covariates. For each outcome, the first row reports the baseline estimate, the second row controls for ethnic fractionalization, the third row controls for the presence of a directly elected mayor, the fourth row controls for log General Grant revenue per capita and its square, and the fifth row controls for all of the aforementioned covariates.

Figure 9: Average and Heterogeneous Effects of the First District Split on Village Road Quality

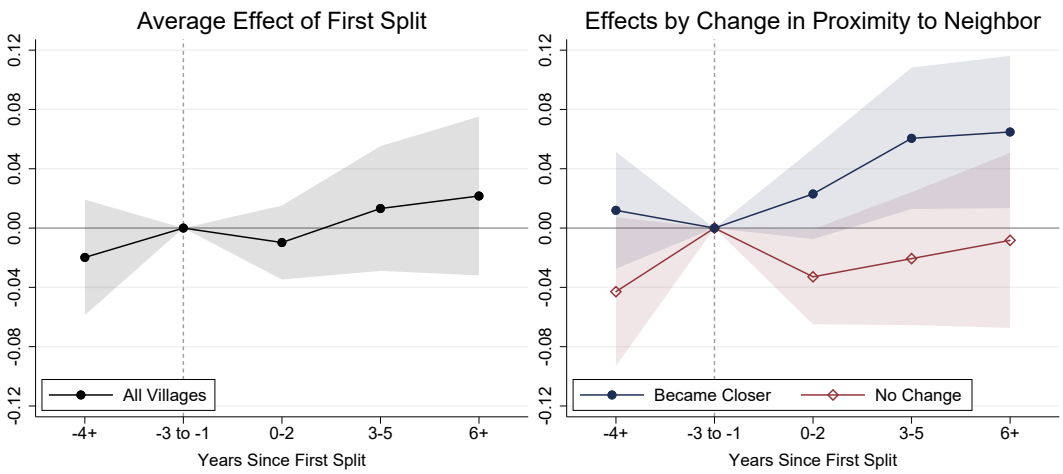
(a) Outcome: Main Village Road Is Made of Asphalt



(b) Outcome: Main Village Road Is Passable Year Round

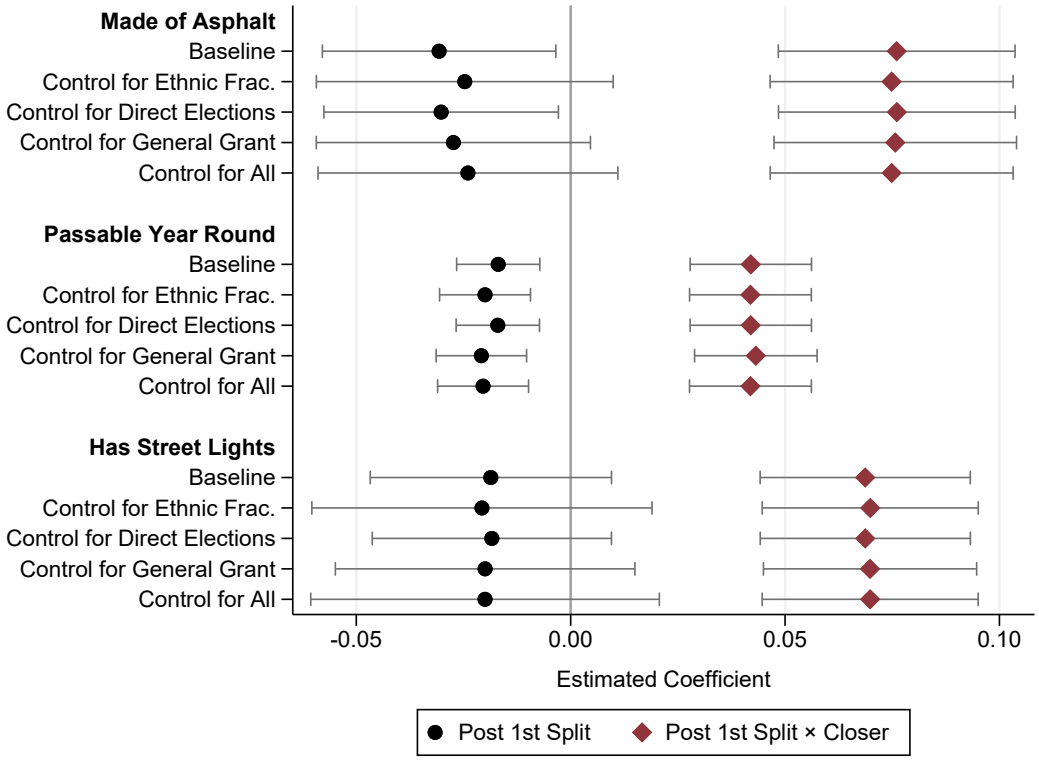


(c) Outcome: Main Village Road Has Street Lights



Notes: This figure plots the estimates of $\{\beta_s\}$ in (4) (left-hand side) and $\{\beta_s^C, \beta_s^N\}$ in (5) (right-hand side), along with their 95-percent confidence intervals.

Figure 10: Robustness of Village Road Quality Results to Adding Controls



Notes: This figure plots the estimates of β and δ from (3) and their 95-percent confidence intervals after controlling for various covariates. For each outcome, the first row reports the baseline estimate, the second row controls for ethnic fractionalization, the third row controls for the presence of a directly elected mayor, the fourth row controls for log General Grant revenue per capita and its square, and the fifth row controls for all of the aforementioned covariates.

A Additional Heterogeneity Analysis

A.1 Heterogeneity by Firm Size

Change below: interpretation of Bai et al is incorrect. In their model moving costs are *increasing* and concave in firm size, which is determined by TFP. Concavity implies that when the firm grows, the benefit of moving to another province with a lower bribe rate increases more than the increase in moving costs. This is because net-of-bribe profits increase linearly in TFP.

Next we test whether the effect of district splits varies according to the size of the firm. Bai et al. (2019) find that when Vietnamese firms grow larger, they pay less in bribes as a share of firm revenue. They claim that the result operates through intergovernmental competition, arguing that larger firms are more mobile and thus can more credibly threaten to leave the jurisdiction. If firm size is negatively correlated with mobility costs, we would also expect that the fragmentation of districts would benefit larger firms more.

We do not find strong evidence of the impact of firm size on district splits in this context. Larger firms are somewhat less likely to see an increase in bribes as a result of splits in their original district and also less likely to see an increase in gift payments as a share of total revenue. Table B.1 displays the results from augmenting the baseline specification with the interaction term *Post 1st Split* × *Large Firm*, where *Large Firm* is an indicator variable that equals 1 for firms that recorded 200 or more employees at least once during the years prior to the district's first split. Now the coefficient on *Post 1st Split* measures the effect of splitting on small firms, and the coefficient on *Post 1st Split* × *Firm Size* measures how the effect of the first split varies according to firm size.

The full-sample results in Panel A show that overall, consistent with the baseline results, there is very litte impact on either the intensive or extensive margin of tax payments. On the other hand, there is a positive impact on gift payments in the both the intensive and extensive margin that is decreasing with firm size. If we assume that the effect is linear as we do in this specification, the impact of splits only decreases the probability of gift payments for very large firms. An increase in the number of employees at a firm by 100 is associate with a decrease in the effect of splits of gift payments by 0.1 percentage points. However, the impact of firm size on the effect of district splits is not statistically significant. Results in the sub sample of districts that split around the moratorium are consistent with the full sample results. The major difference in this sub-sample is that the impact of the first split on the probability of any gift payments is negative in magnitude but not statistically significant. Figures C.3 and C.4 show that for all outcomes in both groups of large and small firms, districts that split do not vary systematically from those that do not or have not yet split, before splits.

A.2 Heterogeneity by Spatial Concentration of Industry

Firm size is only one indicator of moving costs for a firm. Firms might rely on location-specific resources that make it more difficult for them to move in search of better local governance. Rothenberg, Bazzi, Nataraj and Chari (2016) show that certain industries in Indonesia are more spatially concentrated than others because they tend to rely on locally available inputs such as natural resources and labor. The more spatially concentrated an industry, the higher the firms' moving costs and lower their bargaining power with district officials. We would therefore expect that firms in less spatially concentrated industries are more likely to benefit from district splits.

We examine how the impact of the district splits varies by industrial concentration of a firm's industry prior to the split using the Ellison and Glaeser (1997) measure of industrial concentration of industry j as

$$\theta_j = \frac{G_j - \left(1 - \sum_d x_d^2\right) H_j}{\left(1 - \sum_d x_d^2\right)(1 - H_j)}, \tag{A.1}$$

where $H_j = \sum_f z_f^2$ is the Herfindahl index of concentration of employment across all establishments f within industry j, and $G_j = \sum_d \left(x_d - s_{dj}\right)^2$ measures the sum of squared deviations between s_{dj}, the share of industry j's national employment in district d, and x_d, the share of national employment in all industries in district d, where the district is defined according to the boundaries in 2000. Our variable of interest is the average value of spatial concentration of the firm's industry prior to the any district split where the firm was located in 2000 or in the earliest year they are observed in the data. Industry is identified by 4-digit ISIC codes. Higher values of θ_j indicate greater spatial concentration.

Table B.2 shows the results of interacting the post-split variable with this spatial concentration measure. We see some weak evidence of heterogeneity by spatial concentration. The probability of any bribe payments is decreasing in spatial concentration, contrary to what we would expect since firms with high moving costs should have less bargaining power. However, on the intensive-margin, bribe payments as a share of total revenue increase by more for firms in more spatially concentrated industries. Tax payments decrease following splits on both the intensive and intensive margin and they fall by more for more spatially concentrated industries. Results from only the districts that split around the moratorium are shown in panel B and are very similar to results from the full sample.

Figures C.5 and C.6 show the results of the event-study specification 2 split by a sample of "low-concentration" firms, which are firms in industries with below-median spatial concentration (i.e. less than 0.028), and a sample of above-median or "high-concetration" firms. We see that for all of our outcome variables of interest, firms in these two groups exhibit parallel trends prior to the first split.

B Appendix Tables

Table B.1: Heterogeneous Effects of the First Split by Firm Size

Panel A: All Districts

	Firm Paid Any:		Payments as % of Revenue:	
	(1) Taxes/Fees	(2) Gifts	(3) Taxes/Fees	(4) Gifts
Post 1st Split	0.001	0.030***	−0.127**	−0.024
	(0.015)	(0.009)	(0.055)	(0.039)
Post 1st Split × Firm Size	−0.002	−0.011	−0.017	0.031***
	(0.003)	(0.007)	(0.076)	(0.008)
Observations	286,183	284,904	282,697	281,674
District Clusters	325	325	325	325

Panel B: Balanced Panel

	Firm Paid Any:		Payments as % of Revenue:	
	(1) Taxes/Fees	(2) Gifts	(3) Taxes/Fees	(4) Gifts
Post 1st Split	−0.018	0.013	−0.140***	−0.031
	(0.016)	(0.010)	(0.054)	(0.026)
Post 1st Split × Firm Size	−0.004	−0.007	−0.032	0.024***
	(0.004)	(0.005)	(0.104)	(0.006)
Observations	250,638	249,476	247,562	246,653
District Clusters	290	290	290	290

Notes: Panel A reports results for the full sample, and Panel B reports results for the subsample that restricts the set of event periods to $\{-3, -2, -1, 0, 1, 2, 3, 4\}$ and the set of treated districts to those that are observed in every event period included in the sample. *Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. *Firm Size* measured the maximum number of employees (in 1000s) recorded for a firm prior to the first split in their district. In columns 1 and 2, the outcome is an indicator variable that equals 1 if the firm paid any formal taxes or gifts, respectively. In columns 3 and 4, the outcome is the value of taxes or gifts, respectively, paid by the firm as a percentage of firm revenue. Each regression includes a full set of firm fixed effects and island × year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: Heterogeneous Effects of the First Split by Industry Spatial Concentration

Panel A: All Districts

	Firm Paid Any:		Payments as % of Revenue:	
	(1) Taxes/Fees	(2) Gifts	(3) Taxes/Fees	(4) Gifts
Post 1st Split	0.002	0.033***	−0.119**	−0.039
	(0.019)	(0.010)	(0.059)	(0.047)
Post 1st Split × Industry Spatial Concentration	−0.240	−0.603	−1.418	2.490
	(0.990)	(0.574)	(4.469)	(2.129)
Observations	286,127	284,854	282,646	281,629
District Clusters	325	325	325	325

Panel B: Balanced Panel

	Firm Paid Any:		Payments as % of Revenue:	
	(1) Taxes/Fees	(2) Gifts	(3) Taxes/Fees	(4) Gifts
Post 1st Split	−0.019	0.015	−0.069	−0.032
	(0.019)	(0.011)	(0.061)	(0.029)
Post 1st Split × Industry Spatial Concentration	0.005	−0.418	−7.641***	0.800
	(0.703)	(0.525)	(2.855)	(1.917)
Observations	250,596	249,440	247,520	246,617
District Clusters	290	290	290	290

Notes: Panel A reports results for the full sample, and Panel B reports results for the subsample that restricts the set of event periods to $\{-3, -2, -1, 0, 1, 2, 3, 4\}$ and the set of treated districts to those that are observed in every event period included in the sample. *Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. *Concentration* the value of the Ellison-Glaeser index of spatial concentration as given in equation (A.1). Higher values of the index indicate greater spatial concentration. In columns 1 and 2, the outcome is an indicator variable that equals 1 if the firm paid any formal taxes or gifts, respectively. In columns 3 and 4, the outcome is the value of taxes or gifts, respectively, paid by the firm as a percentage of firm revenue. Each regression includes a full set of firm fixed effects and island × year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: The Effect of the First District Split on District Expenditure

Panel A: All Districts

	Log Expenditure per Capita			
	(1) Total	(2) Infrastructure	(3) Personnel	(4) Administration
Post 1st Split	0.020	0.164**	−0.050*	0.175***
	(0.026)	(0.066)	(0.025)	(0.033)
Observations	4,381	3,754	4,411	3,791
District Clusters	336	336	336	336

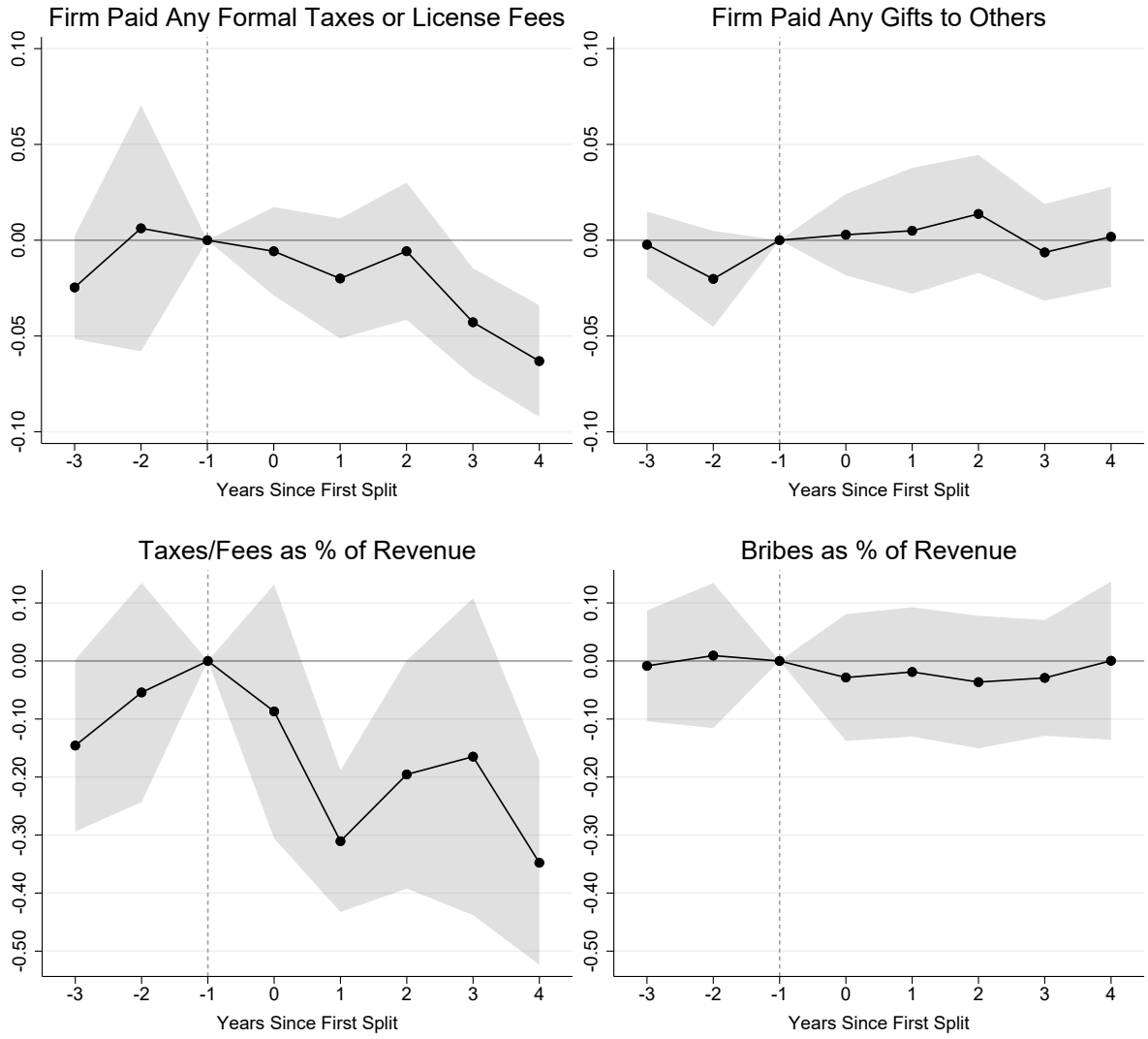
Panel B: Balanced Panel

	Log Expenditure per Capita			
	(1) Total	(2) Infrastructure	(3) Personnel	(4) Administration
Post 1st Split	−0.018	0.206**	−0.093***	0.147***
	(0.038)	(0.088)	(0.030)	(0.037)
Observations	3,523	3,081	3,553	3,125
District Clusters	301	301	301	301

Notes: The dependent variables are log district-level expenditure per capita in four expenditure categories: *Total*, *Personnel*, *Administration*, and *Infrastructure*. *Post 1st Split* is an indicator variable that equals 1 after the first time the district splits into two or more districts. Each regression controls for the log of General Grant revenue per capita and its square, and includes a full set of district fixed effects and island × year effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustering by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

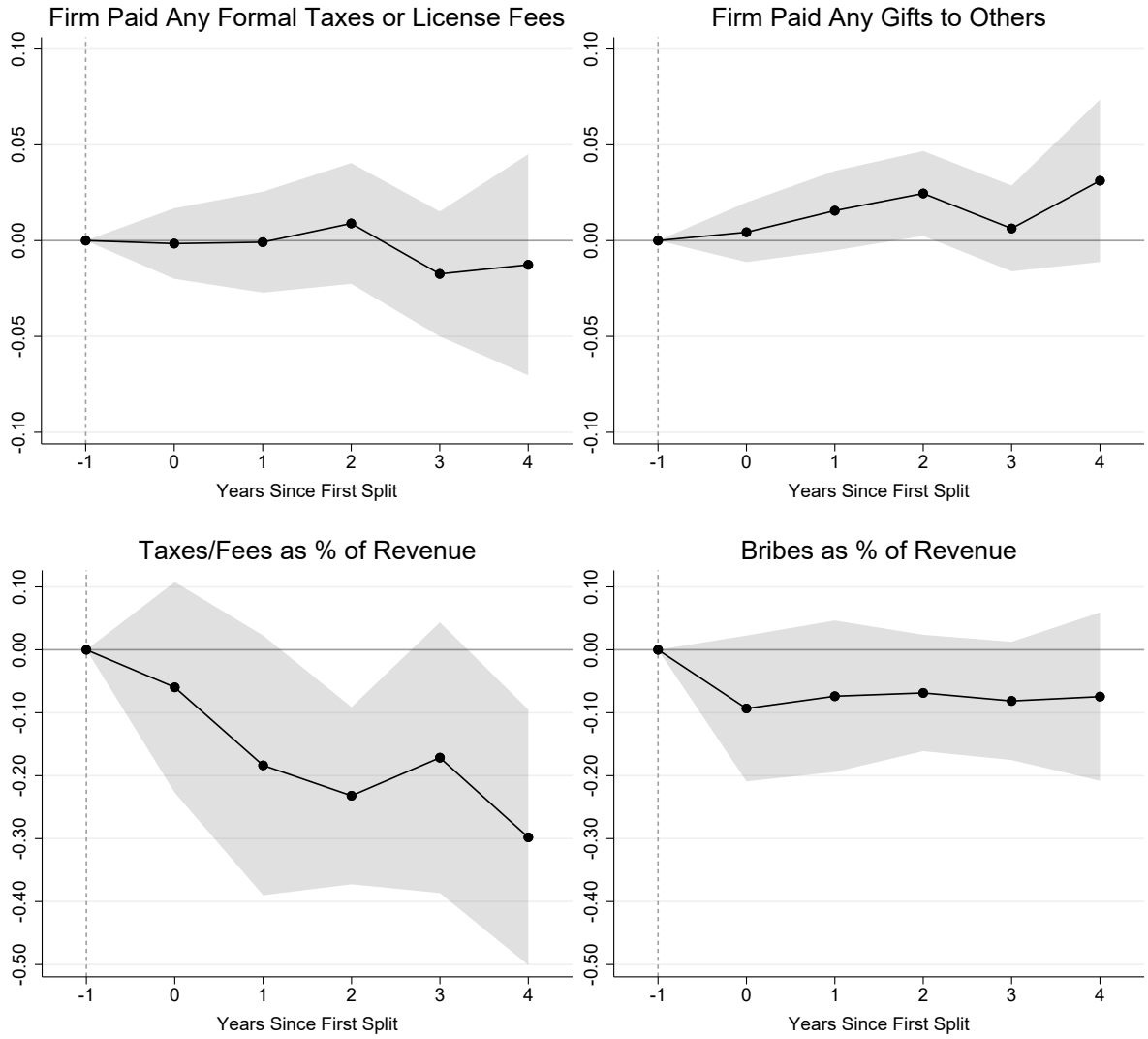
C Appendix Figures

Figure C.1: The Effect of the First District Split on Firm-Level Outcomes, Balanced Panel



Notes: This figure plots the estimates of $\{\beta_s\}_{s \in \mathcal{S}}$ from (2) and their 95-percent confidence intervals.

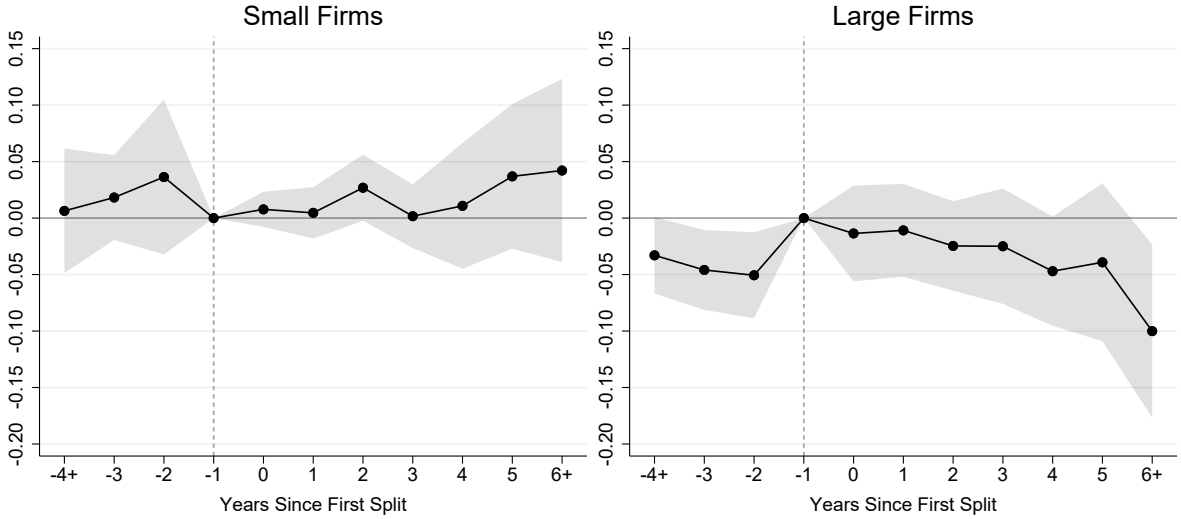
Figure C.2: The Effect of the First District Split on Firm-Level Outcomes, Short Balanced Panel



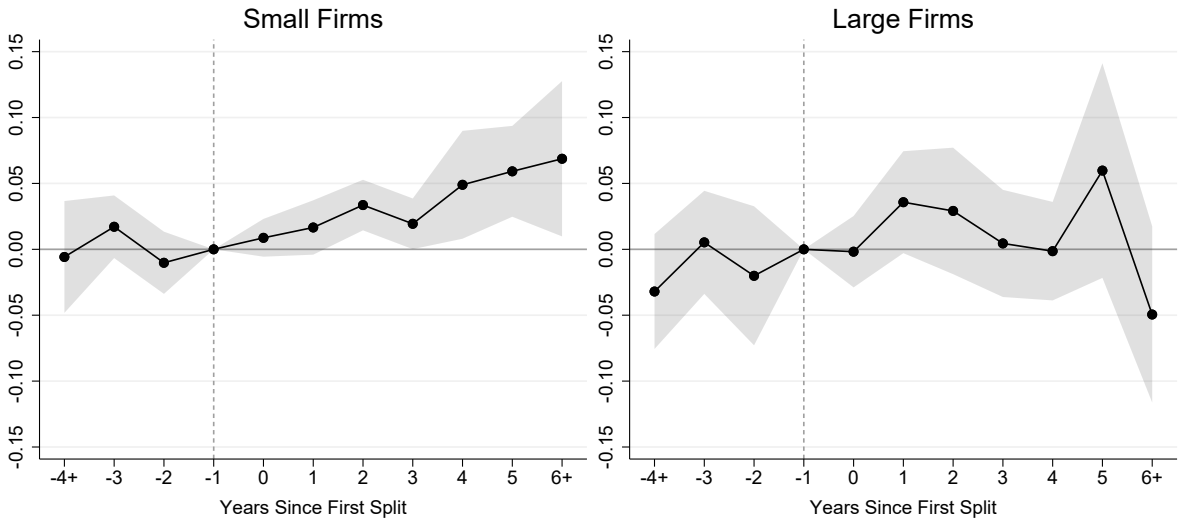
Notes: This figure plots the estimates of $\{\beta_s\}_{s \in \mathcal{S}}$ from (2) and their 95-percent confidence intervals.

Figure C.3: Heterogeneous Effects of the First Split by Firm Size: Extensive-Margin Outcomes

(a) Outcome: Firm Remitted Any Formal Taxes or License Fees



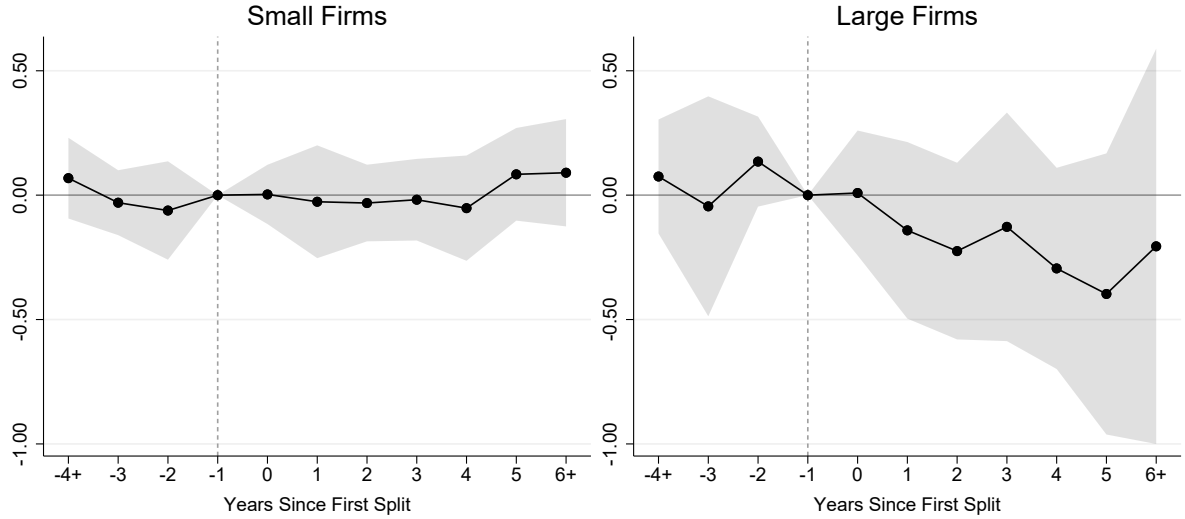
(b) Outcome: Firm Recorded Any Gifts to Others



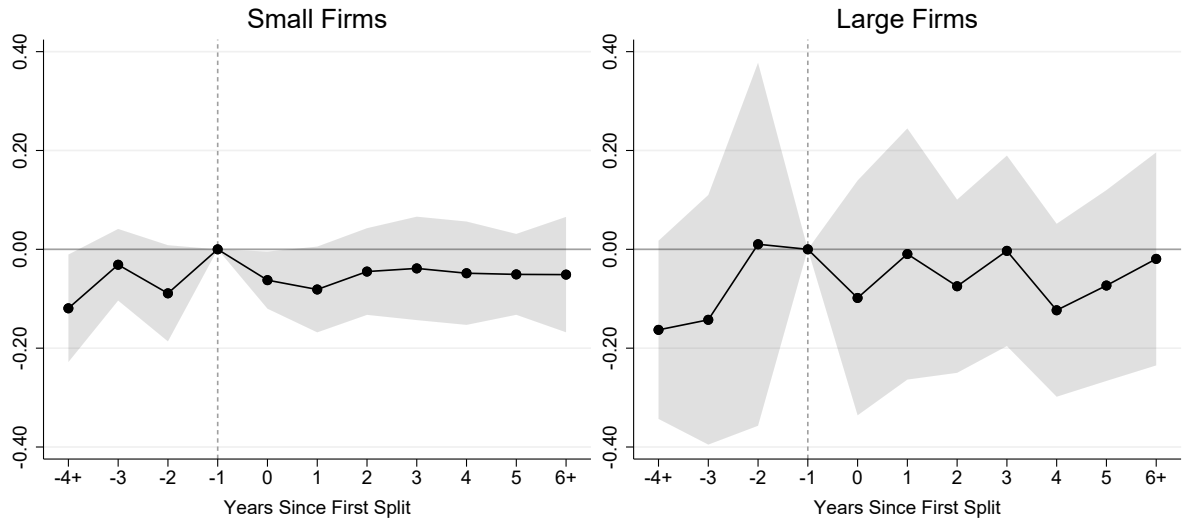
Notes: This figure plots the estimates of $\{\delta_s\}_{s \in \mathcal{S}}$ from (2) and their 95-percent confidence intervals. In each panel, the graph on the left uses the full sample of districts, and the graph on the right uses only districts that first split during 2001–03 or 2007–08 (right before or after the 2004–06 moratorium on splitting).

Figure C.4: Heterogeneous Effects of the First Split by Firm Size: Intensive-Margin Outcomes

(a) Outcome: Formal Taxes and License Fees as % of Revenue



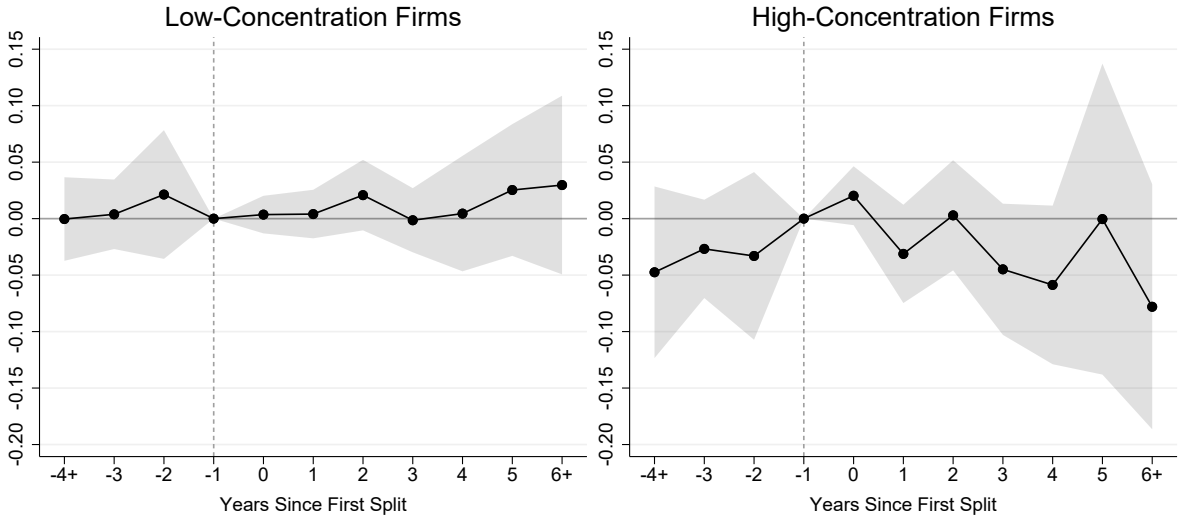
(b) Outcome: Gifts to Others as % of Revenue



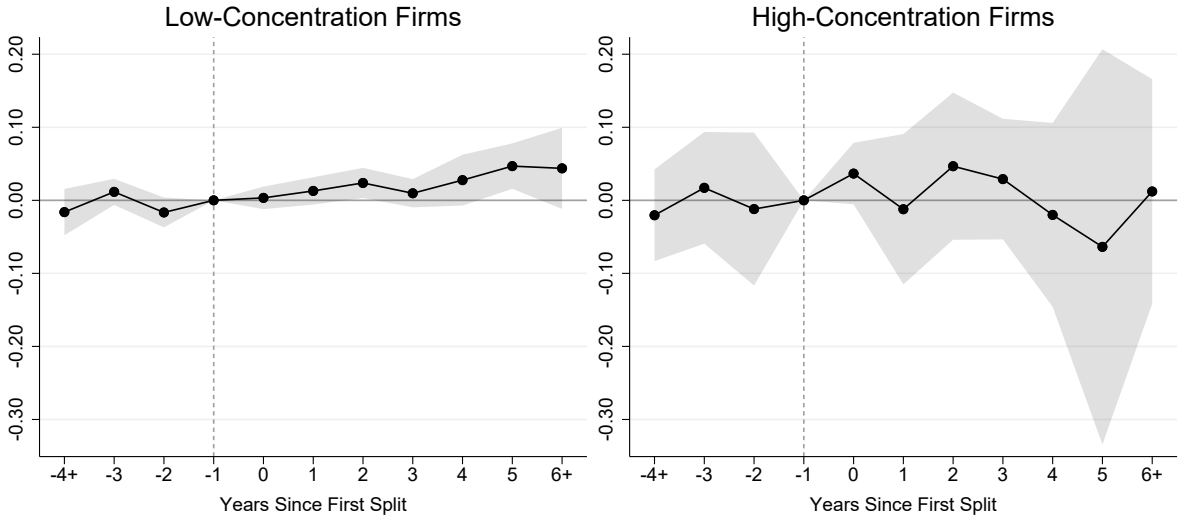
Notes: This figure plots the estimates of $\{\beta_s\}_{s \in \mathcal{S}}$ from (2) and their 95-percent confidence intervals. In each panel, the graph on the left uses the full sample of districts, and the graph on the right uses only districts that first split during 2001–03 or 2007–08 (right before or after the 2004–06 moratorium on splitting).

Figure C.5: Heterogeneous Effects of the First Split by Industry Concentration: Extensive-Margin Outcomes

(a) Outcome: Firm Remitted Any Formal Taxes or License Fees



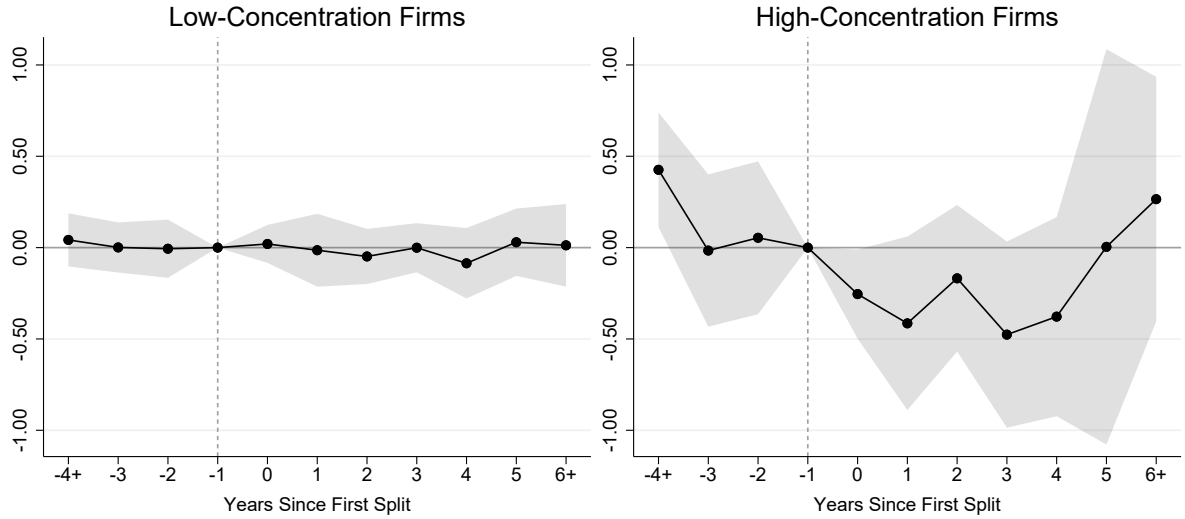
(b) Outcome: Firm Recorded Any Gifts to Others



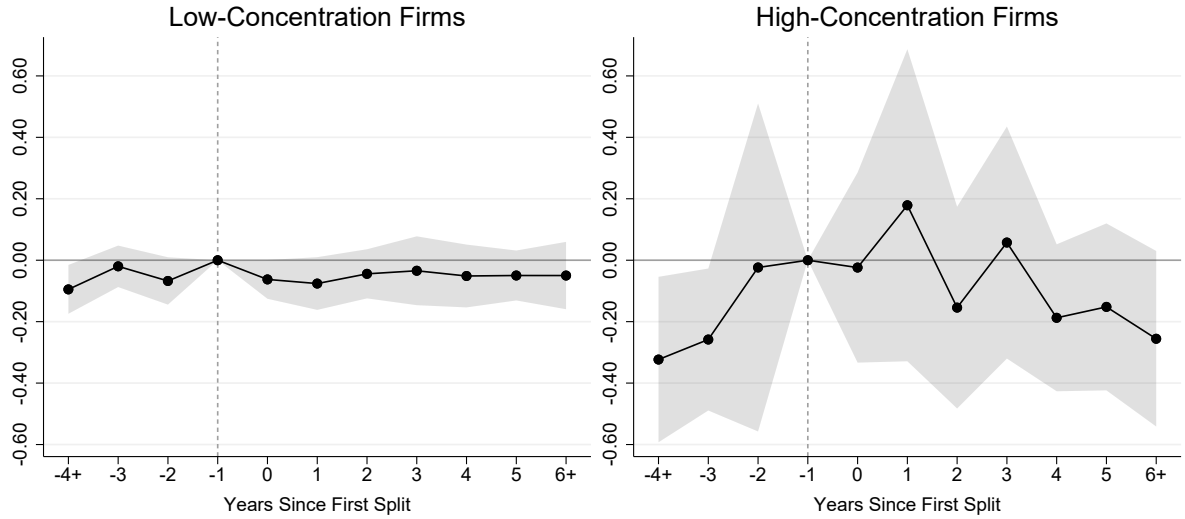
Notes: This figure plots the estimates of $\{\beta_s\}_{s\in\mathcal{S}}$ from (2) and their 95-percent confidence intervals. In each panel, the graph on the left is for a sample of "low-concentration" firms (i.e. firms in industries with below-median value of spatial concentration), and the graph on the right uses is for a sample of "high-concentration" firms.

Figure C.6: Heterogeneous Effects of the First Split by Industry Concentration: Intensive-Margin Outcomes

(a) Outcome: Formal Taxes and License Fees as % of Revenue



(b) Outcome: Gifts to Others as % of Revenue



Notes: This figure plots the estimates of $\{\beta_s\}_{s\in\mathcal{S}}$ from (2) and their 95-percent confidence intervals. In each panel, the graph on the left is for a sample of "low-concentration" firms (i.e. firms in industries with below-median value of spatial concentration), and the graph on the right uses is for a sample of "high-concentration" firms.