

# Adjusting to Government Fragmentation: Investment Dynamics and Beyond\*

Beata S. Javorcik<sup>a</sup> and Steven Poelhekke<sup>b</sup>

<sup>a</sup>EBRD, University of Oxford & CEPR

<sup>b</sup>Vrije Universiteit Amsterdam & CEPR

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## Abstract

Changes to administrative division of countries are not uncommon, prompting the question of whether government fragmentation matters for economic activity. If administrative capacity is limited, proliferation of local governments may increase the tax and compliance burden and heighten uncertainty, worsening the business climate with potentially detrimental effects for private investment. This study analyses implications of district proliferation in Indonesia. It finds that manufacturing plants operating in the splitting districts reduced investment and experienced an increase in the tax burden. In contrast, state-owned establishments did not register declining investment or higher taxation suggesting that links to the central or regional governments may have sheltered them from uncertainty and higher taxation.

**JEL Codes:** H77, H11, D22, D73

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Beata S. Javorcik: University of Oxford, Department of Economics, Manor Road Building, Manor Road, Oxford OX1 3UQ, United Kingdom. Email: beata.javorcik@economics.ox.ac.uk.

Steven Poelhekke (corresponding author): Vrije Universiteit Amsterdam, De Boelelaan 1105, 1081 HV Amsterdam, Netherlands. Email: steven.poelhekke@vu.nl.

# 1 Introduction

Over the last several decades some countries have devolved power to local governments and created new local jurisdictions (Grossman and Lewis, 2014), while other countries have seen consolidation of local governments.<sup>1</sup> Supporters of decentralization point out that bringing governments closer to their constituencies leads to better public service delivery, allows policies to closer reflect local preferences and facilitates monitoring (Tiebout, 1956; Besley and Case, 1995). Critics of decentralization focus on the increased cost of government proliferation and vulnerability to capture by special interests (Boffa et al., 2016). The literature has mostly examined the implications of government proliferation for government financing, efficiency and public services delivery. But surely the administrative and political upheaval caused by government fragmentation must have implications for private business. These implications are likely to be greater in the context of developing countries, where administrative capacity is limited and thus the administrative changes may not always be introduced in a smooth and efficient manner.<sup>2</sup>

This paper argues that proliferation of governments in a developing country setting can create fiscal and policy uncertainty and increase the tax and compliance burden for private businesses. We find that this has detrimental effects on investment in private establishments, though not in state-owned establishments (SOEs). This is intuitive, as the latter have direct links to the government.

We consider a clean and plausibly exogenous policy shock, namely the rapid process of government proliferation in Indonesia, which took place after the sudden fall of President Suharto in 1998. This episode of decentralization has been previously exploited in the literature with Burgess et al. (2012), Alesina et al. (2019), Bazzi and Gudgeon (2021), and Cassidy and Velayudhan (2024) making claims about its plausible exogeneity. Within the decentralization process, which was going on for over a decade, two waves of district splits stand out with respect to their exogenous nature due to unanticipated political events: 1999 and 2001 waves (there were no districts split in year 2000). Following widespread riots, the regime

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<sup>1</sup> In 1952, the U.S. Census Bureau reported a total of 116,755 local governments, of which only 90,075 remained by 2017 (Goodman, 2019).

<sup>2</sup> This was the case in Indonesia, which is the focus of our study (Nasution, 2016).

of President Suharto collapsed in May 1998, starting a process known as pemekaran (blossoming) and subsequent ‘big bang’ decentralization in 1999. Very rapidly laws on regional governance (Law 22/1999) and on fiscal relations (Law 25/1999) were drafted and passed under President Habibie. It allowed multiple requests for district splits by local politicians from earlier years to be suddenly approved by the president (who had veto rights) to preserve stability in a country with high ethnic diversity and influential local rulers (Fitriani et al., 2005; Burgess et al., 2012; Bazzi and Gudgeon, 2021). Then in 2000, the presidential veto over district splits was suddenly lifted, starting a new wave of district splits. The 1999 and 2001 wave, encompassing 51 districts splitting, give us a setting in which the unexpected turn of events determined the timing of district proliferation, making them an attractive setting for our analysis. These two waves will form the core of our analysis, although we will also consider the proliferation of districts that took place between 1993 and 2003.

The decentralization process in Indonesia was extensive, and the number of districts increased from 284 to 511 between 1989 and 2014. This process was associated with increased costs of running new administrations and a decline in central government transfers as a share of total revenue, thus leading to proliferation of local taxes, deterioration of business climate and creation of uncertainty about future policies and the quality of governance in the newly created districts. However, it did not have a *direct* effect on economic output, thus offering a good setting to study the implications of decentralization in isolation from other factors. Moreover, the staggered nature of the changes has created an ideal laboratory for our analysis that allows us to trace the impact of decentralization over time.

We set the stage for our analysis by demonstrating that Indonesia’s decentralization had a profound impact on local budgets. The detailed information on public finances shows that splitting districts received fewer earmarked transfers (in the first few years after the split) from the national government relative to their population size and area. At the same time, they increased the share of own-source revenue and cut back on public investment to pay for the burden of self-administration. These patterns are in line with the premise of our analysis that decentralization may increase the tax burden on private businesses, in addition to creating uncertainty about other aspects of local policies.

Our analysis is based on plant-level data from the Survei Manufaktur, the Indonesian

Census of Manufacturing, covering all registered manufacturing plants with more than 20 employees during the period 1990-2006. Our main variable of interest is the investment rate, defined as the ratio of total investment relative to the value of fixed assets, both reported directly in the census. We also consider a plethora of other plant-level outcomes, including taxes paid, capital-labor ratio, employment, donations and others.

Our main focus is on the investment response of individual plants. Under irreversibility of capital projects, investment is expected to react negatively to higher actual or expected taxation, deterioration in business climate as well as uncertainty. Irreversibility in combination with uncertainty leads to a positive option value of delaying investment until more information arrives (Bernanke, 1983). This mechanism relies on plants facing high costs in adjusting and reversing investment, resulting in periods with no investment followed by positive bursts.<sup>3</sup>

We show that plants located in splitting districts invest less (relative to the size of their capital stock) after the split takes place relative to plants operating in non-splitting districts. We take into account unobserved plant-level heterogeneity, sector-year heterogeneity and democratization at the local level.<sup>4</sup> Our finding is robust to controlling for the cashflow and output (both normalized by capital stock, in line with the accelerator model of investment), focusing on various subsets of splits, using various ways of clustering standard errors and excluding state-owned enterprises (SOEs).

If effect of decentralization is (at least to some extent) due to the more precarious fiscal

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<sup>3</sup> Adjustment costs are described as costs related to disruption when new capital is installed. They include costs associated with machine set-up, learning about new processes and routines, intangible organization capital, delivery lags and time to build, and the lack of secondary markets for capital goods (Cooper and Haltiwanger, 2006).

Although industry level studies from the US found only modest adjustment costs (Shapiro, 1986; Hall, 2004), a more recent micro-level analysis by Cooper and Haltiwanger (2006) finds that irreversible investment and adjustment costs are key to explaining the occurrence of both periods of zero investment and of peaks of high investment rates in plant-level data. Based on a sample of large manufacturing plants in the U.S., they find both zeros in 10% of observations and positive bursts of more than 20% in 18% of cases. In our data, both the zeros and the burst are more common: we observe zeros in 50% of observations, while bursts of above 20% are found in 31% plant-years. This suggests that adjustment costs of investing in capital may be substantially higher in Indonesia, where on average management may be less sophisticated than in the US (Javorcik and Poelhekke, 2017), thus amplifying the potential effect of uncertainty.

<sup>4</sup> The fall of Suharto has also led to democratisation at the local level. Although local mayors appointed by Suharto were allowed to complete their term after his fall, they were then replaced by mayors elected by local parliaments and from 2005 by mayors chosen through direct elections.

position of the newly created districts, then SOEs, and in particular those owned by the central government, should be sheltered from its impacts due to their close links to the authorities and their ability to rely on potential bailouts by the central government. To abstract from the potential impacts of decentralization on state ownership, we zoom in on SOEs that were in operation prior to 1999 and compare their post-decentralization investment patterns to those of private establishments. We find that while private establishments reduced their investment after district splits, state-owned establishments were not affected. These findings are robust to defining SOEs as having at least 50 percent or being fully owned by the state. As expected, these results are driven by SOEs belonging to the central government. SOEs belonging to regional governments do not behave any differently from private establishments.

Moreover, we show that establishments in the splitting districts see an increase in the tax burden. The ratio of indirect taxes paid to value added increases after a split for private establishments but not for SOEs. Furthermore, our difference-in-differences analysis documents a decline in capital-labor ratio for private establishments, but not for SOEs.

We conduct an event study using the Sun and Abraham (2021) approach and find confirmation of our baseline conclusions. We find no evidence of differential pre-trends in splitting and non-splitting districts. The impact on investment is first observed in the year after the split takes place, persists for four more years and then disappears. We also find a temporary decline in the capital-labor ratio and a short-lived increase in employment. This explains how producers manage to keep their output unchanged. These findings are suggestive of businesses dealing with increased taxation, deterioration in business climate and increased uncertainty by substituting labor for capital in order to avoid expenditures that are hard to reverse.<sup>5</sup>

Finally, we extend our analysis to consider the impact on foreign direct investment and find that splitting districts saw a decline in the likelihood of foreign acquisitions and greenfield projects. This decline was particularly pronounced in the year of district splitting, which is in line with decentralization causing uncertainty detrimental to investment. We also observe a

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<sup>5</sup> This pattern is consistent with the findings of Fetzner and Wang (2020) who show that UK firms increase their employment while reducing investment in the aftermath of the 2016 Brexit referendum, which introduced huge uncertainty about the future trading relationship between the UK and the European Union.

drop in the establishment entry rates in the year of district splits, reinforcing the same point.

We contribute to the empirical literature on the causes and consequences of government proliferation and decentralization spanning both political science and economics, including Fisman and Gatti (2002), Arzaghi and Henderson (2005), Grossman et al. (2017), Lima and Silveira Neto (2018), Narasimhan and Weaver (2022), Dahis and Szerman (2022), and Cohen (2024).<sup>6</sup> This literature is primarily concerned with the impacts of decentralization on budgets and public services delivery. The studies most related to our paper examine the economic impacts of government proliferation using macro (regional) data or nighttime luminosity and provide conflicting evidence. While Dahis and Szerman (2022) show that redistricting in Brazil leads to an increase in luminosity and no change in employment, Cohen (2024) finds the opposite effect with respect to luminosity in Uganda, and Cassidy and Velayudhan (2024) (examining the period 2000-2014) find a reduction in regional GDP in Indonesia despite an increase in fiscal transfers.

We also add to the literature focusing explicitly on the Indonesia context examining the implications of government proliferation and decentralization for public services (Lewis, 2017; Singhanian, 2022), public finances (Cassidy and Velayudhan, 2024), ethnic conflict (Bazzi and Gudgeon, 2021), and deforestation (Burgess et al., 2012; Alesina et al., 2019).

Our contribution lies in taking a micro perspective and using establishment-level data to examine the impact of government proliferation on firm behavior. We document a particularly striking negative effect on investment among private establishments, but not among SOEs. The negative consequences of government proliferation extend to foreign investors as establishments in splitting districts are less likely to become targets of foreign acquisitions and fewer foreign greenfield projects are observed. In sum, we show that the decentralization process itself is associated with economic costs by creating uncertainty, leading to a higher tax burden and (at least temporary) a deterioration in business climate. All of these factors have a detrimental effect on investment.

Why did the unintended consequences take place? The process of decentralization and government proliferation in Indonesia bestowed new powers on local governments, including giving them responsibility in the area of transport, agriculture, manufacturing industry and

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<sup>6</sup> See also a survey by Mookherjee (2015).

trade.<sup>7</sup> But as concluded by an Asian Development Bank study (Nasution, 2016), “the decentralization program was ill prepared” because “the capacity of subnational governments to produce public and private goods, increase productivity and employment, and promote economic growth in their jurisdictions, was not increased”. This capacity was lacking at the lower level due to the long tradition of centralization and the fact that before the reform local governments mainly served as implementing agencies of national policies and programs. The 2016 study concluded that “after over 15 years of reform, institutions have yet to be built to allow both the provincial and subprovincial governments to implement the newly acquired responsibilities.” These challenges applied to existing districts and were compounded by the challenges of adjusting to district splits that include the creation of new administrations in newly created districts. Hence, while decentralization may have brought about some benefits, the suboptimal way in which it was introduced proved costly for the private sector.

The rest of the paper is structured as follows. The next section sets the scene for our analysis. Section 3 discusses the data sources. Section 4 explains the empirical strategy for the main analysis. The results are presented in Section 5. Section 6 explores additional angles by documenting a detrimental effect of district splits on foreign direct investment and accounting for institutional quality. The last section concludes.

## 2 Setting the scene

This section sets the scene for our analysis by providing an overview of the district splitting process. It demonstrates that decentralization was associated with proliferation of district-level taxes and deterioration in institutional quality.

### 2.1 District proliferation in Indonesia

Indonesia had 284 districts in 1989, 301 in 1998, 440 by 2003, 497 by the year 2009 and 511 by 2014. Figure 1 shows the proliferation of subnational administrative districts (kabupaten

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<sup>7</sup> More precisely, Laws No. 22/1999 and No. 32/2004 gave local governments authority to execute a wide range of responsibilities in areas of health, education, public works, environment, communication, transport, agriculture, manufacturing industry and trade, capital investment, land, cooperatives, labor force, and infrastructure services (Nasution, 2016).

or regencies) in Indonesia by year between 1989 and 2009. The figure also shows the political timeline. During the reign of Suharto, who was in power since 1967, the country was more centrally governed and district splits were rare. Following widespread riots Suharto fell from power in May 1998, starting a process known as *pemekaran* (blossoming) and subsequent ‘big bang’ decentralization in 1999. This included the rapid drafting and passing of Law 22/1999 on regional governance and Law 25/1999 on fiscal relations under President Habibie. It allowed for multiple requests for district splits by local politicians from earlier years to be suddenly approved by the president (who held veto rights) in order to preserve stability in a country with high ethnic diversity and influential local rulers (Fitriani et al., 2005; Burgess et al., 2012; Bazzi and Gudgeon, 2021).

A second wave of decentralization took place from 2001, the year after the sudden lifting of the presidential veto over district splits. Redistricting stopped equally abruptly in 2004, when a sudden suspension of further splits and decentralization was implemented. Applications for new districts continued to arrive but were put on hold during this period (UNDP, 2008). The suspension ended in 2007 but was then reinstated between 2009 and 2012.

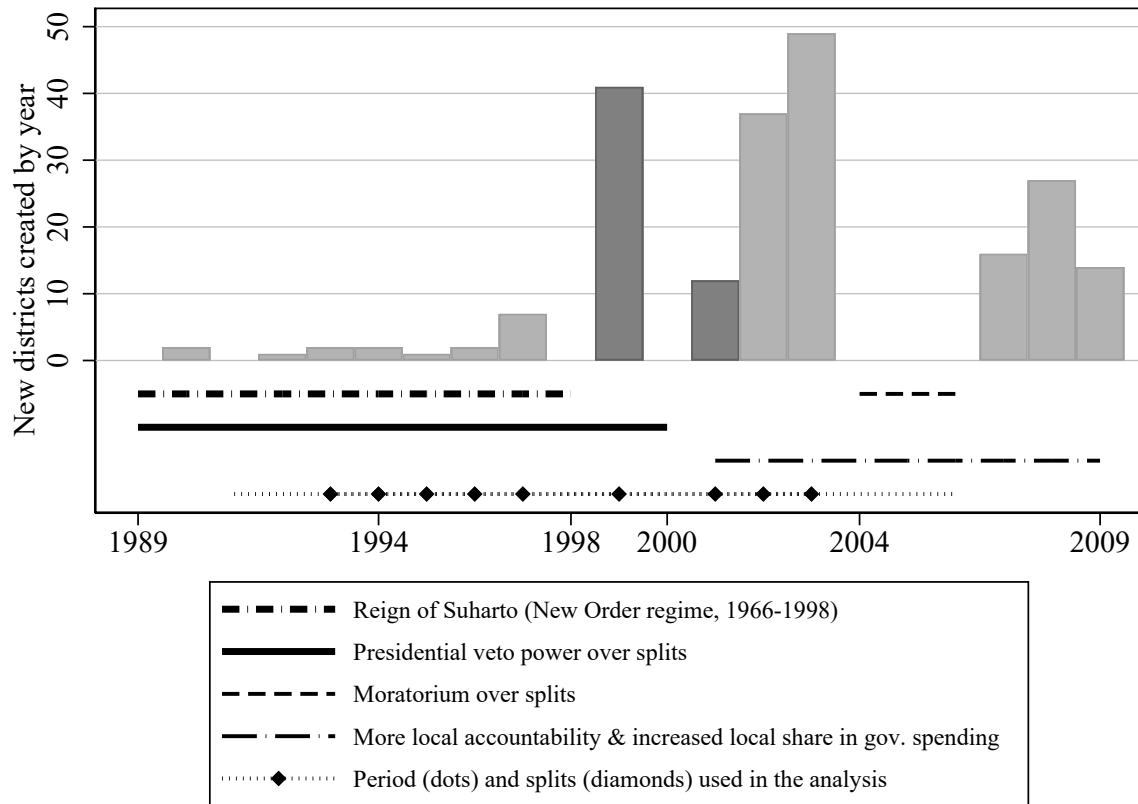
Our analysis follows a two-pronged approach. The first prong is based on district splits taking place between 1993 and 2003 (as marked in the figure). This allows us to consider the maximum number of districts splitting while allowing us to observe plants for a sufficient number of years prior to the first split and after the last split. The second prong focuses on district splits, which took place in 1999 and 2001 and whose timing was determined by unanticipated political events, thus making them plausibly exogenous.

## 2.2 Democratization

President Habibie held the first national elections after Suharto’s reign on 7 June 1999, democratizing the country after decades of dictatorship. At the local level, however, mayors that were appointed by the Suharto regime were allowed to finish their term, after which the locally elected parliament appointed a new mayor. Direct elections of mayors started in 2005. This process resulted in staggered democratization at the local level (Martinez-Bravo et al.,



Figure 1: New districts created by year and political timeline



Notes: Bars count the number of new districts that are created in a given year due to a split of the parent district. Horizontal lines denote specific political time frames.

2017) and happened independently of the timing of district splits.<sup>8</sup>

## 2.3 Proliferation of taxes

decentralization was accompanied by turmoil in the taxation landscape. In 1997, Law 18/1997 allowed local governments to issue a wide range of local government taxes. Note however that own-source revenue was only 8% of total revenue on average:<sup>9</sup> the new taxes had little revenue potential, but nevertheless posed high compliance costs to taxpayers and the economy. The ability of local authorities to tax was briefly restricted to a closed list by Law 18/1999, until Law 34/2000 again expanded the scope for local government revenues. Regional governments were allowed to add taxes through regional regulations approved by the regional government council with (in practice) limited national supervision (Brodjonegoro, 2004).<sup>10</sup>

Districts used the new laws to create taxes that ranged from advertisement taxes to road maintenance levies (Hofman and Kaiser, 2004; Luebke, 2005). This allowed districts to raise the share of local revenue and become somewhat less dependent on national transfers, some of which were of uncertain quantity and tended to arrive with as much as a six-month-long delay. In a non-random sample of 231 places surveyed by Lewis (2003), creation of new districts led to creation of up to 1,000 new taxes and charges in the year 2001 alone, 60% of which were not submitted for a national review.<sup>11</sup> Of the minority that was reviewed by the national government and covering two-thirds of districts, 40% applied directly to the primary sector (inputs), 10% to the secondary sector (manufacturing), and another 10% and 20% to trade and distribution, and services, respectively. According to LPEM-FEUI (2005) up to another 6,000 were created between 2000 and 2005. New decentralization laws were passed in September 2004 in order to strengthen central government control over local officials and

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<sup>8</sup> Of the 178 split events between 1990 and 2009, 63 preceded local democratization (of which 18 happened under Suharto), 22 coincided with local democratization, and 14 happened one year and 79 two or more years after local democratization.

<sup>9</sup> See Table OA1. This is equivalent of 6.70 USD per capita per year.

<sup>10</sup> However, the main candidate for local taxation, the property tax, remained under the jurisdiction of the national government (Brodjonegoro, 2004).

<sup>11</sup> These were, therefore, implemented illegally, partly on purpose, because if the national government did not invalidate a local law submitted to it within 60 days it came into force and the national government lost its power of annulment (Butt, 2015).

budgets (Soesastro and Atje, 2005).

We conduct an econometric analysis examining the relationship between revenue sources and district splits (see Online Appendix OA1). Its results are consistent with the survey evidence documenting proliferation of taxes in the aftermath of decentralization in Indonesia and suggesting that taxation could have been a source of uncertainty during and after district splits. The findings of the analysis can be summarized as follows. The share of direct national government transfers, namely the General Allocation Grant (DAU) in the total revenue, drops by 5.2%. This decline is accompanied by the increasing share of district own-source revenue, which goes up by 3.4% as well an increase in the revenue from the natural resource revenue sharing scheme (2.8%).

What happens to expenditures? As illustrated in Online Appendix Table OA3, post-split districts spent a higher share of their tax revenue on general administration (with the difference of 4.6% relative to non-splitting districts) and personnel (2.6%), while decreasing spending on public law and order and health.

In summary, splitting districts increase administrative expenditure and personnel costs. They attempted to compensate for these developments by levying new local taxes, thus potentially increasing uncertainty and compliance burden.

## 2.4 Deteriorating institutional quality and heightened uncertainty

Deterioration of the business climate is visible in survey data. The Survey of Regional Investment Attractiveness, carried out in 2004 by the Regional Autonomy Implementation Monitoring Committee (*Komite Pemantauan Pelaksanaan Otonomi Daerah*, or KPPOD), found that business owners reported local tax regimes as an important constraint on investment. These constraints arose in the form of compliance costs, such as business licensing, even when the tax or charge itself was moderate.<sup>12</sup> These developments increases the scope for corruption and exacerbated the impact of pre-existing corruption (Kuncoro, 2004; Luebke, 2005), adding to the overall economic harm done by the newly established local revenue

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<sup>12</sup> This is consistent with the trends observed in district revenues. While the total district revenue grew on average by 15% per year in constant rupiah between 1994 and 2003, other own source revenue (business licenses and fees) increased by 20% per year, with the biggest increase after 1999, and became more important than electricity taxes (Lewis and Sjahrir, 2009).

sources (Barnes et al., 2005).

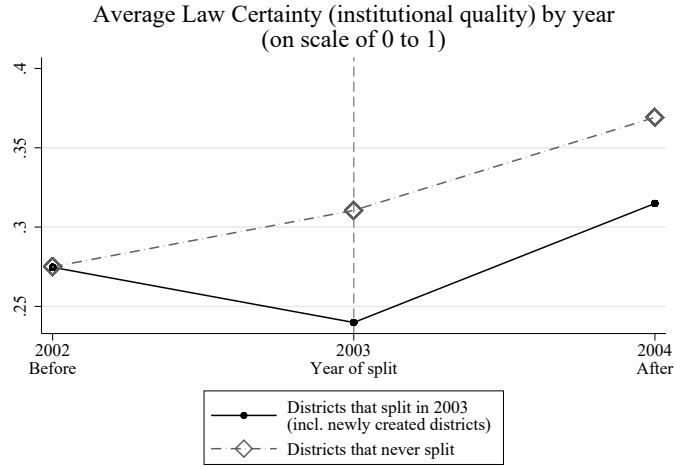
Data from KPPOD (2003) also allow us to shed light on the impact of decentralization on uncertainty. KPPOD compiled measures of local institutional quality in a subset of 124 districts during the years 2002, 2003 and 2004, by collecting views from local businesses and a panel of experts. Each district was scored along multiple dimensions using Likert scales (1 to 5, with 5 being best).<sup>13</sup> Law Certainty is of particular interest, as it refers to the consistency of rules and law enforcement in the region, whether court verdicts discriminate law subjects, the presence of illegal levies, and the strength of enforcement of formal rules, which depends on overlapping jurisdictions.

Because the data are available for three years, we can only examine district splits that took place in 2003. Although 50 new districts were created in 2003, only 10 districts that split in that year were surveyed by KPPOD. With this caveat in mind, Figure 2 shows the mean institutional score in districts that split in 2003, versus those that never split. Institutional quality was surprisingly similar in the two groups before splits occurred in 2003, but then deteriorated substantially in the splitting districts, while improving in the others, with the gap continuing in 2004 (the last year of KPPOD data).

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<sup>13</sup> The dimensions of business climate considered were: Apparatus & Service (22%), Regulation of Regional Legal Products (25), Regional Finance (14%), and Law Certainty (39%), with weights in brackets being used to arrive at a total institutional score.

Figure 2: District splitting and Uncertainty



*Notes:* This figure shows the average annual institutional quality score across districts according to KPPOD, for two groups of districts: those that never split (dashed line), and those that split in 2003 (solid line). Institutional quality is measured as ‘law certainty’ and is defined between 0 and 1, where 1 is best.

## 3 Data

### 3.1 Plant-level information

Our primary data source is the Survei Manufaktur, the Indonesian Census of Manufacturing conducted by the BPS on an annual basis. The census surveys all registered manufacturing plants with more than 20 employees. It contains detailed information on a large number of variables, including the four-digit sector classification, output, inputs, ownership and participation in international trade. It is a plant-level panel data set covering the period 1990-2006 and containing 351,603 plant-year observations. The average spell a plant remains in our sample is about 12 years. The data set also includes the location of the plant: a province and district code that allows us to track precisely which establishment is affected by a district split in which year.

Following the finance literature (Julio and Yook, 2012; Gulen and Ion, 2016), we truncate plant-level financial variables so that investment and cashflow to capital ratios fall between -5 and +5, dropping 4% of observations on investment.<sup>14</sup>

<sup>14</sup> Their respective means (and standard deviations) are then 0.35 (0.83) and 0.77 (0.99), while using the raw data results in -52.94 (124,883.90) and 939.30 (194,968.80), respectively. All our results are robust to alternative cut-offs, such as dropping the top 10 or 5 percentile of positive values and the bottom 10 or 5 percentile of negative values (to avoid dropping all zero observations).

We use the ownership information to distinguish between privately owned and state-owned plants. Because state-ownership shares may endogenously change over time and be influenced by government fragmentation, we define state-owned plants as those where central and regional governments combined owned stakes above a certain threshold in any year prior to 1999, the year before the large majority of district splits take place. We consider four thresholds: 50% and 100%, and distinguish between plants owned by the central government and plants owned by the regional government.<sup>15</sup> Private plants are defined as plants that have no state-ownership at all in *every* period. We thus drop from the analysis all (partial) nationalisations of plants that happen in 1999 or later. This specifically drops plants that were supported by government direct investment in the wake of the Asian financial crisis.<sup>16</sup>

Appendix Tables A1 and A2 list all the variables, definitions, and distributions for private and state-owned establishments, respectively.

### 3.2 Identifying splitting districts

We identify district splits using the Indonesia Database for Policy and Economic Research (INDO-DAPOER), which includes district-level information on public revenues by source and expenditure by category. It also includes a ‘walkthrough’ that relates districts to their predecessors: the parent district that split into new child districts. Each administrative district has a unique code that also appears in the Survey of Manufacturing. We validated these with the Master File Kabupaten of the Badan Pusat Statistik (BPS, Central Bureau of Statistics). The year of split is the year in which two or more districts are reported instead of the single parent district that existed the year before.<sup>17</sup>

Of the 284 districts that existed in 1989, 63% (179 districts) did not split by 2009, 17%

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<sup>15</sup> In 76% of plant-years with non-zero state-ownership, the share of state ownership is equal to 100%. 50% is the second most common ownership share. The share of plant-years with non-zero regional government ownership that is 100% owned by regional governments is 60%, and it is 80% for central government owned plants.

<sup>16</sup> We observe a spike in government ownership in 2000 (an increase from 633 plants in 1999 to 4,312 plants in 2000) that is unwound in the following years. These plants are excluded from the analysis.

<sup>17</sup> In some instances, a plant (or its surveyor) is late to start using the new district code. We are careful to clean the data for these occurrences and take INDO-DAPOER and the Master File Kabupaten as leading sources for the timing of splits. If a plant changes to a district code that is not a descendent of the parent district, then we consider the plant to have relocated. This is, however, very rare.

split once, 10% split two ways, 4% split three ways, 3% split four ways, and the remaining 2% had split five to eight ways. In some cases a district splits two ways in the same year, while it is more common that splitting happens sequentially, leading to smaller and smaller districts, but often with a gap of several years between splits.

For example, the 1989 district Padang Pariaman (with code 1305) in the province of Sumatera Barat had split two ways by 2009. This started with a one-way split in 1999 into Kepulauan Mentawai (code 1301) and the remaining Padang Pariaman (with new code 1306). In 2002, the new district Pariaman (code 1377) was carved out of the larger Padang Pariaman (which kept code 1306). Manufacturing plants located in Kepulauan Mentawai have thus experienced one split (in 1999), while plants in Pariaman have experience two splits, one in 1999 when Kepulauan Mentawai seceded, and one in 2002 when Pariaman became its own district. Plants in the remaining rump of Padang Pariaman have also experienced two splits, which are the two secession events.

## 4 Empirical strategy

### 4.1 Baseline specification

We start with a basic difference-in-differences setup:

$$y_{jt} = \beta \text{DistrictSplit}_{dt} + \gamma X_{jt} + \alpha_j + \nu_{mt} + \epsilon_{jt} \quad (1)$$

where  $y$  refers to outcome variables of interest observed for an individual plant  $j$  in year  $t$ .  $\text{DistrictSplit}$  is an indicator variable taking on the value of one in the year when district  $d$ , where plant  $j$  is operating, splits as well as in all subsequent years, and taking on the value of zero otherwise.<sup>18</sup> In all specifications, we control for whether or not district  $d$  had a democratically elected mayor in year  $t$ , following Martinez-Bravo et al. (2017), and in some specifications account for the output to capital stock and cashflow to capital stock ( $X_{jt}$ ). The specification includes plant fixed effects  $\alpha_j$  as well as 4-digit ISIC industry-year fixed

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<sup>18</sup> We will also experiment with treating plants located in either of the new ‘child’ districts separately, according to whether they are in the district that receives a new capital city, versus those that are in the district that keeps the parent district capital city.

effects  $\nu_{mt}$ . In our baseline results, we experiment with various kinds of clustering to test the robustness of our results. We cluster standard errors by plant, four-digit industry-year, and alternatively also on pre-split parent district or the initial 1989 districts.

## 4.2 Event study

We also conduct event studies and follow the estimation method of Sun and Abraham (2021) to estimate dynamic effects in settings of two-way fixed effects and staggered treatment, which takes into account heterogeneity in treatment effects. Given that treatment happens at a more aggregate level (districts) than our unit of observation (manufacturing plants) this appears especially relevant. We exclude  $t - 1$  as the baseline period and add three leads and nine lags to equation (1):

$$y_{jt} = \sum_{s=t-4}^{t-2} \beta_s \text{DistrictSplit}_{ds} + \sum_{s=t}^{t+9} \beta_s \text{DistrictSplit}_{ds} + \gamma X_{jt} + \alpha_j + \nu_{mt} + \epsilon_{jt} \quad (2)$$

In our baseline sample, we focus on private plants that experience a district split at most one time. While treatment is staggered over time and affects 2,255 plants in this baseline sample, we also have districts in the sample that never split, such that 15,758 plants are never treated. For this reason we only exclude one period as the baseline period, following Borusyak et al. (2024). When we analyze all district splits, we bin earlier leads with  $t - 4$  and later lags with  $t + 9$  because the number of observations that we can include beyond these horizons drops off quickly, due to the staggered nature of treatment and the entry and exit of plants from the sample in which the average plant is observed for 12 years. When we focus on the preferred subsample of 1999 and 2001 district splits,  $t + 7$  is the latest outcome period we can consider. Our initial analysis of investment is based on unrestricted sample, while later in the paper we restrict our attention to plants that we can observe for at least 14 consecutive years, thus making the panel more balanced.



### 4.3 Identifying assumptions

Our identification rests on the unexpected nature of the district splits, as in Burgess et al. (2012), Alesina et al. (2019), Bazzi and Gudgeon (2021), and Cassidy and Velayudhan (2024). The political events, highlighted in Section 2, suggest that decentralization and the approval and moratorium on splits were not anticipated. We follow a two-pronged approach. In the first prong, we focus on district splits taking place between 1993 and 2003 in order to maximize the number of instances of decentralization we can analyze. The choice of years is determined by the need to observe plants for at least four years prior to the first split and at least several years after the last split. In the second prong, we focus exclusively on the 1999 and 2001 splits, as these followed immediately the sudden fall of the Suharto regime and thus could not have been anticipated. Figure 1 graphs the distribution of district splits by year and depicts the period, covered in the first prong, with a dotted line.

The combination of two-way fixed effects and staggered treatment may create additional challenges for identification, for example when all units are treated as described in Goodman-Bacon (2021). Later treated units may then become controls for early treated units, while it is not clear that later treated units are similar enough to never treated units even before they are treated. However, 105 districts do not split between 1989 and 2009, such that this issue is not relevant in our case: almost all identification comes from comparing treated districts with never treated districts. In Online Appendix OA2 we perform a decomposition of the treatment effect to confirm this.

In Appendix OA6, we ask what district characteristics predict splitting. We find that neither natural resources, nor manufacturing employment, nor the share of plants with positive investment can predict district splits in our sample. Only surface area and population size have predictive power when it comes to future splits. Our analysis will control for surface area by including plant fixed effects. And we will show in robustness checks that controlling for the population size does not affect our main findings (see Appendix OA3).

## 5 Main results

### 5.1 Do manufacturing plants reduce investment after districts split?

Our main outcome of interest is the ratio of investment  $I$  in year  $t$  to fixed assets  $K$  at the start of the period  $t$ . We scale investment by the initial stock of capital to exploit the full variation in investment, as is standard in the literature. Establishments do not invest every year, so investment contains zeros in almost 50% of cases within our baseline sample and is reported to be negative in less than 2% of cases (net divestment or sale of assets). Taking logs of the level of investment would drop all these observations, hence the focus on a scaled variable instead. We first present the results for the full sample (see the upper panel of Table 1), followed by the results for our preferred subsample, encompassing only the 1999-2001 district splits (see the lower panel of Table 1).

The estimation results provide strong evidence in support of our hypothesis that decentralization is associated with a deterioration of the business climate and heightened uncertainty, which are detrimental to investment. Column 1 in the upper panel shows the results from a baseline specification including just the treatment indicator and controlling for local democracy, plant fixed effects, sector-year fixed effects and two-way clustering of standard errors on plant and sector-year. The coefficient of interest is negative and statistically significant at the one percent level. It suggests that plants operating in districts that have split reduced their investment by about 6 percentage points, on a mean investment rate of of 35%. In Column 2, we follow the Q-model of investment and additionally control for output-capital and cash-flow-capital ratios.<sup>19</sup> The coefficient of interest remains statistically significant at the one percent level and increases slightly in magnitude. In column 3, we exclude SOEs (plants with a positive state-ownership share in at least one year) and events where a plant's district splits for the second or more times.<sup>20</sup> The results remain robust. As evident from columns 5, 6 and 7, the results are robust to alternative clustering on current districts, 1989

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<sup>19</sup> Consistent with the Q-model of investment (Tobin, 1971; Tobin and Brainard, 1977; Blundell et al., 1992), we find that these positively predict investment, suggesting borrowing constraints, but they do not change the effect of splits.

<sup>20</sup> Specifically, we exclude plant-years from the moment a second split happens onward and the two years preceding that second split.

districts (recall that 1989 is the first year under study) and pre-split districts, respectively.<sup>21</sup> In the final column, we still cluster on pre-split districts, while additionally excluding control plants that will be treated in the future, beyond our sample period, after the moratorium on splits of 2004-2006. The effect of interest remains statistically significant at the one percent level with a magnitude of -0.080 being very similar to those found in the other columns. In sum, we conclude that district splits have a strong negative impact on investment decisions in the affected businesses.

In the lower panel, we consider only district splits that took place in 1999 and 2001, i.e., those immediately following the sudden fall of the Suharto regime. Thus we focus on a shorter time period and a smaller subsample.<sup>22</sup> The analysis in the lower panel mirrors the specifications presented in the top panel, and the results confirm the findings based on the full sample. In all columns, we find a negative and statistically significant (at the one percent level) effect of district splits. In all cases, the magnitudes of the estimated effects are larger than those found for the full sample. Districts splits are associated with an investment decline of between 9 and 14%.

The magnitudes of the estimated effect are larger than the magnitudes found in studies focusing on advanced economies. For instance, they are more than twice the effect of uncertainty around national elections (Julio and Yook, 2012); larger than the effect of a doubling of the Baker et al. (2016) policy uncertainty index (Gulen and Ion, 2016); and an order of magnitude larger than the effect of Brexit uncertainty for UK firms (Hassan et al., 2024). As argued earlier, less sophisticated management techniques prevalent in developing countries may increase the adjustment costs of capital, thus exacerbating the impact of uncertainty. Moreover, while these studies focus primarily on publicly listed firms, we study the behavior of non-listed establishments, including relatively small and medium-sized ones.

We also find that plants invest more after the staggered arrival of democracy at the local

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<sup>21</sup> The specification found in column 7 (column 15 for the preferred subsample) will constitute the estimation approach when we consider additional outcomes.

<sup>22</sup> This subsample includes plants in districts that never split, and plants in districts that experience their first split in 1999 or 2001. It excludes plants that experience a split in any other year. All plants in this sample experience at most one split.

district level.<sup>23</sup> In the subsequent analysis, we always control for local democracy.

In Online Appendix Section OA3, we show that the result, for our preferred subsample of 1999 and 2001 district splits as well as for the full sample, are also robust to: separately controlling for secondary splits; distinguishing child districts according to whether they keep the splitting parent district capital city or create a new capital; controlling for the district-level endowments of non-oil and oil natural resources interacted with indices of world commodity prices; controlling for (lagged) population; and adjusting the timing of splits by using the first availability of separate reporting on revenue by new districts (as opposed to the BPS walkthrough). Finally, our findings are robust to controlling for region-year fixed effects.

## 5.2 Are SOEs behaving differently after splits?

If uncertainty associated with the districts splitting is linked to the more precarious fiscal position of the newly created districts (recall discussion in Section 2.3), then SOEs, and in particular SOEs owned by the central government, may be sheltered from its impacts due to their close links to the authorities and their ability to rely on potential central government bailouts. By the virtue of their close connections with the local governments, SOEs may be also sheltered from uncertainty associated with policy changes in splitting districts and may enjoy preferential tax treatment. Thus overall, we would expect that their investment should be less affected than that of private establishments.

To test this hypothesis we allow for an interaction between the post-district-split period and an indicator for an establishment being an SOE. In order to abstract from the potential impacts of government fragmentation on state ownership via investment and divestment, we focus on SOEs that were in operation prior to 1999 and had at least 50% state ownership or were fully owned by the state prior to 1999. In other words, our SOE status is a plant-specific time-invariant variable.<sup>24</sup> Moreover, we include in our sample only establishments that were privately owned throughout the period, as well as the SOEs as defined above.

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<sup>23</sup> This supports the findings of Abeberese et al. (2023), who show that democratization increased firm productivity and improved the business environment in Indonesia.

<sup>24</sup> This also means that in the presence of plant fixed effects, there is no need to include a self-standing SOE indicator.

Table 1: District splits and investment

Dependent variable →	I/K							
	[1] + Q-model of investment	[1] + excluding second splits	[3] + excluding SOEs	[4] + cluster on current districts	[4] + cluster on 1989 districts	[4] + cluster on pre-split districts	[7] + Excl. control plants with first split after moratorium	
<b>Panel A: All splits</b>	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Year of split and after	-0.059*** (0.019)	-0.069*** (0.019)	-0.055*** (0.019)	-0.077*** (0.025)	-0.077*** (0.026)	-0.077*** (0.027)	-0.077*** (0.026)	-0.080*** (0.027)
Output/K		0.040*** (0.002)						
Cashflow/K		0.027*** (0.005)						
Local democracy period	0.044*** (0.012)	0.045*** (0.011)	0.042*** (0.012)	0.036*** (0.014)	0.036** (0.016)	0.036** (0.017)	0.036** (0.016)	0.037** (0.016)
Observations	163,005	163,005	162,804	124,821	124,821	124,821	124,821	124,267
R-squared	0.466	0.480	0.467	0.478	0.478	0.478	0.478	0.477
Clusters	1841	1841	1841	1763	316	257	315	315
<b>Panel B: 1999 and 2001 splits</b>	[9]	[10]	[11]	[12]	[13]	[14]	[15]	[16]
Year of split and after	-0.091*** (0.025)	-0.108*** (0.025)	-0.087*** (0.025)	-0.135*** (0.034)	-0.135*** (0.033)	-0.135*** (0.030)	-0.135*** (0.034)	-0.142*** (0.033)
Output/K		0.040*** (0.002)						
Cashflow/K		0.027*** (0.005)						
Local democracy period	0.036*** (0.012)	0.037*** (0.012)	0.035*** (0.012)	0.029** (0.014)	0.029* (0.016)	0.029* (0.016)	0.029* (0.016)	0.031** (0.016)
Observations	151,643	151,643	151,493	117,538	117,538	117,538	117,538	117,053
R-squared	0.469	0.483	0.470	0.483	0.483	0.483	0.483	0.482
Clusters	1825	1825	1825	1741	302	254	301	301

*Notes:* All regressions control for plant and sector-year fixed effects. Sample includes splits between 1993 and 2006 (Panel A) or splits in 1999 and 2001 (Panel B) and thus exclude splits that occur after the moratorium of 2004-2006. In column [8] we exclude control plants that are treated beyond our sample period (after the moratorium). SOEs denotes state-owned enterprises (plants with a positive state-ownership share in at least one year). \*\*\*Significant at 1% level; \*\*Significant at 5% level; \*Significant at 10% level. Standard errors clustered on plant and sector-year unless specified otherwise.

The results, based on the full sample and presented in the top panel of Table 2, are in line with our hypothesis. They indicate that while privately-owned establishments reduced their investment after district splits, state-owned establishments were not affected. This latter conclusion is drawn based on the p-values listed in the table, which test if the sum of the regressions coefficients is significantly different from zero. These findings are robust to both definitions of SOEs. The magnitude of the estimated investment decline in private plants of about 8 percentage points is in line with the results found in Table 1.

Next we distinguish between SOEs owned by the central government and those owned by regional governments.<sup>25</sup> We confirm the conclusion that private plants reduced their investment in the post-split period and find that neither centrally- nor regionally-owned SOEs were affected by the splits. As reflected in the p-values reported in the table, in neither of the columns can we reject the hypothesis of no effect on SOEs.

In the lower panel of the table, we repeat the same exercise for our preferred subsample of 1999 and 2001 district splits. Their results are very similar to those in the upper panel. We confirm the negative and statistically significant effect of district splits on investment in private establishments and find no statistically significant effects on investment by SOEs.

To foreshadow the results presented later in the paper, in Section 5.4 we will also show that while the indirect tax burden of private plants increased in the post-split period, the tax burden born by state-owned establishments remained unchanged. This is consistent with the view that SOEs enjoy a preferential tax treatment.

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<sup>25</sup> We drop 47 plants that had equal ownership between regional and central government and an additional 43 that switched between levels of government before 1999, affecting 756 observations. Our results are robust to including these plants.

Table 2: State-Owned Enterprises (SOE): Investment

Dependent variable →	I/K			
	> 50%	> 50%	100%	100%
SOE definition →				
<b>Panel A: All splits</b>	[1]	[2]	[3]	[4]
Year of split and after	-0.079*** (0.026)	-0.079*** (0.027)	-0.078*** (0.026)	-0.077*** (0.027)
SOE * Year of split and after	0.119* (0.062)		0.138*** (0.052)	
Central SOE * Year of split and after		0.154** (0.075)		0.129** (0.055)
Region SOE * Year of split and after		0.137 (0.103)		0.151 (0.141)
<i>Effect of split for SOEs (p-value)</i>	<i>.487</i>		<i>.231</i>	
<i>Effect of split for Central SOEs (p-value)</i>		<i>.303</i>		<i>.319</i>
<i>Effect of split for Region SOEs (p-value)</i>		<i>.569</i>		<i>.599</i>
Observations	132,424	131,678	131,557	130,815
R-squared	0.473	0.474	0.474	0.475
Districts	324	324	321	321
<b>Panel B: 1999 and 2001 splits</b>	[5]	[6]	[7]	[8]
Year of split and after	-0.136*** (0.033)	-0.138*** (0.033)	-0.135*** (0.033)	-0.135*** (0.033)
SOE * Year of split and after	0.086 (0.078)		0.142* (0.074)	
Central SOE * Year of split and after		0.111 (0.101)		0.127 (0.080)
Region SOE * Year of split and after		0.134 (0.140)		0.274 (0.244)
<i>Effect of split for SOEs (p-value)</i>	<i>0.517</i>		<i>0.922</i>	
<i>Effect of split for Central SOEs (p-value)</i>		<i>0.776</i>		<i>0.913</i>
<i>Effect of split for Region SOEs (p-value)</i>		<i>0.981</i>		<i>0.574</i>
Observations	124,422	123,715	123,732	123,029
R-squared	0.479	0.479	0.479	0.480
Districts	306	306	304	304

*Notes:* SOE denotes state-owned enterprise. Sample excludes second splits and control plants that experience their first split after the moratorium. Sample includes plants that are always privately owned and plants that had at least x% state ownership before 1999, where x is stated in the column header. All regressions control for plant and sector-year fixed effects, and the local democracy period and its interaction with state ownership. Observations and R-squared are the same in both panels. P-values in italics refer to testing  $Year\ of\ split\ and\ after + SOE * Year\ of\ split\ and\ after = 0$ . \*\*\*Significant at 1% level; \*\*Significant at 5% level; \*Significant at 10% level. Standard errors in brackets clustered on plant and pre-split districts. Sample years include 1993 to 2006.

### 5.3 Event study

Next, we turn to an event study analysis (see equation 2) and follow the methodology of Sun and Abraham (2021) that allows for heterogeneous treatment effects. Starting with the left-hand-side panel of Figure 3, based on the full sample, there is no evidence of differential pre-trends between plants in the treated and control districts. The negative impact of district splits on investment appears in the year following a split and persists for a further six years, after which it disappears. When we consider the subsample of 1999-2001 district splits (see the right hand side panel), the effect last only for 5 years in total.

Our empirical results are consistent with the time it takes for a brand new local government to form and for a new policy to be implemented by the (new) local bureaucracy. It is instructive to consider the average delay between the year a new district is created and the year in which the new district first publishes revenue and expenditure data, according to the World Bank’s INDO-DAPOER database. For revenue, this gap is on average modest: one year with a standard deviation of 1.3 years. However, for district expenditure data, the gap increases to 2.8 years on average with a standard deviation of 2 years. In other words, 20% of splitting districts have not reported their revenues two years after the split and their expenditures four years after the split.<sup>26</sup>

Moreover, once revenues and expenditures are published, they fluctuate substantially over the years. In new ‘child’ districts, defined as new districts that also have a new district capital, the annual growth in total revenue is 40% in the first four years after split, and only 17% in the subsequent 5 to 8 years after split. For expenditure, these numbers are 34% and 20%, respectively.<sup>27</sup> This suggests that it takes at least several years for district finances to stabilize and become more predictable. Moreover, as described in Section 2, new taxes and charges introduced by the new districts should be reviewed by national government before being implemented, further lengthening the period of policy uncertainty.

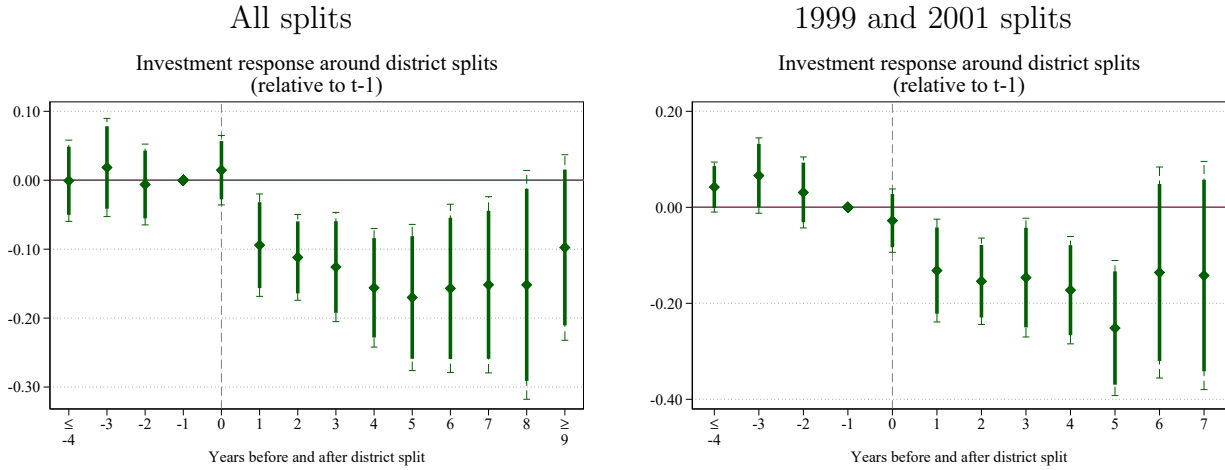
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<sup>26</sup> In Online Appendix OA3, we use the year of first availability of revenue data as an alternative definition of the timing of district splits and find that our results are robust.

<sup>27</sup> For all district splits combined, revenue grows 30% annually in the first four years, and 14% in the next four years. For expenditure, the numbers are 21% and 18%, respectively. All figures are in nominal terms.



Figure 3: District splits event study graph: Investment rate (I/K)



*Notes:* Sample includes plants that experience at most one split or experience no splits, are never state owned, and are observed for at least 14 consecutive periods. Thick spikes depict 90% confidence intervals, while the thin caps depict 95% confidence intervals. Corresponding Sun and Abraham (2021) regressions are presented in Online Appendix OA4.

## 5.4 What about other outcomes?

We also consider the impact of district splitting on other outcomes in our preferred subsample of districts splitting in 1999 and 2001. The top panel of Table 3 presents the results from our baseline difference-in-difference specification where we just focus on private establishments, the first splits occurring in a district and cluster standard errors on the pre-split districts. This is equivalent to specification from column 15 in Table 1, which is now reproduced in the first column. As our analysis later in the paper will include event studies estimated on plants that are observed for at least 14 consecutive periods, the middle panel of Table 3 restricts the data to the subsample used in the event studies.

**Tax burden.** The results indicate that establishments in splitting districts are subject to a higher tax burden, as proxied by the ratio of indirect tax payments to value added.<sup>28</sup> The coefficient of interest is statistically significant at the five and ten percent level in the upper and middle panel, respectively. It suggests that in splitting districts businesses experience a 1.2-1.4 percentage-point increase in the tax burden, which is equivalent to a 27% increase relative to the average value of 3 percentage points found in the sample of private establishments

<sup>28</sup> Both variables are taken directly from the Manufacturing Census. These also include central government administered taxes such as sales taxes, building and land tax (*PBB*).

(see summary statistics in Table A1).

**Other outcomes.** As evident from column 3, lower investment activity of establishments in split districts leads to a decline in capital-intensity of the production process, as one would expect. One worry may be that, rather than capturing deterioration of the business climate or uncertainty, we are capturing a disruption to the local economy, such as that occurring after civil unrest. While we do not observe civil unrest directly, we can look at other outcomes, such as output. The overall plant performance shows no indication of district splits affecting total output, goods output or employment, though event study analysis will show a more nuanced picture. Finally, we consider donations, which could be used as thinly veiled bribes, though fail to find a stastically significant effect on this outcome.

**Private plants vs SOEs.** The bottom panel of the table adds to the sample of Panel B plants that were state owned in the years preceding 1999. This allows us to compare the effect of district splits on private plants versus state-owned plants by means of an interaction term. Because our definition of an SOE is time-invariant, the direct effect of the variable SOE in Panel C is absorbed by plant fixed effects. The results first show that the effects on privately-owned establishments mirror those from the top panel and include a statistically significant decline in investment and capital intensity and a statistically significant increase in taxation. More importantly, the results also suggest that SOEs behave differently from private establishments. As evident from p-values, SOEs do not seem to have been affected by district splits when it comes to investment, capital intensity or taxation. They also seem to register an increase in output of goods in the aftermath of decentralization, which contrasts with the results for private plants who do not see a similar effect.

**Event studies.** In order to understand the dynamics of the adjustment process, we conduct event studies for the outcomes considered in the last table, namely: investment, indirect taxes, capital-labor ratio, employment, output and goods output as well as donations. We follow the methodology of Sun and Abraham (2021) and restrict the sample to private establishments that are observed for at least 14 consecutive years in the data. The choice of the latter

restriction is motivated by the length of the pre- and post-period treatment we consider in the event study. We focus on our preferred subsample of districts splitting in 1999 and 2001. The results are presented in Figure 4 with the underlying regression results presented in Table OA8. The thick spikes depict 90% confidence intervals, while the thin caps depict 95% confidence intervals.

The first graph shows the trajectory of investment, which confirms the pattern found in Figure 3 presented earlier. There is no evidence to suggest violation of the parallel trends assumption. The negative effect of decentralization is visible already in the year following a district split and persists for five years in total, after which it disappears.

The pattern for donations is more noisy, given that this variable may capture both legitimate charitable donations as well as lobbying expenditures and bribes. Nevertheless, the results are quite interesting, as they suggest a bump in donations in the first few years after a district split (with the effects for  $t + 1$  and  $t + 3$  being statically significant), followed by donations returning to their pre-treatment period level and then somewhat declining (with a decline in  $t + 5$  being statistically significant). The latter finding is consistent with a results of a cross-country analysis showing that fiscal decentralization in government expenditure is associated with lower corruption (Fisman and Gatti, 2002). Again, there is no evidence of differential pre-trends.

Unlike the difference-in-differences analysis, the event study shows a temporary spike in employment. It reinforces the decline in capital intensity precipitated by lower investment and explains why producers are able to keep their output constant.

**Entry and exit.** As explained earlier, the industrial census includes all plants with more than 20 employees. This means that the data set is not well suited for examining entry and exit as these events cannot be distinguished from employment crossing the 20 worker threshold. Keeping this caveat in mind, we nevertheless examine the impact of district splits on entry and exit of individual plants. The entry and exit rates are defined as the number of new (exiting) plants relative to the number of plants in operation in a given year in a given district and a given 2-digit sector. We see a statistically significant decline in the entry rates in the year of the split, which is consistent with decentralization causing uncertainty

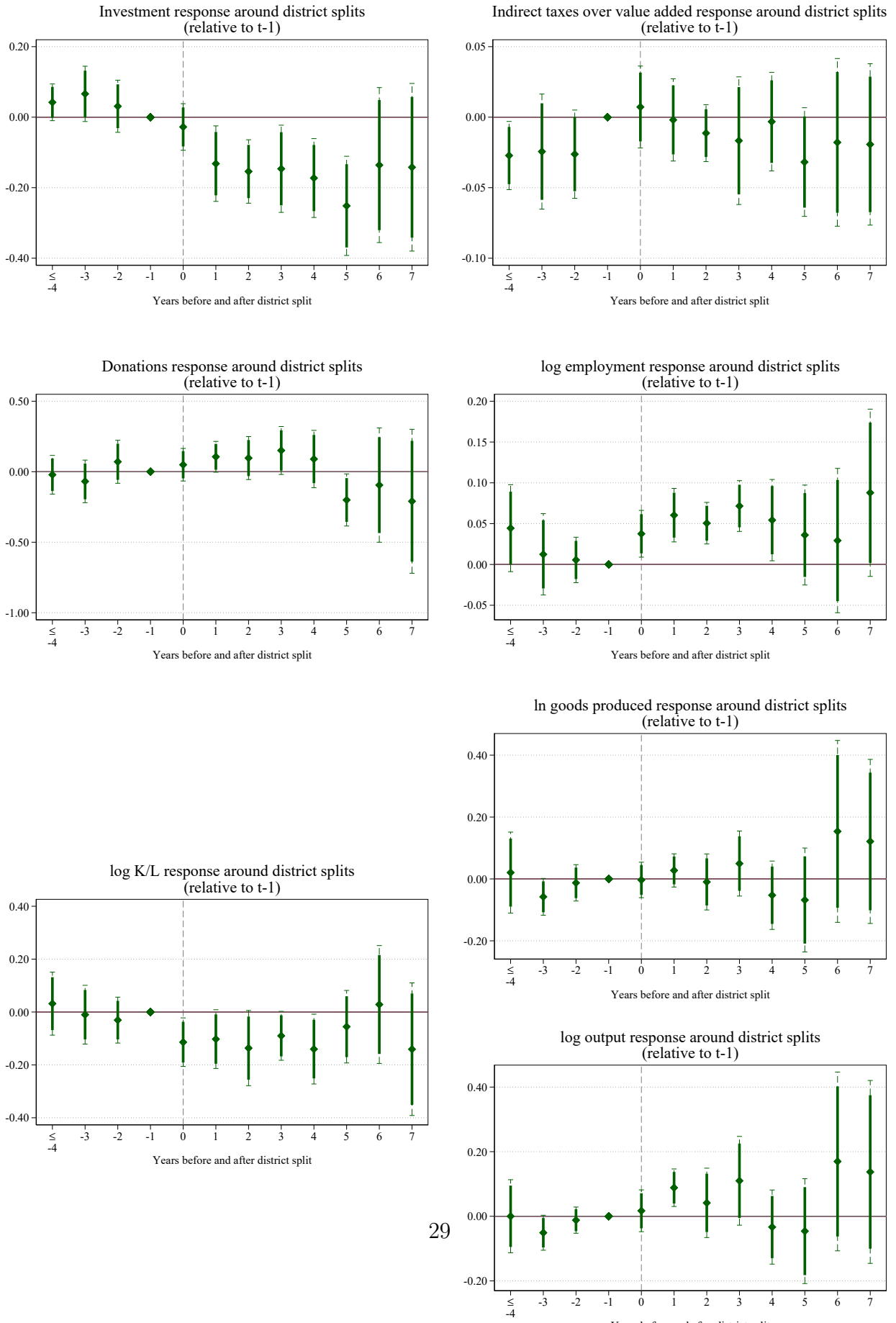
that discourages entry. Three years later the data show an increase in plant turnover, i.e., an increase in both in entry and exit rates. See Appendix OA5.

Table 3: 1999 and 2001 district splits and other outcomes

Dependent variable →	I/K	Indirect taxes over value added	log K/L	log employment	log output goods	log output	log donations
<b>Panel A: Private plants</b>	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Year of split and after	-0.135*** (0.034)	0.012** (0.006)	-0.123*** (0.047)	0.022 (0.019)	-0.000 (0.055)	0.044 (0.056)	0.065 (0.056)
Observations	117,538	117,505	117,538	117,538	109,980	117,537	97,360
R-squared	0.483	0.381	0.804	0.936	0.919	0.923	0.800
Districts	301	301	301	301	301	301	291
<b>Panel B: Private plants observed ≥ 14 consecutive periods</b>	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Year of split and after	-0.164*** (0.042)	0.014* (0.007)	-0.144*** (0.053)	0.028 (0.021)	0.014 (0.063)	0.061 (0.062)	0.089 (0.061)
Observations	59,794	59,774	59,794	59,794	56,535	59,794	50,319
R-squared	0.429	0.376	0.767	0.934	0.918	0.922	0.783
Districts	217	217	217	217	217	217	215
<b>Panel C: Private plants and SOEs observed ≥ 14 consecutive periods</b>	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Year of split and after	-0.164*** (0.043)	0.015** (0.007)	-0.126** (0.054)	0.024 (0.019)	0.023 (0.061)	0.063 (0.061)	0.095 (0.059)
SOE * Year of split and after	0.132 (0.081)	-0.023* (0.013)	0.226 (0.197)	-0.034 (0.083)	0.141 (0.108)	0.085 (0.105)	-0.041 (0.180)
<i>Effect of split for SOEs (p-value)</i>	.711	.389	.616	.906	.079	.132	.746
Observations	65,548	67,842	65,743	67,867	64,224	67,866	55,363
R-squared	0.424	0.360	0.747	0.934	0.924	0.928	0.791
Districts	231	232	231	232	232	232	228

*Notes:* Panels A and B exclude SOEs (plants that are ever state-owned), while Panel C includes SOEs: plants with at least 50% state ownership before 1999. All panels exclude second splits and control plants that experience their first split after the moratorium. All regressions control for plant and sector-year fixed effects and the local democracy period (also interacted with SOE in Panel C). *Indirect taxes* includes central government administered taxes (sales taxes, building and land tax (*PBB*)), fees for business permits, road use tax (*SWP3D*), import duties, custom fees, and other levies (not including income and personal taxes). \*\*\*Significant at 1% level; \*\*Significant at 5% level; \*Significant at 10% level. Standard errors clustered on plant, industry-year, and pre-split districts. Sample years include 1993 to 2006.

Figure 4: 1999 and 2001 District splits event study graphs: Other outcomes



## 6 Additional evidence

### 6.1 District splits and foreign direct investment (FDI)

Deterioration of the business climate and uncertainty introduced by district splits are likely to have had a negative effect not only on investment decision of domestic firms, but also on inflows of foreign direct investment. Therefore, next we examine whether district splits had a negative impact on foreign acquisitions and greenfield projects.

We define foreign acquisitions as a change in foreign ownership at the establishment level from under 10 percent to over 50 percent. Greenfield projects are defined as new establishments appearing in the data with at least 50 percent of foreign equity. The dependent variable in our analysis is then defined as an indicator for (i) at least one foreign acquisition, (ii) at least one greenfield entry; or (iii) at least one acquisition or greenfield entry taking place in a given two-digit sector in a given district in a given year. We conduct an event study following the approach of Sun and Abraham (2021) and control for the post-democratisation period, logged number of plants in operation, 1989 district fixed effects, and sector-year fixed effects. Standard errors are clustered at the level of 1989 districts. We conduct this exercise on the full sample as well as on the sample of 1999-01 district splits.<sup>29</sup>

The results, depicted in Figure 5, support our priors. In the top panel, they indicate that splitting districts were less likely to experience foreign acquisitions. There was an immediate effect taking place in the year of the decentralization shock and then a smaller dip lasting for several years. The immediate effect is consistent with decentralization causing uncertainty about future policies of newly created districts and this uncertainty deterring foreign acquisitions.

The graphs in the middle panel, focusing on the entry of greenfield FDI, also show a big drop in the year of decentralization followed by a modest decline for a few years that follow. These patterns are confirmed in the bottom panel, we considers both foreign acquisitions and greenfield FDI.

The detrimental effect of district splits on FDI flows, which particularly pronounced in

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<sup>29</sup> In the full sample, around 3% of cells experience one foreign acquisition, 0.7% two acquisitions, and the maximum equals 44.

the year when decentralization takes place, is consistent with the policy shock introducing uncertainty about future policies.

## 6.2 Institutional quality and investment

Based on the discussion of the deterioration of institutional quality in Section 2.4, we are interested in understanding whether institutional quality mattered for the trajectory of investment and donations in the splitting districts. The indices of institutional quality, based on the KPPOD survey, are available only for 2002-04. This forces us to focus only on districts that split in 2003 (versus those that did not split at all) utilizing institutional variables from the pre-split period, namely 2002.<sup>30</sup> We estimate the following equation:

$$\begin{aligned}
 y_{jt} = & \beta_1 DistrictSplit_{d,2003} + \beta_2 DistrictSplit_{d,2003} * Institutions_{d,2002} & (3) \\
 & + \beta_3 LocalDemocracy_{jt} + \alpha_j + \nu_{mt} + \epsilon_{jt}
 \end{aligned}$$

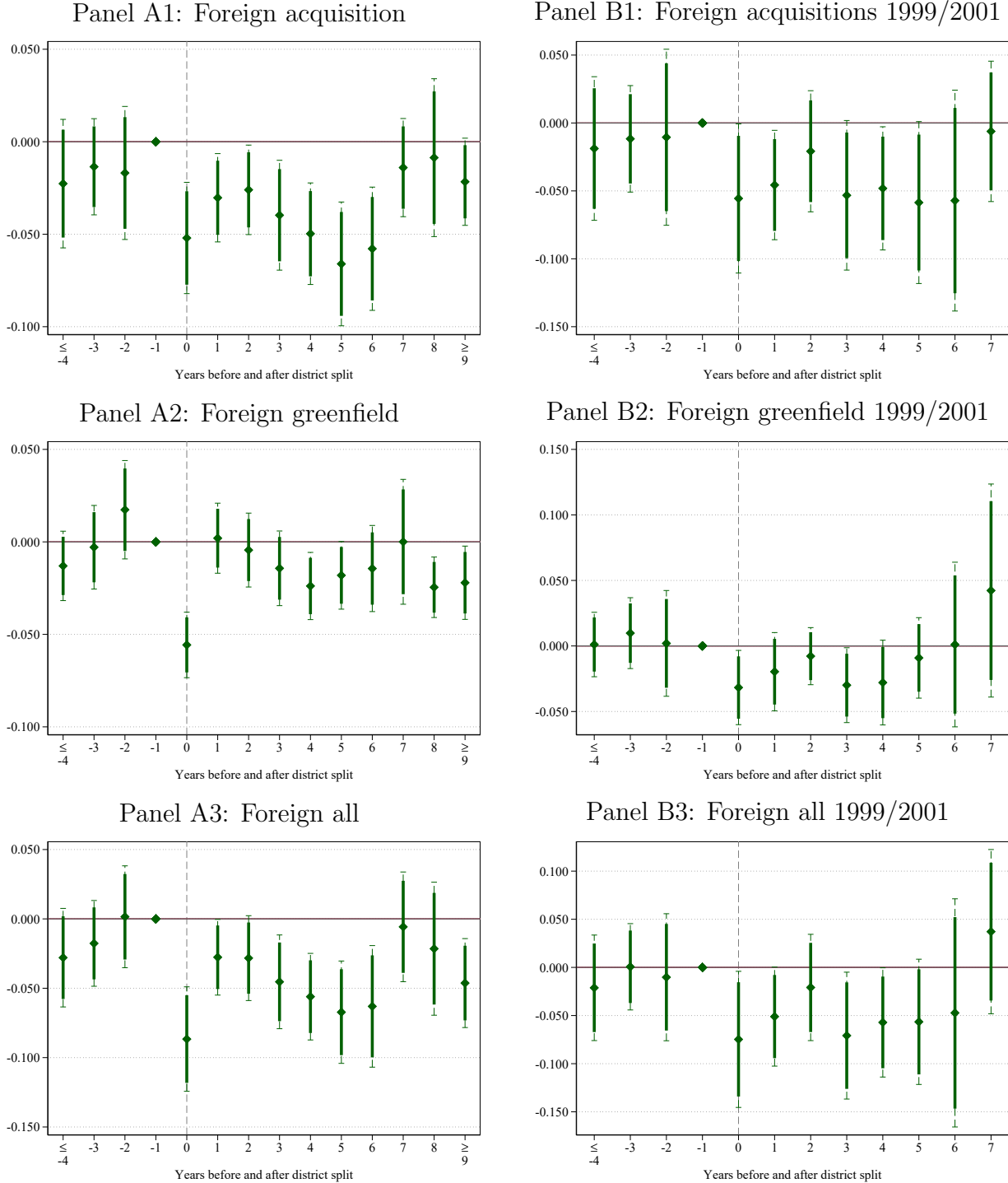
where  $y$  refers to outcome variables of interest observed for an individual plant  $j$  in year  $t$ , with  $t$  ranging from 2002 to 2003. *DistrictSplit* is an indicator variable taking on the value of one in 2003 when district  $d$ , where plant  $j$  is operating, splits, and taking on the value of zero otherwise. The specification includes plant fixed effects  $\alpha_j$  as well as 4-digit ISIC industry-year fixed effects  $\nu_{mt}$ . We cluster standard errors by plant, four-digit industry-year, and pre-split parent district.

*Institutions* is the Law certainty measure (introduced in Section 2.4) or one of its sub-components. Law certainty is defined between 0 and 1 (which is a rescaling of the underlying Likert scales) and is itself an aggregate of four underlying measures, with weights listed in the column headings and means in brackets. These four measures are: *Consistency of regulations* which “measures the certainty, clarity, and consistency in enforcement of regional regulations and other policies regulating business”; *Law enforcement* which “measures law certainty such as protection on work contract and ownership right, consistency of court decisions especially those related to business”; *Illegal levies outside bureaucracy* which “portrays

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<sup>30</sup> We focus on 2002 values to avoid capturing institutional changes brought about by the splits themselves.

Figure 5: FDI events at the sector-district-year level



*Notes:* Panels A include all district splits, while panel B includes only 1999 and 2001 district splits. Panels 1 show the effect of district splits on a sector-district-year level indicator variable equal to 1 if at least one foreign acquisition took place where foreign ownership increased from  $< 10\%$  to  $> 50\%$ . In Panels 2 the plant entered with 100% foreign ownership. Panels 3 combine both types of events. The unit of observation is a sector-district-year ( $N=39,610$ ). The controls include: an indicator for the post-democratization period, 1989 district $\times$ sector and sector $\times$ year fixed effects. Thick spikes depict 90% confidence intervals, while the thin caps depict 95% confidence intervals, clustered by 1989 districts.



regional government’s settlement of illegal practice in levy conducted by people or group of people outside bureaucracy that disturbs business”; and *Executive-Legislative relations* which “captures problems caused by poor relations between district parliament (DPRD) and Regional Government” (KPPOD, 2003, p.108).

Each column in Table 4 presents an interaction of the post-split indicator with the full index or its different subcomponent. The estimation results exhibit intuitive patterns. Greater certainty about law mitigates the increase in donations brought about by the district splits (column 1), with the effect being driven by ‘law enforcement’ (column 4) and ‘executive-legislative relations’ (column 6).

In the case of investment (see Panel B), both the aggregate index of law certainty as well as its three components (columns 9-11) mitigate the effects of districts splits. Limits on illegal levies have the largest mitigating effect, which is consistent with the proliferation of taxes discussed in Section 2.3.<sup>31</sup>

Although the results, presented in this section, exhibit the expected patterns, they should be interpreted with caution, given the small sample of districts that were surveyed by KPPOD and thus can be included in the analysis as well as the short time span covered by the KPPOD data.

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<sup>31</sup> Surprisingly, poor relations between the district parliament and its government appear conducive to investment.

Table 4: Initial institutional quality

Dependent variable →	I/K					
Interaction variable (t=2002) →	Law certainty					
...of which → (weight in total)	Full index (100%)  (mean=0.32)	Consistency of Regulations (28%)  (mean=0.37)	Law En- forcement (44%)  (mean=0.24)	Illegal Levy outside Bu- reaucracy (15%)  (mean=0.21)	Executive- Legislative Relations (13%)  (mean=0.39)	
	[1]	[2]	[3]	[4]	[5]	[6]
Year of split and after	-0.047** (0.023)	-0.103*** (0.033)	-0.116*** (0.041)	-0.056*** (0.020)	-0.120*** (0.035)	0.120** (0.057)
Year of split * interaction		0.719** (0.360)	0.720* (0.382)	0.780*** (0.285)	0.936** (0.367)	-0.465*** (0.151)
Local democracy period	-0.032* (0.019)	-0.033* (0.019)	-0.033* (0.019)	-0.033* (0.019)	-0.033* (0.019)	-0.032* (0.019)
Observations	3,584	3,584	3,584	3,584	3,584	3,584
Districts	72	72	72	72	72	72
R-squared	0.928	0.928	0.928	0.928	0.928	0.928

*Notes:* Percentages are sub-index weights in the aggregate index *Law certainty*. Means are sample means of the institutional indices used for calculating the marginal effects. Sample excludes SOEs (plants that are ever state owned) and second splits. Sample years include 2002 and 2003, with splits occurring in 2003. All regressions control for local democracy period, plant and sector-year fixed effects.\*\*\*Significant at 1% level; \*\*Significant at 5% level; \*Significant at 10% level. Standard errors clustered on plant, sector-year, and pre-split districts.

## 7 Conclusions

This study documents some transitory negative consequences of government fragmentation for economic outcomes in a developing country setting. Taking advantage of plausibly exogenous proliferation of districts in Indonesia, it documents a reduction in investment rates among plants located in splitting districts. This decline is substantial, with investment rates declining by 9 to 14 percentage points for several years. The magnitude of the investment decline is larger than the effects typically documented among listed firms in advanced economies. This differential could be due to less sophisticated management techniques and inclusion of relatively small and medium-sized unlisted establishments in our data.

In contrast to private plants, establishments owned by the government do not register a drop in investment. This is intuitive as by the virtue of their ownership structure they are not subject to the same uncertainty about future policies and tax burden as private entities. The data also indicate that decentralization leads to a decline in firm entry rates in the year when a district splits. Finally, foreign direct investment also seems to be adversely affected as splitting districts are less likely to see foreign acquisitions and greenfield investments, with the effects being particularly pronounced in the year of district split.

Our results also show an increase in the plant-level tax burden and as well as a temporary decline in the capital-labor ratio in private, but not in state-owned, establishments. In the absence of an adverse effect on demand, businesses compensate for lower investment by increasing employment, which is in line with hiring being less costly to adjust than fixed asset purchases in Indonesia. Intuitively, the effects of district splits on investment are smaller in districts with a higher initial level of institutional quality.

In summary, our findings are in line with government proliferation leading to at least a temporary decline in business climate and a period of heightened uncertainty, both of which had a detrimental effects on private investment, both domestic and foreign. Thus although bringing government closer to citizens may have its benefits, our paper suggests that it may also be associated with substantial transitory costs.

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# Appendix

Table A1: Summary statistics: private plants

<b>Panel A:</b> By private plant-year	N	mean	s.d.	min	max
Dummy =1 in year of split	124,905	0.01	0.10	0.00	1.00
Dummy =1 in year of split and after	124,905	0.06	0.24	0.00	1.00
Investment (I/K)	124,905	0.35	0.78	-5.00	5.00
Indirect taxes over value added	124,868	0.03	0.12	0.00	4.98
log K/L	124,905	9.31	1.37	3.01	20.34
log Employment	124,905	4.00	1.04	2.56	10.66
log Output goods	117,360	13.85	1.87	-0.34	23.53
log Output	124,904	13.81	1.87	7.60	23.53
log Donations	103,908	7.12	1.75	0.46	19.70
Output/K	124,905	2.83	3.02	0.00	16.67
Cashflow/K	124,905	0.63	0.83	-4.95	5.00
Dummy =1 if local democracy period	124,893	0.32	0.47	0.00	1.00

<b>Panel B:</b> By 2-digit sector-district-year	N	mean	s.d.	min	max
Entries as a share of all private plants	35,506	0.03	0.10	0.00	1.00
Exits as a share of all private plants	35,506	0.03	0.10	0.00	1.00
At least one greenfield FDI event	35,863	0.04	0.19	0.00	1.00
At least one FDI event	35,863	0.06	0.23	0.00	1.00

*Notes:* *Year of split* is derived from the Indonesia Database for Policy and Economic Research (INDO-DAPOER) and the Master File Kabupaten of the Badan Pusat Statistik (BPS, Central Bureau of Statistics). *Indirect taxes* includes central government administered taxes (sales taxes, building and land tax (*PBB*)), fees for business permits, road use tax (*SWP3D*), import duties, custom fees, and other levies (not including income and personal taxes). *Local democracy period* is from Martinez-Bravo and Stegmann (2018). All other variables are from the Indonesian Census of Manufacturing.

Table A2: Summary statistics: SOEs

<b>Panel A:</b> Private plants and SOEs	N	mean	s.d.	min	max
Dummy =1 if plant >50% SOE before 1999	138,250	0.08	0.27	0.00	1.00
Dummy =1 if plant >100% SOE before 1999	138,250	0.07	0.25	0.00	1.00
<b>Panel B:</b> SOEs (>50% before 1999)	N	mean	s.d.	min	max
Dummy =1 if plant >50% Central gov. SOE before 1999	13,345	0.52	0.50	0.00	1.00
Dummy =1 if plant >50% Region gov. SOE before 1999	13,345	0.35	0.48	0.00	1.00
Dummy =1 in year of split	13,345	0.02	0.13	0.00	1.00
Dummy =1 in year of split and after	13,345	0.10	0.30	0.00	1.00
Investment (I/K)	9,153	0.31	0.74	-4.34	4.98
Indirect taxes over value added	13,335	0.04	0.17	0.00	4.77
log K/L	9,509	9.98	1.82	-6.24	18.95
log Employment	13,345	5.00	1.41	3.00	10.62
log Output goods	12,703	15.63	2.27	6.96	23.52
log Output	13,344	15.65	2.26	8.21	23.52
log Donations	8,322	8.43	2.22	0.03	18.47
Output/K	8,704	2.63	3.20	0.00	16.67
Cashflow/K	8,676	0.72	0.97	-5.00	4.99
Dummy =1 if local democracy period	13,345	0.37	0.48	0.00	1.00

*Notes:*

Table A3: Summary statistics: private plants (controls and 1999 and 2001 splits)

<b>Panel A:</b> By private plant-year	N	mean	s.d.	min	max
Dummy =1 in year of split	117,626	0.00	0.07	0.00	1.00
Dummy =1 in year of split and after	117,626	0.03	0.16	0.00	1.00
Investment (I/K)	117,626	0.35	0.77	-5.00	5.00
Indirect taxes over value added	117,594	0.03	0.12	0.00	4.98
log K/L	117,626	9.27	1.37	3.01	20.34
log Employment	117,626	3.97	1.02	3.00	10.66
log Output goods	110,320	13.78	1.85	-0.34	23.53
log Output	117,625	13.75	1.85	7.60	23.53
log Donations	98,420	7.07	1.74	0.46	19.70
Output/K	117,626	2.84	3.02	0.00	16.67
Cashflow/K	117,626	0.63	0.82	-4.95	5.00
Dummy =1 if local democracy period	117,620	0.33	0.47	0.00	1.00

<b>Panel B:</b> By 2-digit sector-district-year	N	mean	s.d.	min	max
Entries as a share of all private plants	31,497	0.02	0.09	0.00	1.00
Exits as a share of all private plants	31,497	0.02	0.09	0.00	1.00
At least one greenfield FDI event	31,678	0.04	0.19	0.00	1.00
At least one FDI event	31,678	0.05	0.23	0.00	1.00

*Notes:* *Year of split* is derived from the Indonesia Database for Policy and Economic Research (INDO-DAPOER) and the Master File Kabupaten of the Badan Pusat Statistik (BPS, Central Bureau of Statistics). *Indirect taxes* includes central government administered taxes (sales taxes, building and land tax (*PBB*)), fees for business permits, road use tax (*SWP3D*), import duties, custom fees, and other levies (not including income and personal taxes). *Local democracy period* is from Martinez-Bravo and Stegmann (2018). All other variables are from the Indonesian Census of Manufacturing.

Table A4: Summary statistics: SOEs (controls and 1999 and 2001 splits)

<b>Panel A:</b> Private plants and SOEs	N	mean	s.d.	min	max
Dummy =1 if plant >50% SOE before 1999	129,589	0.07	0.26	0.00	1.00
Dummy =1 if plant >100% SOE before 1999	129,589	0.07	0.25	0.00	1.00
<b>Panel B:</b> SOEs (>50% before 1999)	N	mean	s.d.	min	max
Dummy =1 if plant >50% Central gov. SOE before 1999	11,963	0.52	0.50	0.00	1.00
Dummy =1 if plant >50% Region gov. SOE before 1999	11,963	0.35	0.48	0.00	1.00
Dummy =1 in year of split	11,963	0.01	0.10	0.00	1.00
Dummy =1 in year of split and after	11,963	0.05	0.23	0.00	1.00
Investment (I/K)	8,280	0.30	0.74	-4.34	4.98
Indirect taxes over value added	11,955	0.04	0.17	0.00	4.77
log K/L	8,600	9.96	1.82	-6.24	18.95
log Employment	11,963	4.99	1.42	3.00	10.62
log Output goods	11,354	15.57	2.28	6.96	23.52
log Output	11,962	15.58	2.27	8.21	23.52
log Donations	7,569	8.39	2.24	2.36	18.47
Output/K	7,895	2.62	3.20	0.00	16.67
Cashflow/K	7,867	0.71	0.95	-5.00	4.99
Dummy =1 if local democracy period	11,963	0.36	0.48	0.00	1.00

*Notes:*

# Online Appendix

## Adjusting to Government Fragmentation: Investment Dynamics and Beyond

*Beata Javorcik and Steven Poelhekke*

March 19, 2025

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## **OA1 Analysis of district revenue sources**

In this Section we analyse the effect of district splits on sources of revenue and on expenditure at the district level.

### **OA1.1 Data on district revenue sources**

Information on district revenue by source comes from INDO-DAPOER. These include the Special and the General Allocation Grants (DAK and DAU) which are direct transfers from the national government, natural resource revenue, own-source revenue, other revenue, and national tax revenue sharing. The DAK are earmarked transfers such as for health and education infrastructure and has been growing after decentralization. The DAU are much larger and give full freedom to local government spending. These are based on a formula including population, area, 'geographical circumstances', and poverty. In the 2001 formula, each of these had equal weight, while in 2002 population and area received higher weights. After decentralization, the DAU included a lump-sum amount, thus creating incentives for each region to split up (Hofman and Kaiser, 2004). Own source revenue includes local taxes, user charges, receipts from license fees, and state-owned enterprises (including water utility companies, PDAMs). These include taxes on electricity, charges for health services provided by local public clinics (Puskesmas), issuance of building permits and public market fees. Each of taxes, charges, and others contribute roughly one third of total own-source revenues. Other revenue consists of other minor transfers from the central government, transfer from the province, transfers from other regions, emergency funds, and non-specified others (The World Bank, 2008, p.153).

Table OA1: Summary statistics: districts

District revenue and source shares (1994-2009)	N	mean	s.d.	min	max
DAK (Special Allocation Grant)	5,105	0.15	0.15	0.00	0.59
DAU (General Allocation Grant)	5,166	0.57	0.20	0.03	0.89
NRRV (Natural Resource Revenue Sharing)	5,060	0.04	0.09	0.00	0.61
OSRV (Own Source Revenue)	5,184	0.08	0.07	0.01	0.60
OTHR (Other Revenue)	5,184	0.05	0.05	0.00	0.26
TXRV (Tax Revenue Sharing)	5,137	0.10	0.05	0.02	0.38
District expenditure share (2001-2009)	N	mean	s.d.	min	max
STAF (Personnel)	2,977	0.46	0.14	0.08	0.78
CAP (Capital)	2,958	0.23	0.12	0.04	0.74
GSR (Goods and services)	2,963	0.19	0.06	0.07	0.38
OTHR (Others)	2,944	0.08	0.05	0.01	0.39
ADMN (General administration)	3,166	0.33	0.13	0.12	0.96
AGR (Agriculture)	3,090	0.04	0.02	0.00	0.12
ECON (Economy)	3,087	0.02	0.01	0.00	0.10
EDU (Education)	3,111	0.30	0.12	0.02	0.57
ENVR (Environment)	2,811	0.02	0.02	0.00	0.11
HE (Health)	3,104	0.08	0.03	0.01	0.18
HOUS (Housing and public facilities)	2,120	0.03	0.04	0.00	0.20
INFR (Infrastructure)	3,093	0.15	0.08	0.01	0.48
PROT (Social protection)	2,672	0.01	0.01	0.00	0.04
PUBL (Public, law and order)	1,915	0.01	0.01	0.00	0.05
RELG (Religions)	690	0.01	0.01	0.00	0.03
TOUR (Tourism and culture)	2,706	0.01	0.00	0.00	0.03

*Notes:* All shares have been trimmed at the top and bottom percentile.

## OA1.2 Estimating equation

In Appendix Table OA2 we report the results of the following specification, where we regress revenue sources as a share of total revenue, on district split event dummies:

$$DistrictOutcome_{it} = \beta_s DistrictSplit_{it} + \alpha_i + \mu_t + \epsilon_{it} \quad (4)$$

where *DistrictOutcome* denotes various outcomes of interest for district *i* observed in year *t* (with *t* ranging from 1989 to 2009). The year of the split (*DistrictSplit*) is a dummy equal to one from the year of the split onward. The sample includes all parent and child districts as well as districts that never split. The  $\alpha_i$  are district fixed effects, which, depending on the specification, can refer to the initial 1989 district fixed effects or alternatively to both parent and child fixed effects as they are created over time.

## OA1.3 Findings

The results in Table OA2 are consistent with the survey evidence documenting proliferation of taxes in the aftermath of decentralization in Indonesia and suggest that these could be a source of uncertainty during and after district splits.

In columns 1 to 6, we change the dependent variable in each column to a different source of revenue, distinguishing between the Special and the General Allocation Grants (DAK and DAU), natural resource revenue, own-source revenue, other revenue, and tax revenue sharing. We find a clear increase in natural resource revenues that is consistent with the new fiscal redistribution rules, which were implemented with decentralization. Moreover, the own source revenue component starts to make up a larger share of revenue. The share increases by 3.4%-points, when compared to a district that does not split.<sup>32</sup>

Table OA3 focuses on expenditures (as a share of revenue). However, the sample is small as these data are only available from 2000 and there is a break in the data after 2003. Keeping these caveats in mind and noting that this is a short period, we find a relative increase in spending on personnel and general administration and a reduction in the year of the split. This

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<sup>32</sup> In Table OA4 we show that the effect of own source revenue was also present in splits that precede decentralization Law 34/2000. However, the DAK would still increase in importance after splits while natural resource revenue did not change.



Table OA2: District-level revenue sources

Dependent variable →	Revenue source as a share of total revenue					
	DAK (Special Allocation Grant)	DAU (General Allocation Grant)	NRRV (Natural Resource Revenue Sharing)	OSRV (Own Source Revenue)	OTHR (Other Revenue)	TXRV (Tax Revenue Sharing)
	[1]	[2]	[3]	[4]	[5]	[6]
Year of split and after	-0.019** (0.009)	-0.039* (0.021)	0.034** (0.013)	0.037*** (0.005)	-0.005* (0.003)	-0.004 (0.006)
Local democracy period	0.003 (0.007)	-0.006 (0.009)	-0.001 (0.005)	-0.000 (0.003)	0.006** (0.003)	-0.002 (0.003)
District, and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations (district-years)	5,087	5,148	5,041	5,169	5,168	5,119
R-squared	0.823	0.788	0.680	0.770	0.644	0.636
1999 AND 2001 SPLITS:						
Year of split and after	0.001 (0.018)	-0.098** (0.041)	0.072** (0.036)	0.040*** (0.008)	0.002 (0.010)	0.003 (0.007)
Local democracy period	0.004 (0.007)	-0.001 (0.010)	-0.005 (0.005)	-0.002 (0.003)	0.006* (0.003)	-0.002 (0.003)
District, and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations (district-years)	4,013	4,060	3,990	4,077	4,075	4,042
R-squared	0.824	0.801	0.711	0.769	0.647	0.658

Notes:

is suggestive of splits being costly in terms of restructuring or building up a new government. Moreover, and consistent with the discussion of the results on donations, we find a small reduction in spending on law and order.

In summary, splitting districts increase administrative expenditure and personnel costs. They attempted to compensate for these developments by levying new local taxes, thus potentially increasing uncertainty and compliance burden. The Survey of Regional Investment Attractiveness carried out in 2004 by the Regional Autonomy Watch (Komite Pemantauan Pelaksanaan Otonomi Daerah, or KPPOD) found that business owners reported local tax regimes as an important constraint on investment. These constraints arise in the form of compliance costs such as to business licensing, even when the tax or charge itself is moderate. Moreover, it increases the scope for and impact of existing corruption (Kuncoro, 2004; Luebke, 2005), adding to the overall economic harm done by newly established local revenues (Barnes et al., 2005).

Table OA3: District expenditures (years 2001-2002: almost the same if keep 1999,2001,2002 splits, multicol issue if drop 2002 splits)

Dependent variable →	Expenditure as a share of total revenue							
	STAF (Personnel)	CAP (Capital)	GSR (Goods and services)	OTHR (Others)	ADMIN (General administration)	AGR (Agriculture)	ECON (Economy)	EDU (Education)
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Year of split and after	0.043*** (0.014)	-0.026* (0.015)	-0.017 (0.014)	-0.010 (0.013)	0.029 (0.019)	0.008 (0.008)	-0.005 (0.005)	0.002 (0.014)
Observations	516	498	516	512	514	510	496	482
R-squared	0.930	0.841	0.852	0.775	0.746	0.845	0.739	0.882
Clusters	229	224	230	227	228	228	224	215

Dependent variable →	Expenditure as a share of total revenue							
	ENVR (Environment)	HE (Health)	HOUS (Housing and public facilities)	INFRA (Infrastructure)	PROT (Social protection)	PUBL (Public, law and order)	RELIG (Religious)	TOUR (Tourism and culture)
	[9]	[10]	[11]	[12]	[13]	[14]	[15]	[16]
Year of split and after	0.004 (0.005)	-0.003 (0.003)	0.002 (0.009)	-0.034*** (0.012)	-0.002 (0.001)	-0.003*** (0.001)	-0.001 (0.001)	-0.001 (0.001)
Observations	510	512	516	526	476	480	426	488
R-squared	0.784	0.841	0.762	0.728	0.715	0.762	0.836	0.742
Clusters	227	230	230	233	218	217	193	225

Notes: All regressions control for district and year fixed effects, local democracy period, and log population. Clustered by 1989 district.

Table OA4: District revenue before fiscal decentralisation (1994-2000)

Dependent variable →	Revenue source as a share of total revenue					
	DAK (Special Allocation Grant)	DAU (General Allocation Grant)	NRRV (Natural Resource Revenue Sharing)	OSRV (Own Source Revenue)	OTHR (Other Revenue)	TXRV (Tax Revenue Sharing)
	[1]	[2]	[3]	[4]	[5]	[6]
Year of split and after	0.049** (0.025)	-0.089*** (0.032)	0.001 (0.005)	0.023** (0.010)	0.017* (0.009)	0.009 (0.007)
Local democracy period	0.016 (0.013)	-0.021 (0.016)	0.001 (0.004)	0.001 (0.006)	0.004 (0.003)	-0.000 (0.005)
District and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,937	1,955	1,982	1,964	1,982	1,951
R-squared	0.590	0.655	0.616	0.877	0.402	0.759
Clusters	271	270	271	271	271	270

*Notes:*

## OA2 Difference-in-difference decomposition weights

This section performs the decomposition analysis of Goodman-Bacon (2021). He shows that the two-way fixed effects estimator with staggered treatment is a variance-weighted average of all possible two-group/two-period difference-in-difference estimators in the data. It is therefore possible to gauge the weight given within the overall treatment effect on those four estimators. The four estimators compare earlier treated to later controls (e.g. later controls are units that are treated later but can serve as controls before they are treated), later treated to earlier controls, treated to never treated, and treated to already treated (that are treated before the sample starts). For example, if most of the treatment effect is due to comparing earlier treated to later controls, then one needs stronger assumptions about whether later treated units (that are controls until they are treated) are not already on a different trend before being treated. Also, it is preferable to have a low weight on already treated plants (treated before the sample starts) that act as controls for later treated plants because treatment effects may not be stable over time.

In Table OA2 we perform seven decompositions where we progressively change a balanced sub-sample of plants to include those observed 14 years to those observed 7 years from within our baseline estimation sample. Highlighted in bold is the estimator that receives virtually all weight in our average treatment effect in all samples: the treated versus never-treated plants. This suggests that there is little bias from comparing earlier treated versus later controls or later treated versus earlier controls because any differential trend in these timing groups has little influence on the average treatment effect.

Table OA5: Difference-indifference decomposition weights

Plant years observed →	14	13	12	11	10	9	8	7
Earlier T vs. Later C	0.003	0.003	0.005	0.006	0.007	0.009	0.010	0.012
	[-0.012]	[0.027]	[-0.123]	[-0.181]	[-0.106]	[-0.207]	[-0.165]	[-0.089]
Later T vs. Earlier C	0.010	0.006	0.013	0.009	0.010	0.016	0.014	0.016
	[0.053]	[0.073]	[0.213]	[0.334]	[0.048]	[0.092]	[0.068]	[0.049]
<b>T vs. Never treated</b>	<b>0.941</b>	<b>0.944</b>	<b>0.952</b>	<b>0.956</b>	<b>0.954</b>	<b>0.952</b>	<b>0.950</b>	<b>0.951</b>
	<b>[-0.069]</b>	<b>[-0.099]</b>	<b>[-0.130]</b>	<b>[-0.186]</b>	<b>[-0.206]</b>	<b>[-0.268]</b>	<b>[-0.286]</b>	<b>[-0.286]</b>
T vs. Already treated	0.046	0.046	0.029	0.030	0.029	0.023	0.025	0.022
	[0.013]	[0.021]	[-0.063]	[-0.012]	[0.052]	[0.021]	[-0.078]	[-0.093]
Sample years from	1992	1993	1994	1995	1996	1997	1998	1999
Observations	11,606	12,077	12,732	13,970	15,190	16,866	17,152	17,136
Plants	829	929	1,061	1,270	1,519	1,874	2,144	2,488

*Notes:* This table shows weights and average d-i-d estimates in square brackets for balanced sub-samples that vary according to how many periods each plant is observed in the data. For example, the first column selects, starting from 1992, all plants that are observed 14 periods each, which is the maximum in our data. The third column selects all plants that are observed 12 periods each, within the years 1995-2006, and thus also includes plants that are observed 14 years in the whole dataset. In all samples, virtually the entire treatment effect is due to comparisons between treated and never treated plants. Estimates performed with BACONDECOMP (version 1.0.5 16sep2022). (Goodman-Bacon et al., 2019; Goodman-Bacon, 2021).

### OA3 Further robustness tests of split effect on investment

This section presents additional robustness tests of the main effect of district splits on investment.

In columns 1 and 2, we allow for the fact that districts may split more than once over time, and include separate dummies for secondary splits. This is important because, arguably, the first instance of political uncertainty due to district splits may be more unanticipated than subsequent ones. The results show that in the sample that includes SOEs and private plants (column 1) and in the sample that drop SOEs (column 2) the first split results in significant reduction of investment, while a secondary split does not have a significant compounding effect. As column 3 shows, leaving secondary splits in the sample does not lead to different conclusions.

In column 4 we add an interaction between the post-split dummy and an indicator of whether the plant is located in the new ‘child’ district (which has a new district capital). We find no evidence that such plants are any worse off than plants that experience a split but are located in the new post-split district that keeps the parent district capital city.

Columns 5 to 7 control for variables that potentially predict district splits, such as natural resource wealth and population trends. Natural resource wealth at the level of initial districts are a fixed effect, but their value may increase over time. Motivated by Fitriani et al. (2005) and using data from Pelzl and Poelhekke (2021), we include interactions of initial resource wealth with changes in an index of relevant world mineral prices. None of these affects the main results.

Finally, in the last three columns we allow for the possibility that both the INDO-DAPOER database and the Master File Kabupaten of the BPS misreported the timing of splits and/or if the timing refers to approval rather than implementation. Although many districts report separate revenue data from the year of split as thus far defined, some report individual revenue only one or more years later, although this improves over time. In fact, district revenue is missing in 48% of split years, while for the 2007 splits district revenue is missing in only 21% of splits. Therefore, columns 9-10 use as timing of the split the first year in which a new district’s budgets is recorded in INDO-DAPOER. The two versions allow for a gap of one

or two to three years between INDO-DAPOER's walkthrough and the first recorded budget. The estimates are robust to this exercise, despite the potential measurement error.

Table OA6: Robustness of firm-level analysis (1999 and 2001 splits)

Dependent variable →	I/K							
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Separate second split	Second splits incl.	Split years 1999 and 2001 versus never split	using first district revenue availability as delayed split timing up to $t$ years later	1 year	2-3 years		
Year of split and after	-0.087** (0.035)	-0.116*** (0.035)	-0.125*** (0.036)	-0.123*** (0.040)	-0.139*** (0.034)	-0.175*** (0.044)	-0.135*** (0.033)	-0.138*** (0.034)
Year of second split and after	-0.175 (0.245)	-0.316 (0.295)						
Year of split and after * child district				-0.010 (0.090)				
Local democracy period	0.035** (0.016)	0.030* (0.016)	0.031* (0.016)	0.031* (0.016)	0.027* (0.014)	0.023 (0.017)	0.031* (0.016)	0.030* (0.016)
Resources 1990 * ΔWPI					-0.049 (0.047)	-0.059 (0.053)		
Resources 1990 * ΔWPI t-1					-0.071 (0.062)	-0.062 (0.062)		
Oil 1990 * ΔWPI					-0.006*** (0.002)	-0.031* (0.017)		
Oil 1990 * ΔWPI t-1					0.002 (0.002)	-0.076*** (0.025)		
log population						0.008 (0.042)		
log population t-1						0.005 (0.041)		
log population t-2						-0.021 (0.050)		
Excluding SOEs	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Excluding second splits	No	No	No	No	Yes	Yes	Yes	Yes
Observations	151,643	118,036	118,036	118,036	108,355	90,581	117,532	117,522
R-squared	0.469	0.481	0.481	0.481	0.501	0.519	0.483	0.483
Clusters	316	303	303	303	298	281	300	300

Notes:  $WPI$  is world price index. All regressions control for plant and sector-year fixed effects. \*\*\*Significant at 1% level; \*\*Significant at 5% level; \*Significant at 10% level. Standard errors clustered on plant, sector-year, and pre-split district.



Table OA7: Robustness of firm-level analysis (all splits)

Dependent variable →	I/K								
	Separate second split			Second splits incl.			using first district revenue availability as delayed split timing up to $t$ years later		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Year of split and after	-0.056** (0.025)	-0.069*** (0.027)	-0.076*** (0.027)	-0.080** (0.031)	-0.097*** (0.027)	-0.120*** (0.032)	-0.088*** (0.026)	-0.088*** (0.026)	-0.087*** (0.026)
Year of second split and after	-0.218 (0.212)	-0.366 (0.255)							
Year of split and after * child district				0.009 (0.058)					
Local democracy period	0.043*** (0.015)	0.036** (0.016)	0.037** (0.016)	0.036** (0.016)	0.034** (0.016)	0.031* (0.016)	0.037** (0.016)	0.037** (0.016)	0.037** (0.016)
Resources 1990 * ΔWPI					-0.047 (0.046)	-0.056 (0.052)			
Resources 1990 * ΔWPI t-1					-0.071 (0.061)	-0.063 (0.063)			
Oil 1990 * ΔWPI					-0.004*** (0.002)	-0.035** (0.017)			
Oil 1990 * ΔWPI t-1					0.001 (0.001)	-0.076*** (0.026)			
log population						-0.009 (0.038)			
log population t-1						-0.015 (0.052)			
log population t-2						0.040 (0.057)			
Excluding SOEs	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Excluding second splits	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Observations	163,005	125,380	125,380	124,821	114,661	95,908	124,705	124,625	124,619
R-squared	0.466	0.476	0.476	0.478	0.497	0.515	0.478	0.478	0.478
Clusters	340	318	318	315	310	290	314	314	314

Notes: WPI is world price index. All regressions control for plant and sector-year fixed effects. \*\*\*Significant at 1% level; \*\*Significant at 5% level; \*Significant at 10% level. Standard errors clustered on plant, sector-year, and pre-split district.

## OA4 Event-study regressions

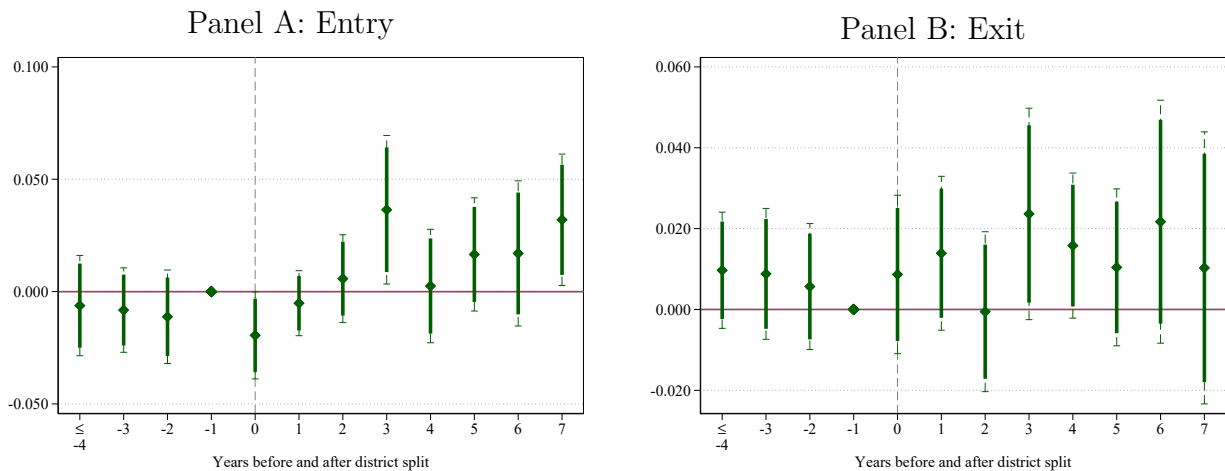
Table OA8: Regressions underlying Figure 4 (1999 and 2001 district splits)

Dependent variable →	I/K	Indirect taxes over value added	log K/L	log employment	log output goods	log output	log donations
Four or more years before split	0.042 (0.027)	-0.027** (0.012)	0.032 (0.061)	0.044 (0.027)	0.020 (0.067)	0.000 (0.058)	-0.021 (0.070)
Three years before split	0.066* (0.040)	-0.024 (0.021)	-0.010 (0.057)	0.012 (0.025)	-0.058* (0.030)	-0.051* (0.028)	-0.069 (0.077)
Two years before split	0.031 (0.038)	-0.026 (0.016)	-0.031 (0.044)	0.006 (0.014)	-0.013 (0.030)	-0.012 (0.021)	0.071 (0.078)
One year before split							
Year of split	-0.028 (0.034)	0.007 (0.015)	-0.114** (0.047)	0.038** (0.015)	-0.003 (0.029)	0.017 (0.033)	0.050 (0.059)
One year after split	-0.132** (0.055)	-0.002 (0.015)	-0.102* (0.057)	0.060*** (0.017)	0.027 (0.027)	0.088*** (0.030)	0.106* (0.056)
Two years after split	-0.154*** (0.046)	-0.011 (0.010)	-0.136* (0.073)	0.051*** (0.013)	-0.010 (0.046)	0.041 (0.055)	0.097 (0.078)
Three years after split	-0.146** (0.063)	-0.017 (0.023)	-0.090* (0.047)	0.072*** (0.016)	0.050 (0.054)	0.110 (0.070)	0.151* (0.087)
Four years after split	-0.173*** (0.057)	-0.003 (0.018)	-0.140** (0.067)	0.054** (0.025)	-0.053 (0.056)	-0.033 (0.059)	0.090 (0.104)
Five years after split	-0.252*** (0.072)	-0.032 (0.020)	-0.056 (0.070)	0.036 (0.031)	-0.068 (0.086)	-0.046 (0.083)	-0.200** (0.094)
Six years after split	-0.136 (0.112)	-0.018 (0.030)	0.028 (0.114)	0.029 (0.045)	0.154 (0.150)	0.170 (0.141)	-0.095 (0.207)
Seven years after split	-0.142 (0.121)	-0.019 (0.029)	-0.141 (0.128)	0.088* (0.052)	0.121 (0.135)	0.137 (0.144)	-0.209 (0.260)
Observations	59,794	59,774	59,794	59,794	56,535	59,794	50,319
R-squared	0.430	0.377	0.767	0.934	0.918	0.922	0.783
Districts	217	217	217	217	217	217	215

*Notes:* All regressions control for plant and sector-year fixed effects. Sample years include 1993 to 2006 and thus excludes splits that occur after the moratorium of 2004-2006. *Indirect taxes* includes central government administered taxes (sales taxes, building and land tax (*PBB*)), fees for business permits, road use tax (*SWP3D*), import duties, custom fees, and other levies (not including income and personal taxes). \*\*\*Significant at 1% level; \*\*Significant at 5% level; \*Significant at 10% level. Standard errors clustered on pre-split district, plant and sector-year.

## OA5 Entry and exit

Figure OA1: Entry and exit as a share of all private plants at the sector-district-year level (1999 and 2001 district splits)



*Notes:* This figure shows the effect of district splits on the 2-digit sector-by-district-by-year rate of entry and exit of individual plants. The unit of observation is a 2-digit sector-district-year. The controls include: an indicator for the post-democratization period, 1989 district and sector-times-year fixed effects. Thick spikes depict 90% confidence intervals, while the thin caps depict 95% confidence intervals, clustered by 1989 districts.

## OA6 Predicting district splits

In this Section, we use the district-level data to test whether splits could be foreseen. We build on Fitrani et al. (2005) who examine in a cross-sectional setting factors that contributed to splitting of districts. Except for the surface area, they find few robust results. When they consider natural resource wealth, which due to the fiscal changes implied a larger share of natural resource revenues accruing to a local government after 2001, they find some indication of a positive effect on 1999 splits and a negative effect on 2001-2003 splits, thus suggesting a zero average effect.

In Table OA9, we find that only surface area and population size robustly predict that a district that existed in 1991 (column 1) or in 1998 (columns 2) will subsequently split. We control for surface area through fixed effects in our main regression, and we show in our robustness checks that controlling for the population size does not affect our findings (see Appendix OA3. More importantly, we find that neither natural resources, nor manufacturing employment, nor the share of plants with positive investment can predict district splits in our sample.

Table OA9: Predicting splits

Dependent variable →	Dummy =1 if a district splits in:	
	<i>All districts as of 1991</i>	<i>All districts as of 1998</i>
<i>Sample →</i>	<i>1991 to 2006</i>	<i>1999 or 2001</i>
<i>Splits that happen in years →</i>	[1]	[2]
Non-oil mineral natural resources, 1990	-0.030 (0.028)	-0.002 (0.019)
Oil natural resources, 1990	0.007 (0.005)	-0.003 (0.003)
log surface area	0.191*** (0.013)	0.068*** (0.011)
log manufacturing employment	-0.000 (0.016)	0.016 (0.011)
% of plants with positive investment	0.018 (0.071)	-0.032 (0.044)
log population	-0.134*** (0.035)	-0.049** (0.021)
Observations	240	253
R-squared	0.440	0.168

*Notes:* Column 1 is a cross-section of districts that exist in 1991, while columns 2 is a cross-section of districts that exist in 1998. The dependent variable is a dummy equal to 1 if one of these districts splits: in any year post 1991 (column 1); in 1999 or in 2001 (column 7). Splits in years 2007 and later are excluded, but their inclusion does not change results (not shown for brevity). \*\*\*Significant at 1% level; \*\*Significant at 5% level; \*Significant at 10% level. Standard errors clustered on 1989 districts.

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