Navigating Uncertainty: Investment Dynamics and Beyond*

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April 6, 2023

Abstract

Countries have been increasingly decentralizing and devolving powers to lower levels of government in the hope of improving services delivery. Yet proliferation of governments, particularly in a developing country setting, may create fiscal and policy uncertainty and increase the tax and compliance burden for private businesses, with potentially detrimental effects for investment. This hypothesis is tested in the context of Indonesia, which has increased the number of districts from 284 in 1989 to 511 by 2014. The data show that districts that split received fewer earmarked transfers from the national government, increased the share of own-source revenue and cut back on public investment to pay for the burden of self-administration. Plants operating in the splitting districts responded by reducing investment, and (because demand was not affected) by increasing employment, which is in line with hiring being less costly to reverse than fixed asset purchases. The results also show an increase in the plant-level tax burden and 'donations', as well as a persistent decline in the capital-labor ratio. In contrast to private plants, state-owned establishments did not register a drop in investment or an increase in the tax burden.

JEL Codes: F2, P16, H11, H00
Keywords: policy uncertainty, investment, decentralization

*We thank Massimiliano Cali, Devaki Ghose, Peter Phillips, seminar participants at Monash University, EBRD, University of Auckland, Victoria University Wellington, as well as conference participants at the Empirical Investigations in International Trade 2020 workshop, the New Zealand Association of Economists 2021, the LACEA LAMES annual meeting 2021, and the 2022 NOVAFRICA Conference on Economic Development for helpful comments. All errors are our own. 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.
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1 Introduction

Although there is little doubt that uncertainty is detrimental to economic activity, capturing uncertainty has remained elusive. That is because shocks causing uncertainty typically bring about an economic downturn making it virtually impossible to separately assess the impact of uncertainty (Bloom et al., 2007). Recent events, such as Brexit, the Covid-19 pandemic and Russia's war on Ukraine have led to renewed interest in the topic and stimulated research proposing new ways of capturing uncertainty, such as an index based on the occurrence of keywords related to policy uncertainty in newspaper articles (Baker et al., 2016), recurring election events (Julio and Yook, 2012), analysis of earning calls conducted by publicly traded firms (Hassan et al., 2019), or novel instrumental variables (Alfaro et al., 2022), each with its own advantages and downsides. The literature on uncertainty has focused almost exclusively on advanced economies. And yet, the implications of uncertainty may be felt more acutely in developing economies, where less sophisticated management techniques may make the adjustment process more difficult.[1]

This paper contributes to our understanding of the effects of uncertainty by considering a clean and plausibly exogenous policy shock, namely the rapid process of government decentralisation in Indonesia, which took place after the sudden fall of President Suharto in 1998 and the subsequent lifting of the presidential veto over district splits. As a result, the number of Indonesia districts increased from 284 to 511 between 1989 and 2014. The decentralisation process was associated with increased costs of running new administrations and a decline in central government transfers, thus creating huge uncertainty about the future taxation policy 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 implications of uncertainty 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 uncertainty over time.[2]

We set the stage for our analysis by demonstrating that Indonesia's decentralisation had

[1] Across countries management scores tend to closely track levels of economic development (Scur et al., 2021).

[2] Claims about the plausibly exogenous nature of Indonesian decentralisation have been made by Burgess et al. (2012), Alesina et al. (2019), and Bazzi and Gudgeon (2021).

a profound impact on local budgets. The detailed information on public finances shows that splitting districts received fewer earmarked transfers (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 decentralisation is associated with fiscal uncertainty and may increase the tax burden on private businesses, in addition to creating uncertainty about other aspects of local policies.

Our core 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 1990-2009. 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 variables, including employment, donations, taxes paid, etc.

Our main focus is on the investment response of individual plants. Under irreversibility of capital projects, investment is expected to react negatively to uncertainty. Irreversibility in combination with uncertainty leads to a positive option value of delaying investment until more information arrives (Bernanke, 1983). This mechanisms relies on plants facing high costs in adjusting and reversing investment, resulting in periods with no investment followed by positive bursts.[3,4]

We also examine whether 'donations' are a potential mitigating factor in reducing uncertainty, as donations could be in reality thinly veiled bribes to corrupt local politicians (Transparency International, 2018). Since we find that output is unaffected, we gauge whether plants

[3] 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).

[4] 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 U.S. (Javorcik and Poelhekke, 2017), thus amplifying the potential effect of uncertainty.

simultaneously reduce capital investment and switch towards more labor-intensive production methods. Given the likely high adjustment costs of fixed assets and irreversibility of physical investment, it may be less costly and less risky to adjust the size of the labor force.

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.[5] 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). 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 observed first in the year after the split takes place, persists for seven years and then disappears.

Several extensions boost our confidence that these findings capture the response of investment to increased uncertainty. First, we posit that by the virtue of their government ownership SOEs are not subject to the same kind of uncertainty that afflicts private establishments. We test this hypothesis by allowing for a differential investment response of private and state-owned establishments in the post-split years. We define SOEs based on their state ownership status before 1999, i.e., prior to the first district split considered in our sample to abstract from the possibility of state ownership being affected by decentralisation. We consider four thresholds of state ownership share: above 10%, above 20%, above 50% as well as 100% state ownership. All four approaches produce consistent results. While in the post-split period, private establishments *reduce* their investment rate on average by 6-9 percentage points, SOEs *increase* their investment rate by about 10-14 percentage points. As one would expect, this effect is driven primarily by SOEs owned by the central government. SOEs belonging to regional governments reduce their investments as much as private establishments do. This is intuitive, given the decline in transfers regional governments obtained from the

[5] 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.

central government and the general decline in public investment in the splitting districts we document in the paper.

Second, 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. Third, an increase in 'donations' is observed among establishment operating in the splitting districts, which is consistent with businesses trying to lobby or hedge against adverse actions of regional governments through political donations or bribes. Fourth, we observe an increase in employment and a decline in the capital-labor ratio. This is suggestive of businesses dealing with uncertainty by substituting labor for capital in order to avoid expenditures that are hard to reverse.[6] Fifth, we find no statistically significant impact of district splits on other variables, such as output, exports, imports, share of output exported, reliance on imported inputs, which is also intuitive as international trade is unlikely to be affected by a change in local policies.

Finally, we conduct event studies on these additional variables and show the transitory nature of the shock. We also find a temporary decline in the total factor productivity. The impact on capital intensity is an exception: the decline in the capital-labor ratio seems to persist over the full time horizon considered in the study. This is not surprising, given that the negative impact on investment is observed for seven years.

Our paper contributes to the fast growing literature on firm responses to uncertainty. This literature explored uncertainty associated with business cycles (Bernanke, 1983; Bloom et al., 2007, 2018), national elections (Julio and Yook, 2012), as well as general political uncertainty (Hassan et al., 2019), general economic uncertainty (Handley and Limão, 2015), uncertainty related to regulatory changes (Gulen and Ion, 2016) and uncertainty related to trade policy changes (Handley and Limão, 2015; Hassan et al., 2021). Our contribution lies in demonstrating the implications of an increasingly common source of uncertainty, namely,

[6] This pattern is consistent with the findings of Fetzer 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.

fiscal decentralisation.[7] It also lies in investigating the implications of uncertainty in a developing country setting. As argued earlier, less sophisticated management techniques prevalent in developing countries may increase the adjustment costs of capital, thus exacerbating the impact of uncertainty. The magnitude of our effect is large: about twice the effect of uncertainty around national elections (Julio and Yook, 2012); similar to 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., 2021). Moreover, where these studies include publicly listed firms in their sample, we are able to study the behavior of non-listed establishments, including small and medium-sized ones as well as state-owned enterprises.

We also contribute to the literature on the causes of and reasons for decentralization (Arzaghi and Henderson, 2005), and the effects on public service provision and democratisation as surveyed by Mookherjee (2015). We show that the decentralization process itself is associated with economic costs by creating uncertainty. As this uncertainty has a detrimental effect on investment, a slow down in structural transformation may be an unintended consequence of decentralisation.

The rest of the paper is structured as follows. The next section sets the scene for our analysis. Section 3 discusses the data sources, while Section 4 explains the empirical strategy. All the results are presented in Section 5. 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 decentralisation was associated with a decline in transfers from

[7] Developed and developing countries have increasingly decentralized and devolved powers to lower levels of government in the hope of improving services delivery. The examples include the creation of regional parliaments in the UK, increasing the number of municipalities from 3,974 in 1980 to 5,560 in 2000 in Brazil, increasing the number of states from 22 to 37 between 1990 to 2010 in Nigeria, Uganda increasing the number of districts from 34 to 112, Kenya from 47 to 70, and Vietnam increasing the number of provinces from 40 to 64 between 1996 and 2003 (Grossman and Lewis, 2014). A typical motivation is the theory of fiscal federalism, which prescribes that a public function should be performed at the lowest level of government where such functions are still effective within their jurisdictions (Musgrave, 1959; Oates, 1972).

the central government to the post-split districts, proliferation of district-level taxes and deterioration in institutional quality.

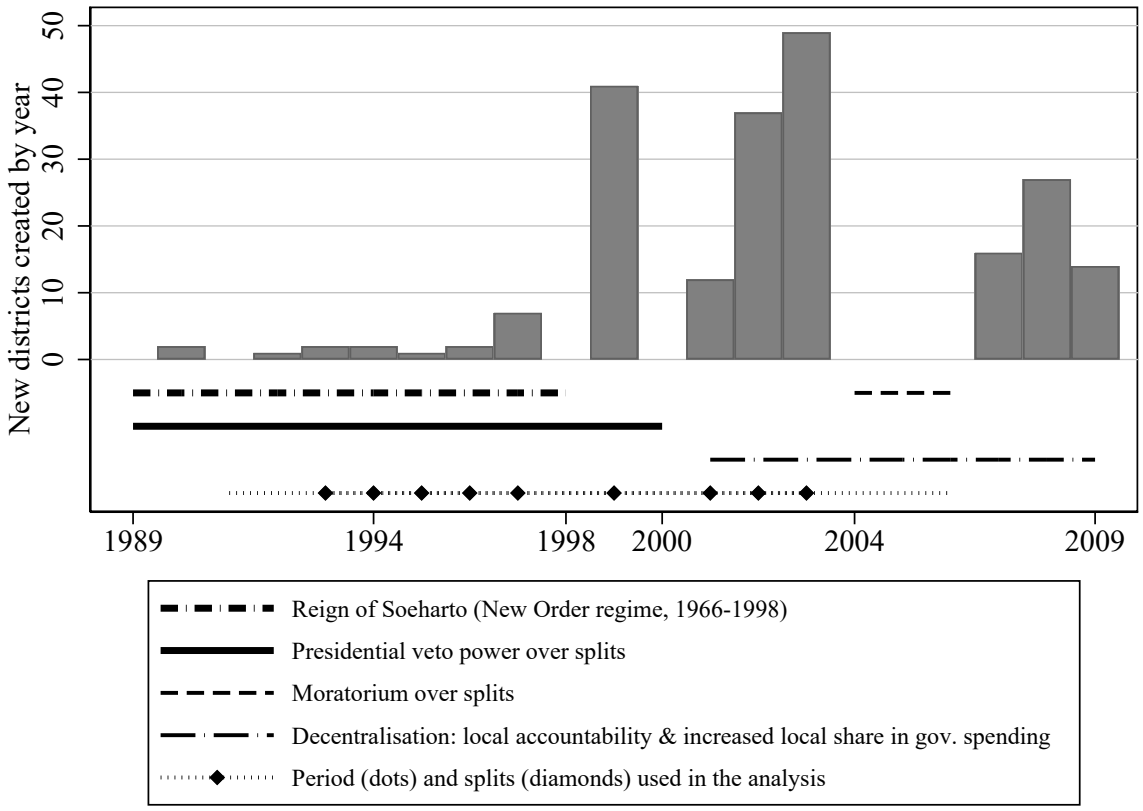
2.1 District proliferation in Indonesia

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 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 (Fitrani et al., 2005; Burgess et al., 2012; Bazzi and Gudgeon, 2021).

A second wave of decentralisation 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.

As marked in the figure, our analysis is based on district splits taking place between 1993 and 2003. This is driven by the need to observe plants for the sufficient length of time prior to the first split and after the last split. As argued above, unexpected political developments that drove the 'big bang' decentralization between 1999 and 2001 made district splits plausibly exogenous, and our findings are robust to focusing on just splits taking place during that period.

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.

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., 2017) and happened independently of the timing of district splits.[8]

2.3 Proliferation of taxes

Decentralisation 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, with little revenue potential, but high costs to taxpayers and the economy. This 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 could add taxes through regional regulations approved by the regional government council with in practice limited national supervision (Brodjonegoro, 2004).[9]

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 new taxes and charges: up to 1,000 in the year 2001 alone, 60% of which were not submitted for a national review.[10] Of the minority that was reviewed by the national government and covering two-thirds of districts, 40% applied directly to the primary

[8] 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.

[9] However, the main candidate for local taxation, the property tax, remained under the jurisdiction of the national government (Brodjonegoro, 2004).

[10] These were therefore implemented illegally, partly on purpose, because if the national government does not invalidate a local law submitted to it within 60 days it comes into force and the national government loses its power of annulment (Butt, 2015).

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 decentralisation laws were passed in September 2004 in order to strengthen central government control over local officials and budgets (Soesastro and Atje, 2005).

We conduct an econometric analysis examining the relationship between tax revenues 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 post-split period is associated with a 11% drop in the district-level tax revenue, after controlling for the population size. This is primarily driven by a decline in the share of direct national government transfers, namely the General Allocation Grant (DAU) and the Special Allocation Grant (DAK), in the total revenue. The share of the former in the total district-level revenue drops by 5.5%, while the latter declines by 1.6%. This decline is accompanied by the increasing share of district own source revenue, which goes up by 3.5% as well an increase in the revenue from the natural resource revenue sharing scheme (4.6%).

An event study, which follows the Sun and Abraham (2021) methodology and whose results are presented in Figure 2, illustrates a large drop (on the order of 25%) in the total district revenue in the year immediately following a split. This drop becomes smaller in the two subsequent years before disappearing in the fourth year after a split. There is no evidence of differential pre-trends between splitting and non-splitting districts.

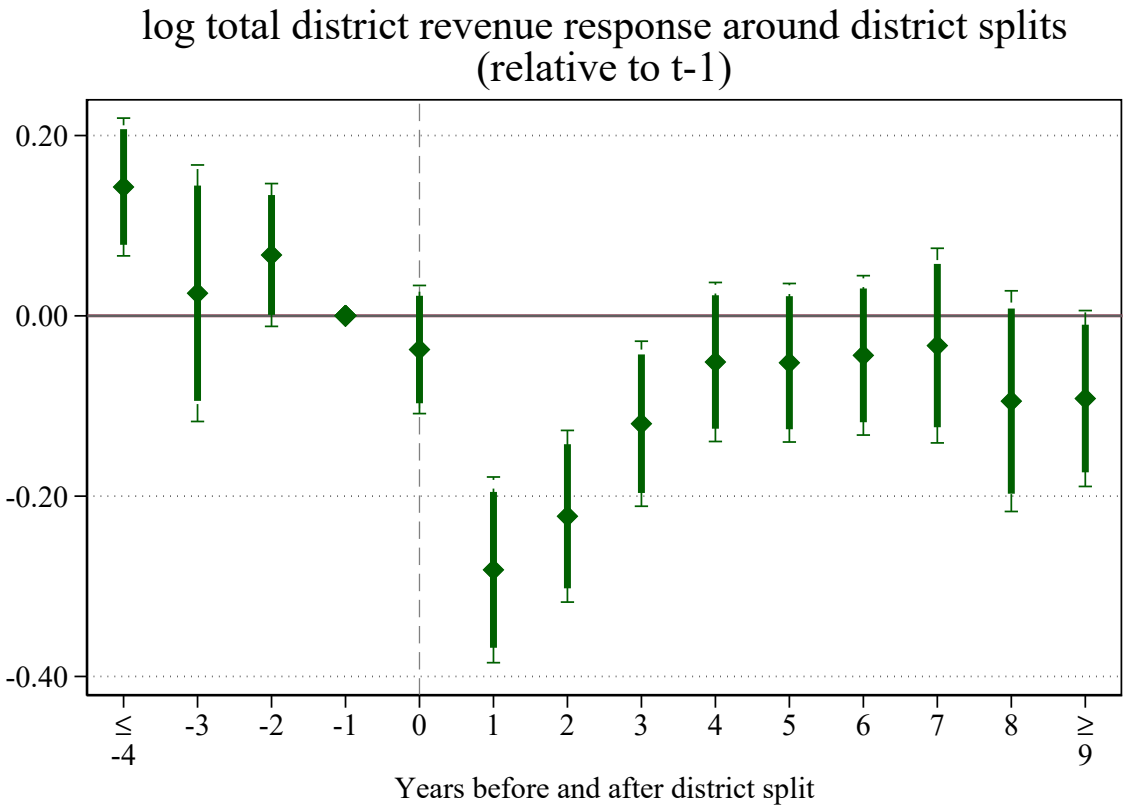
What are the implications of this substantial (even if temporary) decline in revenues? As illustrated in Online Appendix Table OA2, post-split districts spent a higher share of their tax revenue on general administration (with the difference of 4.1% relative to non-splitting districts) and personnel (2.7%), while decreasing spending on public law and order and health.

In summary, splitting districts experienced a simultaneous decline in total revenues and an increase in administrative expenditure and personnel costs. They attempted to compensate for these developments by levying new local taxes, thus potentially increasing uncertainty.

This increase in uncertainty is visible in survey data. 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 arose in the form of compliance costs, such as business licensing, even when the tax or charge itself was moderate. 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). 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 sources (Barnes et al., 2005).

Figure 2: District revenue and splits



Notes: Thick spikes depict 90% confidence intervals, while the thin caps depict 95% confidence intervals.

2.4 Deteriorating institutional quality

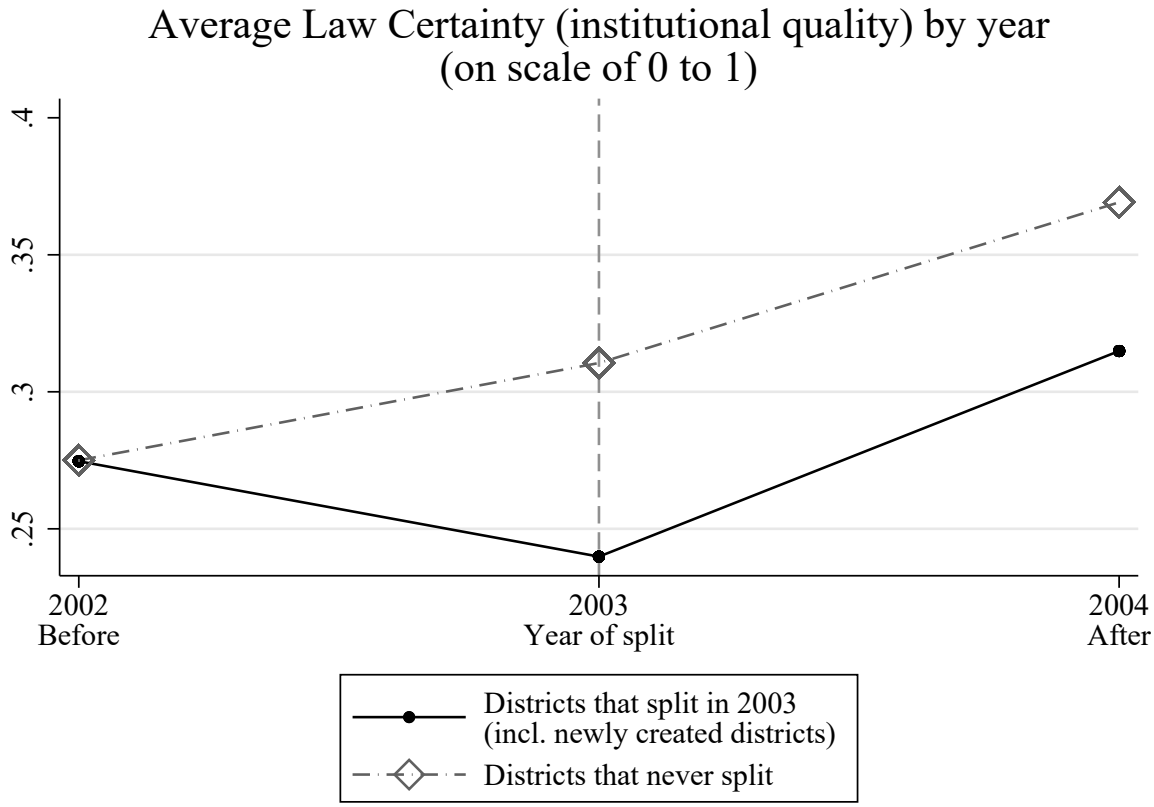
In this subsection, we present some evidence supporting the view that district splits have led to greater uncertainty due to deterioration in the business environment.

Regional Autonomy Watch KPPOD (2003) 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).[11] Law Certainty is of particular interest in the context of our study. 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 KKPOD. With this caveat in mind, Figure 3 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 of KKPOD data).

[11] 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 3: Institutions



Notes: This figure shows the average annual institutional quality score across districts according to KKPOD, 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 1990-2009 and containing 392,416 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.[12]

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 decentralisation, 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: 10%, 20%, 50% and 100%, and distinguish between plants owned by the central government and plants owned by the regional government.[13] 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

[12] 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).

[13] 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.

government direct investment in the wake of the Asian financial crisis.[14]

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.[15]

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

Appendix Tables A1 and A2 list all the variables, definitions, and distributions for private

[14] 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.

[15] 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.

and state-owned establishments, respectively.

4 Empirical strategy

4.1 Baseline specification

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

$$PlantOutcome_{jt} = \beta DistrictSplit_{dt} + \gamma X_{jt} + \alpha_j + \nu_{mt} + \epsilon_{jt} \quad (1)$$

where *PlantOutcome* for plant j refer to outcome variables of interest observed for an individual plant j in year t. *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. 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 α_j as well as 4-digit ISIC industry-year fixed effects ν_{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):

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

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. (2021). 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. We perform regressions on this unrestricted sample, but also on a sample where we restrict 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), and Bazzi and Gudgeon (2021). The political events, highlighted in Section 2, suggest that decentralization and the approval and moratorium on splits were not anticipated. In our baseline analysis, we focus on district splits taking place between 1993 and 2003. This choice 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 robustness checks, 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 baseline sample period with a dotted line.

In Online Appendix OA2 we regress dummies for whether a 1989 district ever splits, on observable district characteristics, such as, area, population, natural resources, and manufacturing employment. For a sample of districts that does eventually split, we regress a count variable of the number of years expired before as split happened on similar district-level characteristics. In each case, we take the 1989 districts as point of departure and in a second

set the 1999 set of districts. Conditional on district fixed effects, we find little evidence that splits could be predicted. The only time-varying exception is natural resources in some specifications, but our results are robust to controlling for these (see Section OA4).[16]

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 OA3 we perform a decomposition of the treatment effect to confirm this.

5 Results

5.1 Do manufacturing plants reduce investment after district splits?

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.

The estimation results, presented in Table 1, provide strong evidence in support of our hypothesis that decentralisation is associated with heightened uncertainty which is detrimental to investment. Column 1 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

[16] Our fixed effects also control for the role of ethnicity (Pierskalla, 2016).

plants operating in districts that have split reduced their investment by about 6 percent. In Column 2, we follow the Q-model of investment and additionally control for output-capital and cash-flow-capital ratios.[17] The coefficient of interest remains statistically significant at the one percent level and increases slightly in magnitude. In Column 3, we consider only district splits that took place in 1991 and 2001, i.e., those immediately following the sudden fall of the Suharto regime, thus focusing on a shorter time period and a smaller subsample.[18] The negative effect of district splits remains significant at the one percent level and increases in magnitude to -0.087. In column 4, we exclude SOEs and events where a plant's district splits for the second or more times.[19] The results remain robust. As evident from columns 5, 6 and 7, the results are robust to alternative clustering on current districts, 1989 districts and pre-split districts, respectively.[20] 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.

The magnitude of our effect is larger than the magnitudes found in studies focusing on advanced economies. For instance, it is about twice the effect of uncertainty around national elections (Julio and Yook, 2012); similar to 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., 2021). 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

[17] 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.

[18] The sample in column 3 includes plants in districts that never split, and plants in districts that experience their first split in 1999 or 2001. The sample excludes plants that experience a split in any other year. All plants in this sample experience at most one split.

[19] Specifically, we exclude plant-years from the moment a second split happens onward and the two years preceding that second split.

[20] The specification found in column 7 will constitute the estimation approach when we consider additional outcomes.

establishments, including small and medium-sized ones.

We also find that plants invest more after the staggered 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).

Table 1: District splits and investment

Dependent variable →	I/K							
		[1] + Q-model of investment	[1] + split in 1999 and 2001 only	[1] + excluding SOEs and second splits	[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
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Year of split and after	-0.059***	-0.069***	-0.087***	-0.077***	-0.077***	-0.077***	-0.077***	-0.080***
	(0.019)	(0.019)	(0.025)	(0.025)	(0.026)	(0.027)	(0.027)	(0.027)
Output/K		0.040***						
		(0.002)						
Cashflow/K		0.027***						
		(0.005)						
Local democracy period	0.044***	0.045***	0.035**	0.036***	0.036**	0.036**	0.036**	0.037**
	(0.012)	(0.012)	(0.012)	(0.014)	(0.016)	(0.017)	(0.016)	(0.016)
Observations	163,005	163,005	151,493	124,821	124,821	124,821	124,821	124,267
R-squared	0.466	0.480	0.470	0.478	0.478	0.478	0.478	0.477
Clusters	1,841	1,841	1,825	1,763	316	257	315	315

Notes: All regressions control for plant and sector-year fixed effects. Sample years include 1993 to 2006 (except in [3]) and thus exclude splits that occur after the moratorium of 2004-2006. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level. Standard errors clustered on plant and sector-year unless specified otherwise.

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, should be sheltered from its impacts due to their close links to the authorities and their ability to rely on potential central government bailouts.

Therefore, next we allow for a differential impact of district splits on SOEs. In order to abstract from the potential impacts of decentralisation on state ownership via investment and divestment, we focus on SOEs that were in operation prior to 1999. In other words, our SOE

status is a plant-specific time-invariant variable.[21] Moreover, we include in our sample only establishments that were privately owned throughout the period as well as establishments with at least 10% state ownership prior to 1999.

The results, presented in the top panel of Table 2, are in line with our hypothesis. They indicate what while privately-owned establishments reduced their investment after district splits, state-owned establishment increased their investment outlays. Both effects are statistically significant at the one percent level in all specifications. They are robust to defining SOEs as having at least 10% state ownership, at least 20%, at least 50% or being fully owned by the state. The magnitude of the investment decline in private plants of about 8 percentage points is in line with the results found in Table 1. The increase in investment outlays by SOEs is at the level of about 10% of their capital stock.

In the bottom panel of Table 2, we distinguish between SOEs owned by the central government and those owned by regional governments.[22] We find that while establishments belonging to regional governments reduce their investments as much as private establishments do, establishment owned by the central government register a substantial increase in their investment outlays. This is intuitive, given the decline in transfers regional governments obtain from the central government and the general decline in public investment in the splitting districts we document in the paper. To foreshadow the results from section 5.4 we will also show that while the indirect tax burden of private plants increases, the tax burden born by state-owned establishments remains unchanged.

[21] This also means that in the presence of plant fixed effects, there is no need to include a self-standing SOE indicator.

[22] 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			
SOE definition →	> 10%	> 20%	> 50%	100%
Panel A	[1]	[2]	[3]	[4]
Year of split and after	-0.080***	-0.079***	-0.079***	-0.077***
	(0.027)	(0.026)	(0.026)	(0.026)
SOE * Year of split and after	0.183***	0.178***	0.201***	0.216***
	(0.061)	(0.062)	(0.066)	(0.063)
Local democracy period	0.036**	0.036**	0.036**	0.036**
	(0.015)	(0.015)	(0.015)	(0.015)
Panel B	[5]	[6]	[7]	[8]
Year of split and after	-0.080***	-0.079***	-0.080***	-0.075***
	(0.027)	(0.026)	(0.026)	(0.026)
Central SOE * Year of split and after	0.246***	0.248***	0.304***	0.294***
	(0.074)	(0.077)	(0.072)	(0.066)
Region SOE * Year of split and after	0.089	0.064	0.106	0.060
	(0.103)	(0.100)	(0.121)	(0.147)
Local democracy period	0.035**	0.035**	0.035**	0.035**
	(0.015)	(0.015)	(0.015)	(0.015)
Observations	133,127	133,127	133,127	133,127
R-squared	0.473	0.473	0.473	0.473
Districts	332	332	332	332

Notes: 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 10% state ownership before 1999. All regressions control for plant and sector-year fixed effects. Observations and R-squared are the same in both panels. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level. Standard errors clustered on plant, sector-year, 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. As evident from Figure 4, 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 immediately following a split and persists for further six years, after which it disappears.

The literature does not offer a clear benchmark of how long it takes for uncertainty to resolve. Most measures of uncertainty used in the literature are continuous, such as the news-based index by Baker et al. (2016) and the dispersion of TFP shocks in Bloom et al. (2018), or regularly recurring events as in Julio and Yook (2012).[23] In Bloom et al. (2007), based on an empirical dynamic model with up to two lags, investment is most depressed during the first two years of high uncertainty after which the difference with counterfactual low uncertainty becomes small. In Gulen and Ion (2016), a news shock containing key terms related to policy uncertainty (from Baker et al., 2016) affects investment two years into the future in a sample of firms from Compustat. Furthermore, Alfaro et al. (2022) find that financial frictions prolong the effects of uncertainty by 50%.

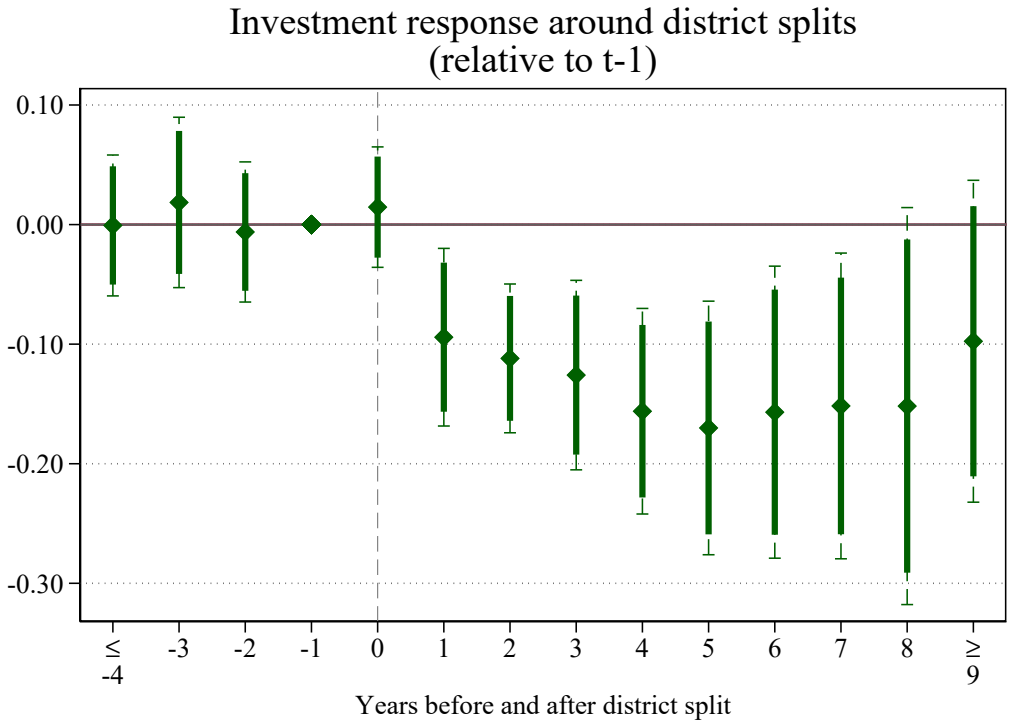
Our empirical results are, however, consistent with the time it takes for a brand new local government to form and for a new policy to be implemented by the (new) local bureaucracy. It is instructive to consider the average delay between the year a new district is created and the year in which the new 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.[24] Moreover, once revenues and expenditures are published, they fluctuate substantially over the years. In new 'breakaway' districts, defined as

[23] They test for uncertainty during the year of a national election, and for a dummy that groups the 'post election' period. However, because national elections typically happen every four years, this may include a pre-election effect as well.

[24] In Online Appendix OA4, 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.

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.[25] This suggests that is 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.

Figure 4: District splits event study graph: Investment rate (I/K)



Notes: Sample includes plants that experience at most one split and are never state owned. 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 OA7.

[25] 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.

5.4 Other outcomes

In this section, we consider the impact of district splitting on other outcomes. 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 7 in Table 1, which is now reproduced in the first column.

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.[26] The coefficient of interest is statistically significant at the five percent level. It suggests that in splitting districts businesses experience a one-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 (see Column 3 and Table A1).

'Donations.' Further, the results suggests that establishments in splitting districts increase their 'donations'.[27] On the one hand, donations could be in reality thinly veiled bribes to corrupt local politicians (Transparency International, 2018).[28] We can only speculate that gifts and donations relate to corruption, but at a minimum, political donations may help to avert some of the uncertainty in times of political change. On the other hand, donations could also be legitimate donations to political actors for lobbying purposes or they could take the form of legitimate charitable donations.[29] And indeed Hassan et al. (2019) find that a one-standard-deviation increase in political risk raises political donations and lobbying expenditures of US listed firms by 8.7% and 18.6%, respectively. Unfortunately, our data do not allow us to distinguish between the three types of donations, which may be the reason

[26] 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*).

[27] In the Census, donations are defined as "Expenditures (Other) gifts, charities, donations and the like". (Variable ICOVCU = "*Pengeluaran(Lainnya) hadiah, sumbangan dan sejenisnya*".)

[28] In 2022, Transparency International ranked Indonesia 110th out of 180 on their corruption perceptions index (implying that 109 countries were *less* corrupt) with a low score of 34/100. While this is an improvement over the 32/100 score of 2012, it is still below the world average. See: `https://www.transparency.org/en/cpi/2022/index/idn`

[29] In a cross-country data set Fisman and Gatti (2002) find that fiscal decentralization in government expenditure is associated with lower corruption.

why the event study results presented in the next section will be quite noisy. The results in Column 2, show a 9% increase in donation outlays among establishments operating in splitting districts. Given the uncertainty introduced by the splits, it is quite likely that the results capture an increase in the bribes and lobbying rather than legitimate charitable donations. Businesses may opt to engage in this way with new local administration in order to mitigate policy surprises, lower the chances of tax audits or introduction of regulations that may adversely affect them.

Substitution of capital with labor. One worry may be that, rather than capturing 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 (Column 11), production of goods (Column 10), or exports and imports (Columns 6-9). Although the results for exports and imports are intuitive, one may wonder how businesses keep up their production level while simultaneously lowering investment. The answer is found in Columns 4 and 5, which show that plants in splitting districts increase employment and see a drop in their capital-labor ratio. Given the low level of compliance with labor market regulations in Indonesia (Dhanani et al., 2009), substituting capital with labor may be a good strategy when it comes to dealing with uncertainty if getting rid of capital goods and other fixed assets carries higher adjustment and transaction costs than reducing the size of the labor force.

More balanced sample. With an eye on conducting event studies that will allow us to understand better the dynamics of businesses adjusting to decentralisation, we repeat all the regressions from the top panel of the table restricting the sample to establishments that can be observed for at least 14 consecutive years. This is the sample that we will be most suited to conducting event study using the Sun and Abraham (2021) approach. The results, produced by this more balanced sample and presented in the middle panel, are similar to those obtained earlier in terms of magnitudes and significance levels, despite the sample being only half as large. The only exception is indirect taxes where the coefficient of interest is of a very similar

magnitude as before but does not reach conventional significance levels.[30]

[30] In Online Appendix OA5, we present two versions of each panel in Table 3: one where we keep only plants that are observed for at least 14 consecutive periods and one for all plants.

Table 3: District splits and other outcomes

Dependent variable →	I/K	Indirect taxes over value added	log donations	log employment	K/L	% exported	IHS exports	% imported	IHS imports	log output goods	log output	log TFP
Panel A: Private plants	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Year of split and after	-0.077***	0.010**	0.091**	0.035**	-0.068*	-0.010	-0.009	0.013	-0.135	0.021	0.050	-0.000
	(0.027)	(0.004)	(0.045)	(0.015)	(0.036)	(0.007)	(0.006)	(0.009)	(0.131)	(0.035)	(0.037)	(0.001)
Local democracy period	0.036**	-0.002	0.024	-0.003	0.010	-0.003	-0.003	-0.007**	-0.059	-0.003	0.007	0.000
	(0.016)	(0.002)	(0.025)	(0.007)	(0.020)	(0.005)	(0.004)	(0.003)	(0.049)	(0.018)	(0.017)	(0.001)
Observations	124,821	124,783	102,789	124,821	124,821	124,821	124,821	120,052	124,821	117,021	124,820	119,525
R-squared	0.478	0.372	0.800	0.936	0.802	0.712	0.712	0.742	0.799	0.919	0.923	0.931
Districts	315	315	301	315	315	315	315	315	315	315	315	313
Panel B: Private plants observed ≥ 14 consecutive periods	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Year of split and after	-0.097***	0.009	0.095*	0.045**	-0.110**	-0.002	-0.002	0.009	-0.138	0.035	0.071	-0.001
	(0.033)	(0.006)	(0.050)	(0.018)	(0.043)	(0.008)	(0.007)	(0.012)	(0.180)	(0.043)	(0.044)	(0.002)
Local democracy period	0.011	-0.003	0.015	-0.004	0.014	-0.004	-0.003	-0.007**	-0.057	-0.026	-0.014	-0.000
	(0.018)	(0.003)	(0.033)	(0.009)	(0.024)	(0.005)	(0.004)	(0.004)	(0.072)	(0.021)	(0.021)	(0.001)
Observations	64,529	64,505	53,836	64,529	64,529	64,529	64,529	62,441	64,529	61,114	64,529	62,060
R-squared	0.422	0.364	0.783	0.934	0.764	0.692	0.692	0.697	0.773	0.918	0.922	0.920
Districts	247	247	244	247	247	247	247	247	247	247	247	247
Panel C: Private plants and SOEs observed ≥ 14 consecutive periods	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Year of split and after	-0.102***	0.010*	0.102**	0.047***	-0.084**	-0.006	-0.005	0.012	-0.104	0.040	0.068	-0.001
	(0.033)	(0.006)	(0.049)	(0.017)	(0.042)	(0.008)	(0.007)	(0.013)	(0.167)	(0.042)	(0.044)	(0.002)
SOE * Year of split and after	0.204***	-0.016*	0.036	-0.045	0.124	-0.001	-0.002	-0.007	-0.058	0.110	0.077	0.015
	(0.071)	(0.010)	(0.137)	(0.064)	(0.135)	(0.019)	(0.017)	(0.012)	(0.286)	(0.081)	(0.077)	(0.011)
Local democracy period	0.014	-0.002	0.012	-0.003	0.017	-0.002	-0.002	-0.005	-0.056	-0.029	-0.024	-0.001
	(0.017)	(0.003)	(0.031)	(0.009)	(0.023)	(0.005)	(0.004)	(0.003)	(0.072)	(0.020)	(0.020)	(0.001)
Observations	70,964	73,650	59,449	73,681	71,188	73,681	73,681	71,337	73,678	69,855	73,680	70,727
R-squared	0.417	0.346	0.791	0.932	0.743	0.671	0.671	0.696	0.767	0.924	0.928	0.900
Districts	276	282	277	282	276	282	282	282	282	282	282	282

Notes: IHS = inverse hyperbolic sine transformation. Panels A and B exclude SOEs. 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. Observations and R-squared are the same in both panels. *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). *% exported* as a share of production. *% imported* as a share of materials. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level. Standard errors clustered on plant, sector-year, and pre-split districts. Sample years include 1993 to 2006.

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 fixed effects. The results first show that the effects on privately-owned establishments mirror those from the top panel and include a statistically significant increase in taxation. However, the results also suggest that SOEs behave differently from private establishments along two dimensions: they see a decline in the tax burden and increase their investment outlays. These two patterns may be connected, as a lower tax burden may leave extra funds that can be utilized for investment purposes.

Ownership by central vs regional governments. In Online Appendix OA5, we also split SOEs into national and regional government owned plants. The results confirm the previously discussed differential effect of decentralisation on investment, which increased in establishments owned by the central government but decreased in establishments owned by regional governments (with the effect being the same as for private establishments). Further, we find that the burden of indirect taxes declined for establishments belonging to the central government, while increasing for regionally-owned ones (but by less than for private plants).[31] Finally, central-government-owned establishments increased their production of goods and TFP, which is in line with their increase in investment. At the same time, the TFP in establishments owned by regional governments went down.

Event studies. Finally, we conduct event studies on the variables, for which we found statistically significant effects in Table 3, in order to understand the dynamics of the adjustment. 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

[31] The increase in tax burden in SOEs owned by regional governments is in line with the their mission, which is defined as being "a public servant, as a source of Local Revenue (PAD), and also as an agent for driving regional economic growth" (Wibowo, 2020).

consider in event study.[32]

The results for investment, donations, indirect taxes, employment, capital-labor ratio, and TFP are presented in Figure 5. 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 4 presented earlier. There is no evidence to suggest violation of the parallel trends assumption. The negative effect of decentralisation is visible already in the year following a district split and persists for additional six years, 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, followed by donations returning to their pre-treatment period level or somewhat declining. Again, there is no evidence of differential pre-trends.

The results for indirect taxes are very noisy. They suggest that splitting districts tended to have a somewhat lower level of taxation in the pre-decentralisation period, even though these include national-level taxes. Pretty much all coefficients in the post-decentralisation period lack statistical significance at conventional levels.

A very striking pattern of a one-time level adjustment is observed for employment. Businesses seem to react immediately to the shock by increasing employment in the year of a district splitting and the year after. This level adjustment seems to become permanent as it persists throughout the time horizon considered, albeit with the estimates becoming more noisy.

The capital-labor ratio mirrors the trajectory of investment and employment. As one would expect, a decline in investment accompanied by growing employment results in a declining capital per worker. The magnitude of the decline increases over time mirroring the long lasting negative effect of decentralisation on investment.

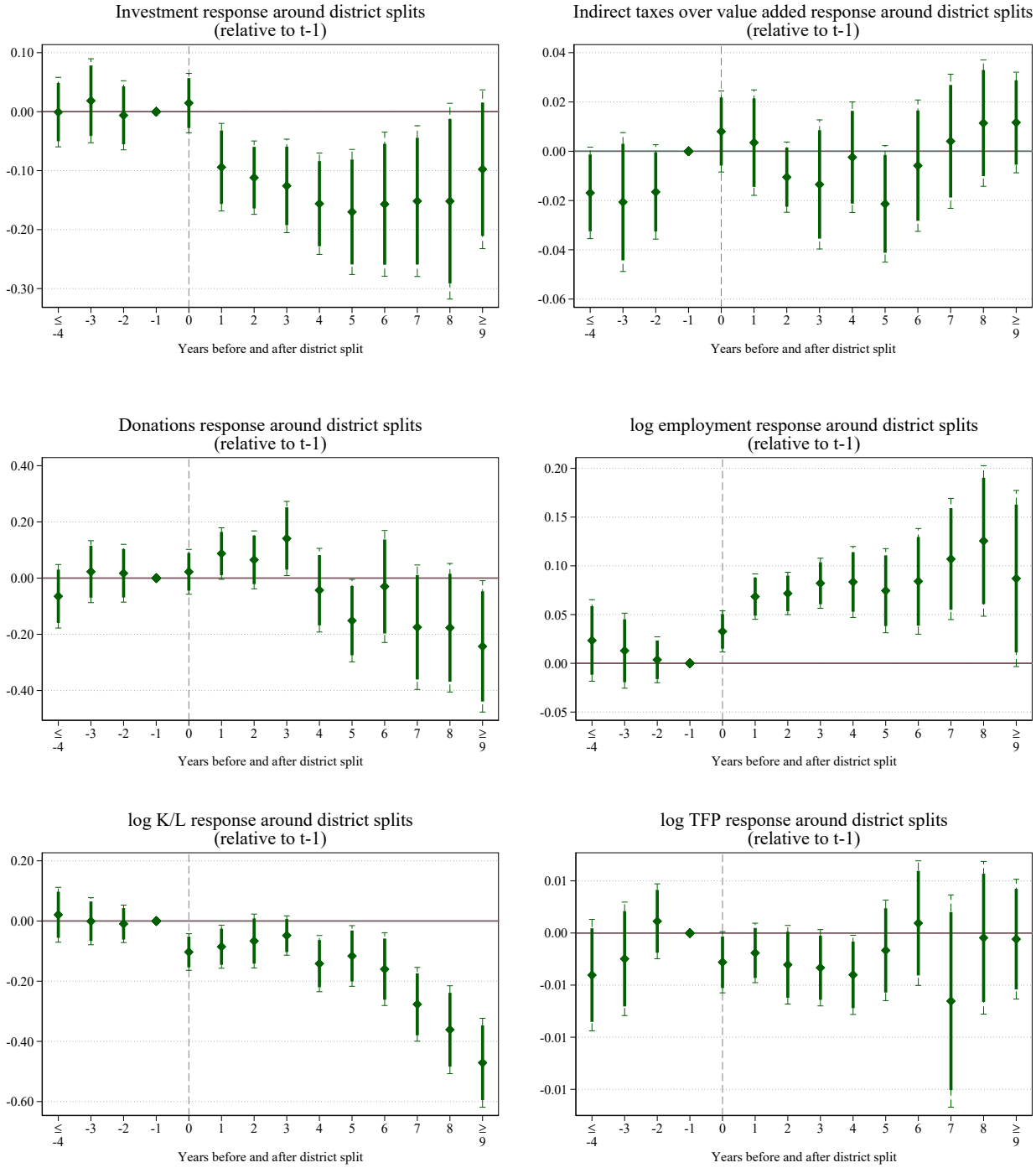
Finally, there is some evidence that the restructuring of the plant affects total factor

[32] Event study graphs for all other variables are presented in Appendix Figure A1 and show relatively more noisy effects over time. In Online Appendix OA6, we present analogous figures using the full sample. The underlying regressions are presented in Online Appendix OA7.

productivity (TFP) negatively, though the estimates are noisy.[33]

[33] TFP, as measured by the innovations to a regression of plant-year level deviations from industry-level expected value added on lagged capital, materials, and labor, is obtained from Javorcik and Poelhekke (2017), and in turn based on De Loecker and Warzynski (2012) and Ackerberg et al. (2006).

Figure 5: District splits event study graphs: Other outcomes



Note: 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 OA7.

5.5 Additional evidence

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. Although the results, presented in this section, exhibit intuitive 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.

Each column in Table 4 presents an interaction of the post-split indicator with the Law certainty measure or one of its subcomponents: *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 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). 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.

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 compoents (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.[34]

[34] Surprisingly, poor relations between the district parliament and its government appear conducive to investment.

Table 4: Initial institutional quality

Panel A						
Dependent variable →	log donations					
Interaction variable (t=2002) →		Law certainty				
...of which →			Consistency of Regulations 28%	Law Enforcement 44%	Illegal Levy outside Bureaucracy 15%	Executive-Legislative Relations 13%
		(mean=0.32)	(mean=0.37)	(mean=0.24)	(mean=0.21)	(mean=0.39)
	[1]	[2]	[3]	[4]	[5]	[6]
Year of split and after	0.223*	0.447**	0.410**	0.255**	0.433*	1.578***
	(0.122)	(0.190)	(0.205)	(0.122)	(0.220)	(0.580)
Year of split ∗ interaction		-2.951**	-1.931	-4.508**	-2.709	-3.739**
		(1.358)	(1.512)	(1.700)	(2.121)	(1.410)
Observations	2,802	2,802	2,802	2,802	2,802	2,802
Districts	67	67	67	67	67	67
R-squared	0.945	0.946	0.946	0.946	0.946	0.946

Panel B						
Dependent variable →	I/K					
	[7]	[8]	[9]	[10]	[11]	[12]
Year of split and after	-0.047**	-0.103***	-0.116***	-0.056***	-0.120***	0.120**
	(0.023)	(0.033)	(0.041)	(0.020)	(0.035)	(0.057)
Year of split ∗ interaction		0.719**	0.720*	0.780***	0.936**	-0.465***
		(0.360)	(0.382)	(0.285)	(0.367)	(0.151)
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 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, and in panel A also for employment quintiles. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level. Standard errors clustered on plant, sector-year, and pre-split districts.

6 Conclusions

This paper investigates the implications of uncertainty, created by administrative and fiscal decentralisation, on business activity. Unlike the existing literature, it focuses on a developing country and leverages a plausibly exogenous shock, namely the substantial increase in the number of Indonesian administrative districts. The data show that districts that split received fewer earmarked transfers from the national government, increased the share of own-source revenue and cut back on public investment to pay for the burden of self-administration.

Establishments operating in the splitting districts responded by reducing investment, and (in the absence of an adverse effect on demand) by increasing employment, which is in line with hiring being less costly to reverse than fixed asset purchases. The results also show an increase in the plant-level tax burden and 'donations', as well as a persistent decline in the capital-labor ratio. In contrast to private plants, establishments owned by the central government did not register a drop in investment or an increase in the tax burden.

We conclude that the effects of uncertainty in a developing country setting are substantial, with investment rates declining from a pre-shock mean of 35% by at least 8 percentage points for a duration of up to seven years. This magnitude are much 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 small and medium-sized establishments in our sample.

Although decentralisation can potentially bring many benefits, one of its unintended consequences may be a slow down in structural transformation of the manufacturing sector.

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Appendix

Table A1: Summary statistics: private plants

Private plants	N	mean	s.d.	min	max
=1 in year of split	124,905	0.01	0.10	0.00	1.00
=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 Donations	103,908	7.12	1.75	0.46	19.70
log Employment	124,905	4.00	1.04	2.56	10.66
log K/L	124,905	9.31	1.37	3.01	20.34
log TFP	119,795	2.23	0.11	-0.03	2.70
% Exported	124,905	0.10	0.28	0.00	1.00
IHS exports	124,905	0.09	0.25	0.00	0.88
% Materials imported	120,304	0.06	0.19	0.00	2.68
IHS materials imports	124,905	2.06	4.97	0.00	22.24
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
Output/K	124,905	2.83	3.02	0.00	16.67
Cashflow/K	124,905	0.63	0.83	-4.95	5.00
=1 if local democracy period	124,893	0.32	0.47	0.00	1.00

Notes: IHS equals the inverse hyperbolic sine transformation. *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). *Local democracy period* is from Martinez-Bravo and Stegmann (2018). All other variables are from the Indonesian Census of Manufacturing, including variables used in the calculation of *TFP*, which is obtained from Javorcik and Poelhekke (2017).

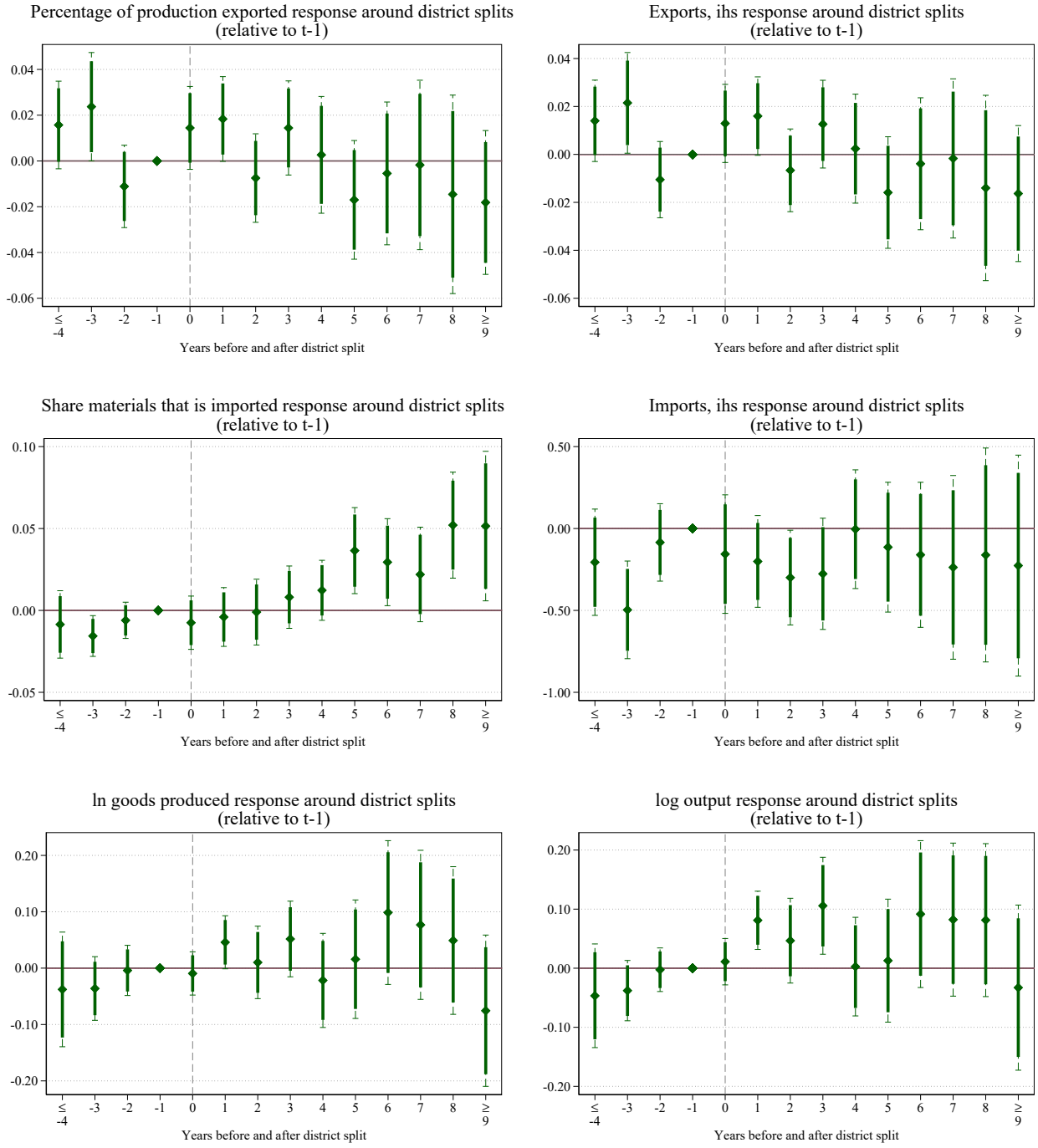
Table A2: Summary statistics: SOEs

Private plants and SOEs	N	mean	s.d.	min	max
=1 if plant >10% SOE before 1999	138,250	0.10	0.30	0.00	1.00
=1 if plant >20% SOE before 1999	138,250	0.09	0.29	0.00	1.00
=1 if plant >50% SOE before 1999	138,250	0.08	0.27	0.00	1.00
=1 if plant >100% SOE before 1999	138,250	0.07	0.25	0.00	1.00

SOEs (>50% before 1999)	N	mean	s.d.	min	max
=1 if plant >50% Central gov. SOE before 1999	13,345	0.52	0.50	0.00	1.00
=1 if plant >50% Region gov. SOE before 1999	13,345	0.35	0.48	0.00	1.00
=1 in year of split	13,345	0.02	0.13	0.00	1.00
=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 Donations	8,322	8.43	2.22	0.03	18.47
log Employment	13,345	5.00	1.41	3.00	10.62
log K/L	9,509	9.98	1.82	-6.24	18.95
log TFP	12,353	2.21	0.14	0.98	2.76
% Exported	13,345	0.15	0.32	0.00	1.00
IHS exports	13,345	0.13	0.28	0.00	0.88
% Materials imported	12,894	0.07	0.20	0.00	1.95
IHS materials imports	13,342	2.89	5.99	0.00	23.13
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
Output/K	8,704	2.63	3.20	0.00	16.67
Cashflow/K	8,676	0.72	0.97	-5.00	4.99
=1 if local democracy period	13,345	0.37	0.48	0.00	1.00

Notes: IHS equals the inverse hyperbolic sine transformation. SOE equals state-owned enterprise.

Figure A1: District splits event study graphs: Other outcomes (continued)



Note: Thick spikes depict 90% confidence intervals, while the thin caps depict 95% confidence intervals.

Online Appendix

Uncertainty, investment, and 'donations'

Beata Javorcik and Steven Poelhekke

April 6, 2023

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OA1 Analysis of district revenues

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

OA1.1 Data on district revenues

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).

OA1.2 Estimating equation

In Appendix Table OA1 we report the results of the following specification, where we regress log total district revenue, and its components by source as a share of total revenue, on district split event dummies:

$$DistrictOutcome_{it} = \beta_s DistrictSplit_{it+s} + \gamma lnPop_{it} + \alpha_i + \mu_t + \epsilon_{it} \quad (3)$$

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*) corresponds to s being equal 0. s can also take the values -2, -1, 1, 2, and 3 which denotes up to two leads and three lags of splits, such that we estimate the effect of a split on the outcome from two and one year(s) before the split, to one year after, two years after, and three or more years after. The sample includes all parent and child districts as well as districts that never split. The α_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.

lnPop stands for the log of population size. Information on population size is available for all years and all districts. However, a population sample census was performed only every decade until 2000, with a full census in 2000, and then every five year through the Population Survey Between Census (SUPAS), implying that the BPS relies on other additional surveys such as the annual SAKERNAS labour force survey in the intermediate years. Because this introduces measurement error and because most other major components of the formula for fiscal transfers are geographic characteristics, such as the area, that are fixed over time, we do not scale district-level financial variables by population and instead include population as a control variable in addition to district fixed effects. Moreover, it is impossible to follow the allocation rules exactly. For example, the largest component (about 60%) is the general allocation grant (DAU). It has two components, the basic allocation (which covers a portion of the wage bill) and the fiscal gap, which is the difference between fiscal capacity and expenditure needs. Fiscal capacity is the sum of own revenue and revenue sharing, while fiscal need is estimated (presumably by means of some unspecified formula) on the basis of five variables: population, area, local prices of construction materials, regional per capita income and the regional 'human development index' (Soesastro and Atje, 2005; The World Bank, 2008). Because of endogeneity concerns, we control for population explicitly and for area via fixed effects.

OA1.3 Findings

The results in Table OA1 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 the year of the split and afterwards, district revenue drops significantly by about ten percent. This holds whether we control for initial 1989 district fixed effects or not.[35]

Does the composition of revenue change after district splits? In columns 4 to 9, we change the dependent variable to the source of revenue as a share of total 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, presumably to make up for the drop in revenue that is due to a reduction in (earmarked) transfers DAK and DAU and consistent with laws 18/1997 and 34/2000 on the proliferation of local taxes and fees, the own source revenue component starts to make up a larger share of revenue. The share increases by 3.5%-points, when compared to a district that does not split.[36]

Table OA2 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 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 experience a simultaneous decline in total revenues and

[35] In column 3, we gauge if there is a significant difference between a seceding district and the remainder of its parent district. Often, when a district splits, one of the two post-split districts keeps the name and the seat of government of the parent district, while the seceding district chooses a new name and has to form a new government. A seceding district is labeled as "a new breakaway district" in the table. We find no statistically significant difference in effects between the seceding and remainder districts.

[36] In Table OA3 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.

an increase in expenditure and attempt to compensate for these by levying new local taxes, potentially increasing uncertainty. 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. While total district revenue grew 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 becoming more important than electricity taxes (Lewis and Sjahrir, 2009). 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 OA1: District-level revenue and sources

Dependent variable →	log total district revenue			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]	[7]	[8]	[9]
Year of split and after	-0.117***	-0.114***	-0.113***	-0.016*	-0.055***	0.046***	0.035***	-0.002	-0.004
	(0.029)	(0.038)	(0.040)	(0.010)	(0.018)	(0.014)	(0.004)	(0.004)	(0.008)
Local democracy period	-0.000	0.013	0.013	0.001	0.002	-0.005	-0.002	0.007**	-0.004
	(0.021)	(0.022)	(0.022)	(0.008)	(0.010)	(0.006)	(0.005)	(0.003)	(0.005)
log population	0.489***	0.501***	0.501***						
	(0.025)	(0.039)	(0.040)						
Year of split and after ∗ New breakaway district			-0.005						
			(0.083)						
1989 District, and year FE	Yes	No	No	No	No	No	No	No	No
District, and year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations (district-years)	5,198	5,191	5,191	5,135	5,246	5,091	5,224	5,196	5,211
Number of clusters	274	455	455	274	274	274	274	274	274
R-squared	0.964	0.969	0.969	0.798	0.764	0.705	0.742	0.584	0.477

Notes:

Table OA2: District expenditures

Dependent variable →	Expenditure as a share of total revenue							
	STAF (Personnel)	CAP (Capital)	GSR (Goods and services)	OTHR (Others)	ADMN (General administration)	AGR (Agriculture)	ECON (Economy)	EDU (Education)
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Year of split and after	0.027*	-0.020	-0.009	-0.008	0.041*	0.009	-0.005	-0.016
	(0.015)	(0.016)	(0.015)	(0.014)	(0.022)	(0.007)	(0.006)	(0.017)
Observations	584	564	582	582	584	578	560	548
R-squared	0.933	0.853	0.866	0.791	0.735	0.866	0.742	0.871
Districts	297	287	295	296	297	294	285	279
	ENVR (Environment)	HE (Health)	HOUS (Housing and public facilities)	INFR (Infrastructure)	PROT (Social protection)	PUBL (Public, law and order)	RELG (Religious)	TOUR (Tourism and culture)
	[9]	[10]	[11]	[12]	[13]	[14]	[15]	[16]
Year of split and after	0.006	-0.007**	0.003	-0.019	-0.002	-0.005**	-0.001	-0.000
	(0.008)	(0.004)	(0.009)	(0.018)	(0.002)	(0.002)	(0.001)	(0.001)
Observations	572	580	584	594	528	540	472	546
R-squared	0.751	0.849	0.775	0.744	0.729	0.744	0.825	0.743
Districts	290	295	297	302	269	274	238	277

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

Table OA3: District revenue before fiscal decentralisation (1993-2000)

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	0.042*	-0.049*	0.009	0.022***	0.002	-0.004
	(0.022)	(0.027)	(0.012)	(0.007)	(0.004)	(0.011)
Local democracy period	0.020	-0.019	0.004	-0.006	0.002	0.001
	(0.015)	(0.017)	(0.004)	(0.006)	(0.003)	(0.006)
log population	-0.075***	0.046***	0.011	0.014	0.005*	0.009
	(0.016)	(0.017)	(0.008)	(0.010)	(0.003)	(0.009)
District and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,898	1,923	1,937	1,922	1,937	1,913
R-squared	0.590	0.669	0.570	0.872	0.443	0.737
Districts	313	314	315	313	315	314

Notes:

OA2 Predicting district splits

In this Section, we use the district-level data to test whether the timing of splits was unanticipated.

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, they find some indication of a positive effect on 1998–2000 splits and a negative effect on 2001-2003 splits, thus suggesting a zero average effect.

In Table OA4 below and conditional on district fixed effects, we find evidence that an increase in the value of non-oil natural resources, surface area and population help predict whether a 1989 district eventually splits. However, when we look at the timing as captured by the number of years since 1989 to the first split of a district, we find no significant results, nor is this effect visible when we repeat the exercise with the districts that existed in 1999, just before decentralization. In the latter case, only non-oil mineral natural resources are significant, which we include in a robustness test of our main results (see Table OA6).

Table OA4: Plausibly Exogenous Timing of Splits Conditional on Observed Fixed Effects

Dependent variable →	Dummy =1 if 1989 district ever split				Number of years to first split of district			
Sample →	*All districts as of 1991*				*All districts as of 1991 that subsequently split*			
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Non-oil mineral natural resources	-0.026	-0.026	-0.033	-0.034	-0.031	-0.031	-0.143	-0.090
	(0.021)	(0.021)	(0.028)	(0.028)	(0.137)	(0.137)	(0.156)	(0.166)
Oil natural resources	-0.005	-0.005	-0.003	-0.002	1.076**	1.076**	1.287**	1.414***
	(0.063)	(0.063)	(0.055)	(0.055)	(0.524)	(0.524)	(0.531)	(0.505)
log surface area	0.178***	0.178***	0.206***	0.202***	0.214	0.214	0.480	0.420
	(0.013)	(0.013)	(0.013)	(0.014)	(0.652)	(0.652)	(0.674)	(0.639)
% of plants with positive investment	0.065	0.065	0.059	0.062	0.588	0.588	0.993	1.526
	(0.072)	(0.072)	(0.071)	(0.073)	(1.076)	(1.076)	(1.125)	(1.187)
log manufacturing employment	-0.033**	-0.033**	0.002	-0.003	-0.236	-0.236	-0.497**	-0.506**
	(0.013)	(0.013)	(0.016)	(0.016)	(0.229)	(0.229)	(0.248)	(0.244)
log population			-0.137***	-0.126***			1.217	1.303
			(0.036)	(0.039)			(0.794)	(0.809)
% manuf. empl. in plants with FDI				0.117				5.784*
				(0.168)				(3.352)
% manuf. empl. in plants that export				0.138				0.519
				(0.202)				(3.407)
% manuf. empl. in state-owned plants				-0.022				3.129
				(0.147)				(1.993)
Observations	238	238	236	236	69	69	69	69
Standard errors:	robust	clustered on 1989 districts			robust	clustered on 1989 districts		
R-squared	0.418	0.418	0.445	0.448	0.045	0.045	0.080	0.142

Dependent variable →	Dummy =1 if 1999 district ever split				Number of years to first split of district			
Sample →	*All districts as of 1999*				*All districts as of 1999 that subsequently split*			
	[9]	[10]	[11]	[12]	[13]	[14]	[15]	[16]
Non-oil mineral natural resources	-0.006	-0.006	-0.016	-0.017	-0.255***	-0.255***	-0.236***	-0.198***
	(0.022)	(0.022)	(0.025)	(0.025)	(0.057)	(0.057)	(0.056)	(0.048)
Oil natural resources	0.000	0.000	0.013	0.015	-0.176	-0.176	-0.099	-0.164
	(0.055)	(0.049)	(0.044)	(0.044)	(0.390)	(0.393)	(0.397)	(0.440)
log surface area	0.127***	0.127***	0.145***	0.139***	-0.163	-0.163	-0.122	0.033
	(0.013)	(0.014)	(0.019)	(0.019)	(0.398)	(0.416)	(0.399)	(0.424)
% of plants with positive investment	-0.138*	-0.138*	-0.136*	-0.108	-0.494	-0.494	-0.178	-0.146
	(0.074)	(0.075)	(0.079)	(0.079)	(0.738)	(0.765)	(0.749)	(0.789)
log manufacturing employment	-0.025**	-0.025**	-0.027	-0.024	0.182	0.182	0.067	0.019
	(0.012)	(0.012)	(0.018)	(0.019)	(0.170)	(0.175)	(0.187)	(0.197)
Local democracy period (mayor)	-0.070	-0.070	0.048	0.053	0.767	0.767	0.948	1.095
	(0.059)	(0.059)	(0.070)	(0.069)	(0.730)	(0.764)	(0.836)	(0.867)
log population			-0.025	-0.023			0.327	0.564
			(0.044)	(0.044)			(0.542)	(0.603)
% manuf. empl. in plants with FDI				0.505***				1.908
				(0.158)				(1.828)
% manuf. empl. in plants that export				0.043				0.616
				(0.116)				(0.997)
% manuf. empl. in state-owned plants				0.232				-1.011
				(0.165)				(0.785)
Observations	287	287	251	251	74	74	68	68
Standard errors:	robust	clustered on 1989 districts			robust	clustered on 1989 districts		
R-squared	0.273	0.273	0.328	0.348	0.068	0.068	0.074	0.127

OA3 Difference-indifference 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 OA3 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]
T vs. Never treated	**0.941**	**0.944**	**0.952**	**0.956**	**0.954**	**0.952**	**0.950**	**0.951**
	[-0.069]	**[-0.099]**	**[-0.130]**	**[-0.186]**	**[-0.206]**	**[-0.268]**	**[-0.286]**	**[-0.286]**
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).

OA4 Further robustness tests of split effect on investment

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

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.

Columns 4 to 6 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 Fitrani 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 7-9 use as timing of the split the first year in which a new district's budgets is recorded in INDO-DAPOER. The three versions allow for a gap of one up 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

Dependent variable →	I/K								
	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 years	3 years
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Year of split and after	-0.056**	-0.069***	-0.076***	-0.097***	-0.139***	-0.175***	-0.088***	-0.088***	-0.087***
	(0.025)	(0.027)	(0.027)	(0.027)	(0.034)	(0.044)	(0.026)	(0.026)	(0.026)
Year of second split and after	-0.218	-0.366							
	(0.212)	(0.255)							
Local democracy period	0.043***	0.036**	0.037**	0.034**	0.027*	0.023	0.037**	0.037**	0.037**
	(0.015)	(0.016)	(0.016)	(0.016)	(0.015)	(0.017)	(0.016)	(0.016)	(0.016)
Resources 1990 * ΔWPI				-0.047	-0.049	-0.059			
				(0.046)	(0.047)	(0.053)			
Resources 1990 * ΔWPI t-1				-0.071	-0.071	-0.062			
				(0.061)	(0.061)	(0.062)			
Oil 1990 * ΔWPI				-0.004***	-0.006***	-0.031*			
				(0.002)	(0.002)	(0.017)			
Oil 1990 * ΔWPI t-1				0.001	0.002	-0.076***			
				(0.001)	(0.001)	(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	Yes
Excluding second splits	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	163,005	125,380	125,380	114,661	108,355	90,581	124,705	124,625	124,619
R-squared	0.466	0.476	0.476	0.497	0.501	0.519	0.478	0.478	0.478
Clusters	340	318	318	310	298	281	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.

OA5 District splits and other outcomes: all firms versus those observed at least 14 periods

Table OA7: District splits and other outcomes, plants observed for any number of years

Dependent variable →	I/K	Indirect taxes over value added	log donations	log employment	K/L	% exported	IHS exports	% imported	IHS imports	log output goods	log output	log TFP
Panel A: Private plants	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Year of split and after	-0.077***	0.010**	0.091**	0.035**	-0.068*	-0.010	-0.009	0.013	-0.135	0.021	0.050	-0.000
	(0.027)	(0.004)	(0.045)	(0.015)	(0.036)	(0.007)	(0.006)	(0.009)	(0.131)	(0.035)	(0.037)	(0.001)
Local democracy period	0.036**	-0.002	0.024	-0.003	0.010	-0.003	-0.003	-0.007**	-0.059	-0.003	0.007	0.000
	(0.016)	(0.002)	(0.025)	(0.007)	(0.020)	(0.005)	(0.004)	(0.003)	(0.049)	(0.018)	(0.017)	(0.001)
Observations	124,821	124,783	102,789	124,821	124,821	124,821	124,821	120,052	124,821	117,021	124,820	119,525
R-squared	0.478	0.372	0.800	0.936	0.802	0.712	0.712	0.742	0.799	0.919	0.923	0.931
Districts	315	315	301	315	315	315	315	315	315	315	315	313
Panel B: Private plants and SOEs	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Year of split and after	-0.079***	0.011**	0.085*	0.037**	-0.047	-0.012*	-0.010*	0.015	-0.099	0.023	0.048	-0.001
	(0.026)	(0.004)	(0.044)	(0.015)	(0.035)	(0.007)	(0.006)	(0.010)	(0.118)	(0.036)	(0.037)	(0.001)
SOE * Year of split and after	0.201***	-0.018**	0.008	-0.063	0.133	0.002	0.000	-0.013	-0.210	0.116	0.083	0.017*
	(0.066)	(0.008)	(0.119)	(0.054)	(0.121)	(0.018)	(0.016)	(0.010)	(0.308)	(0.073)	(0.068)	(0.010)
Local democracy period	0.036**	-0.001	0.019	-0.001	0.014	-0.002	-0.002	-0.006*	-0.050	-0.006	0.000	-0.000
	(0.015)	(0.002)	(0.024)	(0.007)	(0.019)	(0.005)	(0.004)	(0.003)	(0.048)	(0.017)	(0.017)	(0.001)
Panel C: Private plants and SOEs	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Year of split and after	-0.080***	0.011**	0.093**	0.035**	-0.049	-0.012*	-0.011*	0.015	-0.103	0.023	0.048	-0.000
	(0.026)	(0.005)	(0.044)	(0.015)	(0.035)	(0.006)	(0.006)	(0.010)	(0.120)	(0.036)	(0.037)	(0.001)
Central SOE * Year of split and after	0.304***	-0.018**	0.101	-0.072	0.137	0.005	0.004	-0.003	-0.191	0.168**	0.103	0.022**
	(0.072)	(0.009)	(0.140)	(0.060)	(0.179)	(0.022)	(0.020)	(0.011)	(0.256)	(0.083)	(0.074)	(0.010)
Region SOE * Year of split and after	0.106	-0.029**	-0.048	0.061	0.142	-0.033	-0.030	-0.050*	-0.786	-0.063	-0.028	-0.014*
	(0.121)	(0.012)	(0.213)	(0.073)	(0.170)	(0.025)	(0.022)	(0.030)	(0.730)	(0.115)	(0.106)	(0.008)
Local democracy period	0.035**	-0.000	0.019	-0.000	0.016	-0.003	-0.003	-0.006*	-0.047	-0.007	-0.001	-0.000
	(0.015)	(0.002)	(0.024)	(0.007)	(0.019)	(0.005)	(0.004)	(0.003)	(0.048)	(0.017)	(0.017)	(0.001)
Observations	133,127	136,526	110,135	136,574	133,454	136,574	136,574	131,380	136,571	128,157	136,572	130,427
R-squared	0.473	0.358	0.805	0.935	0.788	0.698	0.698	0.736	0.793	0.924	0.928	0.919
Districts	332	338	323	338	332	338	338	338	338	338	338	337

Notes: IHS = inverse hyperbolic sine. Sample excludes SOEs and second splits and control plants that experience their first split after the moratorium. All regressions control for plant and sector-year fixed effects. Observations and R-squared are the same in both panels. *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). *% exported* as a share of production. *% imported* as a share of materials. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level. Standard errors clustered on plant, sector-year, and pre-split districts. Sample years include 1993 to 2006.

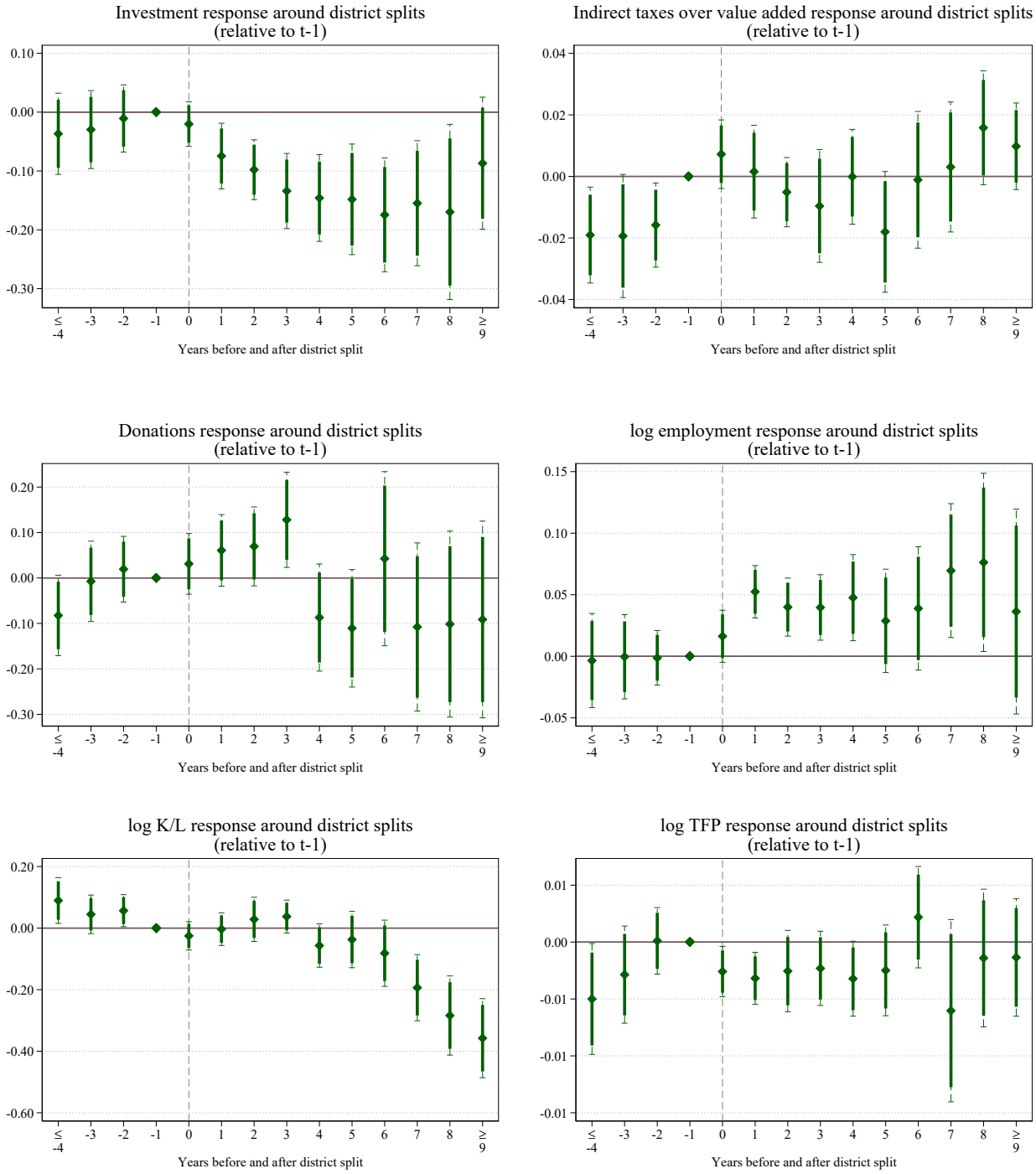
Table OA8: District splits and other outcomes, plants observed at least 14 consecutive years

Dependent variable →	I/K	Indirect taxes over value added	log donations	log employment	K/L	% exported	IHS exports	% imported	IHS imports	log output goods	log output	log TFP
Panel A: Private plants	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Year of split and after	-0.097***	0.009	0.095*	0.045**	-0.110**	-0.002	-0.002	0.009	-0.138	0.035	0.071	-0.001
	(0.033)	(0.006)	(0.050)	(0.018)	(0.043)	(0.008)	(0.007)	(0.012)	(0.180)	(0.043)	(0.044)	(0.002)
Local democracy period	0.011	-0.003	0.015	-0.004	0.014	-0.004	-0.003	-0.007**	-0.057	-0.026	-0.014	-0.000
	(0.018)	(0.003)	(0.033)	(0.009)	(0.024)	(0.005)	(0.004)	(0.004)	(0.072)	(0.021)	(0.021)	(0.001)
Observations	64,529	64,505	53,836	64,529	64,529	64,529	64,529	62,441	64,529	61,114	64,529	62,060
R-squared	0.422	0.364	0.783	0.934	0.764	0.692	0.692	0.697	0.773	0.918	0.922	0.920
Districts	247	247	244	247	247	247	247	247	247	247	247	247
Panel B: Private plants and SOEs	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Year of split and after	-0.102***	0.010*	0.102**	0.047***	-0.084**	-0.006	-0.005	0.012	-0.104	0.040	0.068	-0.001
	(0.033)	(0.006)	(0.049)	(0.017)	(0.042)	(0.008)	(0.007)	(0.013)	(0.167)	(0.042)	(0.044)	(0.002)
SOE * Year of split and after	0.204***	-0.016*	0.036	-0.045	0.124	-0.001	-0.002	-0.007	-0.058	0.110	0.077	0.015
	(0.071)	(0.010)	(0.137)	(0.064)	(0.135)	(0.019)	(0.017)	(0.012)	(0.286)	(0.081)	(0.077)	(0.011)
Local democracy period	0.014	-0.002	0.012	-0.003	0.017	-0.002	-0.002	-0.005	-0.056	-0.029	-0.024	-0.001
	(0.017)	(0.003)	(0.031)	(0.009)	(0.023)	(0.005)	(0.004)	(0.003)	(0.072)	(0.020)	(0.020)	(0.001)
Panel C: Private plants and SOEs	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Year of split and after	-0.103***	0.009*	0.113**	0.046***	-0.087**	-0.006	-0.005	0.012	-0.114	0.039	0.067	-0.001
	(0.033)	(0.006)	(0.049)	(0.016)	(0.042)	(0.008)	(0.007)	(0.013)	(0.171)	(0.042)	(0.044)	(0.002)
Central SOE * Year of split and after	0.329***	-0.016	0.156	-0.046	0.057	-0.001	-0.002	0.002	-0.023	0.161*	0.102	0.018
	(0.074)	(0.010)	(0.166)	(0.070)	(0.191)	(0.024)	(0.022)	(0.015)	(0.311)	(0.088)	(0.082)	(0.011)
Region SOE * Year of split and after	0.037	-0.033**	-0.066	0.128	0.321	-0.035**	-0.032*	-0.048	-0.722	-0.098	-0.069	-0.025***
	(0.142)	(0.016)	(0.236)	(0.079)	(0.200)	(0.018)	(0.016)	(0.036)	(0.837)	(0.136)	(0.127)	(0.009)
Local democracy period	0.014	-0.002	0.012	-0.002	0.019	-0.004	-0.003	-0.005	-0.048	-0.030	-0.024	-0.001
	(0.017)	(0.003)	(0.032)	(0.009)	(0.023)	(0.005)	(0.004)	(0.004)	(0.073)	(0.020)	(0.020)	(0.001)
Observations	70,362	72,768	58,986	72,799	70,563	72,799	72,799	70,469	72,796	68,986	72,798	69,894
R-squared	0.418	0.349	0.791	0.933	0.747	0.675	0.674	0.696	0.770	0.923	0.927	0.902
Districts	275	281	276	281	275	281	281	281	281	281	281	281

Notes: IHS = inverse hyperbolic sine. Sample excludes SOEs and second splits and control plants that experience their first split after the moratorium. All regressions control for plant and sector-year fixed effects. Observations and R-squared are the same in both panels. *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). *% exported* as a share of production. *% imported* as a share of materials. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level. Standard errors clustered on plant, sector-year, and pre-split districts. Sample years include 1993 to 2006.

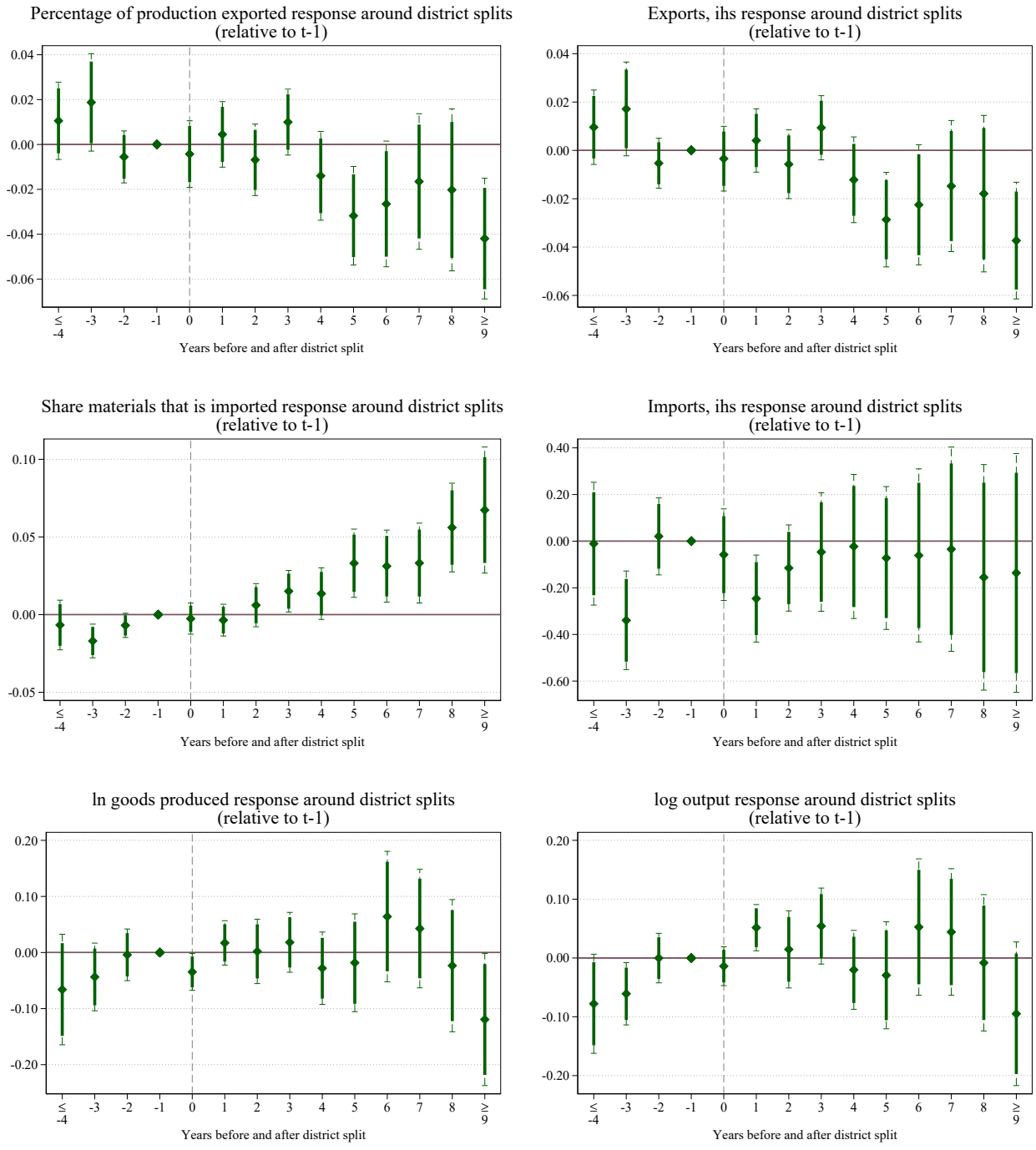
OA6 District splits event study graphs: full sample

Figure OA1: Event study graphs: full sample



Note: Thick spikes depict 90% confidence intervals, while the thin caps depict 95% confidence intervals.

Figure OA2: Event study graphs: full sample (continued)



Note: Thick spikes depict 90% confidence intervals, while the thin caps depict 95% confidence intervals.

OA7 Event-study regressions

Table OA9: Regressions of Figure 5

Dependent variable →	I/K	Indirect taxes over value added	log donations	log employment	K/L	log TFP
Four or more years before split	-0.001	-0.017*	-0.065	0.023	0.021	-0.004
	(0.030)	(0.009)	(0.058)	(0.021)	(0.047)	(0.003)
Three years before split	0.019	-0.021	0.023	0.013	-0.001	-0.002
	(0.036)	(0.014)	(0.056)	(0.020)	(0.040)	(0.003)
Two years before split	-0.006	-0.017*	0.017	0.004	-0.009	0.001
	(0.030)	(0.010)	(0.052)	(0.012)	(0.032)	(0.002)
One year before split						
Year of split	0.015	0.008	0.022	0.033***	-0.103***	-0.003*
	(0.026)	(0.008)	(0.041)	(0.011)	(0.031)	(0.001)
One year after split	-0.094**	0.003	0.087*	0.068***	-0.085**	-0.002
	(0.038)	(0.011)	(0.047)	(0.012)	(0.036)	(0.001)
Two years after split	-0.112***	-0.011	0.065	0.072***	-0.066	-0.003
	(0.032)	(0.007)	(0.053)	(0.011)	(0.046)	(0.002)
Three years after split	-0.126***	-0.013	0.141**	0.082***	-0.048	-0.003*
	(0.040)	(0.013)	(0.067)	(0.013)	(0.033)	(0.002)
Four years after split	-0.156***	-0.002	-0.043	0.083***	-0.142***	-0.004**
	(0.044)	(0.011)	(0.076)	(0.019)	(0.048)	(0.002)
Five years after split	-0.170***	-0.021*	-0.151**	0.075***	-0.116**	-0.002
	(0.054)	(0.012)	(0.075)	(0.022)	(0.051)	(0.002)
Six years after split	-0.157**	-0.006	-0.03	0.084***	-0.160**	0.001
	(0.062)	(0.014)	(0.102)	(0.028)	(0.062)	(0.003)
Seven years after split	-0.152**	0.004	-0.175	0.107***	-0.277***	-0.007
	(0.065)	(0.014)	(0.113)	(0.032)	(0.063)	(0.005)
Eight years after split	-0.152*	0.011	-0.177	0.126***	-0.361***	-0.000
	(0.085)	(0.013)	(0.117)	(0.039)	(0.075)	(0.004)
Nine or more years after split	-0.098	0.012	-0.243**	0.087*	-0.471***	-0.001
	(0.069)	(0.010)	(0.119)	(0.046)	(0.075)	(0.003)
Observations	64529	64505	53836	64529	64529	62,060
R-squared	0.427	0.367	0.785	0.934	0.766	0.920
Districts	247	247	244	247	247	247

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. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level. Standard errors clustered on pre-split district, plant and sector-year.

Table OA10: Regressions of Figure A1

Dependent variable →	% exported	IHS exports	% imported	IHS imports	log output goods	log output
Four or more years before split	0.016	0.014	-0.009	-0.206	-0.038	-0.047
	(0.010)	(0.009)	(0.010)	(0.166)	(0.052)	(0.045)
Three years before split	0.024*	0.021**	-0.016**	-0.497***	-0.036	-0.038
	(0.012)	(0.011)	(0.006)	(0.152)	(0.029)	(0.026)
Two years before split	-0.011	-0.011	-0.006	-0.085	-0.004	-0.003
	(0.009)	(0.008)	(0.006)	(0.121)	(0.023)	(0.019)
One year before split						
Year of split	0.014	0.013	-0.008	-0.156	-0.009	0.011
	(0.009)	(0.008)	(0.008)	(0.185)	(0.020)	(0.020)
One year after split	0.018*	0.016*	-0.004	-0.201	0.046*	0.081***
	(0.009)	(0.008)	(0.009)	(0.143)	(0.024)	(0.025)
Two years after split	-0.007	-0.007	-0.001	-0.300**	0.010	0.047
	(0.010)	(0.009)	(0.010)	(0.147)	(0.033)	(0.037)
Three years after split	0.014	0.013	0.008	-0.277	0.052	0.106**
	(0.010)	(0.009)	(0.010)	(0.173)	(0.034)	(0.042)
Four years after split	0.003	0.002	0.012	-0.004	-0.022	0.003
	(0.013)	(0.012)	(0.009)	(0.185)	(0.043)	(0.043)
Five years after split	-0.017	-0.016	0.037***	-0.114	0.016	0.013
	(0.013)	(0.012)	(0.013)	(0.202)	(0.054)	(0.053)
Six years after split	-0.005	-0.004	0.029**	-0.161	0.099	0.092
	(0.016)	(0.014)	(0.014)	(0.226)	(0.065)	(0.063)
Seven years after split	-0.002	-0.002	0.022	-0.237	0.077	0.082
	(0.019)	(0.017)	(0.015)	(0.286)	(0.067)	(0.066)
Eight years after split	-0.015	-0.014	0.052***	-0.162	0.049	0.081
	(0.022)	(0.020)	(0.017)	(0.333)	(0.067)	(0.066)
Nine or more years after split	-0.018	-0.016	0.051**	-0.226	-0.076	-0.033
	(0.016)	(0.014)	(0.023)	(0.344)	(0.068)	(0.071)
Observations	64,529	64,529	62,441	64,529	61,114	64,529
R-squred	0.694	0.693	0.699	0.774	0.918	0.922
Districts	247	247	247	247	247	247

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. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level. Standard errors clustered on pre-split district, plant and sector-year.

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