Uncertainty, investment, and 'donations'

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Abstract Countries have been increasingly decentralizing and devolving powers to lower levels of government in the hope of improving service delivery. This paper hypothesizes that proliferation of governments in a developing country setting creates fiscal and policy uncertainty, with potentially detrimental effects on 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. Plants operating in the districts that unexpectedly split reduce investment. The decline in investment takes place in the year following a district split and persists over time. The magnitude of the drop is sizeable, reaching about 11 percent three or more years after the split. This likely reflects uncertainty because output, exports, and employment are not affected. Simultaneously, plants increase gifts, charity and 'donations' in the years following district splits, which might reflect their efforts to mitigate political uncertainty. However, donations only go up in districts with low initial levels of institutions as captured by law enforcement. Outward-oriented establishments, such as exporters and importers, seem to be particularly strongly affected by uncertainty. These findings suggest that lower export competitiveness and increased corruption may be unanticipated byproducts of decentralization.

Keywords: investment; traded sector; decentralization; policy uncertainty

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1. Introduction

Developed and developing countries have increasingly decentralized and devolved powers to lower levels of government in the hope of improving service delivery. The examples include creation of regional parliaments in the UK, increasing the number of municipalities from 3,974 in 1980 to 5,560 in 2000 in Brazil, and increasing the number of states from 22 to 37 between 1990 to 2010 in Nigeria.[1] 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, 1999).

This paper hypothesizes that proliferation of governments in a developing country setting creates fiscal and policy uncertainty and increases the tax and compliance burden for private businesses, with potentially detrimental effects on investment and competitiveness. This hypothesis is tested in the context of Indonesia, which has decentralized and increased the number of districts from 284 in 1989 to 511 by 2014. The proliferation of Indonesian subnational districts that greatly increased after the sudden fall of Suharto in 1998 and the subsequent lifting or the presidential veto over district splits in 2000, both of which mitigate endogeneity concerns in our analysis focusing on splits taking place during the period 1989-2004.

The analysis focuses on a panel of manufacturing plants from the Indonesian Census of Manufacturing. The results suggest that plants operating in the splitting districts reduce their investments. The decline in investment appears in the year following a district split and persists over time. The magnitude of the drop is sizeable, reaching about 11 percent three of more years after the split. Outward-oriented establishments, such as foreign-owned plants, exporters and importers seem to be particularly strongly affected. Perhaps with a view of mitigating uncertainty, we find that manufacturing plants increase 'donations' following district splits. Compared to exporters, importers increase donations more and also reduce investment less. In an extension, we examine detailed information on district finances. The data show that districts that split receive fewer earmarked transfers from the national government relative to their population and geography. At the same time, they increase the share of own-source revenue, such as through local (import) taxes, and cut back on public investment to pay for the burden of self-administration.

[1] Grossman and Lewis (2014) document more examples, such as, Uganda increasing the number of districts from 34 to 112, and Kenya from 47 to 70. Vietnam increased the number of provinces from 40 to 64 between 1996 and 2003.

The results are robust to a battery of checks. There is no evidence of anticipation effects. If anything, investment reacts to decentralization with a delay of one year. The results are not driven by general uncertainty about new districts' policy. They are also robust to controlling for financing constraints at the plant level; to clustering at the level of 1989 (i.e., original) districts, pre-split districts or more aggregate industry-year levels; to dropping post-moratorium district splits, to controlling for unobserved and observed predictors of district splitting such as natural resource wealth, population density, district area, and ethnic fractionalization; and to changing the timing of district splits from *de jure* to the *de facto* year of first reporting of post-split district budgets.

The results are consistent with the survey evidence documenting proliferation of taxes in the aftermath of decentralization in Indonesia. In a non-random sample of 231 places surveyed by Lewis (2003), creation of new districts appears to have 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. The minority of taxes and charges, which were reviewed by the national government and covered two-thirds of districts, 40% applied directly to the primary sector (inputs), 10% to the secondary sector (manufacturing), and another 10% and 20% to trade and distribution, and services, respectively. According to LPEM-FEUI (2005) up to another 6,000 were created between 2000 and 2005.

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.[2]

In conclusion, although decentralization may have led to some desirable outcomes, it seems to have worsened the quality of the business climate, as perceived by private establishments, which have responded by scaling down their investment. A particularly strong response from export-oriented establishments suggests that lower export competitiveness may be an unanticipated byproduct of decentralization.

This paper is related to several strands of the economic literature. [TO BE COMPLETED].

[2] 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). These tax increases also increased the scope for and impact of existing corruption (Kuncoro, 2004; von Luebke, 2005), adding to the overall economic harm done by newly established local revenues (Barnes et al., 2005).

2. Data and background

2.1 District proliferation in Indonesia

Figure 1 shows the proliferation of subnational administrative districts (*kabupaten* or regencies) in Indonesia between 1989 and 2009. In 1989, there were 284 districts, which increased to 497 by the year 2009 and 511 by 2014 (not shown). The figure also shows the political timeline. During the reign of Suharto, which ended abruptly in May 1998, governing power was quite centralized and district splits were rare. This changed in 1999, the year after Suharto's reign ended, in a process known as *pemekaran* (blossoming) and subsequent 'big bang' decentralization.

The first wave of decentralization included the rapid drafting and passing of Law 22/1999 on regional governance and Law 25/1999 on fiscal relations under President Habibie. This allowed requests for district splits to be rapidly approved 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, 2020). Local politicians from various political parties lobbied their counterparts in the central committees to propose the split to the central government. However, the Ministry of Interior was responsible for making the final proposal to the president, who held veto rights.

A second wave is visible 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 of decentralization took place. It ended in 2007 but was then reinstated between 2009 and 2012. Applications for new districts continued to arrive, but were put on hold during this period (UNDP, 2007). The year 2004 also saw new decentralisation laws passed in September in order to strengthen central government control over local officials and budgets (Soesastro and Atje, 2005).

Of the districts existing 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.

In our baseline analysis, we focus on the period 1989 up to and including 2006, meaning that we exclude the splits that occurred in 2007 or just after the moratorium on splitting ends. These later splits and their fiscal implications may have been anticipated more because the application for splits may have dated from as early as 2004, but were delayed by the moratorium. Moreover, we want to track the same manufacturing plant over time for at least three periods after a district split. As we observe manufacturing plants only until 2009, we would not have enough periods after the splits taking place in 2008 and 2009. In robustness tests, we also drop the 2004-2009 period altogether; keep districts that do not split during 2007, 2008 or 2009 as controls; and focus exclusively on the 1999 and 2001 splits for districts that do not also split two years before or three years after those splits. In Figure 1, the baseline sample period is depicted by the dotted line with diamonds denoting years with district splits.

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. However, the main candidate for local taxation, the property tax, remained under the jurisdiction of the national government (Brodjonegoro, 2004). Instead, regional governments added other taxes through regional regulations approved by the regional government council, with in practice limited national supervision as ministerial decisions can be repealed in the Supreme Court. One reason for doing so was to raise the share of local revenue and become somewhat less dependent on national transfers, some of which are of uncertain quantity and tend to arrive with as much as a six-month-long delay. These new local taxes ranged from advertisement taxes to district-level internal trade barriers in the form of import and export taxes (Hofman and Kaiser, 2004; Brodjonegoro, 2004).

According to Brodjonegoro (2004), these changes have led to deterioration of the business climate. Surveyed firms cited uncertainty over local taxes and charges, the cost of bureaucracy, and policy surprises that disrupt business licensing as a major contributing factor to production costs (see survey run by LPEM-FEUI (2003)).[3] Large and medium-sized firms

[3] Business licenses include a license to start a business, but also licenses to operate, such as: environmental permit, building license, location permit, principle permit, nuisance permit, and work

were expected to pay relatively more, and the increase in costs for all firms was estimated to be in the order of 10%. At the province level, this may have contributed to the drop in investment in almost every province from 33.7% of GDP in 1996 to 16% in 2001 (Brodjonegoro, 2004).

Up to 1,000 such taxes and charges were created in the year 2001 alone, of which only 40% were submitted for national review (Lewis, 2003). The remainder 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). The Jakarta Post suggests that these have led to a 'high-cost' economy ('Local Autonomy Creating High Cost Economy', JP, 21/3/03), despite the relatively small amount of revenue that these taxes represent.[4] Of the minority that was reviewed by the national government and covering two-thirds of districts, 40% applied directly to the primary sector (inputs), 10% to the secondary sector (manufacturing), and another 10% and 20% to trade and distribution, and services, respectively.

2.2 Data sources

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. 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.

Each district has a unique code that also appears in our main manufacturing plant-level panel data, the *Survei Manufaktur*, the Indonesian Census of Manufacturing conducted by the BPS on an annual basis. This allows us to track precisely which establishment is affected by a district split in which year.[5] The census surveys all registered manufacturing plants with more than 20 employees. It contains detailed information on a large number of variables,

safety permit, each taking up to 43 days to obtain. Another 14 licenses exist. See Lewis and Sjahrir (2009).

[4] The share of own-source revenue in total revenue is on average 8%.

[5] 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.

including the investment flow, fixed assets, 'donations' (to which we come back further down), and the four-digit sector classification. In addition, we observe output, (imported) inputs, ownership and participation in international trade. This allows us to control for a wide set of observed and unobserved characteristics of plants, and for fine-grained sectoral business cycle stances that may coincide with district splits. Our dataset covers the period 1990-2009 and contains 392,416 plant-year observations, of which about 7% belong to foreign-owned plants, 15% to plant-years with exports, and 20% to plant-years with imports. The average spell a plant remains in our sample is about 12 years. Figure 2 shows the number of plants that are affected by district splits in each year, which reflects the timeline of district splits shown in Figure 1.

Information on district revenue by source comes from the Indonesia Database for Policy and Economic Research (INDO-DAPOER: World Bank, 2015). This database is maintained by the World Bank, Jakarta Office, and collects province and district-level economic and fiscal data. The underlying sources are mainly the Central Bureau of Statistics (BPS) and the Ministry of Finance. The fiscal series 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 (such as, Regional Drinking Water 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 contributes 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 (World Bank, 2008, p153).

The year of democratization comes from Martinez-Bravo et al. (2017).

Table A1 list all the variables, their definitions, sources and distributions. Following the finance literature (Julio and Yook, 2012; Gulen and Ion, 2016), we winsorize plant-level

financial variables such that investment and cashflow to capital ratios fall between -5 and +5, dropping 4% of observations on investment.[6]

3. Empirical Strategy

3.1 Empirical strategy

In our main manufacturing plant-level regressions, we regress *PlantOutcome* which captures various outcomes of interest for plant *j* observed in year *t* (with *t* ranging from 1989 to 2006) on a series of events of districts splitting:

$$PlantOutcome_{jt} = \beta_s DistrictSplit_{jt+s} + \gamma X_{jt} + \alpha_j + \upsilon_{mt} + \varepsilon_{jt} \quad (1)$$

where *j* is an individual plant observed in year *t* and the υ_{mt} are 4-digit ISIC industry-year fixed effects. *s* denotes 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 year of the split corresponds to *s* being equal 0, the year before to s being equal to -1, etc. Our main outcome of interest is the ratio of current investment *I* to fixed assets *K*, where the latter are directly observed in the census and which we time at the start of the period. We scale investment by the initial stock of capital to exploit the full variation in investment. Since firms do not investment every year, investment contains zeros in almost 50% of cases within out baseline sample, and is reported to be negative in less than 2% of cases (divestment or sale of assets). Taking logs of the level of investment would drop all these observations, hence our focus on a scaled variable instead. We always control for the years in which a district is a local democracy, following Martinez-Bravo et al. (2017). After Suharto's reign ended, mayors that were appointed by the regime were allowed to finish their term, after which the local elected parliament appointed a new mayor. Since this democratic transition may also result in uncertainty, we control for a dummy equal to one from the year in which a district has a democratically appointed mayor. Direct elections of mayors started in 2005. We cluster standard errors by plant, four-digit industry-year, and alternatively also on pre-split parent district or the initial 1989 districts.

We extend the main analysis by exploring heterogeneity at the plant level to ask if

[6] 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).

outward-oriented plants are affected more by uncertainty. Because manufacturing plants may enter and exit from international markets over time, which may also be related to uncertainty, we construct matched pairs of plants that are equal in observable ways, including whether they were exporting before they experienced a district split. More specifically, we use Coarsened Exact Matching (CEM; see Iacus et al., 2011), which creates exact matches on all binary and count variables and exact matches within bins of continuous variables. This is straightforward for our many dummy variables such as two-digit sector, year, exporter status (0, >10% or >50% of output), traded sector status, and year of democratization. For continuous variables, we split each variable into bins to build exact matches to the extend that treated and control firms are observed in the same bin. The benefit of this method is that balancing is automatically achieved on matching variables and that it is not affected by model mis-specification. For example, it does not have to assume a probit model as is the case in propensity-score matching. Our matching variables are: one and two lags of capital and employment deciles, and one and two lags of 11 investment rate bins (-5 to -1, -1 to -0.5, -0.5 to 0, 0, 0 to 0.25, 0.25 to 0.50, 0.50 to 0.75, 0.75 to 1, 1 to 2, 2 to 4, 4 to 5). After matching, we verify that also lagged continuous variables such as the investment rate, log employment, % exported, log exports (+1), log output, log donations and foreign owned status are mean-balanced in the sample of matched pairs. Using our matched sample, we estimate the difference-in-difference specification (1) again, thereby eliminating plant fixed effects, but in addition we control for pre-split export and import intensity, and interact the district split event with initial import or with export intensity.

In the second extension, we also examine the effect of district splits on district-level outcomes, such as total revenue, revenue by source, and expenditure, controlling for population size and district and year fixed effects:

$$DistrictOutcome_{it} = \beta_s DistrictSplit_{it+s} + \gamma lnPop_{it} + \alpha_i + \mu_t + \varepsilon_{it} \qquad (2)$$

where *DistrictOutcome* denotes various outcomes of interest for district *i* observed in year *t* (with *t* ranging from 1989 to 2009). 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; World Bank, 2007). Because of endogeneity concerns, we control for population explicitly and for area via fixed effects.

3.2 Exogenous timing of district splits

Our empirical strategy exploits the exogenous timing of district splits, following Burgess et al. (2012), Alesina et al. (2019), and Bazzi and Gudgean (2020). 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 the splits that occurred before the 2004 moratorium on splits, because they were implemented in a short period of time and can be plausibly treated as unanticipated events. In contrast, the post-moratorium splits, taking place in 2007 and later years, may have been waiting for approval since 2004 and hence may have been anticipated. To track manufacturing plants during post-split periods we include the years 2004-2006 in the baseline results. In robustness tests (i) we exclude districts from that window where splitting may have been anticipated (those districts that eventually split in 2007 or later), (ii) drop the 2004-2009 period altogether, and (iii) focus only on the 1999 and 2001 splits. We also exclude state-owned enterprises (SOEs) and plants that experience more than one split. The reason is that SOEs are directly linked to government and may have more information about impending changes, and plants that experience a split for the second or even third time may have learned from past experience.

Fitriani et al. (2005) 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.

Our identifying assumption is that the timing of redistricting is not driven by trends in plant-level investment. We test this assumption using plant-level data and find no evidence of pre-trends.

In the online appendix Table OA1, we further use the district-level data to test whether the timing of splits was unanticipated. 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.

4. Results

4.1. Do manufacturing plants reduce investment after splits?

Column 1 in Table 1 presents our baseline estimate of equation (1), based on 16,252 manufacturing plants from 127 four-digit sectors observed between 1989 and 2006. We find an unanticipated and persistent drop in plant investment rate, ranging between 6 and 10% points starting in the year after their district splits. This effect is robust to including standard determinants of investment in incomplete markets, such as, output to capital ratio and cashflow to capital ratio, which capture financing constraints (see column 2). Consistent with the Q-model of investment (Tobin, 1971; Tobin and Brainard, 1977; Blundell et al., 1992), we find that these positively predict investment, but they do not change the effect of splits. In column 3, we show that the splits that occurred during the years of big bang decentralization had even stronger and more immediate negative effects on investment. Importantly, we find no evidence of anticipation effects.

In column 4, we drop state-owned enterprises (SOEs), i.e., plants that ever report a (fractional) ownership by state entities. These plants were relatively common due to the effects

of bailouts and restructuring of corporate debt in the aftermath of the Asian crisis.[7] More importantly, such plants may have been more 'connected' to local government. Their revenue stream may have been more stable, thus making them less affected by splits, or they were better informed, reducing uncertainty compared to private sector plants. Reduced uncertainty may also occur during secondary splits: we thus drop the two years before secondary splits and the year of the secondary split and subsequent years from the sample. To exclude potential anticipation effects altogether we make sure that we examine only plant-years that experience at most one spell of district splits. Comparing columns 1 and 4 we find that the effect of splits was nearly twice as negative among private sector plants that experience a split for the first time, relative to private sector plants that did not experience a split.

So far, the regressions do not show evidence for anticipation effects at one or two years before the event of a district split. In Figure 3 (top panel), we further examine the absence of evidence for pre-split trends in investment, by using the same sample as column 4, but including a set of dummies equal to 1 in up to 5 years before splits, and 5 years after split, but excluding the year t-1 which becomes the baseline. The first dummy is also equal to 1 in all periods before *t-5*, and the last dummy is 1 in all periods after *t+5*. There is clearly no evidence for a pre-split trends, while investment significantly and persistently drops after a district splits.[8]

The period of democratization at the local level appears unrelated to investment in all regressions once SOEs are excluded. In Appendix Figure 3 we also show pre- and post-split effects for multiple periods before and after democratization, but we find no significant effects. A district that splits is thus a more major event than the transition to democracy at the local level.

Columns 5 to 8 show stability and robustness of the estimates to (i) dropping the anticipation controls, (ii) clustering on initial 1989 districts, (iii) clustering on pre-split parent districts, and (iv) restricting the sample to the years before the moratorium on splits that started in 2004.

[7] About 20% of manufacturing plants briefly received state support in 2000 to prevent them from failing. In the data, this shows up as a short period of full state-ownership shares. By 2002 this was greatly reduced to 8% of plants, which was still higher than the 2% of 1999.

[8] In Appendix Figure 1 we instead saturate the model with a full set of separate dummies (green and solid spikes) following Callaway and Sant'Anna (2020), or collapse the t-8 and earlier and t+10 and later into single dummies (orange and dashed spikes) for a more parsimonious variable bandwidth model. The reason for varying the bandwidth is that the number of treated plants drops off steeply at early and late horizons because few plants are observed that long in the sample as shown in the bottom panel, leading the noisy estimates at the tails. Comparing the saturated with the two variable bandwidth models shows little bias from estimating the more parsimonious specification.

The census of manufacturing records total investment by each plant in each year, and also the source by which investment was financed. Appendix Table A5 shows that the reduction in investment is not driven by any particular source of funding drying up in the first few years after the split. However, three years after the split we find a significant reduction in direct domestic loans and private equity investment.

In Table 2, we slightly alter the econometric specification and collapse the indicators for individual post-split years into one indicator, effectively changing the specification to a more standard difference-in-difference:

$$PlantOutcome_{jt} = \beta DistrictSplit_{jt} + \gamma X_{jt} + \alpha_j + \upsilon_{mt} + \varepsilon_{jt}$$

where $DistrictSplit_{jt}$ is an indicator variable equal to one in the year of split and subsequent years, and taking on the value of zero otherwise. We repeat all specifications of Table 1, and find that our main result is very stable.

4.2. Robustness checks

In Appendix Table A2, we perform a wide range of additional robustness tests where we change the method of clustering, the sample period, and the set of control variables, none of which changes the main results.

Bloom et al. (2007) show that irreversibility causes the responsiveness of investment to demand shocks to be weaker during periods of high uncertainty, an effect that holds in a sample of 672 publicly traded U.K. manufacturing firms. In our setting, district splits may create uncertainty which in turn would make firms more cautious to invest when output to capital ratios are high. We test this by following Bloom et al. in augmenting a Q-model of investment (for reference, we also show estimates of a pure Q-model of investment in Column 1). As visible in column 2, we also find that more uncertainty (captured in our context by a district split) reduces investment relatively more for high output establishments.

Column 3 clusters standard errors at a more aggregate two-digit sector level. The results remain robust.

In columns 4 and 5, we allow for the fact that districts may split more than once over time, and include separate dummies for the years surrounding secondary splits. This is important because, arguably, the first instance of political uncertainty due to district splits may be more unanticipated than subsequent ones, and may also be more disruptive due to its novelty. This view is supported by the data, which show that in our baseline sample of SOEs and private plants (column 4) and in a sample of only private plants (column 5) only the first split results

in significant reduction of investment.[9] As column 6 shows, leaving secondary splits in the sample would lead to some underestimation of the effect of splits on investment.

Column 7 shows the reason for excluding splits that occurred after the moratorium on splits ended. These later splits were put on hold but may have been requested as early as 2004 and thus be more anticipated, invalidating the identification strategy. Column 7 shows indeed that post-moratorium splits have significantly negative anticipation effects in contrast to the earlier splits, which is the main reason to focus our attention on the 1989-2006 period in the main results. Because the plant-level data ends in 2009 we cannot estimate more lags in this sample.

Column 8 adds pre-split district clusters to the specification of Table 2, column 1, and drops the period starting in 2004.

Columns 9 to 11 control for variables that potentially predict district splits, such as natural resource wealth and population trends. Natural resource wealth at the level of initial districts are a fixed effect, but their value may increase over time. Motivated by Fitriani et al. (2005) and using data from Pelzl and Poelhekke (2020), 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 Kabupate* 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 12-14 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.[10]

[9] Note that in columns 4 and 5 the dummy 'Three+ years after split' equals 1 three years after the first split and remains 1 up to three years before the second split. In column 6 the dummy 'Three+ years after split' equals 1 three years after the first split and remains one after that.

[10] In Online Appendix Table OA4 we show that the investment effects are not worse in years when the district budget is unknown. Rather than adding to uncertainty, it appears that this is mostly due to poor reporting. Other sources of uncertainty, such as whether the new districts receives a new name and has to build a new government (new breakaway district) or when both have new names which may signify a larger change in policy (both new), also do not change the main detrimental effect of splits on investment.

4.3. Can 'donations' avoid uncertainty?

The strong response of investment to sudden district splits raises the question if plants have ways of reducing uncertainty. The Census of Manufacturing also records "Expenditures (Other) gifts, donations and the like", which have been used in literature as a proxy for bribes.[11] In 2019, Transparency International ranked Indonesia 85th out of 198 on their corruption perceptions index with a score of 40/100. While this is an improvement over the 32/100 score of 2012, it is still below the world average. 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.

In Table 3, we therefore change the dependent variable to the log of donations reported at the individual plant-year level, and re-examine the effect of district splits. The sample is smaller, but donations are common: only 17% of plants *never* report any donations. Excluding SOEs and secondary splits, we find in column 1 an increase is donations in the year of the split, that is also significant two years after the split.[12]

In Figure 3, bottom panel, we also explore pre-split trends in donations. None of the pre-split dummies are significant, and donations appear to fluctuate above and below zero somewhat. Appendix Figure 2 shows the corresponding figure with a saturated set of dummies. In all cases, donations only go up significantly in the year of the split and stay high up to three years after the split. Dropping anticipation in column 2, the results do not change much. Columns 3 and 4 instead report a more standard diff-in-diff specification. In column 3a the dummy compares the entire post-split period to the pre-split period and finds a less significant positive effect after splits. Interacting with the local democracy dummy suggests that donations go up mainly in districts that split during non-democratic periods.

So far, we find that while investment decreases, donations increase. However, while this may be consistent with an increase in uncertainty and an attempt to reduce uncertainty, it is also consistent with direct disruption of the economy. We next look at other outcomes to see if this is the case.

4.4. Uncertainty versus disruption of the local economy

In Table 4, we look at real as opposed to financial effects, in the sense that we ask

[11] Variable ICOVCU = "*Pengeluaran(Lainnya) hadiah, sumbangan dan sejenisnya*".
[12] Result are robust to clustering on 1989 districts.

whether the reduction in investment rates is accompanied by reductions in employment, exports, and output. Column 1 repeats the baseline estimate for the investment rate, in a sample of privately owned manufacturing plants, and columns 2 to 5 each replace the dependent variable with employment growth, the change in the share of output exported, growth in the value of exports, and output growth, respectively. We find very little effect of district splits on these variables: the coefficients are small and almost never statistically significant. This may imply that it takes time for a drop in investment rates to translate into worsened performance. It also suggests that since the only effect of the district split is a reduction of investment (and an increase in donations), we are capturing the effect of uncertainty as opposed to a disruption to the (local) economy.

5. Extension I: Are outward-oriented plants more affected by district splits?

In this section, we explore heterogeneity in manufacturing plant characteristics, such as belonging to the traded sector, domestic importer status, domestic exporter status, and foreign-owned status. We first do so by interacting the post-split period dummies with the plant characteristics such as whether a plant ever exports in its lifetime, and then look closer by constructing matched pairs of firms that both exported or not exported in the year before a district split.

Table 5 presents the interaction results. For comparison, in column 1, we start with the baseline specification of a sample of privately owned plants. In column 2, we track whether a plant belongs to the traded four-digit sector as captured by average travel distance of their goods (Holmes and Stevens, 2014). We find strong evidence that outward-oriented plants (i.e., those belonging to traded sectors) reduce their investment by more during and after district splits. Moreover, plants in non-traded sectors do not show a statistically significant response. Partly, this may be due to the much higher average annual investment rate of traded sector firms, which is 42% on average (median 3%) compared to only 28% for non-traded sector plants (median 0%). The traded sector may be inherently more sensitive to uncertainty, which shows up as a very strong negative effect on investment of district splits.

Next, we split plants in domestic private and foreign-owned plants, and examine importer and exporter status of domestic private plants. Importer and exporter status is time invariant and defined as a dummy equal to 1 if a plant ever imports or exports a positive share of material inputs or output, respectively. We thus assume that plants that exported in the past

or will export in the future are structurally different from plants that never export during our sample period.[13] Foreign owned plants are defined as those reporting a positive foreign ownership share in at least one year. The result indicate that importers respond somewhat stronger to district splits than exporters. There is no evidence of statistically significant responses to splits on the part of plants active only in the domestic market. The strong effect on importers is in line with the observation made in the literature that local governments were mainly raising own-source revenue by imposing local import taxes. The effect on foreign-owned plants is also negative but more noisy, partly because foreign status is relatively rare and only a subset of such firms is ever affected by a district split.[14]

Table 6 reports the results of district splits on four outcomes for a sample of *matched* pairs of plants that are observationally similar in the year before a split occurs. The benefit is that we can make pairs of plants that were both exporters, or both not exporters just before splits occur. We then interact the split event with a measure of export or import intensity, and estimate outcomes cumulatively at five different horizons. For example, plants that do no export in *t-1* do not significantly change investment after a district split (top row, upper left panel A). However, the more a plant exported, the more it reduced investment up to three years after a split occurs (second row). The marginal effects, estimated at the mean export intensity of plants that export, suggest that export-intensive plants reduce investment by up to 22% three years after a district splits, which echoes the results in Table 5. However, they do *not* increase donations more than non-exporters. They do tend to reduce the share of raw materials that is imported more, but exports are not affected at this horizon. Panels B show the effects for plants that are intensive importers. They are less likely to reduce investment (unless they import a lot, and then only with a delay), but they are showing a much stronger increase in donations. They also reduce imports less, despite anecdotal evidence suggesting that new districts increased own-source revenue by imposing local fees, duties and local import taxes. This suggests that donations may help to reduce the adverse effects of district splits, and that importers are more active in doing so than exporters that sell to foreign companies. We come back to this in Section 7.

6. Extension II: Does local institutional quality prevent donations?

[13] This is done to avoid the possibility that a plant's importing or exporting status is affected by uncertainty.
[14] We experimented by defining international status by using the plants' status as of 1990 (the start of the sample), or at the first year they enter our dataset, but these measures either focus on a very narrow set of plants, or are more noisy as they fail to incorporate the high rate of globalisation of Indonesian manufacturing during the 1990s.

We next explore if local institutional quality at the district level can prevent an increase in 'donations'. KKPOD (Regional Autonomy Watch) surveyed local institutional quality in a subset of 124 districts during the years 2002, 2003 and 2004. Each district was scored along multiple dimensions using Likert scales (1 to 5, with 5 being best). These include 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. The note to Table 7 provides more detail to these. For example, Law Certainty refers to the consistency of rules and law enforcement in the region, which are not subject to frequent change due to succession of officials. It also weighs in 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 is only available for three years, we can only examine district splits that take 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 4 shows the mean institutional score in districts that split in 2003, versus those that never split. Institutional quality was surprisingly similar before splits occurred in 2003, but then deteriorated, and did not catch up again by 2004. More formally, Table 7 finds that donations went up after district splits (and investment down), mostly in districts that had a *low* institutional score in 2002. The significant difference of the effect of splits among districts with high versus low institutions in 2002, appears to be driven by the component of law certainty, which in turn mostly reflects law enforcement.

In the lower panel of the table, we examine the impact of institutional quality on investment. The interactions terms appear to be mostly insignificant, with the exception of interaction with Executive-Legislative Relations.

7. Extension III: Does splitting affect the structure of district fiscal revenue sources?

We start by estimating equation (1) to examine the effect of splits on district revenues, controlling for 1989 district fixed effects, year fixed effects, and population, using a panel of districts observed between 1989 and 2009.[15] The results, presented in Appendix Table A3, suggest that a doubling of population increases revenues by about 50%, which is in line with

[15] As mentioned before, we exclude district splits occurring in 2007-2009 because we do not observe the relevant outcomes three years after these splits.

population being only one of several determinants of fiscal transfers. In the two years leading up to the split, there is no significant change in revenue. However, in the year of the split, revenue drops by 6% (conditional on the population change), and then reduces further by 8.5, 13.6, and 10.8% in the first, second, and third or later years after the split, respectively.

In column 3, we drop the leads and in column 4 we replace 1989 district fixed effects by current district fixed effects such that a parent district and its two children districts are each identified by their own fixed effect. This captures (un)observed characteristics such as land area, other geography, and the dispersion of towns and cities. Conditional on this richer set of fixed effects, the results are somewhat larger in magnitude: a district split persistently reduces revenue over and beyond what is expected from the change in its size.

In column 5, 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.

Does the composition of revenue change after district splits? In columns 6 to 11, 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 clearly see a drop in the DAU: the lump sum component received by districts does not appear to make up for the overall loss in the transfers share of revenue. The smaller DAK also decreases in importance. However, we find a clear increase in natural resource revenues that is consistent with the new fiscal redistribution rules, which were implemented with decentralization. Moreover, and consistent with laws 18/1997 and 34/2000 on the proliferation of local taxes and fees described in Section 2, the own source revenue component starts to make up a larger share of revenue after districts split. The share increases by 2.4% points, when compared to a district that does not split.[16]

Additional analysis in the appendix Table A4 focuses on expenditures (as a share of revenue). However, 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

[16] In online appendix Table OA2 we show that the effect of own source revenue was also present in splits that precede Law 34/2000. However, the DAK would still increase in importance after splits while natural resource revenue did not change.

relative increase in spending on personnel and general administration and a reduction in capital and infrastructure spending in the year of the split. This is suggestive of splits being costly in terms of restructuring or building up a new government, at the expense of capital and infrastructure spending.[17]

In summary, splitting districts experience a simultaneous decline in total revenues and an increase in expenditure and attempt to compensate for these by levying new local taxes.

8. Conclusions

Although decentralization and devolution of powers to lower levels of government are often advocated as a way of improving service delivery, they may be costly to the local economy. This paper considers the case of Indonesia, which has experienced proliferation of local governments as it has increased the number of its districts from 284 in 1989 to 511 by 2014. It finds that districts that split receive fewer earmarked transfers from the national government, increase the share of own-source revenue and cut back on public investment to pay for the burden of self-administration. This situation creates fiscal and policy uncertainty and increases the tax and compliance burden for private businesses, resulting in detrimental effects for investment.

The data indicate that plants operating in the splitting districts respond to these changes by reducing their investment. The decline in investment is visible in the year following a district split and persists over time. It is sizeable in magnitude, with the drop in investment (relative to capital stock) of about 11 percent three of more years after the split. Outward-oriented establishments, such as importers and exporters are particularly strongly affected. Moreover, plants respond to the uncertainty by increasing 'donations'. While we cannot be sure that these relate to political donations *per se* of corruption in general, they are suggestive of the latter in an environment of perceived high corruption. The latter is underscored by the fact that donations go up mostly in districts with weak local law enforcement. Importers also increase donations by more, which is consistent with anecdotal evidence on the proliferation of local taxes, including import tariffs, and with our results that show that districts that split rely more on own-source revenue, such as local taxes.

[17] Finally, in online appendix Table OA3, we look at actual expenditure shares on items that are funded by the earmarked DAK (Special Allocation Grant) transfer. This data is only available from 2003 and contains many missing entries. We still find a net increase in spending on the government sector, and a decrease in health and infrastructure such as roads and irrigation.

Taken together, these findings suggest that although decentralization may have led to some desirable outcomes, it has worsened the quality of the business climate. Private businesses responded to this deterioration by scaling down their investment. A particularly strong response from export-oriented establishments suggests that lower export competitiveness may be an unanticipated by-product of decentralization.

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Figure 1
New districts created by year and political timeline

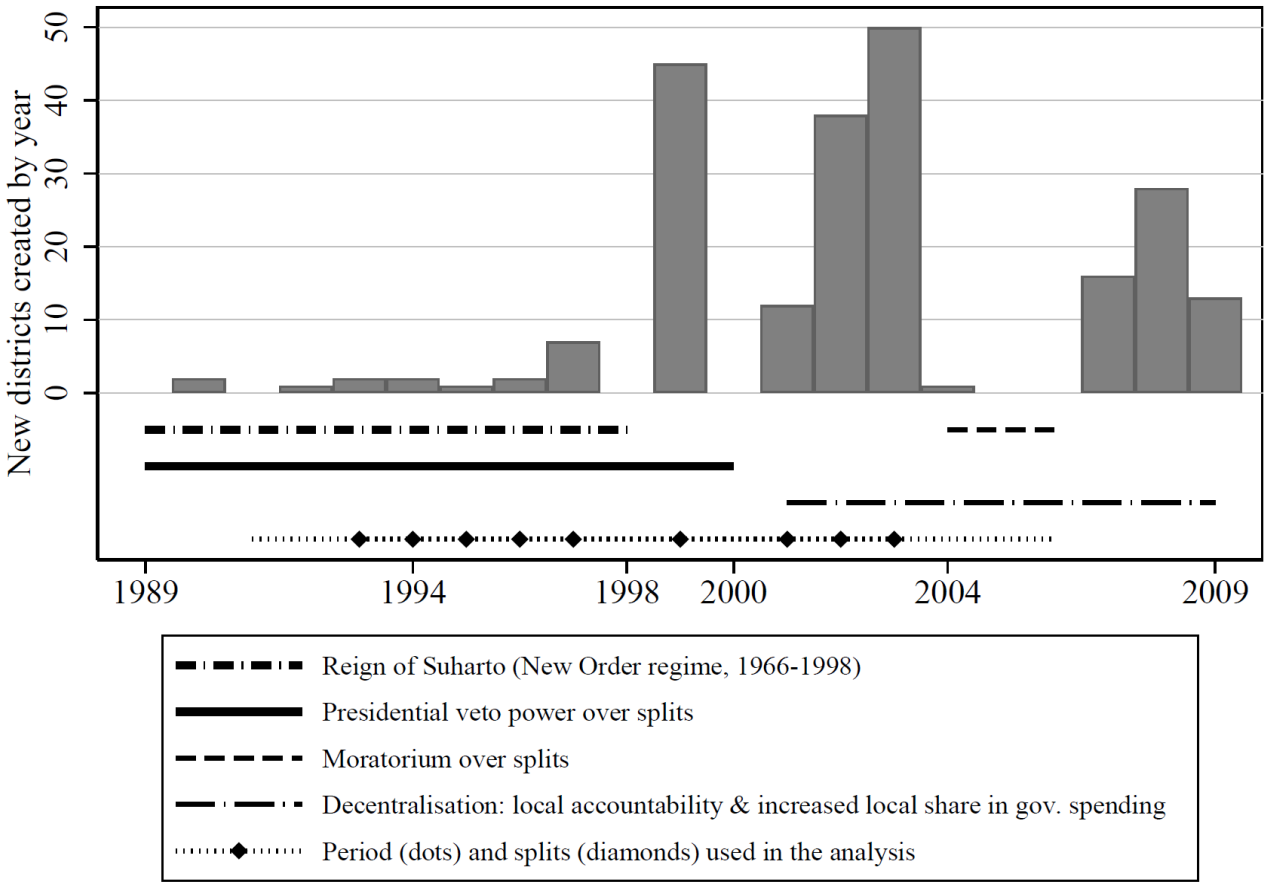


Figure 2
Manufacturing plants affected by district splits

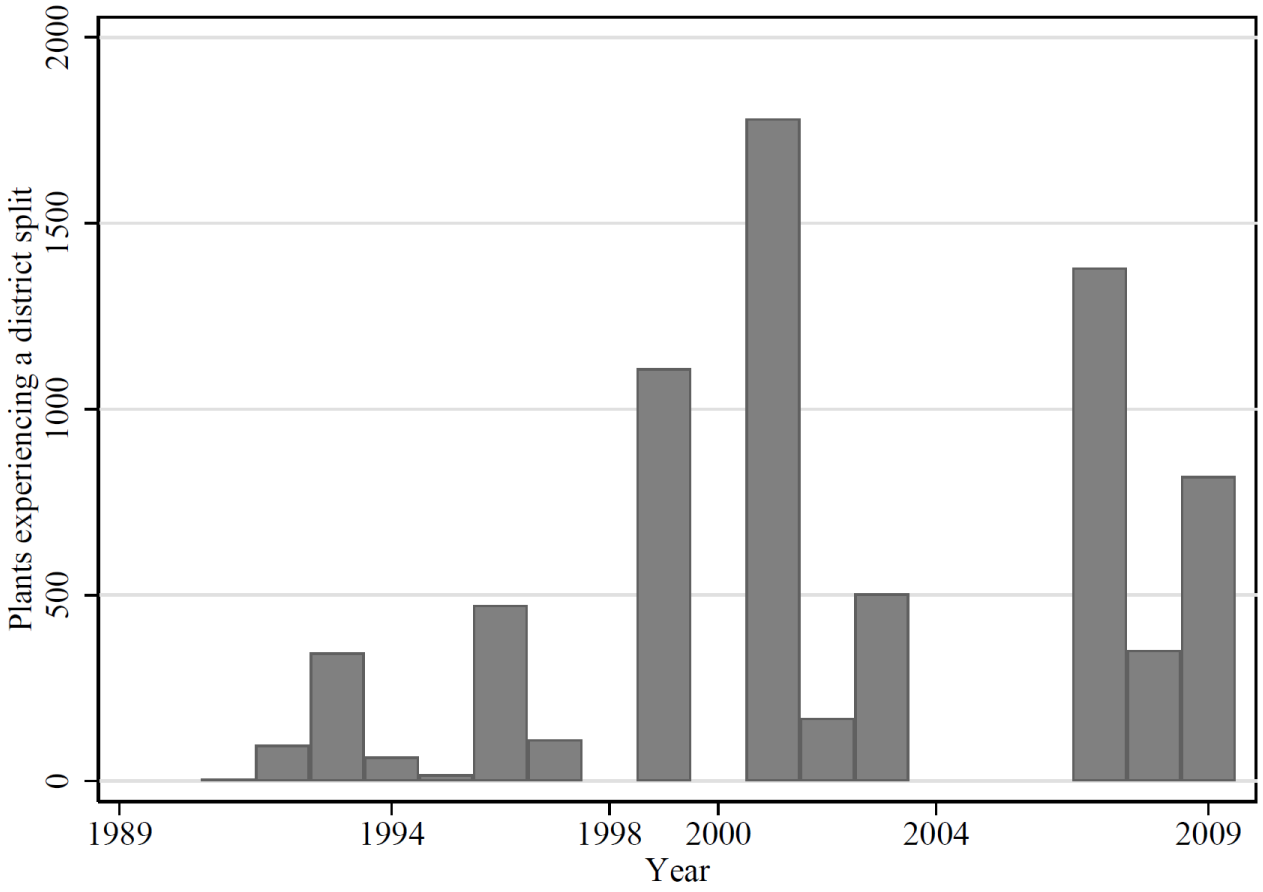
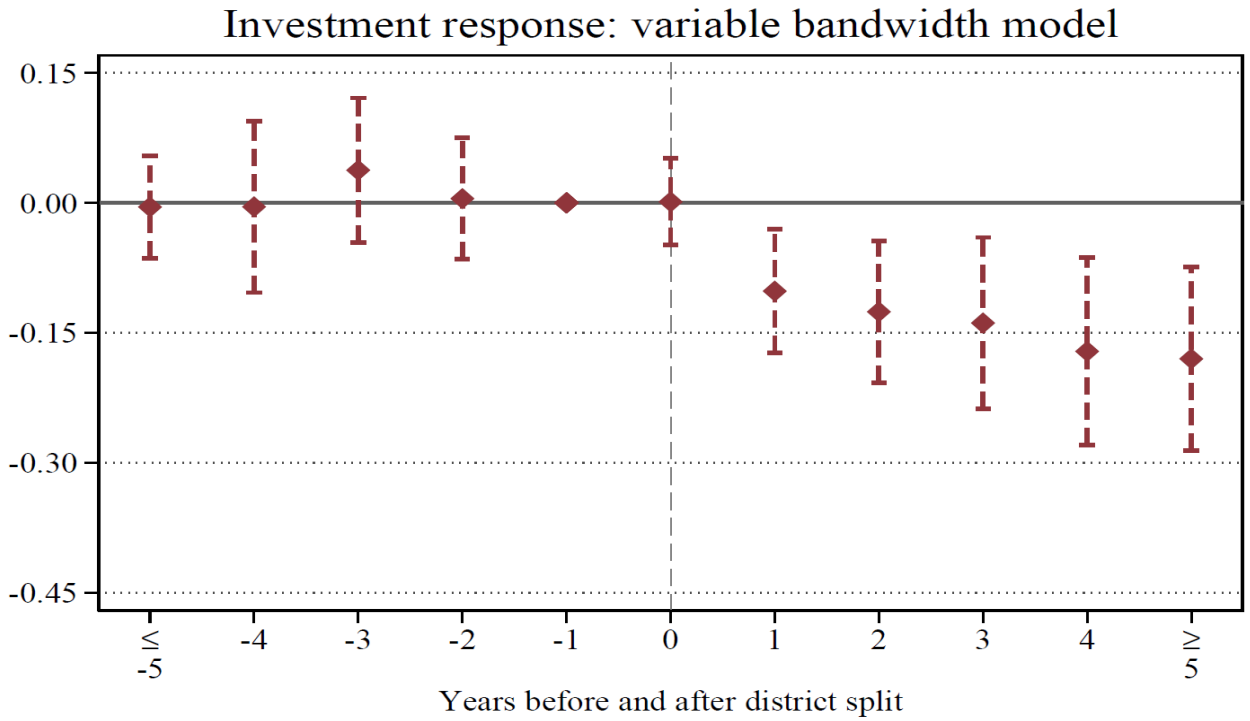
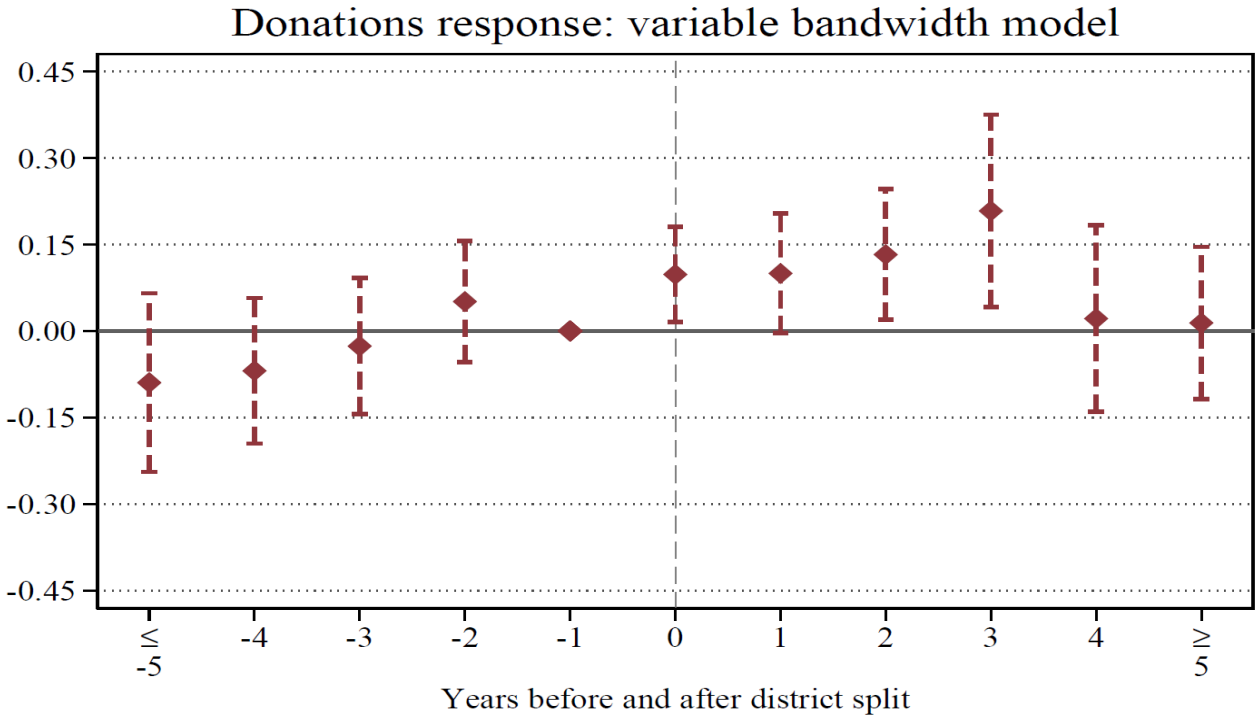


Figure 3
District splits event study graphs



Note: Spikes are 90% confidence bands



Note: Spikes are 90% confidence bands

Figure 4

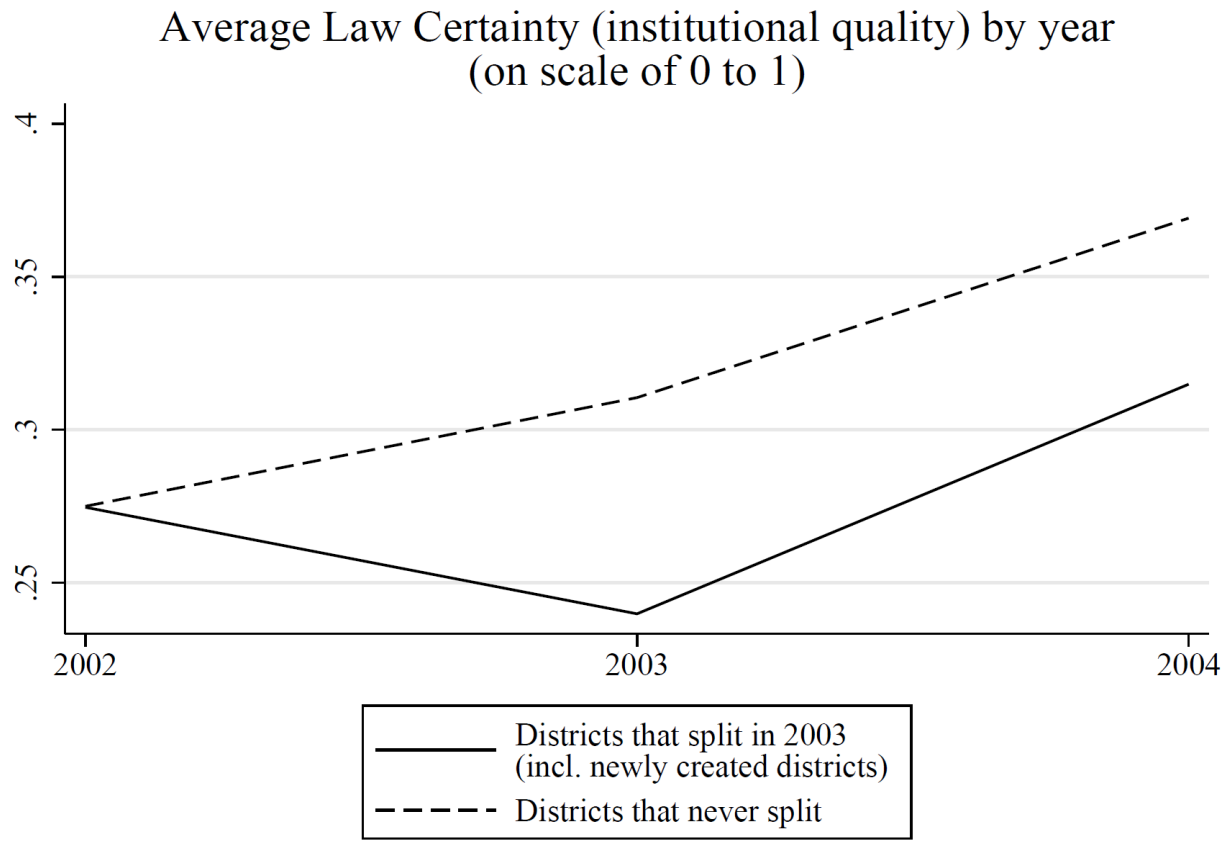

Average Law Certainty (institutional quality) by year
(on scale of 0 to 1)
.4
.35
.3
.25
2002
2003
2004
Districts that split in 2003
(incl. newly created districts)
Districts that never split

Table 1
Firm-level analysis: drop in investment rate

Dependent variable →	I/K							
		[1] + Q-model of investment	[1] + split years 1999 and 2001 only	[1] + excluding SOEs and second splits	[4] + dropping anticipation	[5] + cluster on 1989 districts	[5] + cluster on pre-split districts	[7] + Excluding districts with post moratorium splits and <=2006
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Two years before split	0.035	0.028	-0.039	0.004				
	(0.023)	(0.023)	(0.029)	(0.033)				
One year before split	0.038	0.032	-0.019	-0.001				
	(0.027)	(0.027)	(0.039)	(0.039)				
Year of split	0.009	0.001	-0.082**	-0.001	-0.002	-0.002	-0.002	-0.002
	(0.031)	(0.031)	(0.038)	(0.043)	(0.038)	(0.031)	(0.035)	(0.035)
One year after split	**-0.060***	**-0.068****	**-0.102****	**-0.104****	**-0.104*****	**-0.104*****	**-0.104*****	**-0.107*****
	(0.031)	(0.031)	(0.046)	(0.041)	(0.037)	(0.038)	(0.039)	(0.039)
Two years after split	**-0.061****	**-0.068****	**-0.124*****	**-0.127*****	**-0.128*****	**-0.128*****	**-0.128*****	**-0.130*****
	(0.030)	(0.030)	(0.043)	(0.039)	(0.036)	(0.039)	(0.041)	(0.041)
Three+ years after split	**-0.098*****	**-0.115*****	**-0.203*****	**-0.165*****	**-0.166*****	**-0.166*****	**-0.166*****	**-0.182*****
	(0.031)	(0.031)	(0.048)	(0.044)	(0.041)	(0.053)	(0.051)	(0.052)
Local democracy period (mayor)	0.025*	0.029**	0.006	0.016	0.016	0.016	0.016	0.017
	(0.013)	(0.013)	(0.013)	(0.016)	(0.016)	(0.016)	(0.017)	(0.017)
Output/K		0.039***						
		(0.002)						
Cashflow/K		0.022***						
		(0.006)						
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
4-digit-industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	102,581	102,581	56,317	75,999	75,999	75,999	75,999	75,448
Clusters	1556	1556	878	1462	1462	235	291	291
R-squared	0.522	0.534	0.590	0.529	0.529	0.529	0.529	0.528
F-test leads	1.5783	1.1339	0.9144	0.009301				
F-test leads, p-value	0.2067	0.3220	0.4011	0.9907				

Notes: Robust standard errors are clustered by plant, 4-digit industry-year. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Table 2

Firm-level analysis: diffirence-in-difference

Dependent variable →	I/K							
		[1] + Q-model of investment	[1] + split years 1999 and 2001 only	[1] + excluding SOEs and second splits	[4] + dropping anticipation	[5] + cluster on 1989 districts	[5] + cluster on pre-split districts	[7] + Excluding districts with post moratorium splits and <=2006
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Two years before split	0.031	0.024	-0.023	0.006				
	(0.023)	(0.022)	(0.030)	(0.033)				
One year before split	0.036	0.030	-0.002	0.005				
	(0.026)	(0.026)	(0.040)	(0.039)				
Year of split and after (diff-in-diff)	**-0.056****	**-0.068*****	**-0.100*****	**-0.100*****	**-0.102*****	**-0.102*****	**-0.102*****	**-0.106*****
	(0.026)	(0.026)	(0.037)	(0.035)	(0.031)	(0.031)	(0.034)	(0.034)
Local democracy period (mayor)	0.027**	0.031**	0.009	0.017	0.017	0.017	0.017	0.018
	(0.013)	(0.013)	(0.013)	(0.016)	(0.016)	(0.018)	(0.017)	(0.017)
Output/K		0.039***						
		(0.002)						
Cashflow/K		0.022***						
		(0.006)						
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
4-digit-industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	102,581	102,581	56,317	75,999	75,999	75,999	75,999	75,448
Clusters	1556	1556	878	1462	1462	235	291	291
R-squared	0.522	0.534	0.589	0.529	0.529	0.529	0.529	0.528
F-test leads	1.3894	0.9277	0.3857	0.01964				
F-test leads, p-value	0.2495	0.3957	0.6801	0.9806				

Notes: Robust standard errors are clustered by plant, 4-digit industry-year. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Table 3
Plant-level analysis: donations

Dependent variable →	log Donations				
Sample →	excl. SOEs & only districts that split once or never				
	[1]	[2]	[3]	[3a]	[3b]
Two years before split	0.081		0.078		
	(0.067)		(0.067)		
One year before split	0.109		0.107		
	(0.083)		(0.082)		
Year of split and after (diff-in-diff)			**0.121***	0.083	**0.170****
			(0.067)	(0.053)	(0.075)
Year of split	**0.143****	**0.103****			
	(0.066)	(0.050)			
One year after split	0.109	0.069			
	(0.073)	(0.060)			
Two years after split	**0.153****	**0.114***			
	(0.076)	(0.066)			
Three+ years after split	0.104	0.062			
	(0.088)	(0.077)			
Local democracy period (mayor)	0.039	0.040	0.039	0.040	0.049*
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Local democracy period (mayor) * Year of split and after					-0.119
					(0.075)
Employment and capital decile dummies	Yes	Yes	Yes	Yes	Yes
Pre-split district clustering	Yes	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes	Yes
4-digit-industry-year FE	Yes	Yes	Yes	Yes	Yes
Observations	57,418	57,418	57,418	57,418	57,418
Clusters	278	278	278	278	278
R-squared	0.832	0.831	0.832	0.831	0.831
F-test leads	1.0127		0.9771		
F-test leads, p-value	0.3646		0.3777		

Notes: Robust standard errors are clustered by plant, 4-digit industry-year, and pre-split district. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Table 4
Plant-level analysis: other outcomes

Dependent variable →	I/K	Employment growth	Change in % exported	Export growth	Output growth
	[1]	[2]	[3]	[4]	[5]
Year of split	-0.002	0.015	0.005	0.068	-0.041*
	(0.035)	(0.014)	(0.017)	(0.108)	(0.023)
One year after split	**-0.107*** **	0.003	0.001	0.053	-0.001
	(0.039)	(0.012)	(0.008)	(0.096)	(0.028)
Two years after split	**-0.130*** **	-0.005	-0.014*	0.144	-0.031
	(0.041)	(0.012)	(0.008)	(0.115)	(0.020)
Three+ years after split	**-0.182*** **	-0.003	0.006	0.096	-0.029
	(0.052)	(0.011)	(0.007)	(0.122)	(0.020)
Local democracy period (mayor)	0.017	-0.000	0.006	-0.079	-0.042***
	(0.017)	(0.005)	(0.005)	(0.051)	(0.014)
Plant FE					
4-digit-industry-year FE					
Observations	75,448	75,448	75,448	7,322	70,514
Clusters	291	291	291	199	291
R-squared	0.528	0.148	0.127	0.299	0.174

Notes: Robust standard errors are clustered by plant, 4-digit industry-year, and pre-split district. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Table 5
Plant-level analysis: heterogeneity in plant-level time-invariant characteristics

Dependent variable →	I/K				
Interaction variable →	-	Traded sector	Ever Import	Ever Export	Ever foreign
Sample →	excl. SOEs & only districts that split once or never				
			Domestic plants		
	[1]	[2]	[3]	[4]	[5]
Year of split	-0.002	0.076	0.078	0.064	0.014
	(0.035)	(0.047)	(0.048)	(0.049)	(0.038)
One year after split	**-0.107*** **	0.027	-0.028	-0.036	**-0.073***
	(0.039)	(0.047)	(0.056)	(0.051)	(0.042)
Two years after split	**-0.130*** **	-0.018	-0.061	-0.050	**-0.120*** **
	(0.041)	(0.044)	(0.054)	(0.044)	(0.042)
Three+ years after split	**-0.182*** **	-0.017	-0.085	-0.086	**-0.184*** **
	(0.052)	(0.053)	(0.066)	(0.053)	(0.051)
Year of split * interaction variable		**-0.154*** **	**-0.162****	**-0.129***	-0.126
		(0.056)	(0.069)	(0.068)	(0.122)
One year after split * interaction variable		**-0.237*** **	**-0.131***	**-0.126***	**-0.276***
		(0.069)	(0.068)	(0.066)	(0.142)
Two years after split * interaction variable		**-0.200*** **	**-0.149****	**-0.185****	-0.082
		(0.068)	(0.069)	(0.074)	(0.115)
Three+ years after split * interaction variable		**-0.295*** **	**-0.238*** **	**-0.240*** **	-0.019
		(0.077)	(0.074)	(0.085)	(0.104)
Direct effect interaction variable		0.055			
		(0.047)			
Local democracy period (mayor)	0.017	0.017	0.012	0.014	0.017
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Plant FE	Yes	Yes	Yes	Yes	Yes
4-digit-industry-year FE	Yes	Yes	Yes	Yes	Yes
Observations	75,448	74,429	68,961	69,634	75,448
Clusters	291	289	285	285	291
R-squared	0.528	0.531	0.532	0.533	0.528

Notes: Robust standard errors are clustered by plant, 4-digit industry-year, and pre-split district. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively. Significant marginal effects of significant interaction terms are highlighted in bold.

Table 6

Plant-level heterogeneity: matching results

Dependent variable →	I/K					log donations				
log change from t-1 to →	t	t+1	t+2	t+3	t+4	t	t+1	t+2	t+3	t+4
Panel A: Exporters	[1]	[2]	[3]	[4]	[5]	[1]	[2]	[3]	[4]	[5]
Year of split	0.117	0.045	-0.028	-0.000	-0.054	**0.147***	0.191	0.249	0.171	0.088
	(0.073)	(0.075)	(0.034)	(0.043)	(0.061)	(0.085)	(0.123)	(0.174)	(0.210)	(0.229)
Year of split * **% exported t-1**	**-0.364*****	**-0.320***	**-0.242****	-0.166	-0.051	-0.016	-0.088	**-0.791****	-0.381	0.032
	(0.126)	(0.165)	(0.116)	(0.139)	(0.204)	(0.319)	(0.236)	(0.343)	(0.563)	(0.527)
Marinal effect for % exported 79.1%	***-0.170****	***-0.209****	***-0.220*****	*-0.131*	*-0.094*	*0.135*	*0.121*	*-0.377*	*-0.131*	*0.113*
	(0.068)	*(0.116)*	*(0.080)*	*(0.101)*	*(0.144)*	*(0.266)*	*(0.168)*	*(0.251)*	*(0.468)*	*(0.414)*
Observations	1,177	1,086	1,053	957	779	960	948	929	840	759
R-squared	0.033	0.018	0.014	0.012	0.004	0.007	0.009	0.017	0.004	0.001
Panel B: Importers	[1]	[2]	[3]	[4]	[5]	[1]	[2]	[3]	[4]	[5]
Year of split	0.067	0.035	-0.040	0.006	0.004	0.083	0.102	0.037	0.023	0.003
	(0.056)	(0.065)	(0.040)	(0.046)	(0.063)	(0.097)	(0.119)	(0.150)	(0.194)	(0.217)
Year of split * **% imported t-1**	-0.103	-0.440	-0.280	-0.340	**-0.669***	**0.700****	0.845	0.962	0.976	0.987
	(0.214)	(0.362)	(0.213)	(0.215)	(0.361)	(0.332)	(0.650)	(0.794)	(1.245)	(1.149)
Marinal effect for % imported 29.9%	*0.037*	*-0.097*	***-0.123****	***-0.095****	***-0.195****	***0.292****	***0.354****	*0.324*	*0.315*	*0.297*
	(0.082)	*(0.111)*	*(0.048)*	*(0.057)*	*(0.095)*	*(0.132)*	*(0.179)*	*(0.276)*	*(0.412)*	*(0.371)*
Observations	1,177	1,086	1,053	957	779	960	948	929	840	759
R-squared	0.019	0.017	0.012	0.015	0.018	0.012	0.015	0.015	0.006	0.005

Notes: All regressions control for % exported at t-1 and % imported at t-1. Robust standard errors are clustered by plant, 2-digit industry-year, and pre-split district. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Table 6 (continued)

Plant-level heterogeneity: matching results

Dependent variable →	**log imports +1**					**log exports +1**				
log change from t-1 to →	t	t+1	t+2	t+3	t+4	t	t+1	t+2	t+3	t+4
Panel A: Exporters	[1]	[2]	[3]	[4]	[5]	[1]	[2]	[3]	[4]	[5]
Year of split	-0.117	-0.167	0.025	0.176	0.073	0.459	0.214	0.216	0.099	-0.133
	(0.237)	(0.274)	(0.310)	(0.390)	(0.544)	(0.592)	(0.583)	(0.196)	(0.241)	(0.400)
Year of split *** % exported t-1**	-0.272	**-1.083****	**-1.590****	**-1.781****	-1.291	1.517	1.157	0.800	-0.388	-2.989
	(0.321)	(0.538)	(0.624)	(0.700)	(1.038)	(1.427)	(1.340)	(1.645)	(1.158)	(2.549)
Marinal effect for % exported 79.1%	***-0.332****	***-1.024*****	***-1.232*****	***-1.233****	***-0.949****	*1.659*	*1.129*	*0.849*	*-0.208*	*-2.497*
	(0.143)	*(0.396)*	*(0.421)*	*(0.511)*	*(0.518)*	*(1.285)*	*(0.949)*	*(1.250)*	*(0.967)*	*(1.889)*
Observations	1,177	1,177	1,177	1,089	990	1,177	1,177	1,177	1,089	990
R-squared	0.015	0.013	0.018	0.019	0.019	0.127	0.162	0.205	0.141	0.220
Panel B: Importers	[1]	[2]	[3]	[4]	[5]	[1]	[2]	[3]	[4]	[5]
Year of split	-0.050	-0.273	-0.091	-0.232	-0.251	0.734	0.433	-0.021	-0.236	**-0.837****
	(0.211)	(0.303)	(0.298)	(0.322)	(0.433)	(0.454)	(0.352)	(0.299)	(0.340)	(0.419)
Year of split *** % imported t-1**	**-1.144****	-0.748	-1.495	1.075	0.998	-0.248	-0.275	**3.764****	2.719	1.858
	(0.466)	(0.925)	(1.973)	(1.045)	(1.364)	(2.575)	(2.739)	(1.895)	(1.773)	(2.241)
Marinal effect for % imported 29.9%	***-0.391****	***-0.497****	*-0.538*	*0.089*	*0.047*	*0.660*	*0.351*	***1.103****	*0.576*	*-0.282*
	(0.195)	*(0.219)*	*(0.513)*	*(0.460)*	*(0.486)*	*(1.124)*	*(1.030)*	*(0.526)*	*(0.529)*	*(0.802)*
Observations	1,177	1,177	1,177	1,089	990	1,177	1,177	1,177	1,089	990
R-squared	0.017	0.011	0.016	0.016	0.018	0.125	0.161	0.209	0.144	0.216

Notes: All regressions control for % exported at t-1 and % imported at t-1. Robust standard errors are clustered by plant, 2-digit industry-year, and pre-split district. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Table 7

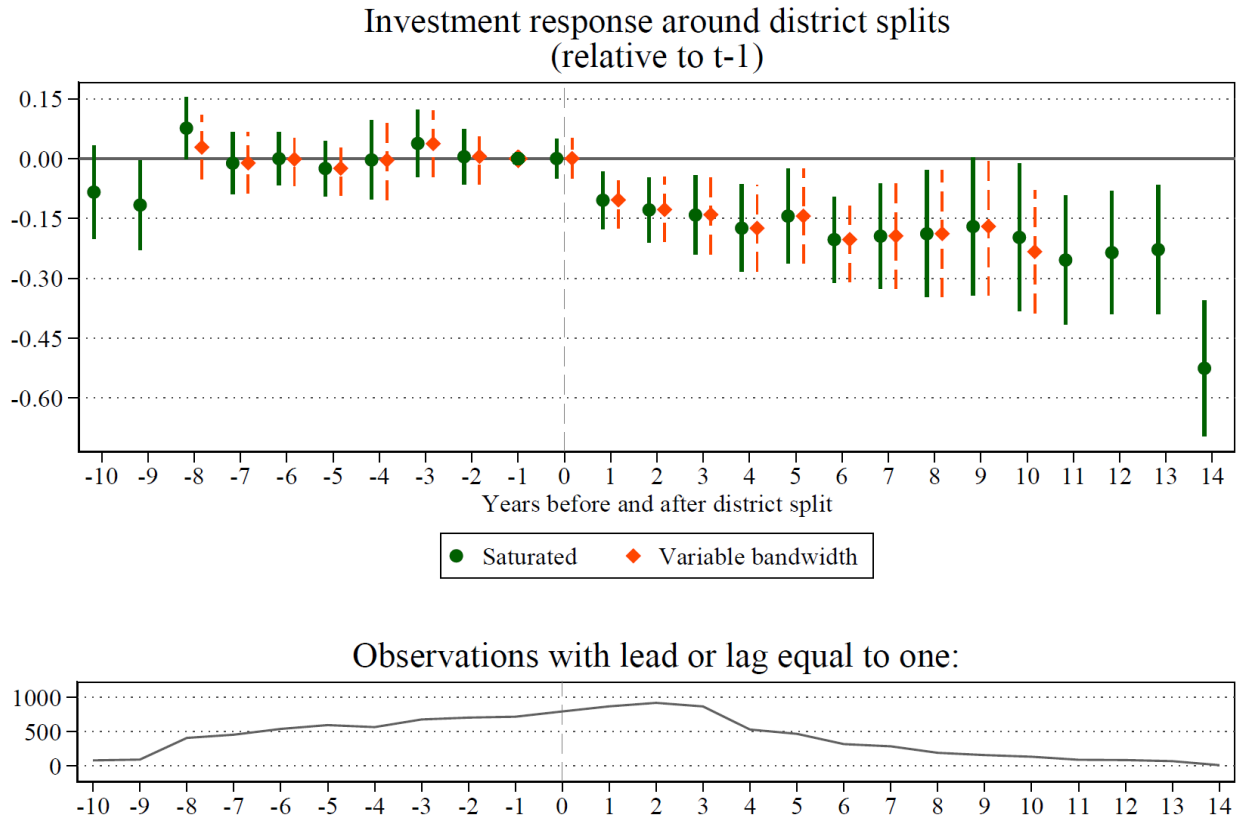
Plant-level analysis: initial institutional quality

	Years 2002-2003, splits in 2003					
Interaction variable (t= 2002) →		Law Certainty				
...of which →			Consistency of Regulations 28%	Law Enforcement 44%	Illegal Levy outside Bureaucracy 15%	Executive-Legislative Relations 13%
Panel A: Dependent variable is **log donations**		(mean=0.32)	(mean=0.37)	(mean=0.24)	(mean=0.21)	(mean=0.39)
	[1]	[2]	[3]	[4]	[5]	[6]
Split (diff-in-diff)	0.276**	0.587***	0.501**	0.305**	0.530**	2.388***
	(0.128)	(0.203)	(0.228)	(0.129)	(0.247)	(0.555)
Split (diff-in-diff) * interaction variable		-4.297**	-2.390	-7.099***	-3.353	-5.888***
		(1.749)	(1.840)	(1.721)	(2.581)	(1.428)
Marginal effect of split at mean of interaction		***-0.795****		***-1.411*****		*0.065*
		(0.421)		*(0.370)*		*(0.126)*
Employment and capital decile dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,329	3,329	3,329	3,329	3,329	3,329
R-squared	0.960	0.960	0.960	0.960	0.960	0.960
Clusters	74	74	74	74	74	74
Panel B: Dependent variable is **I/K**						
	[7]	[8]	[9]	[10]	[11]	[12]
Split (diff-in-diff)	-0.047**	-0.103***	-0.116***	-0.056***	-0.120***	0.120**
	(0.023)	(0.033)	(0.041)	(0.020)	(0.035)	(0.057)
Split (diff-in-diff) * interaction variable		0.719**	0.720*	0.780***	0.936**	-0.465***
		(0.360)	(0.382)	(0.285)	(0.367)	(0.151)
Marginal effect of split at mean of interaction		*0.123*	*0.145*	***0.126****	*0.074*	***-0.061****
		(0.088)	*(0.104)*	*(0.066)*	*(0.051)*	*(0.026)*
Observations	4,274	4,274	4,274	4,274	4,274	4,274
R-squared	0.943	0.943	0.943	0.943	0.943	0.943
Clusters	78	78	78	78	78	78

Notes: Robust standard errors are clustered by plant, 4-digit industry-year, and pre-split district. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively. See Table TA5 for detailed categories of institutions. All regressions control for plant FE and 4-digit-industry-year FE, and for the period of local democracy.

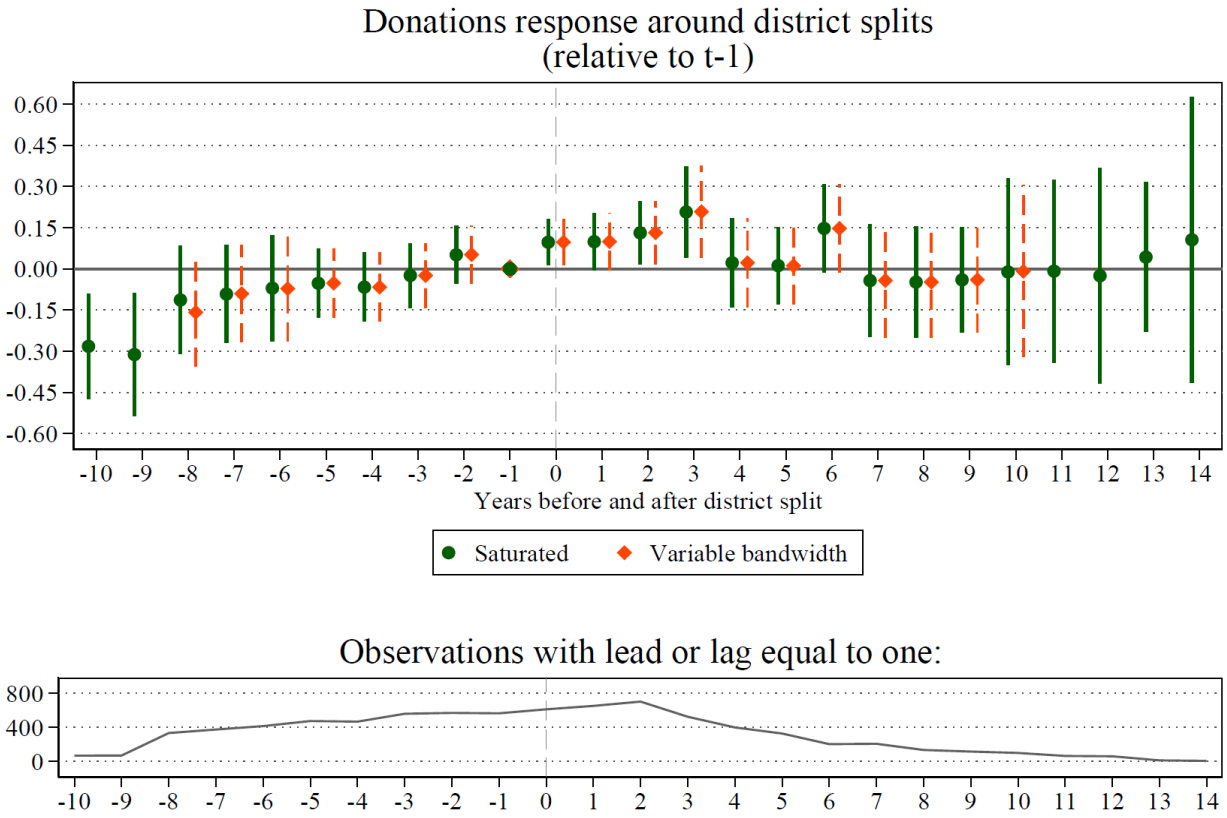
Appendix

Appendix Figure 1
Investment: District splits event study graphs, longer horizon



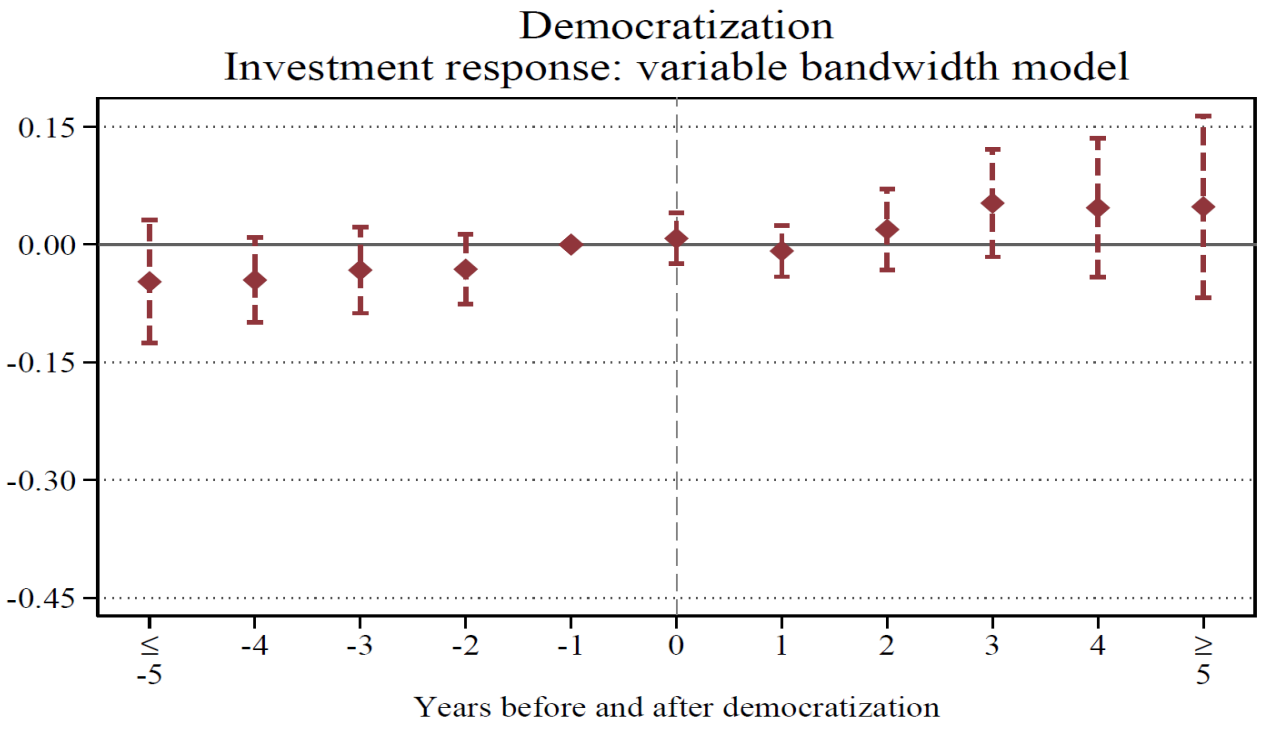
Note: Spikes are 90% confidence bands

Appendix Figure 2
Donations: District splits event study graphs, longer horizon

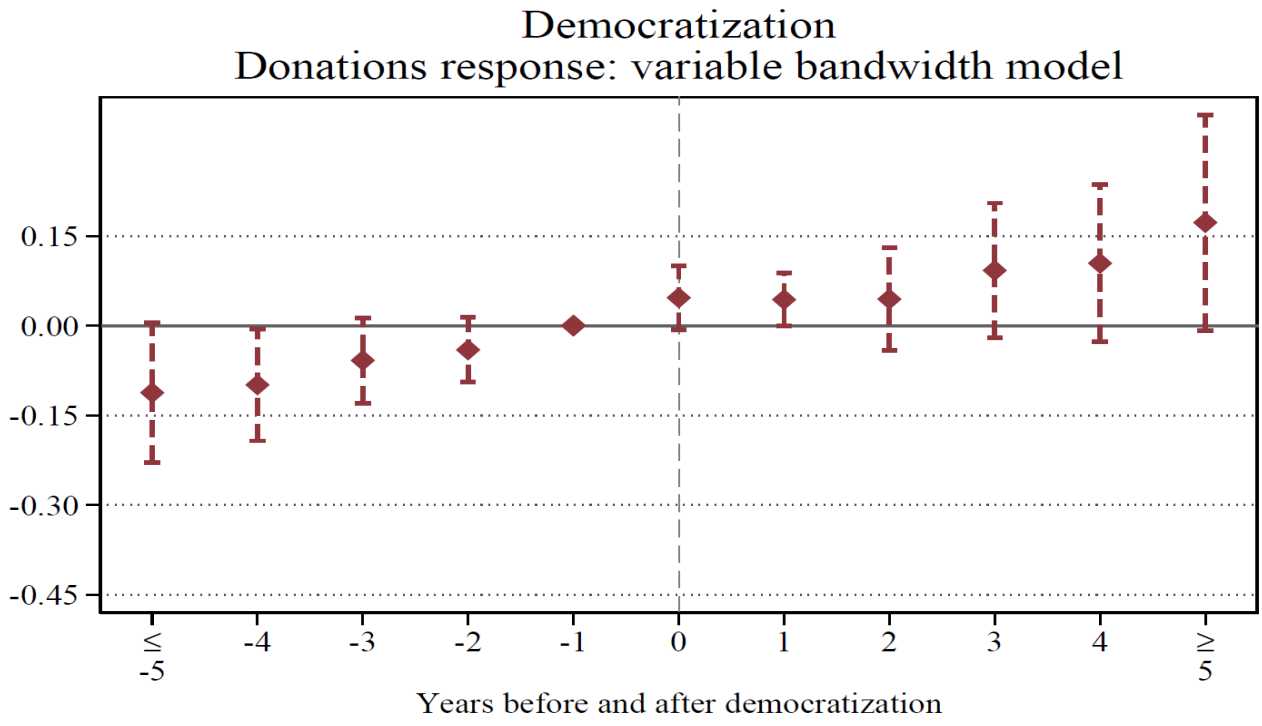


Note: Spikes are 90% confidence bands

Appendix Figure 3
Democratization event study graphs



Note: Spikes are 90% confidence bands



Note: Spikes are 90% confidence bands

Table A2
Robustness of firm-level analysis

Dependent variable →	I/K		
Panel A	pure Q-model of investment	Interaction with Q-model of investment	cluster on 2-digit industry-year FE
	[1]	[2]	[3]
Year of split		0.008	-0.002
		(0.046)	(0.039)
One year after split		-0.023	-0.104***
		(0.041)	(0.038)
Two years after split		-0.055	-0.128***
		(0.042)	(0.035)
Three+ years after split		-0.102**	-0.166***
		(0.045)	(0.042)
Local democracy period (mayor)	0.022	0.020	0.016
	(0.015)	(0.015)	(0.015)
output/K	0.041***	0.043***	
	(0.003)	(0.003)	
cashflow/K	0.029***	0.029***	
	(0.007)	(0.007)	
Year of split * output/K		-0.005	
		(0.014)	
One year after split * output/K		-0.028***	
		(0.010)	
Two years after split * output/K		-0.025**	
		(0.011)	
Three+ years after split * output/K		-0.023***	
		(0.007)	
Excluding SOEs and second splits	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes
4-digit-industry-year FE	Yes	Yes	Yes
Observations	75,999	75,999	75,999
Clusters	1462	1462	307
R-squared	0.543	0.544	0.529

	I/K			
Panel B	Excluding districts with post moratorium splits and <=2006 & incl. SOEs **& Separate second split**	Excluding districts with post moratorium splits and <=2006 & Separate second split **& Excluding SOEs**	Excluding districts with post moratorium splits and <=2006 & incl. 2nd splits **& Excluding SOEs**	Anticipation of post moratorium (2007) splits versus never split & cluster on pre-split districts
	[4]	[5]	[6]	[7]
Two years before split	-0.001	-0.002	0.002	-0.086**
	(0.041)	(0.042)	(0.041)	(0.036)
One year before split	-0.007	-0.009	0.005	-0.129*
	(0.027)	(0.038)	(0.040)	(0.068)
Year of split	-0.014	-0.013	-0.007	-0.215**
	(0.043)	(0.044)	(0.043)	(0.090)
One year after split	-0.089**	-0.108**	-0.097**	
	(0.036)	(0.043)	(0.044)	
Two years after split	-0.085**	-0.130***	-0.127***	
	(0.035)	(0.039)	(0.040)	
Three+ years after split	-0.152***	-0.186***	-0.178***	
	(0.043)	(0.048)	(0.048)	
Local democracy period (mayor)	0.026*	0.017	0.018	(no variation)
	(0.016)	(0.017)	(0.017)	
output/K				
cashflow/K				
Two years before second split	0.051	0.072		
	(0.148)	(0.211)		
One year before second split	0.334	0.363		
	(0.256)	(0.256)		
Year of second split	-0.031	-0.010		
	(0.154)	(0.174)		
One year after second split	-0.136	-0.063		
	(0.207)	(0.253)		
Two years after second split	-0.305	-0.293		
	(0.371)	(0.437)		
Three+ years after second split	-0.248	-0.208		
	(0.223)	(0.276)		
Excluding SOEs and second splits	No	No	No	Yes
Plant FE	Yes	Yes	Yes	Yes
4-digit-industry-year FE	Yes	Yes	Yes	Yes
Observations	101,106	75,575	75,607	13,278
Clusters	328	296	296	194
R-squared	0.521	0.527	0.527	0.881
F-test leads	0.03691	0.02825	0.008243	3.2954
F-test leads, p-value	0.9638	0.9721	0.9918	0.03915

Notes: Robust standard errors are clustered by plant, 4-digit industry-year. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Table A2 (continued)
Robustness of firm-level analysis

Dependent variable →	I/K						
	Pre-moratorium splits & cluster on pre-split districts	[1] with natural resource controls	[1] with natural resource controls, split years 1999 and 2001	[1] with natural resource and population controls, split years 1999 and 2001	[1] + cluster on pre-split districts + using first district revenue availability as split timing up to 1 year later	[1] + cluster on pre-split districts + using first district revenue availability as split timing up to 2 years later	[1] + cluster on pre-split districts + using first district revenue availability as split timing up to 3 years later
	[8]	[9]	[10]	[11]	[12]	[13]	[14]
Two years before split	-0.004	0.009	-0.055	-0.045	0.020	0.020	0.021
	(0.038)	(0.033)	(0.040)	(0.040)	(0.045)	(0.045)	(0.045)
One year before split	0.003	-0.004	-0.039	-0.032	-0.006	-0.020	-0.018
	(0.029)	(0.039)	(0.053)	(0.056)	(0.039)	(0.039)	(0.039)
Year of split	-0.020	-0.013	-0.094*	-0.118**	-0.043	-0.059*	-0.057
	(0.040)	(0.044)	(0.052)	(0.058)	(0.038)	(0.035)	(0.035)
One year after split	-0.110***	-0.104**	-0.126**	-0.163**	-0.113***	-0.113***	-0.109***
	(0.040)	(0.042)	(0.053)	(0.063)	(0.039)	(0.040)	(0.040)
Two years after split	-0.120***	-0.127***	-0.169***	-0.202***	-0.116***	-0.127***	-0.124***
	(0.044)	(0.039)	(0.054)	(0.063)	(0.040)	(0.040)	(0.040)
Three+ years after split	-0.207***	-0.172***	-0.229***	-0.266***	-0.167***	-0.161***	-0.158***
	(0.060)	(0.045)	(0.059)	(0.071)	(0.049)	(0.049)	(0.049)
Local democracy period (mayor)	0.027*	0.018	-0.000	-0.002	0.015	0.016	0.016
	(0.015)	(0.016)	(0.016)	(0.016)	(0.017)	(0.017)	(0.017)
Resources 1990 * Δ world price index		0.016	0.008	0.006			
		(0.036)	(0.048)	(0.048)			
Resources 1990 * Δ world price index t-1		0.000	0.000	-0.063			
		(0.007)	(0.009)	(0.045)			
Oil 1990 * Δ world price index		-0.037	-0.013	-0.017			
		(0.047)	(0.044)	(0.044)			
Oil 1990 * Δ world price index t-1		0.001	0.003	-0.036			
		(0.005)	(0.007)	(0.078)			
log population				-0.016			
				(0.063)			
log population t-1				0.002			
				(0.046)			
log population t-2				-0.058			
				(0.048)			
Excluding SOEs and second splits	No	Yes	Yes	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
4-digit-industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	82,252	74,742	41,918	38,370	75,999	75,995	75,994
Clusters	308	1454	823	792	290	290	290
R-squared	0.527	0.533	0.597	0.600	0.529	0.529	0.529
F-test leads	0.01360	0.07099	0.9332	0.6617	0.1651	0.4222	0.3974
F-test leads, p-value	0.9865	0.9315	0.3937	0.5163	0.8479	0.6560	0.6724

Notes: Robust standard errors are clustered by plant, 4-digit industry-year. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Table A3
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	OSRV (Own Source Revenue)	OTHR (Other Revenue)	TXRV (Tax Revenue Sharing)
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]
Two years before split	0.029										
	(0.022)										
One year before split	-0.032	-0.030									
	(0.025)	(0.021)									
Year of split	-0.061**	-0.062**	-0.051*	-0.079***	-0.070**	-0.018**	-0.001	0.024**	0.024***	-0.009**	-0.013***
	(0.031)	(0.029)	(0.027)	(0.028)	(0.028)	(0.008)	(0.015)	(0.010)	(0.004)	(0.004)	(0.005)
One year after split	-0.084**	-0.080**	-0.084**	-0.106***	-0.107***	0.001	-0.039**	0.021**	0.024***	0.002	-0.005
	(0.039)	(0.038)	(0.037)	(0.036)	(0.038)	(0.012)	(0.017)	(0.010)	(0.005)	(0.003)	(0.006)
Two years after split	-0.135***	-0.136***	-0.135***	-0.139***	-0.151***	-0.027***	-0.031*	0.015	0.024***	0.004	-0.002
	(0.034)	(0.033)	(0.033)	(0.040)	(0.041)	(0.009)	(0.018)	(0.009)	(0.005)	(0.004)	(0.006)
Three+ years after split	-0.109***	-0.109***	-0.114***	-0.088**	-0.091**	-0.028***	-0.044**	0.029**	0.036***	-0.009***	0.005
	(0.038)	(0.036)	(0.033)	(0.044)	(0.046)	(0.010)	(0.022)	(0.012)	(0.006)	(0.003)	(0.008)
Local democracy period (mayor)	-0.010	-0.010	-0.008	0.007	0.006	0.000	-0.001	0.003	-0.002	0.004	-0.002
	(0.022)	(0.021)	(0.021)	(0.023)	(0.023)	(0.007)	(0.009)	(0.004)	(0.003)	(0.003)	(0.003)
log population	0.477***	0.470***	0.460***	0.509***	0.511***	-0.027***	0.017	-0.014*	0.001	0.003	0.010
	(0.032)	(0.030)	(0.029)	(0.043)	(0.044)	(0.010)	(0.019)	(0.007)	(0.007)	(0.003)	(0.008)
Year of split * New breakaway district					-0.240						
					(0.186)						
One year after split * New breakaway district					-0.017						
					(0.107)						
Two years after split * New breakaway district					0.058						
					(0.099)						
Three+ years after split * New breakaway district					0.009						
					(0.081)						
1989 District, and year FE	Yes	Yes	Yes	No	No	No	No	No	No	No	No
District, and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations (district-years)	4,099	4,503	4,906	4,896	4,896	4,752	4,812	4,720	4,839	4,819	4,782
Number of clusters	273	273	274	428	428	427	428	425	426	427	427
R-squared	0.966	0.969	0.968	0.973	0.973	0.833	0.798	0.710	0.773	0.673	0.654
Sum of coefficient of leads	-0.002										
F-test leads	4.1784	2.0157									
F-test leads, p-value	0.01632	0.1568									

Notes: Robust standard errors are clustered by pre-split district. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Sample includes district years **with and without** firm information (less measurement error than using manuf census data only), and excludes 2007, 2008 and 2009 splits

Table A4
District spending and expediture shares

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)	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)
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]	[13]	[14]	[15]	[16]
Year of split	0.022**	-0.031**	-0.016	-0.005	0.035**	0.007	-0.003	-0.015	0.005	-0.006**	-0.005	-0.030***	-0.001	-0.002**	-0.001	-0.001
	(0.011)	(0.013)	(0.011)	(0.010)	(0.016)	(0.006)	(0.004)	(0.014)	(0.006)	(0.003)	(0.008)	(0.009)	(0.001)	(0.001)	(0.001)	(0.001)
One year after split	0.014	-0.014	-0.003	0.003	0.088*	0.017*	-0.000	-0.044	0.001	-0.008	-0.004	-0.013	-0.003	-0.002	-0.001	-0.001
	(0.032)	(0.022)	(0.016)	(0.020)	(0.046)	(0.009)	(0.008)	(0.034)	(0.008)	(0.006)	(0.013)	(0.019)	(0.003)	(0.003)	(0.001)	(0.002)
Two years after split	0.004	0.002	0.002	-0.012	-0.036	-0.010	0.004	-0.028	-0.013	-0.000	0.011	-0.005	0.003**	0.001	0.001	0.001
	(0.028)	(0.040)	(0.027)	(0.014)	(0.066)	(0.009)	(0.003)	(0.020)	(0.010)	(0.005)	(0.009)	(0.037)	(0.001)	(0.002)	(0.002)	(0.001)
Three+ years after split	0.014	0.022	0.018	-0.003	-0.099	-0.002	0.001	0.055	-0.005	0.006	0.019*	0.011	0.001	0.002	0.001	0.002
	(0.031)	(0.043)	(0.028)	(0.014)	(0.075)	(0.010)	(0.005)	(0.033)	(0.011)	(0.006)	(0.011)	(0.040)	(0.001)	(0.002)	(0.003)	(0.001)
Local democracy period (mayor)	-0.023*	-0.027***	-0.001	-0.003	-0.008	-0.001	0.000	-0.027**	0.000	-0.003	-0.008	-0.010	0.003	-0.000	-0.001	-0.000
	(0.013)	(0.008)	(0.009)	(0.007)	(0.016)	(0.003)	(0.005)	(0.010)	(0.002)	(0.002)	(0.005)	(0.009)	(0.003)	(0.001)	(0.002)	(0.001)
log population	-0.070	0.093	0.040	-0.053	-0.055	0.014	0.012	-0.032	-0.027	0.025	0.046	0.053	-0.000	0.011	0.002	-0.008
	(0.105)	(0.072)	(0.079)	(0.062)	(0.199)	(0.025)	(0.015)	(0.098)	(0.028)	(0.027)	(0.035)	(0.076)	(0.008)	(0.010)	(0.006)	(0.006)
District and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations (district-years)	496	478	496	496	498	494	480	462	488	496	496	506	448	458	404	472
Number of districts	248	239	248	248	249	247	240	231	244	248	248	253	224	229	202	236
R-squared	0.916	0.862	0.846	0.783	0.746	0.879	0.744	0.872	0.741	0.838	0.793	0.756	0.739	0.760	0.817	0.740

Notes: Robust standard errors are clustered by pre-split district. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Table A5
Firm-level analysis: sources of investment financing

Dependent variable →	I/K								
of which I financed by →	total	capital markets	foreign investment	foreign loans	government	domestic loan	private	reinvested earnings	stock bonds
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Year of split	-0.002	-0.001	0.004	-0.004	-0.000	-0.004	0.004	0.007	-0.009
	(0.035)	(0.001)	(0.007)	(0.004)	(0.002)	(0.014)	(0.035)	(0.008)	(0.006)
One year after split	-0.107***	0.000	0.001	-0.014*	-0.001	-0.020	-0.017	0.008	-0.014***
	(0.039)	(0.002)	(0.004)	(0.007)	(0.002)	(0.018)	(0.039)	(0.012)	(0.005)
Two years after split	-0.130***	-0.000	0.004	-0.002	0.002	-0.003	-0.040	-0.016	-0.011*
	(0.041)	(0.002)	(0.004)	(0.006)	(0.005)	(0.020)	(0.048)	(0.011)	(0.006)
Three+ years after split	-0.182***	0.000	-0.001	-0.011	-0.003	-0.049**	-0.120***	0.001	-0.008
	(0.052)	(0.002)	(0.006)	(0.008)	(0.002)	(0.022)	(0.046)	(0.011)	(0.005)
Local democracy period (mayor)	0.017	0.000	0.003*	0.000	-0.001	0.004	-0.027*	-0.004	0.000
	(0.017)	(0.000)	(0.002)	(0.002)	(0.001)	(0.006)	(0.015)	(0.005)	(0.001)
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
4-digit-industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	75,448	75,448	75,442	75,447	75,448	75,447	75,397	75,447	75,448
Number of districts	291	291	291	291	291	291	291	291	291
R-squared	0.528	0.390	0.535	0.419	0.461	0.416	0.457	0.418	0.381

Notes: Robust standard errors are clustered by plant, 4-digit industry-year, and pre-split district. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Online Appendix
Not for publication

Table OA1
Plausibly Exogenous Timing of Redistricting Conditional on Observed Fixed Effects

Dependent variable →	Dummy =1 if 1989 district ever split				Number of years to first split of district				Dummy =1 if 1999 district ever split				Number of years to first split of district			
	All districts as of 1991				All districts as of 1991 that subsequently split				All districts as of 1999				All districts as of 1999 that subsequently split			
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]	[13]	[14]	[15]	[16]
Non-oil mineral natural resources	-0.026	-0.026	-0.033	-0.034	-0.031	-0.031	-0.143	-0.090	-0.006	-0.006	-0.016	-0.017	-0.255***	-0.255***	-0.236***	-0.198***
	(0.021)	(0.021)	(0.028)	(0.028)	(0.137)	(0.137)	(0.156)	(0.166)	(0.022)	(0.022)	(0.025)	(0.025)	(0.057)	(0.057)	(0.056)	(0.048)
Oil natural resources	-0.005	-0.005	-0.003	-0.002	1.076**	1.076**	1.287**	1.414***	0.000	0.000	0.013	0.015	-0.176	-0.176	-0.099	-0.164
	(0.063)	(0.063)	(0.055)	(0.055)	(0.524)	(0.524)	(0.531)	(0.505)	(0.055)	(0.049)	(0.044)	(0.044)	(0.390)	(0.393)	(0.397)	(0.440)
log surface area	0.178***	0.178***	0.206***	0.202***	0.214	0.214	0.480	0.420	0.127***	0.127***	0.145***	0.139***	-0.163	-0.163	-0.122	0.033
	(0.013)	(0.013)	(0.013)	(0.014)	(0.652)	(0.652)	(0.674)	(0.639)	(0.013)	(0.014)	(0.019)	(0.019)	(0.398)	(0.416)	(0.399)	(0.424)
% of plants with positive investment	0.065	0.065	0.059	0.062	0.588	0.588	0.993	1.526	-0.138*	-0.138*	-0.136*	-0.108	-0.494	-0.494	-0.178	-0.146
	(0.072)	(0.072)	(0.071)	(0.073)	(1.076)	(1.076)	(1.125)	(1.187)	(0.074)	(0.075)	(0.079)	(0.079)	(0.738)	(0.765)	(0.749)	(0.789)
log manufacturing employment	-0.033**	-0.033**	0.002	-0.003	-0.236	-0.236	-0.497**	-0.506**	-0.025**	-0.025**	-0.027	-0.024	0.182	0.182	0.067	0.019
	(0.013)	(0.013)	(0.016)	(0.016)	(0.229)	(0.229)	(0.248)	(0.244)	(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			-0.137***	-0.126***			1.217	1.303			-0.025	-0.023			0.327	0.564
			(0.036)	(0.039)			(0.794)	(0.809)			(0.044)	(0.044)			(0.542)	(0.603)
% manufacturing employment in plants with FDI				0.117				5.784*				0.505***				1.908
				(0.168)				(3.352)				(0.158)				(1.828)
% manufacturing employment in plants that export				0.138				0.519				0.043				0.616
				(0.202)				(3.407)				(0.116)				(0.997)
% manufacturing employment in state-owned plants				-0.022				3.129				0.232				-1.011
				(0.147)				(1.993)				(0.165)				(0.785)
Observations	238	238	236	236	69	69	69	69	287	287	251	251	74	74	68	68
Standard errors:	robust	clustered on 1989 districts			robust	clustered on 1989 districts			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	0.273	0.273	0.328	0.348	0.068	0.068	0.074	0.127

Notes: ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Table OA2
District revenue and sources Sample: years 1994-2000

Dependent variable →	Revenue source as a share of total revenue					
	DAK (Special Allocation Grant)	DAU (General Allocation Grant)	NRRV (Natural Resource Revenue	OSRV (Own Source Revenue)	OTHR (Other Revenue)	TXRV (Tax Revenue Sharing)
	[1]	[2]	[3]	[4]	[5]	[6]
Year of split	0.011	-0.030	0.006	0.023***	0.006	-0.009
	(0.021)	(0.025)	(0.010)	(0.007)	(0.006)	(0.010)
One year after split	0.068**	-0.071**	0.015	0.021***	-0.000	-0.004
	(0.029)	(0.036)	(0.017)	(0.008)	(0.002)	(0.014)
Two years after split	0.061**	-0.084*	0.006	0.014	0.001	0.025**
	(0.029)	(0.044)	(0.010)	(0.017)	(0.003)	(0.012)
Three+ years after split	0.136***	-0.156**	-0.000	0.038**	-0.005	0.034**
	(0.037)	(0.064)	(0.010)	(0.016)	(0.005)	(0.014)
Local democracy period (mayor)	0.019	-0.018	0.005	-0.007	0.004	0.001
	(0.015)	(0.018)	(0.005)	(0.007)	(0.003)	(0.006)
log population	-0.070***	0.042**	0.011	0.014	0.004	0.012
	(0.016)	(0.017)	(0.009)	(0.010)	(0.003)	(0.009)
District and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations (district-years)	1,818	1,843	1,855	1,842	1,855	1,833
Number of clusters	277	278	278	277	278	278
R-squared	0.588	0.665	0.561	0.869	0.440	0.732

Notes: Robust standard errors are clustered by pre-split district. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Table OA3
District DAK expediture shares, from 2003

Dependent variable →	Expenditure earmarked as a share of total DAK received (Special Allocation Grant)											
	AGR (Agriculture)	EDU (Education)	ENVR (Environment)	FRST (Forestry)	FSH (Fishery)	GOVT (Government Sector)	HE (Health)	INFR (Infrastructure)	INFR_H2O (Infrastructure Sector (Subsect: Water))	INFR_IRIG (Infrastructure Sector (Subsect: Irrigation))	INFR_ROD (Infrastructure Sector (Subsect: Road))	POP (Demographic)
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Year of split		0.050*					-0.046**	-0.147***		-0.039	-0.047*	
		(0.026)					(0.021)	(0.038)		(0.046)	(0.028)	
One year after split		-0.014			-0.021	0.021	-0.043***	-0.021		-0.012	-0.005	
		(0.017)			(0.020)	(0.033)	(0.011)	(0.027)		(0.018)	(0.013)	
Two years after split	-0.011	-0.015			-0.013	-0.044*	-0.014	-0.010	-0.002	-0.005	-0.020	
	(0.009)	(0.014)			(0.013)	(0.026)	(0.010)	(0.021)	(0.004)	(0.016)	(0.013)	
Three+ years after split	0.001	-0.011			0.015	0.084***	-0.001	-0.052*	-0.004	-0.054**	0.001	
	(0.010)	(0.015)			(0.016)	(0.030)	(0.011)	(0.030)	(0.005)	(0.027)	(0.014)	
Local democracy period (mayor)		-0.022					0.015	0.008		0.049	-0.034***	
		(0.017)					(0.010)	(0.061)		(0.077)	(0.009)	
log population	0.063	-0.124***	0.008	-0.050	0.028	-0.055	0.030	0.182***	0.007	0.115	0.065**	0.061
	(0.047)	(0.041)	(0.011)	(0.127)	(0.036)	(0.045)	(0.028)	(0.069)	(0.021)	(0.105)	(0.032)	(0.053)
District and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations (district-years)	1,550	2,199	1,369	70	1,828	351	2,186	2,249	1,666	1,741	2,188	448
Number of districts	409	416	397	35	412	128	417	416	410	362	416	224
R-squared	0.497	0.596	0.686	0.545	0.575	0.871	0.335	0.448	0.469	0.649	0.502	0.701

Notes: Robust standard errors are clustered by pre-split district. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.

Table OA4
Firm-level analysis: is it uncertainty? No

Dependent variable →	I/K		I/K
Worse if district budget unknown?		*Worse if in new breakaway district?*	
	[1]		[2]
Year of split	-0.020	Year of split	-0.017
	(0.039)		(0.037)
One year after split	-0.062	One year after split	-0.107**
	(0.053)		(0.042)
Two years after split	-0.113**	Two years after split	-0.119**
	(0.049)		(0.048)
Three+ years after split	-0.201***	Three+ years after split	-0.169***
	(0.052)		(0.058)
Year of split * budget unknown	0.041	Year of split * New breakaway district	0.046
	(0.066)		(0.085)
One year after split * budget unknown	-0.118	One year after split * New breakaway district	0.005
	(0.073)		(0.098)
Two years after split * budget unknown	-0.049	Two years after split * New breakaway district	-0.027
	(0.053)		(0.079)
Three+ years after split * budget unknown	0.102**	Three+ years after split * New breakaway district	-0.034
	(0.042)		(0.075)
		Year of split * both new	0.169
			(0.230)
		One year after split * both new	-0.102
			(0.179)
		Two years after split * both new	-0.160
			(0.212)
		Three+ years after split * both new	-0.024
			(0.238)
Local democracy period (mayor)	0.019		0.017
	(0.017)		(0.017)
Plant FE	Yes	Plant FE	Yes
4-digit-industry-year FE	Yes	4-digit-industry-year FE	Yes
Observations	75,448	Observations	75,417
Clusters	291	Clusters	291
R-squared	0.528	R-squared	0.528

Notes: Robust standard errors are clustered by plant, 4-digit industry-year, and pre-split district. ***, **, * correspond to the 1%, 5%, and 10% level of significance, respectively.