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Local Government Splits and Economic Activities: Micro-Level Evidence from Indonesia

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Local Government Splits and Economic Activities: Micro-Level Evidence from Indonesia

Esa A. Asyahid*

Abstract

Although local government splits have been widely implemented in developing countries, there is limited empirical evidence on their effects on economic activities. This study investigates the impacts of district splits on household business activities using a rich household-level panel dataset that spans over 20 years and covers an episode of massive district splits in Indonesia. Using a difference-in-differences approach, I found that district splits do not improve non-farm business revenue growth. Instead, they drive more businesses to exit from the industry. On the other hand, district splits improve farm business revenue growth and entry into this industry. However, the growth effect is not driven by productivity improvement as expected, but solely the result of land input expansion, which is likely acquired in unsustainable ways. Additionally, district splits decrease out-migration, aligning with the Tiebout sorting model. Taken together, these findings add another argument for the need to re-evaluate the current practices and regulations on local government splits.

JEL Codes: D13, D73, H77

Keywords: local government splitting, Indonesia, household business, difference-in-differences

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This paper has an online appendix.

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1 Introduction

In a federal or multi-layered polity, in which lower tier or local government has substantial decision-making power, local government plays important roles in shaping the welfare and development of their area and their residents. There are, however, questions on how to optimally balance and structure the central-local government relationships and compositions. Most of the optimal federalism literature focuses on the optimal allocation of power between central and local government, particularly under the discussion of decentralization. Other strands of literature, on the other hand, focus on the compositional aspect, which deals with the optimal number and size of local governments (Pierskalla, 2016).

The discussion of the latter topic becomes not only interesting but also relevant, since local government splits, i.e. increasing the number of local governments by dividing up the existing ones, had been widely implemented in many developing countries (Grossman and Lewis, 2014). Among the hopes of the splits is that it brings the government closer to the people, hence improving public service and publicly provided goods. The skeptics, on the other hand, view that local government splits do more harm than good as it is just an instrument for local elites to extract more resources for their own interest.

In line with these contrasting opinions, the relevant theories on local government splits also predict two opposing impacts on economic activities and public goods provision. On the one hand, local government splits can increase inter-jurisdictional competition in the form of tax competition and sorting of households or firms over their preference of public goods provided (or more well known as Tiebout sorting, after Tiebout (1956)). It could also reduce rent-seeking and corruption by improving direct accountability (Arikan, 2004; Fisman and Gatti, 2002). The redrawing of the border would lead to a more homogeneous preference of the population, thereby reducing the varieties and costs of public goods provisions. Lastly, by reducing the geographic distance to administrative centers, the distribution of public goods would be more effective (Asher et al., 2018).

On the other hand, if the provision of public goods requires fixed costs, local government splits can harm economic activities by inducing diseconomies of scale (Alesina and Spolaore, 1997; Bolton and Roland, 1997; Oates, 1999). In contrast to the hope of reducing rent-seeking, local government splits might in fact increase it if the residents have different abilities to monitor corrupt bureaucrats (Boffa et al., 2016). Furthermore, inter-jurisdictional competition induced by the increasing number of local government might actually boost illegal activities as the competition to lower the costs of illegal activities are also decreasing (Burgess et al., 2012).

Contrasting with these rich theoretical discussions, the empirical evidence of local government splits on economic activities is still scarce. I can only find two studies that examine this issue in developing countries context. The first study is Dahis and Szerman (2021) in Brazil, which finds that local government splits improve public service delivery and economic activities as proxied by nighttime luminosity. They, however, do not find any effect on formal local economic activities which is reflected by the null results on private sector formal employment, number of firms, and local tax revenue. The second study is from Cassidy and Velayudhan (2022) in Indonesia. They find that district splits decrease district economic growth and increase bribery. The driver of this negative growth impact is still unclear because they find null impact on large firms output.

In this study, I investigate the impact of local government splits on household economic activities. Household activities, particularly household businesses deserve important attention because they make up a significant component of developing countries' economies. To achieve this aim, I utilize a rich longitudinal household survey that spans over 20 years. This time range covers an episode of massive district splits in Indonesia following a series of democratization and decentralization reforms. I focus on three sets of outcomes: household migration, entry and exit of household business, and household business growth. The available information allows me to separate two groups of businesses based on sector, namely farm and non-farm businesses. To estimate the average splitting effect on the splitters, I adopt a difference-in-differences approach using an estimator developed by Callaway and Sant'Anna (2021) that overcomes the drawbacks of the standard regression-based two-way fixed effects model. I estimate the dynamic (event study) and static model to allow for examining both the dynamic effect as well as the average treatment effect for over 14 years after splitting.

My analysis yields three main findings. First, district splits decrease household out-migration from the original district. This is in line with the Tiebout (1956) sorting model, in which splitting induces more variation in the combination of price and variety of public service in the original district border hence reducing the need to move to other districts. This is supported by other results, that the decrease is observed only for out-migration which destinations are non-splitters and not visible for out-migration to splitters because the public service price and variety bundles also expanded in the latter.

Second, district splits do not improve household non-farm business activities and instead drive them out of the industry. This is reflected by the null effect on revenue and asset growth and the positive effect on exit rate.

Third, as opposed to non-farm business outcomes, district splits do encourage farm business activities. It improves farm business revenue and asset growth and also drives new households to enter the industry as reflected by the increasing entry rate. However, the revenue growth is fueled solely by land expansion without any sign of productivity improvement. The source of the land is also plausibly from illegal land-clearing activities, which is not an expected mechanism.

This study speaks, in addition to the previously mentioned two studies on the effect of local government splits on economic activities, also to a broader literature on the effect of jurisdictional fragmentation, which is also relatively limited in numbers. Both Lewis (2017) and Singhania (2022) study the effect of district splits in Indonesia on public service delivery. The former uses aggregate level data and finds that splitting does not affect school enrollment but decreases access to water and sanitation. The latter uses more granular village-level data and interacts the splitting variable with direct election. The findings suggest that splitting improves public service delivery, but only when complemented with direct election. Burgess et al. (2012) studies another outcome, namely deforestation, and finds that splitting worsens it by inducing greater rent-seeking competition by allowing more illegal logging. Bazzi and Gudgeon (2021) further finds that splitting induces higher ethnic polarization, which in turn fosters ethnic conflicts. My study adds another argument in favor of the need for reevaluating district splitting.

The rest of the paper is organized as follows. Section 2 provides an institutional context of district splits in Indonesia. Section 3 describes the data used for the analysis, as well as how I construct the sample. Section 4 explains the strategy to identify and estimate the causal effect of splitting. Section 5 presents the results of the estimation and discusses the potential mechanisms. Finally, section 6 concludes.

2 Context

The government of Indonesia is multi-layered with nested hierarchies. In the sub-national level, there are two other main local government layers: provinces which are then subdivided into districts [1]. The majority of districts are rural districts (*kabupaten*) which typically cover

[1]Districts are then subdivided into subdistricts (*kecamatan*) and villages. Subdistricts are merely the districts' apparatus without any decision-making power, while autonomous villages (as opposed to adminis-

large areas with relatively low population density, while around one-fifth of the districts are urban districts (*kota*) which are typically small and dense.

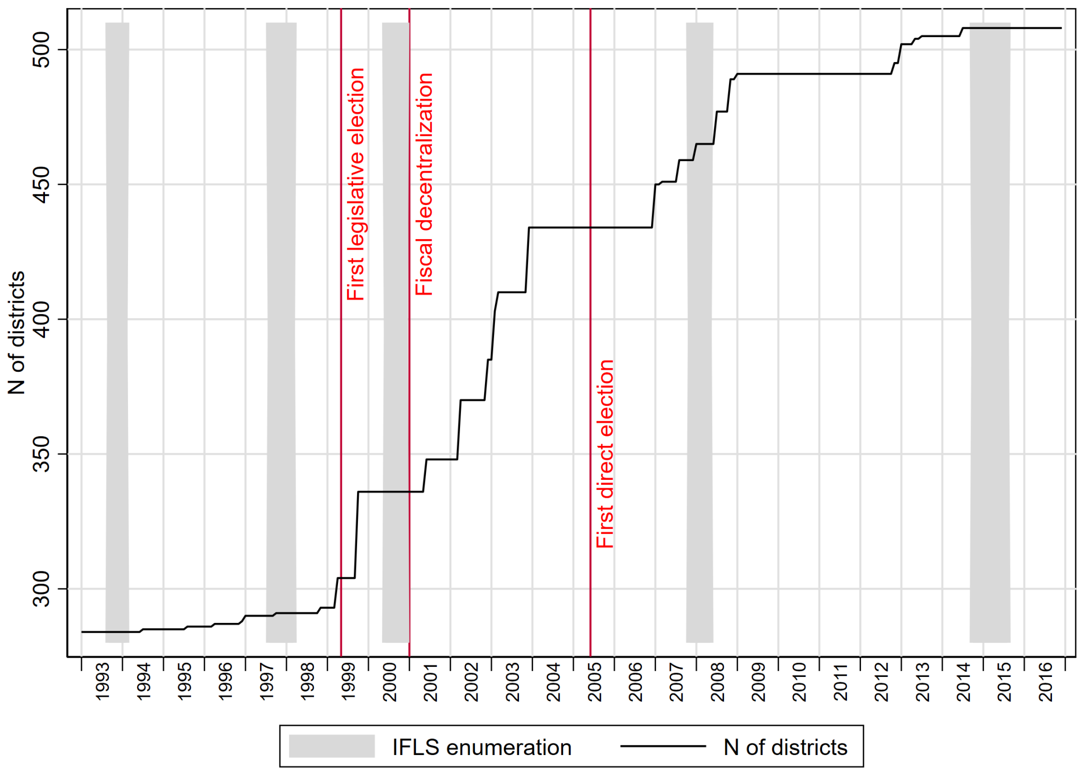
Initially, after its independence from Dutch colonialization, Indonesia mostly retained the inherited sub-national division, particularly at the district level. As time went by, the number of districts (and province) slowly increased as new districts were formed. The formation was mostly done by splitting a district into two or more smaller districts. One district (the "parent" district) would inherit the original name, government, and capital city while the other district(s) (the "child" district) arranges a new government and build a new capital city.

After the fall of Soeharto's authoritarian and highly centralized regime in May 1998, Indonesia underwent a massive series of democratization and decentralization. With the enactment of Law 22/1999, districts were given significantly higher autonomy, both politically and fiscally. District heads were elected by the district parliaments instead of appointed by the central government, as in Soeharto's era. Districts were also given authority over their budgets, funded mainly with non-earmarked transfers from the central government. This fiscal decentralization came into effect in January 2001. With the enactment of this Law, administrative districts, which have no local parliaments, were abolished, with the exception of districts within the capital region of Jakarta. With Law 32/2004, the democratization goes even further, by replacing parliament elections of district heads with direct elections by the public.

Following this great reform was also the massive number of district splits. Figure 1 shows that prior to 1999, the number of districts did not change significantly. Starting in 1999, district splits proliferate causing the number of districts to grow rapidly. Indeed, there were only 336 in the year 2000 while in 2014 this number expanded to 514.

trative villages) have decision-making power.

Figure 1: Number of districts and outcome measurement timeline



District splitting involves several steps. The process is started formally with a decree from the original district head or legislature on the agreement to split the district. This is then followed by other technical decrees such as on the proposed name, capital city location, border, and funding assistance from original districts as well as endorsement from the provincial government where the district is located. The central legislature would then discuss this proposal and enact a law for the new district formation if it is accepted. This entire process could take several years to complete, and the exact duration is pretty much uncertain. From the available dates that I collected, for the splits that took place during 1994-2014, the time lag between initiation to law enactment varies from as short as 2 months to as long as more than 15 years, with a median time lag of 25 months. This variation of time lag implies a lack of control and uncertainty from the district governments' end over the exact timing of the split.

Within 6 months after the law enactment, the newly created district is inaugurated and an ad interim district head is appointed by the central government with a recommendation from the provincial government before the first district head election can be held which typically takes place 1 to 2 years later (Fitrani et al., 2005).

On paper, the motivation for district splits is almost always about shortening the span of control of the local government so as to better provide public service to the people. In reality, however, several other factors could also drive district splitting, such as homogenizing the population in terms of ethnicity (Pierskalla, 2016) and obtaining higher transfer from the central government (Cassidy and Velayudhan, 2022).

3 Data

District level. The main treatment variable used in this study is the date of district formation law enactment, which is the intention-to-treat date of splitting. I collected these dates directly from the government's regulation database publicly available on the internet. To match the child districts with their parents across time, I use a district proliferation crosswalk provided by the World Bank's Indonesia Database for Policy and Economic Research (INDO-DAPOER). This crosswalk also allows me to aggregate newly formed districts back to their original district border[2]. I also derive district-level annual covariates from INDO-DAPOER to perform additional analysis.

Household level. The outcomes of this study are in household level that are derived

[2]This crosswalk actually can serve as the simpler method to determine the timing of district splits. However, several districts have inconsistent codes across years which might confuse actual splits with merely code changes. The timing of the splits from this crosswalk is also not based on the formation law date.

from the Indonesian Family Life Survey (IFLS), one of the most comprehensive longitudinal household surveys with long time coverage. IFLS are enumerated in 5 rounds, which took place mainly in 1993, 1997, 2000, 2008, and 2015 (see Figure 1). Most of the rounds are actually fielded across two calendar years. However, I round them to the years in which most observations lie to retain time discretization of each cross-sectional round [3].

The first IFLS round sample is spread across 149 districts in 13 provinces. Despite not being fully nationally representative, it is designed to represent 83% of the Indonesian population in 1993[4]. It purposively over-samples urban areas and smaller provinces. To account for this over-sampling, the data comes with cross-sectional weight in each round which I use for robustness analysis described in the next section. Other than following the original households across subsequent rounds, IFLS also follows and interviews split-off households of the originals. Therefore, the number of samples grow across rounds, from 7,224 households in 1993 to around 16,000 households in 2015, along with increasing district distribution because of migration. In 2015, the sample households are spread across 297 districts in 24 provinces.

The first outcome is out-migration indicator. To construct this, I match each household with its original district (in 2000 border) in each round. Then, migration is defined as changing original district across subsequent rounds. Note that this definition does not count movement between parent and child districts within the same original district as migration.

The second set of outcomes is regarding household business. IFLS collects information on two different types of business: farm business, which covers all agriculture production activities including hard-stem plants plantation, and non-farm business which covers any other

[3]Another reason is that if the round is split into two years, it is no longer cross-sectionally representative since enumeration time is not random with respect to location characteristics

[4]For a more comprehensive description of the sampling framework and survey technicalities, see Strauss et al. (2016)

business outside farming activities. The definition of owning a farm business is restricted to those who also own agricultural land. In the first two rounds of IFLS, for the non-farm business questions, only the main business is asked, while in the subsequent rounds, it asks all businesses owned by the households. For the sake of consistency, I only keep the main business for all rounds. Earlier rounds also only asked a limited number of questions compared to later rounds. Nevertheless, from the information that is consistently asked across rounds, I can construct two main outcomes: revenue (or production for farm business) and assets of both farm and non-farm business. I also utilize other variables that are only available in later waves for additional analysis.

The limited information in earlier rounds hinders me from matching business across rounds. Therefore, the unit of analysis in this study is household instead of business entities. Still and all, this is not a major drawback as most household businesses are characterized by informality and oftentimes lacks a clear boundary between household and business, so viewing the households themselves as the production unit is deemed appropriate.

Since the observations span across a wide range of time and geographical location, I deflate all monetary values both temporally and spatially. I use monthly consumer price index from the Central Bureau of Statistics (BPS) to construct a temporal deflator, and district poverty line from the same source to construct a spatial deflator. After deflating, all monetary variables are expressed in terms of 2016 Central Jakarta (the downtown of the capital city of Indonesia) price. To prevent outliers from driving the analysis results, I also winsorize all monetary values at the 1% upper tail. For robustness check, I also use other winsorization thresholds in the additional analysis.

Sample selection. For each household, I construct an indicator of (pre-splitting border) cross-district migration sequence since their first observed location [5]. For migration outcomes, I keep only households in their first district plus one round after their first move to other districts. Based on this sample, I use the initial (origin) district location to match the outcomes to the treatment (splitting) variable for the out-migration outcome. For business-related outcomes, I keep households who reside in the district where they stay the longest in terms of the number of rounds they are observed in that district. Furthermore, as I need panel data with at least two time periods for each household, all singletons are dropped. After these selections, the remaining numbers of observations are 45,654 household-years in 211 initial districts in the migration sample and 44,353 household-years in 207 districts in the business growth sample.

My analysis focuses on splittings that happened since 2001. This is because fiscal decentralization had only taken effect since January 2001, thus any new districts after it is always endowed with budget allocation autonomy, ensuring a more homogeneous treatment. However, during the span of my sample (1993 to 2015), many districts split more than once. If district splitting has long-term effects, which is plausible, then the treatment effect of after-2001 splits might be contaminated by the effect of earlier splits. To prevent this, I drop all districts (origin districts in the migration sample) that split between 1993 to 2000 from the sample. This further drops 7.89% of the migration sample and 8.4% of the business growth sample. The remaining sample for out-migration analysis contains 42,053 household-years, which consists of 11,792 unbalanced panel households in 210 origin districts among which 41 are splitters. Meanwhile, the remaining sample for business growth analysis contains 40,630 household-years, which consists of 10,988 unbalanced panel households in 191 districts, among which 32 are splitters.

Descriptive statistics. Table 1 compares baseline characteristics of districts and house-

[5]Note that there are split-off households, implying that not all households are first observed in 1993.

Table 1: Baseline District and Household Characteristics

	(1) Splitters	(2) Non-splitters	(3) Difference
A. District characteristics:			
Log GDP	15.009	14.725	0.283
Log GDP per capita	1.548	1.462	0.086
Log population	13.461	13.263	0.198
Log area	8.677	6.432	2.245***
Share of primary sector in GDP	0.480	0.298	0.182***
Share of secondary sector in GDP	0.204	0.236	-0.032
Share of tertiary sector in GDP	0.316	0.465	-0.150***
Share of own revenue to total revenue	0.100	0.114	-0.014
Share of urban population	0.303	0.504	-0.202**
Share of households with access to electricity (1999)	0.716	0.906	-0.190***
Share of villages with asphalt road	0.509	0.735	-0.226***
Poverty rate (1999)	0.230	0.213	0.017
Observations	28	153	181
B. Household characteristics:			
Share of households owning non-farm business	0.397	0.468	-0.072***
Non-farm business: log(revenue)	16.042	16.355	-0.312***
Non-farm business: log(asset)	14.375	14.805	-0.430***
Share of households owning farm business	0.424	0.264	0.161***
Farm business: log(revenue)	15.642	15.600	0.042
Farm business: log(asset)	17.182	17.781	-0.598***
Observations	1447	6927	8374

Notes: All variables are from the year 2000 except if stated otherwise. * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

holds in the year 2000 using the business growth sample. Splitter districts are similar to non-splitters in terms of the size of the economy and population. Their welfare level is also similar both measured in terms of output per capita and share of population under poverty line. In contrast, splitters are significantly bigger in terms of land area, less urbanized, and have less access to infrastructure (electricity and asphalt roads). The sectoral compositions are also different: splitters' output is dominated by the primary sector. Indeed, in the sample, on average the primary sector contributes to almost half of the total output. This macro picture is also mirrored at the household level. Households in splitter districts are more likely to own farm businesses but less likely to own non-farm businesses compared to their non-splitter counterparts. Among business owners, however, both farm and non-farm businesses tend to be smaller in splitter districts.

4 Empirical Strategy

To empirically examine the average treatment effect on the treated (ATT) of district splits, I employ a difference-in-differences (DID) approach by comparing the evolution of outcomes of splitters versus never-splitters, using the outcomes of the latter as explicit counterfactuals. The ability of this approach to uncover the effect of splitting relies on two assumptions: that the evolution of average outcomes among splitters would have been identical to those of never-splitters had the splits not happened (parallel trend), and that the splitting does affect outcomes before it takes place (no anticipation). The second assumption is arguably realistic since, as described in the previous section, the timing (and even the permission to do so from the central government) of splitting is uncertain and beyond district governments' control, let alone households'. The parallel trend assumption is not directly verifiable, but as the IFLS provides observations prior to splitting, its possibility can be checked by looking for (the lack of) differential trends before the splits. To relax the parallel trend assumption, in several of the outcomes, I also allow for differential parallel trend with respect to baseline covariates.

To allow for examining the dynamic effect of district splits, my preferred specification is a dynamic two-way fixed effect model (TWFE):

$$Y_{idt} = \alpha_i + \theta_t + \sum 1(S_{dt} = s)\beta_s + (\mathbf{X}_i'\boldsymbol{\theta}_t)\boldsymbol{\mu} + \varepsilon_{idt} \tag{1}$$

where Y_{idt} is the outcome of household i in district d (using 2000 border) in callendar time t, α_i is individual fixed effects, θ_t is time fixed effects, $S_{dt} = s$ a an indicators of whether the observation is within time s relative to first splitting (event time), and ε_{idt} is an idiosyncratic error term. Here, the vector $\boldsymbol{\beta} = \{\beta_s\}$ is the parameter of interest. For revenue outcomes, the term $(\mathbf{X}_i'\boldsymbol{\theta}_t)\boldsymbol{\mu}$ is added to allow for parallel trend conditional on a quadratic term of initial revenue. This choice of covariates follows the literature on firm growth in developing countries which finds that initial size is an important determinant of firm growth[6].

Because of the long and irregular gaps in IFLS enumeration time, using annual time in estimating equation (1) would lead to a low number of observations in each event time. This is not to mention that the overall number of splitter clusters is already limited, let alone grouping it by splitting time, which would lead to very imprecise estimates. Another problem from these outcomes observation gaps would be that many β_ss become inestimable as no observations are available in those event times. To improve precision without aggregating too much so that the definition of event time is blurry [7], I use biannual grouping for the event time instead. Still, there are "holes" in the event times under this biannual grouping though it is certainly better than under annual time. Each post-treatment event times also only contain one splitting cohorts[8], so that because of the different dynamic ATT across splitter cohorts, the event-study figure might not show the evolution of ATT smoothly.

Apart from looking at the dynamic effect of splitting, I also estimate the following static TWFE model to get the aggregate effect:

[6]Numerous studies find that contrary to Gibrat's Law, which states that firm growth is independent of its initial size, smaller firms do in fact grow more quickly. See for example Evans (1987a), Evans (1987b), Hall (1987), and Variyam and Kraybill (1992) in developed countries, as well as Bigsten and Gebreeyesus (2007), Coad and Tamvada (2012), and Elston and Weidinger (2023) in developing cointries

[7]Because of time discretization, event time in the event study model does not perfectly represent actual time interval. For instance, if annual time is used, t plus 1 actually covers a whole range of 0 to 2 years after treatment, while t plus 2 covers 1 to 3 years after it. There is clearly an overlap and it gets bigger when longer time aggregation is used.

[8]To be specific, t0-t1 contains 2007-2008 splitter cohorts, t2-t3 is empty, t4-t5 contains 2003-2004 splitter cohorts, t6-t7 contains 2001-2002 splitter cohorts, t8-t9 contains 2007-2008 splitter cohorts, t10-t11 is empty, t12-t13 contains 2003-2004 splitter cohorts, and t14-t15 contains 2001-2002 splitter cohorts.

$$Y_{idt} = \alpha_i + \theta_t + \beta Post_{dt} + (\mathbf{X}_i'\boldsymbol{\theta}_t)\boldsymbol{\mu} + \varepsilon_{idt} \tag{2}$$

where $Post$ indicates if district d has split for the first time since 1993.

As Borusyak et al. (2022), De Chaisemartin and D'Haultfœuille (2020), Goodman-Bacon (2021), and Sun and Abraham (2021) point out, the presence of heterogeneous treatment effect across event time and across treatment cohorts, regression-based TWFE parameters in equation (1) and (2) do not correspond to the correct ATTs. Goodman-Bacon (2021) shows that the TWFE parameters are made up of 2x2 DIDs of all relevant time pairs in all treatment cohorts in the sample, which are then aggregated with weight. The problem is that the weight does not simply reflect the subsample size in each 2x2 DID, but rather is determined by their within-group treatment variance. Even worse, the weight can be negative for some treatment effects, causing a possibility of getting a negative estimated treatment effect even though the actual ATT is positive.

To overcome these drawbacks of the conventional regression-based TWFE estimation, I use an estimator developed by Callaway and Sant'Anna (2021) instead. This estimator circumvents the previously mentioned problem by explicitly estimating all the relevant 2x2 DIDs using only the never splitters as the control group and then aggregating them into either dynamic or static TWFE-like coefficients[9], weighted by their subsample size. For specifications that allow for differential parallel trend conditioned on covariates, each 2X2 DID is estimated using Sant'Anna and Zhao (2020) doubly robust estimator. This estimator is implemented using the *csdid* Stata package.

Because of the time gap problem in the sample, using consistent base period event time in

[9]Other aggregation methods are also available, namely calendar aggregation which estimates the ATT for each calendar period across all treatment cohorts, and group aggregation which estimates the ATT for each treatment cohort across all periods.

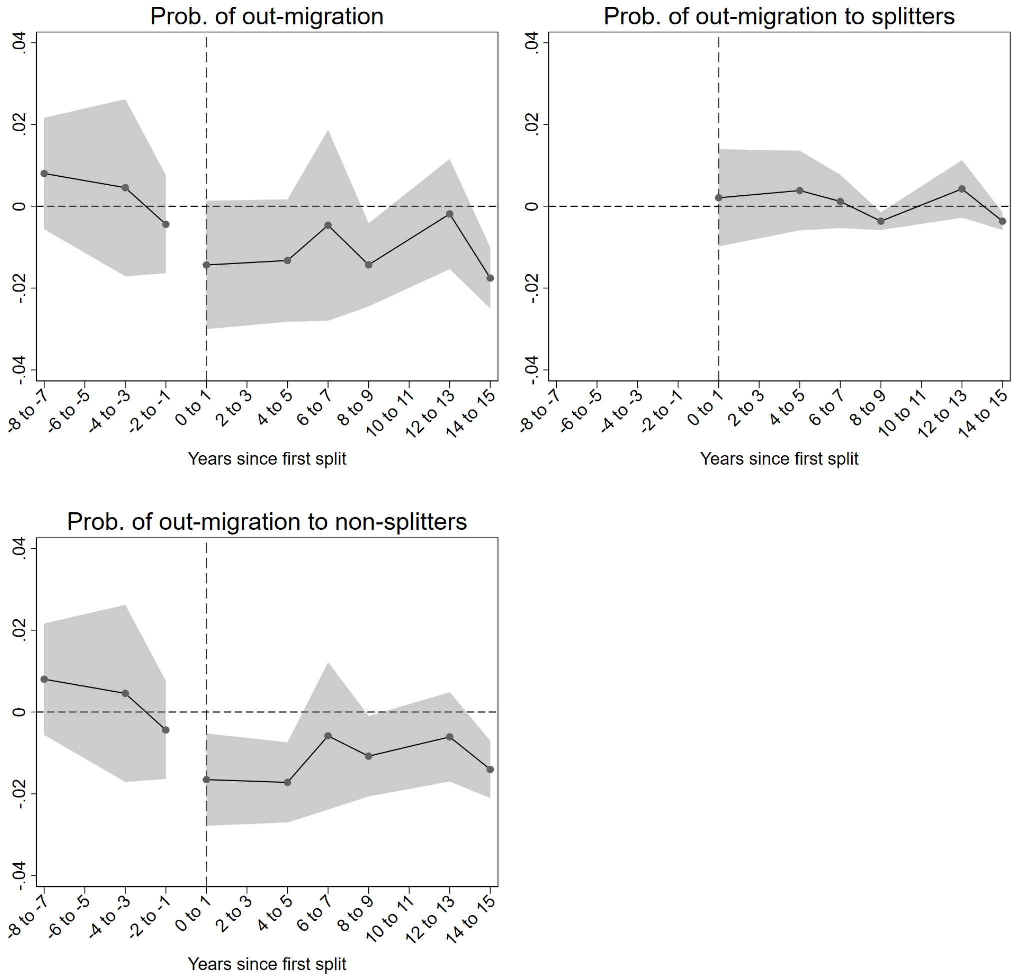
estimating the 2x2 DIDs would leave many of them unestimated. I overcome this by picking the base period using the latest pre-splitting time available in the sample. For instance, the 2x2 DID base period for 2001-2002 splitter cohorts is 1999-2000 observations (t-2 to t-1), and the same observation time is also used as the base for 2007-2008 splitter cohorts (t-8 to t-7). It should be noted that using base periods further away from the treatment period affects nothing in terms of the ability to uncover the ATT except if we consider that the parallel trend assumption becomes less likely to hold the longer the period gap is. If the parallel trend assumption is not violated, however, using an earlier base period could actually reduce the contamination of anticipation effect if there is any.

In all the estimations, the standard errors are clustered at the 2000 border, pre-splitting district. As a robustness check, wild bootstrap standard errors, also clustered at the district level, are also calculated in addition to the asymptotic standard errors. To check the plausibility of the parallel trend assumption, for each specification, if pre-treatment observations are available, I also report chi-squared-based differential pre-treatment tests with the null hypothesis that all pre-treatment 2x2 DIDs are equal to zero.

5 Results and Discussion

This section presents the results as a series of answers to three main questions. First, how do district splits affect the movement of households across jurisdictions? Second, among the non-migrating households, how do the splits affect their dynamics of involvement in the market as producers? Lastly, among the business owners, how do splits affect their business growth?

Figure 2: District Splits and Household Migration



Notes: The figures plot β_s in equation (1) along with their 95% confidence intervals robust to heteroskedasticity and clustering by districts.

5.1 District Splits and Household Migration

Figure 2 plots the β_s of equation 1 for migration outcomes. The upper left panel shows that district splits decrease out-migration. The post-treatment estimated effects are always negative, though only some are significantly different from zero because of imprecision. The corresponding static coefficient can be seen in the first column of table 2, which summarizes all the post-treatment 2x2 DIDs into an average effect of 15 years after splits, which is more precisely estimated than in the dynamic specification. Based on the point estimate, district

Table 2: District Splits and Household Migration

	(1) Prob. of out-migration	(2) Prob. of out-migration to splitters	(3) Prob. of out-migration to non-splitters
Post	-0.010**	0.001	-0.011***
	(0.005)	(0.002)	(0.004)
Pretreatment test p-val.	0.402	.	0.402
Observations	23679	23197	23626
Number of clusters	210	205	208

Notes: This table presents the results from estimating equation (2). The null of the pretreatment test is that all pretreatment 2x2 DIDs are equal to zero. Standard errors are clustered at the district level. * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

splits lower the probability of out-migration by 1 percentage point (pp.). For this outcome, the plausibility of the parallel trend assumption is supported by the lack of statistically significant pre-treatment differential trend which can be inspected visually in the figure and also by noting the high p-value of the pre-treatment test in the table.

The negative impact of splits on out-migration is consistent with Tiebout (1956) sorting model. In this model, different local governments offer different combinations of price (tax) and different variations of baskets of goods (public services). Households and firms can then migrate across jurisdictions to maximize their utility and profit, hence optimum sorting. Under this model, district splits increase the options of different tax and public service provision combinations within the original district border. Consequently, it is an (imperfect) substitute for migration to other districts (Pierskalla, 2016), decreasing the need to move out from the original district border. It should be noted that districts in Indonesia have limited ability to collect tax as opposed to the central government, though they do collect business licensing fees (Cassidy and Velayudhan, 2022). However, the costs faced by households and firms to enjoy the public service in a particular district are not necessarily just a formal tax. Many other informal costs exist, including bribery, which is very common.

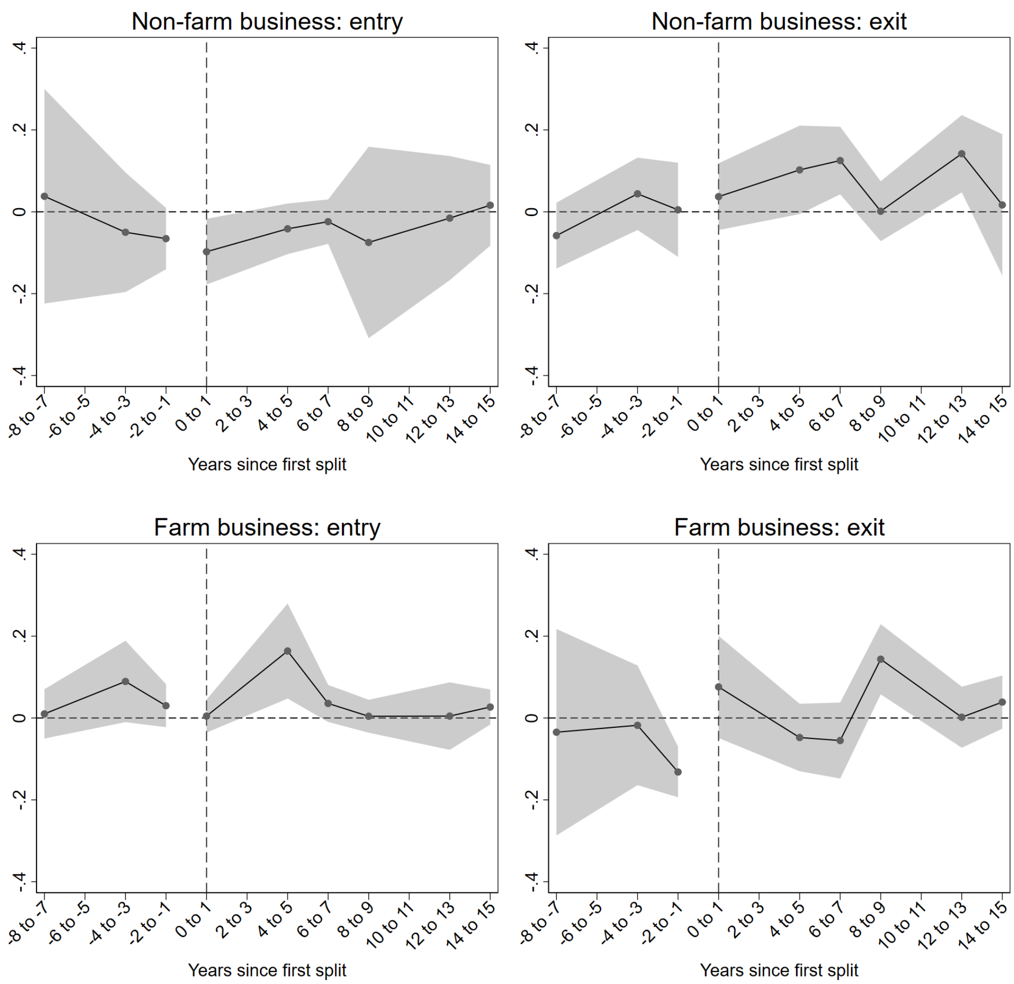
To further check this sorting hypothesis, I separate the out-migration into two groups of destinations. The first is out-migration to splitter districts, which is defined as moving to any splitter districts (which split within the sample time range) after the first split had happened. The second one is out-migration to non-splitter districts, which is defined as moving to any never-splitter districts, or to splitter districts before the first split took place. The rest of the graphs in figure 2 and columns 2 and 3 of table 2 present these analyses. In the out-migration to splitter estimation, any migration to non-splitter is dropped from the sample, and vice versa. Note that in the out-migration to splitter outcomes, no pre-treatment observations are available because prior to the first split in 2001, there are no migration to splitter districts.

The sorting model predicts that the decline in out-migration would be more substantial when the destination is a non-splitter district than if the destination is a splitter district. This is because the price-public service combination variations in the former are lower than the latter. Indeed, the estimation results confirm this. District splits decrease out-migration to non-splitter districts by 1.1 pp. but does not affect out-migration to splitter districts. The negative result in the combined out-migration is driven by out-migration to splitters.

Robustness. To check the robustness of the results, I also estimate the same specifications with wild bootstrap standard error and IFLS-provided cross-sectional weight. The results are presented in Table A1 in the online appendix. The standard error of the estimates remains similar when using the wild bootstrap method. The use of weight, however, decreases the coefficient to 0.7 pp. The IFLS weight mainly corrects for rural-urban composition because of the intentional oversampling of urban areas. The shrink of the coefficient thus suggests that the negative effect of splits on out-migration is dominated by those whose origin districts are more urbanized. It should be noted however that the pre-treatment test is now significant at 10%, which alerts our confidence in the validity of the parallel trend assumption in this result.

5.2 District Splits and Household Business Entry and Exit

Figure 3: District Splits and Household Business Entry and Exit



Notes: The figures plot β_s in equation (1) along with their 95% confidence intervals robust to heteroskedasticity and clustering by districts.

Next, I examine the impact of district splits on household business activities. Before focusing on the performance of businesses among business owners, I check the dynamics of household business entry and exit, into and from both farm and non-farm industries. To do this, I construct entry and exit dummies for both farm and non-farm businesses, which

Table 3: District Splits and Household Business Entry and Exit

	(1) Non-farm business: entry	(2) Non-farm business: exit	(3) Farm business: entry	(4) Farm business: exit
Post	-0.031	0.077***	0.043***	-0.002
	(0.023)	(0.026)	(0.015)	(0.025)
Pretreatment test p-val.	0.355	0.373	0.236	0.023
Observations	11737	6374	13302	6466
Number of clusters	186	179	191	165

Notes: This table presents the results from estimating equation (2). The null of the pretreatment test is that all pretreatment 2x2 DIDs are equal to zero. Standard errors are clustered at the district level. * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

compare the business ownership status in the current to the last round. The time gap of the round pairs is included as a covariate in both equations 1 and 2 to allow for differential trends among different time gaps.

Table 3 shows that district splits have an effect on entry-exit dynamics of farm business that is in the opposite direction to the effect on non-farm business. The static coefficient suggests that district splits increase the probability of *entry* to farm business by 4.3 pp. but at the same time also increase the probability of *exit* from non-farm business by 7.7 pp. The coefficient of non-farm business entry and farm business exit, on the other hand, are not statistically different from zero. However, from the event-study plot in figure 3, we can see that within 0 to 1 year after splits, the entry rate to non-farm business among splitters is significantly lower than that of non-splitters. The figure also reveals that the minuscule coefficient on farm business entry masks the great variation in magnitudes and directions across event times (and hence across splitter cohorts). These results are robust to the use of wild bootstrap standard error and sampling weight as shown in Table A2 in the online appendix. The significant pre-treatment test in the farm business exit outcome and farm business entry with weight, however, suggest that these results should be interpreted rather carefully as the true ATTs.

This finding suggests that district splits discourage household non-farm business activities, as reflected by the economically big impact on exit and also an indicative negative impact on entry. This aligns with Cassidy and Velayudhan (2022) who find, using economic governance survey data in Indonesia, that district splits reduce formality and increase licensing burden among small businesses. Indeed, my finding suggests that the adverse effect of splitting is broader than just hindering business formalization. In contrast to non-farm business, district splits attract households to be involved in farm business. This positive impact is explored further in the next section.

5.3 District Splits and Household Business Growth

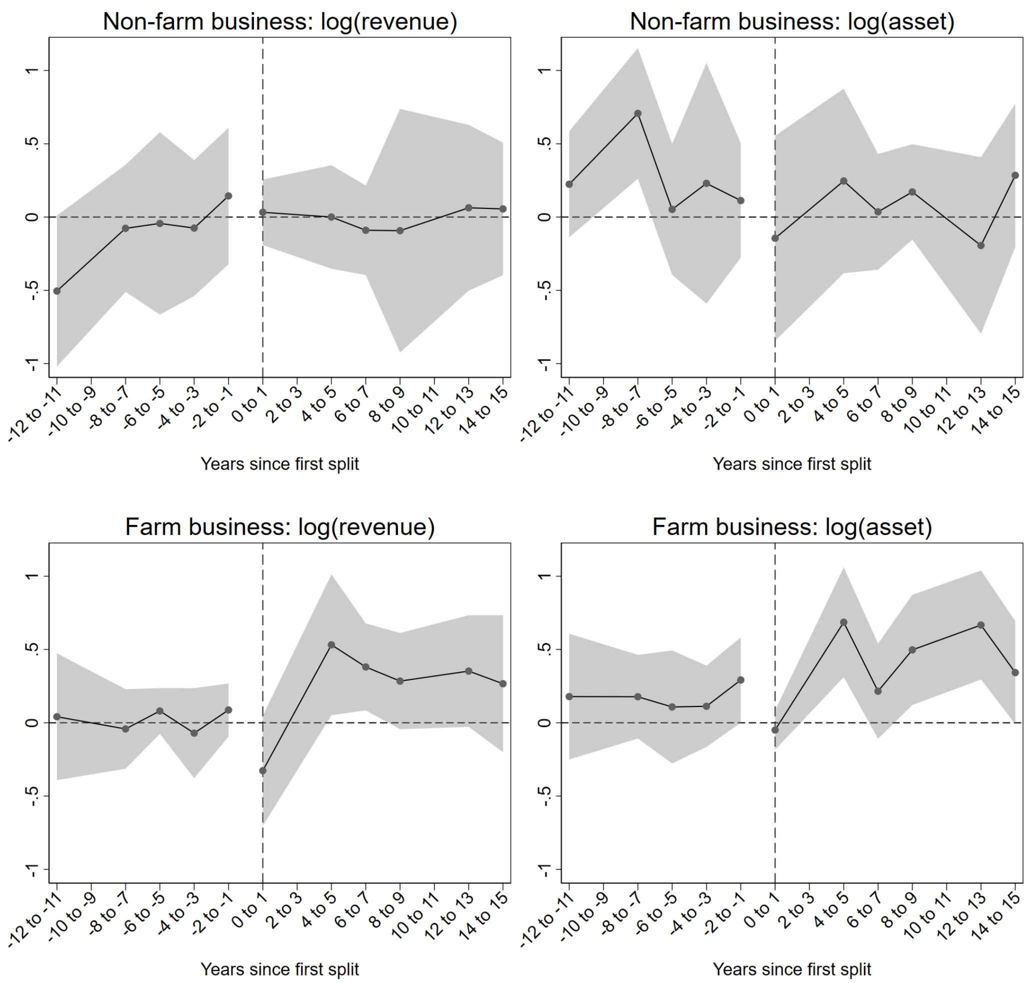
Table 4: District Splits and Household Business Growth

	(1) Non-farm business: log(revenue)	(2) Non-farm business: log(asset)	(3) Farm business: log(revenue)	(4) Farm business: log(asset)
Post	-0.007	0.093	0.351**	0.461***
	(0.111)	(0.141)	(0.142)	(0.131)
Pretreatment test p-val.	0.049	0.000	0.937	0.239
Observations	6370	7870	6890	8302
Number of clusters	180	182	168	171

Notes: This table presents the results from estimating equation (2). The null of the pretreatment test is that all pretreatment 2x2 DIDs are equal to zero. Standard errors are clustered at the district level. * $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Among households that stay involved in either farm or non-farm business, I am able to estimate the effect of district splits on their growth. Figure 4 and table 4 show that district splits do not affect non-farm business revenue and asset growth, but increase both the revenue and assets of farm businesses. Again, with the note of possible parallel trend assumption violations in the non-farm business estimations. The effects on farm-business outcomes is not just statistically significant, but also economically huge: splits boost farm

Figure 4: District Splits and Household Business Growth

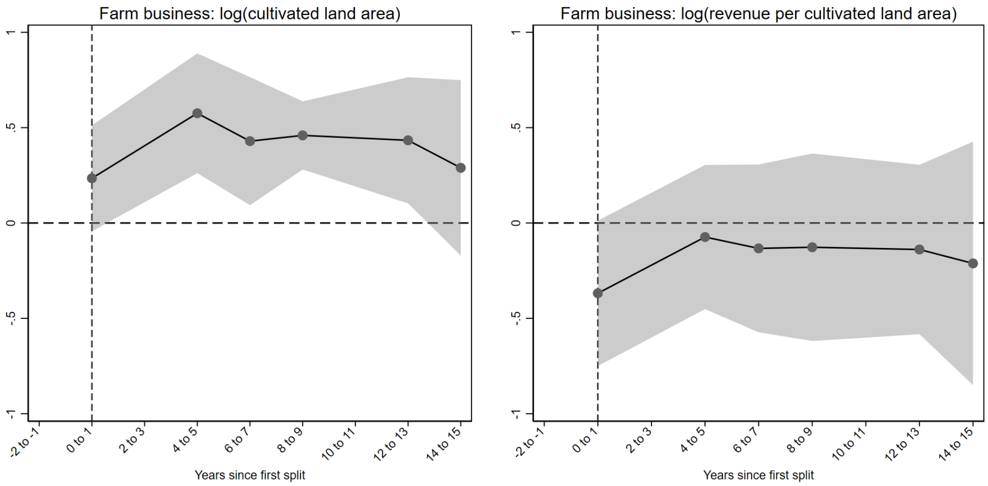


Notes: The figures plot β_s in equation (1) along with their 95% confidence intervals robust to heteroskedasticity and clustering by districts.

business revenue by more than 30% and asset by more than 40%. Table A3 in the online appendix also shows that these results are robust to using wild bootstrap standard error, sampling weight, and also are not driven merely by top outliers as indicated by stable coefficients when using bigger top winsorizations.

The big revenue growth impact leads to another question of whether it is a consequence of improvement in productivity or merely just because of an increase in input. In IFLS 2000 and onwards, more detailed questions are asked about the business activities so that we can

Figure 5: District Splits and Household Business Growth: Mechanism via Land Input

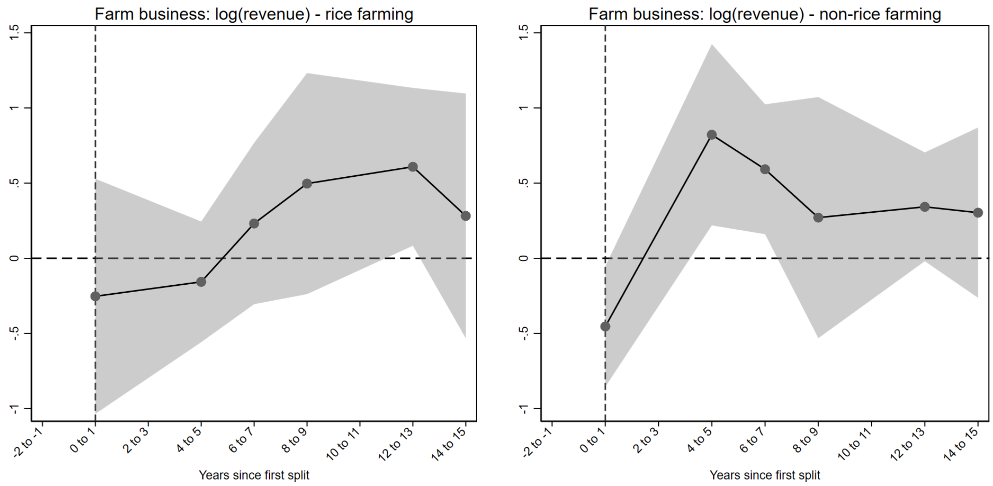


Notes: The figures plot β_s in equation (1) along with their 95% confidence intervals robust to heteroskedasticity and clustering by districts.

take a look at several possibilities driving the main results. Figure 5 shows that district splits increase significantly the cultivated land area of the farm businesses. Combining with the revenue information, in the right-hand plot we can see that the impact on the revenue per land area cultivated is in fact not statistically different from zero. Even, if we look only at the point estimates which are always negative, district splits decrease land productivity, particularly in the very short term.

IFLS 2000 onwards also provides information on the main commodities of the farm businesses. Because the majority of farmers in Indonesia are rice farmers (in the IFLS data, around 50% of farm businesses are rice farmers) it is also interesting to see the results by main commodities. Figure 6 presents this subsample analysis. I lump all non-rice commodities into one category because of the low number of observations. The figure shows that the positive effect on revenue occurs both in rice and non-rice farming. However, the pattern suggests that the overall result is driven mainly by non-rice farming. The positive revenue effect in rice farming takes place in the longer term, while for non-rice farming, the positive effect already takes place within 4-5 years after splitting.

Figure 6: District Splits and Household Business Growth: Results from Different Commodities

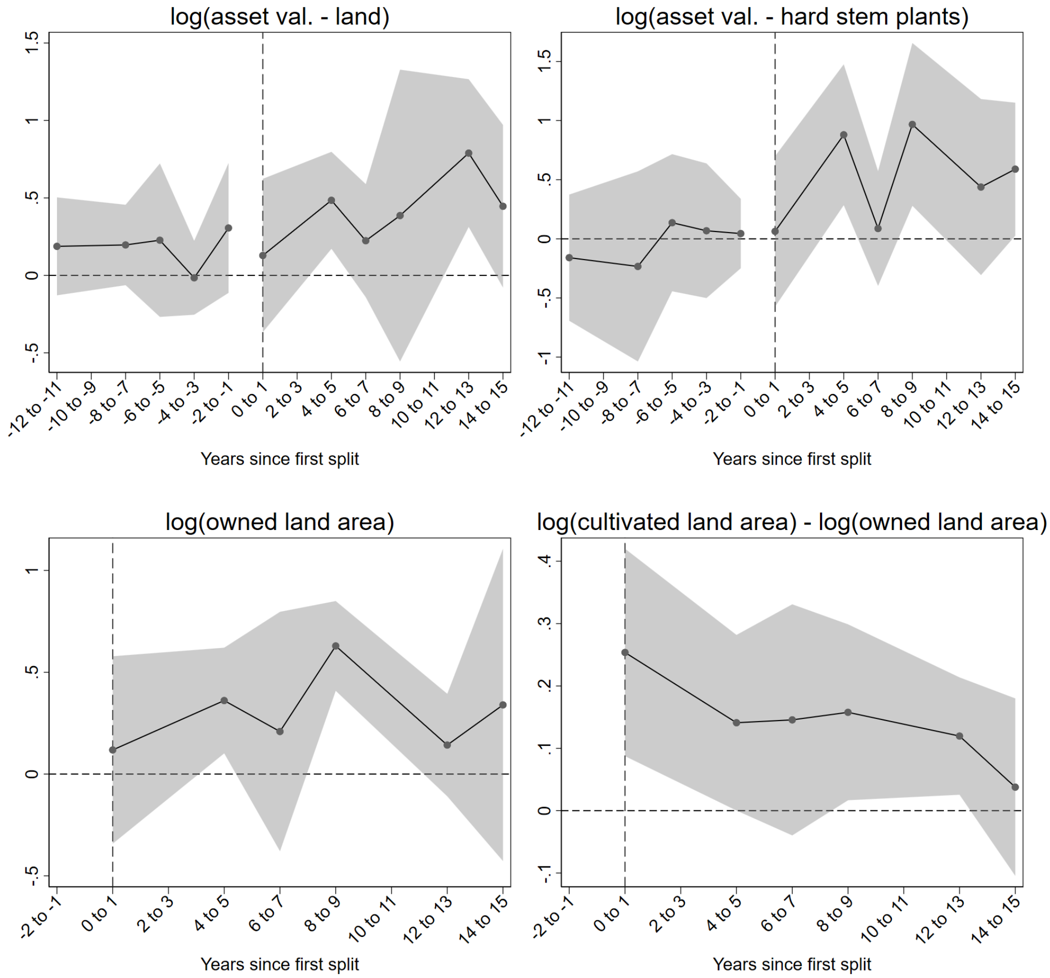


Notes: The figures plot β_s in equation (2) along with their 95% confidence intervals robust to heteroskedasticity and clustering by districts.

Next, I check the details behind the positive effect on farm business assets. The raw data on assets contains questions regarding different asset types. In the upper plots in figure 7, I show two of the asset types that have large enough observations, namely land and hard stem plants. The positive effects are visible both in land and hard-stem plants. Both assets are recorded in monetary values, however, so the positive impact might merely reflect increases in asset prices. Fortunately, in IFLS 2000 onwards, the area of owned land is also recorded. The lower-left plot in figure 7 shows that owned land area also increases following district splits.

Despite the increase in owned land area, which explains the increase in cultivated land area, the coefficients in the former tend to be smaller than those of the latter. This suggests that the farm businesses utilize more land area than what they own. To check this, I estimate the effect of district splits on the difference in log cultivated land area and log owned area. All negative differences are replaced to zero as it reflects idle farmland. The

Figure 7: District Splits and Household Business Growth: Detailed Views on Assets



Notes: The figures plot β_s in equation (1) along with their 95% confidence intervals robust to heteroskedasticity and clustering by districts.

result is shown in the lower-right plot of figure 7. District splits cause a sudden jump in non-owned farmland cultivation which then gradually decreases over time. This non-owned farmland might be rented, or, another source which is possible, acquired by the means of illegal land clearing. Unfortunately, no more detailed questions are available to investigate these possible mechanisms. Burgess et al. (2012) however, have found that the increase in the number of districts within province because of splitting causes an increase in deforestation for illegal wood extraction. They argue that since district governments are the ones who are responsible for enforcing logging rules, the increase in the number of districts causes the heating up of inter-district competition to issue illegal logging permits, which are motivated by maximizing rent-extraction by the bureaucrats. If this story is true, then my result is in line with it and also broadens the consequences to not only wood extraction but also land clearing for farming business.

Furthermore, I checked if the null effect of district splits on non-farm business revenue growth and the positive effect on farm business revenue are aligned with macro pictures. Figure B1 in the online appendix shows that following district splits, only GDP on agriculture is significantly increasing. The GDP on manufacturing, trade, hotel, and restaurants, as well as on other services, which are the three main sectors within which household non-farm businesses belong to[10], are not affected by district splits.

6 Conclusion

In this study, I investigate the effect of local government splits on household economic activities using micro-level data from Indonesia. I utilize a longitudinal household survey that covers more than 20 years time span, within which an episode of massive district splits take place. I employ the difference-in-differences (DID) strategy and make use of the newly developed estimator from Callaway and Sant'Anna (2021) which circumvents the flaws in the conventional two-way fixed effects regression in correctly estimating average treatment effect.

There are three main points from the results that I find. First, district splits decrease household out-migration from the original, pre-split, district. This aligns with Tiebout (1956) sorting model, in which splitting causes more variation in price and variety of public service combinations thereby reducing the need to move to other districts. Second, district splits do not promote non-farm household business activities, reflected by null effects on revenue and asset growth. In fact, it actually causes more non-farm businesses to shut down as reflected

[10] This is based on sectoral information of the non-farm business which is available in IFLS 2000 onwards.

by the increasing exit rate. Third, in contrast with farm-business outcomes, district splits do improve farm business revenue and asset growth. It also attracts new households to enter the production activities as reflected by the increasing entry rate. The revenue growth, however, is merely fueled by increase in land input without any improvement in productivity. The source of the land, however, is possibly from illegal land clearing activities, which can actually incur externalities to the environment and other parties. Thus, the improvement in farm business outcomes is not happening in an expected way and certainly is not in a sustainable manner.

To give some notes on the methodological aspects, the poor precision caused by the small number of splitter districts in my sample, along with the large time gaps between surveys area the main caveat of this study. Another drawback is that in some estimations, differential pre-trends are observed which indicates that parallel trend assumptions might not hold. Nevertheless, the results could at least give some indicative evidence that district splits observed within my sample seem to cause more harm rather than good, thus demanding reevaluation of the system because, at the time this sentence is written, a queuing district split proposals are still awaiting acceptance.

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