STUDIES

Measuring rural economic development from outer space: evidence from the Village Fund policy in Indonesia

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This study examines Indonesia's Village Fund policy. Introduced in 2015, it has been implemented across rural villages (desa) throughout the country. The policy's primary goal is to foster rural development. This study evaluates its effects using high-quality night-time light data, (VIIRS) as an innovative proxy for village-level economic activity from 2013 to 2021. It employs inverse probability weighting difference-in-differences (IPW–DID) to estimate the policy's impact and address selection bias. The results show that, after weighting, the treated and untreated groups are comparable, indicated by a parallel trend before treatment, and the estimates are robust. Moreover, the policy's impact turned positive two years after implementation because 80% of village funds were allocated to infrastructure in the first two years. The heterogeneous effects are significant for villages within 5 km of the city centre. The policy also has a substantial impact on landlocked villages and those in flat areas.

Keywords:
difference-in-differences,
village funds,
desa,
inverse probability weighting

Online first publication date: 17 April 2026

Introduction

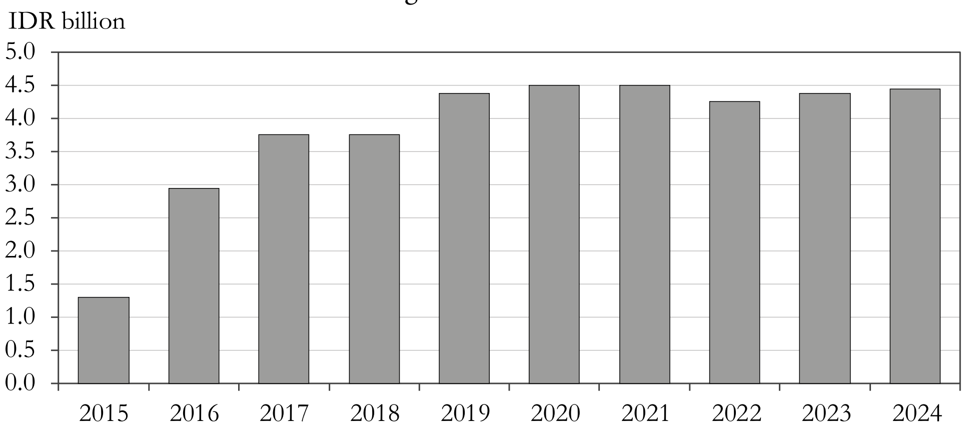
Rural development became a central issue in Indonesia, particularly since the village law was implemented in 2014. The law allows every village in Indonesia to receive funds directly from the central government. The government of Indonesia (2014) issued a policy to strengthen rural development through law 6/2014 on villages. The funds disbursed by the Indonesian government reached 610.6 trillion rupiahs (38.18 billion USD) from 2015 to 2024. In 2015, the first year of implementation, funds were only 20.7 trillion rupiahs, tripling to IDR 70 trillion in the fiscal year of 2023 (Ministry of Finance 2024).

In principle, the Village Fund policy aims to promote village development, improve community welfare, and support equitable growth. Beyond fostering economic advancement, it emphasizes community empowerment, positioning villagers as active participants and primary stakeholders in the development process. This approach is intended to strengthen the social fabric of villages, enabling local populations to take ownership of their progress and development outcomes. Moreover, the Village Fund policy addresses disparities in development by targeting gaps that exist between differing villages, ensuring that growth and improvement are distributed more evenly across rural areas (Imaduddin et al. 2019).

In the early implementation phase, village funds are typically allocated to infrastructure. Imaduddin et al. (2019) documented that from 2015 to 2018, approximately 76%–81% of funds were allocated to infrastructure projects. The next allocation supports rural empowerment, including capacity building for village officials, community enterprises, and society at large. From 2015 to 2018, community development received 11%–16% of village funds. The remaining funds were allocated to official government activities and capacity building for nongovernment actors at the village level. Figure 1 informs the total village funds allocation during 2015–2024.

Figure 1

Village funds allocation



Source: Ministry of Finance 2020 and 2024.

Since 2015, extensive infrastructure has been developed for various functions. By June 2023, projects to enhance intervillage connectivity included 325,400 km of village roads and 1,791,600 km of bridges. Infrastructure to support economic activities comprised 14,168 units, and village-owned enterprises are as many as 42,727 units.

The Village Fund policy also supports health infrastructure, including 31,981 sports facilities, 1,670 clean water connections, 25,713 village health polyclinics, and 43,657 integrated service posts. Communal health facilities include 513,175 baths and foot-washing stations, 86,581 wells, and 50.3 million drainage units. In addition, 573,100 irrigation facilities have been constructed. Since the policy targets rural areas, it also funds agricultural infrastructure. Since 2015, 573,100 irrigation facilities and 6,427 village water reservoirs have been built. The policy also allocates IDR 4.40 trillion for stunting prevention and IDR 5.07 trillion for food security (UPDESA 2024).

Indonesia's massive rural development policy significantly influences rural development outcomes. Fund allocation has played a central role in fostering inclusive growth, particularly by increasing rural incomes through community projects focused on infrastructure development (Ernawati et al. 2021). Hilmawan et al. (2023) use the village development index as a proxy for rural development, finding that the policy positively and significantly affects rural development. Using particular areas such as West Java, Yogyakarta, Riau, East Kalimantan, and Gorontalo as area samples, Indraningsih et al. (2021) found that village funds improve farmers' production and income.

However, given the limitations of official statistical data, it is important to identify proxy measures of rural economic development to estimate the impact of the Village Fund policy. In Indonesia, official data on economic activity are available mainly at the ADM2 (district) to ADM1 (national) levels. This study fills a gap in the rural development literature by using night-time light data as a proxy for economic activity in Indonesia. This study addresses the gap with a methodology that enables large-scale observation, namely inverse probability weighting difference-in-differences (IPW–DID). In previous research, Hartojo et al. (2022) used monthly night-time light data from 2015 to 2019 and applied a regression discontinuity design that focused only on underdeveloped villages – about 26,000 of roughly 70,000 villages.

Related literature

Rural development

Rural development is crucial in developing countries, providing physical infrastructure and essential amenities like schools, hospitals, and good road infrastructure, thereby improving the rural economy. Ernawati et al. (2021) found that allocating village funds in Indonesia promotes inclusive growth. Such funds can increase rural income through community projects that develop rural infrastructure.

Implementing the village fund reflects three key principles of rural development at the smallest administrative level: autonomy, participation, and transparency. The village fund also represents state recognition of the legal community's unity and its authority to regulate and manage government affairs and local interests based on initiatives, rights of origin, and traditional rights (Imaduddin et al. 2019).

Furthermore, rural infrastructure enables the rural economy to diversify beyond agriculture. Wang et al. (2023) show that rural development can expand economic activities to include manufacturing, services, and other sectors. Such diversification increases value added and raises rural labour income, a process combining various rural resources. Marsden–Ploeg (2009) explain that innovatively utilizing rural resources leads to new activities, interactions, transactions, and networks.

Village funds – as an instrument of rural development in Indonesia – boost the rural economy by increasing labour absorption through the stable presence of village-owned enterprises. Arifin et al. (2020) found an increasing number of village-owned enterprises between Java and non-Java regions, with a similar trend regarding the Village Fund policy. Furthermore, Gibson–Olivia (2010) found that data from 4,000 households in rural areas in Indonesia show that the quality of roads and electricity affects employment and income from nonfarm enterprises.

Another mechanism through which rural development improves the rural economy is by strengthening rural–urban linkages, both physical and nonphysical. Villagers with good road access to cities can obtain non-agricultural jobs in nearby towns. The expansion of road networks has made commuting to urban employment a key form of engagement for rural residents, enabling them to access better job opportunities in rural areas (Irwin et al. 2010).

Research on rural development employs various measures of output, including rural household income (Indraningsih et al. 2021), labour mobility (Irwin et al. 2010), agricultural productivity (Bravo-Ureta et al. 2020), as well as other household-level indicators. In Indonesia, however, little research examines rural development outcomes using measures that directly represent economic output.

Hartojo et al. (2022) leveraged spatial analysis and night-time light data as a proxy for measuring rural economic development. Using monthly VIIRS night-time light data, they assessed changes in economic activity before and after the implementation of the Village Fund policy. Their findings show significant improvement in rural economic growth among smaller villages following the funds transfer. Notably, underdeveloped villages in Eastern Indonesia experienced more rapid growth than those in Western and Central Indonesia.

Using night-time light for measuring economic development

Due to breakthroughs in satellite data, consistent information across a large spatial and temporal scale can be obtained. Because shapefiles of village-level administrative boundaries are available, satellite imagery can be used to collect data. This eliminates constraints on data availability at the smallest administrative level. Among the most promising satellite-based measures is night-time light.

In past research, night-time light data have been used not only as an economic proxy at the national level (Doll et al. 2006, Henderson et al. 2012) but also at subnational scales (Gibson–Boe-Gibson 2021, McCord–Rodriguez-Heredia 2022, Pagaduan 2022, Shi et al. 2014). Additionally, night-time light data is utilized as a proxy for inequality (Singhal et al. 2020), poverty (Putri et al. 2022, Yu et al. 2015), and development (Elvidge et al. 2012). Some studies employ night-time light data to capture economic activity in rural areas (Chen et al. 2023, Pan–Hu 2018, Pérez-Sindín et al. 2021, Tan 2015, Yang et al. 2017).

There are two main sources of night-time light data: the Defense Meteorological Satellite Program (DMSP) and the Visible Infrared Imaging Radiometer Suite (VIIRS). DMSP was not initially designed for economic studies. It is operated by the United States Air Force primarily to monitor weather and detect moonlit clouds for military purposes (Henderson et al. 2012). Comparatively, the VIIRS Day/Night Band was designed specifically for scientific research. It is considered far superior to DMSP for economic analysis and has been available since April 2012 (Gibson et al. 2021).

In addition to these two primary night-time light sources, several studies combine DMSP and VIIRS data, such as the harmonized night-time light dataset from Li et al. (2020), covering 1992–2021, and VIIRS night-time light (NTL) (see Chen et al. 2021). Such combinations aim to provide a long time series of night-time light data, but also present challenges. The main challenge in merging these datasets is inconsistent satellite timing and orbital differences. The original and extended DMSP series are from different satellites (F18 and F15, respectively) that observe night-time lights at different hours, around 8:30 p.m. versus 3:30 a.m. VIIRS data, in contrast, are collected around 1:30 a.m. (Zhang–Gibson 2022).

This study uses VIIRS 2.1 yearly night-time light data from Elvidge et al. (2021) as a proxy for economic development at the village level. With a spatial resolution of 0.25 km^2 at the equator, VIIRS 2.1 outperforms older sensors, such as the DMSP, in detecting economic activity. The VIIRS 2.1 version has several advantages, including consistency, saturation, spatial accuracy, and resolution. The DMSP sensor lacks onboard calibration, which automatically adjusts amplification (gain) to detect clouds, but these adjustments are not recorded (Gibson et al. 2020). Thus, brightness values from year to year or across different satellites cannot be directly compared, posing a major problem for time series analysis. In contrast, VIIRS features onboard calibration, ensuring its radiometric data are consistent over time and between satellites. This consistency means that a given brightness value today represents the same light intensity as yesterday or last year, making it far more reliable for analysing policy impact and growth.

Furthermore, VIIRS has a much wider dynamic range – almost seven million times – and does not suffer from saturation issues. It can capture brightness across a broad spectrum, from very dim rural areas to the brightest urban centres (Gibson–Boe-Gibson 2021). Additionally, it records data in 14-bit format, allowing for 16,384 brightness levels (Gibson et al. 2020). This provides far richer detail and nuance in measuring economic intensity. In contrast, the DMSP sensor saturates easily in bright city centres. As a result, many urban areas – from business districts to dimmer suburbs – are recorded with the same maximum digital value (63), rendering differences in economic activity within the city invisible.

VIIRS has a higher spatial resolution of 15 arc-seconds (~500 m) (Elvidge et al. 2021) and more accurately identifies true light sources with minimal blurring. This advantage is crucial for analysis at small geographic scales. In contrast, DMSP suffers from an overglow effect, where light spreads into surrounding dark areas. This exaggerates city size and reduces the accuracy of a small geographic scale (Henderson et al. 2012).

While VIIRS data represent a significant improvement over DMSP, using them as a proxy for rural economic activity has several limitations, including observation timing, the nature of rural activities, and the detection of dimly lit areas. The Suomi satellite carrying the VIIRS sensor observes the Earth at approximately 1:30 a.m. local time. At this hour, many rural economic activities have already concluded, causing luminosity to underrepresent actual economic activity (Gibson–Boe-Gibson 2021, Pagaduan 2022).

Rural economic activity, which relies primarily on the agricultural sector, does not generate substantial night-time light (NTL) (Bhandari–Roychowdhury 2011). Moreover, although VIIRS is more sensitive than DMSP, there is still a threshold below which it cannot capture light. Many low-density areas may still fall below the detection limit (Pagaduan 2022). These two limitations weaken the relationship between NTL and economic indicators, such as gross domestic product (GDP), in rural areas compared to urban areas.

Using GDP at ADM2 level in Indonesia, Gibson et al. (2021) shows that DMSP has lower correlation compared to VIIRS data. The correlation between DMPSP and GDP was 0.01 for the overall R^2 and 0.00 within R^2. Using VIIRS data, the overall R^2 was 0.05, and the within R^2 was 0.00. These findings align with Chen (2025), confirming that night-time light data are more effective for cross-validation than time series analysis in China.

Specifically, in urban areas, NTL effectively reflects economic activity compared to rural areas for both DMSP and VIIRS. In Indonesia, the correlation of DMSP and VIIRS with GDP is 0.36 and 0.68, respectively. In contrast, in rural areas, the correlation is 0.03 for DMSP and 0.01 for VIIRS. These findings confirm that night-time light data are far more effective in urban areas (Gibson et al. 2021).

Therefore, while acknowledging the inherent limitations of NTL data in capturing rural economic activities, this study uses the yearly high-quality VIIRS dataset (2013–2021) as the most optimal available proxy to track broader patterns of rural economic development. This research contributes by extending the analysis of rural economic dynamics, which is crucial in areas where official village statistics are limited.

Methodology and data

Methodology

The study employs causal inference to evaluate the impact of policy, namely, Indonesia's Village Fund policy. The funds are allocated to *desa* (rural villages) across Indonesia. The control group is *kelurahan* (urban villages) and the *UPT (technical implementation unit)*. Ideally, to investigate effects, this study employs a randomized control trial (RCT) as the gold standard for impact evaluation (Deaton–Cartwright 2018).

RCT is an effective measure for several reasons. It uses randomization to minimize selection bias, establish causality, and reduce confounding factors. An RCT design allows for deductive causal inference, as differences in outcomes between the treatment and control groups can be directly attributed to the intervention (Deaton–Cartwright 2018). RCT solves the causal inference problem by generating average treatment and control groups identical in all respects except for the intervention under investigation. RCTs require no modelling, no assumptions about covariates or confounders, and no assumptions about statistical distributions, other than the existence of counterfactual outcomes (Banerjee et al. 2020).

Although RCTs are the gold standard for impact evaluation, they cannot be applied to analyse the impact of village funds in Indonesia. The main reason is that the Village Fund policy is not implemented randomly; it targets specific areas, namely *desa*. Because this policy is not random, RCTs are inappropriate due to their reliance on the randomization assumption (Deaton–Cartwright 2018).

DID is suitable for research designs that use panel data and include both treated and control groups. The basic model specification employs a two-way fixed-effects approach, defined as follows:

$$y_{it} = \alpha + \beta_1 treatment_{it} + \gamma_i + \delta_t + \varepsilon_{it} \quad (1)$$

where y_{it} represents the outcome variable for unit *i* at time *t*. $treatment_{it}$ is the treatment variable. β_1 represents the coefficient of interest, indicating the average treatment effect. Z_{it} is an additional time-varying covariate. γ_i is the unit fixed effects that capture time-invariant unobserved heterogeneity specific to each unit. δ_t denotes time fixed effects, accounting for time-specific factors that are common across all units. ε_{it} represents the error term.

Nevertheless, the standard differences cannot align with the research aims because the policy is not random. Standard DID cannot fully address the research aims because the policy is not randomly assigned. DID assumes that treatment assignment is independent of potential outcomes, an assumption often violated in nonrandomized interventions (Godard-Sebillotte et al. 2019). Moreover, several non-random treatment assignments occur in the policy context, e.g., introducing new laws, government policies, and regulations (Fredriksson–de Oliveira 2019).

Another important consideration is the parallel trends assumption, which underpins the validity of the DID approach. Systematic differences between treatment and control groups before the intervention can bias the estimated treatment effect. In the context of Indonesia's Village Fund policy, the distinction between treatment and control groups is based on administrative status – *desa* (typically rural villages) and *kelurahan* (typically urban villages). Policy-based treatment represents the non-randomness that will lead to a violation of the parallel trends assumption. Violation of the key parallel trends assumption occurs because pre-existing differences between treatment and control groups affect the outcome (Godard-Sebillotte et al. 2019). Moreover, treatment selection is based on unobserved factors that influence outcomes (Waddington et al. 2023).

To address the limitations of standard DID, this study uses propensity scores to correct for selection bias on observed covariates before applying the DID estimator. A propensity score is the probability that an individual receives treatment given their observed covariates (Rosenbaum–Rubin 1983). Methods such as inverse probability weighting (IPW) or propensity score matching (PSM) use propensity scores to summarize confounding information arising from multiple characteristics (covariates).

Several factors determine why some villages receive treatment while others do not, potentially causing selection bias. Directly adjusting for many of these variables is difficult and risks the curse of dimensionality (Black–Smith 2004). Logistic regression helps by combining all selected covariates into a single measure for each unit – the predicted probability of receiving treatment. This measure, called the propensity score, serves as a balancing score that captures the key information needed to correct the bias of observed confounders (Chesnaye et al. 2022, Rosenbaum–Rubin 1983).

Propensity scores can assign weights to each individual, producing a balanced "pseudo-population" where distributions of measured confounders are similar across treatment and control groups (Bettega et al. 2024, Chesnaye et al. 2022). This balancing process aims to make the treatment and control groups comparable, which occurs automatically in RCTs.

The present study adopts an IPW over PSM. While PSM often discards observations that do not exhibit good matches between the treatment and control group, IPW retains all observations by assigning weights, preserving the original sample size and statistical power (Black–Smith 2004). IPW addresses selection bias in observational studies by balancing the distribution of observed confounders between treatment and control groups using weights (Avagyan–Vansteelandt 2021, Carry et al. 2021).

To implement IPW, the first step is to run logistic regression to estimate the propensity score, as shown in the following equation:

$$P(treatment_{it} = 1 \,|X_{it}, \gamma_i, \delta_t) = \Lambda(\beta_0 + \beta_3 X_{it} + \gamma_i + \delta_t) \quad (2)$$

where $P(treatment_{group_{it}} = 1 \,|X_{it}, \gamma_i, \delta_t\,)$ indicates the conditional probability that unit **i** receives treatment at time **t**. Λ represents is the logistic function. X_{it} denotes the covariate. γ_i is the unit fixed effects. δ_t denotes time fixed effects. Since policy implementation was introduced in 2015, t < 2015 (before policy implementation).

The next step is to calculate the IPW using the propensity scores obtained from (2):

$$w_{ATT} = Z + (1 - Z) \cdot \frac{pscore_i}{1-pscore_i} \quad (3)$$

where Z represents the treatment indicator (1 if treated, 0 if control). *pscore* indicates the propensity score for observation *i*. The weight from (3) is incorporated into the estimation.

This study utilizes an event study design primarily to address the impact of policy over time. The research benefits from incorporating event study alongside the standard DID two-way fixed effects (TWFE) model. The event study design supports testing the parallel trends assumption, capturing dynamic effects, and addressing potential biases.

Event studies allow researchers to extend the analysis beyond standard DID. They enable examination of pretreatment trends, which is essential for validating the parallel trends assumption underlying DID designs, both visually and statistically (Marcus–Sant'Anna 2021). Furthermore, event studies can capture dynamic treatment effects that evolve over time, providing a more nuanced understanding of policy impacts than a single average treatment effect (Corral–Yang 2024). Event studies can also identify and mitigate potential biases from standard TWFE regression (Borusyak et al. 2024).

Estimating the event study design relies on the following equation:

$$y_{it} = \alpha_i + \lambda_t + \sum^{k\neq -1} \beta_k . D_{it}^k + \beta_4 X_{it} + \varepsilon_{it} \quad (4)$$

where y_{it} is the outcome for unit *i* at time *t*. α_i is unit (individual) fixed effects. λ_t represent time fixed effects. D_{it}^k is event time indicators (1 if unit *i* is *k* periods away from the treatment at time *t*, and 0 otherwise). The summation excludes $k = -1$ to avoid perfect collinearity. β_k indicates the coefficients of interest, capturing the dynamic treatment effects at different periods relative to the treatment. X_{it} represents the covariates and ε_{it} is the error term.

This study also employs heterogeneous analysis to capture the impact of the Village Fund policy across different village characteristics. This analysis aims to improve policy targeting. For example, a Village Fund policy might be highly effective in coastal areas but have little impact in mountainous areas with distinct needs. Understanding these differences enables policymakers to better allocate resources and tailor interventions to local conditions, avoiding a "one-size-fits-all" approach.

Furthermore, the heterogeneous analysis helps to understand program mechanisms and uncover how the program operates. If the effect is stronger in villages with certain characteristics, it suggests that these characteristics serve as important channels for the program. It also assesses equity to reveal hidden facts, such as the policy benefiting wealthier or better-connected villages while leaving poorer ones behind.

The estimation strategy uses interaction terms rather than a subsample approach. Using a subsample would distort the weights, as the original weights are designed to balance the entire sample, not a specific subset. The estimation strategy is as follows:

$$y_{it} = \alpha + \beta_1 treatment_{it} + \beta_2 (treatment_{it} . D_i) + \gamma_i + \delta_t + \varepsilon_{it} \quad (5)$$

where D_i represents the dummy variable for $D_i = 1$ if village i belongs to the subgroup of interest (e.g., coastal areas, eastern part of Indonesia, mountainous areas); $D_i = 0$ if village i belongs to the reference or base group (e.g., non-coastal areas, western part of Indonesia, non-mountainous areas). This equation includes weight.

Data

The study uses both satellite and official statistical data. Satellite data include night-time light (NTL), terrain roughness, precipitation, and net primary productivity (NPP). Microdata from Statistics Indonesia cover village-level geographic information, such as the distance from the village head office to the district main office, coastal versus non-coastal classification, topography, regional location (West or East Indonesia), and the primary sources of income among villagers.

NTL is used as a proxy for economic development at the village level. The data cover 2013–2021 and are divided into two periods: pretreatment (2013–2014) and posttreatment (2015–2021). To address zero values in NTL data, a constant of 0.01 is added before converting the data to its natural logarithm, following the approach of Bruederle–Hodler (2018) and Hodler–Raschky (2014). The analysis encompasses 78,144 villages, categorized into a treatment group (69,858 observations) and a control group (8,286 observations).

Table 1

Data sources

No.	Data	Unit	Sources
1	NTL (yearly)	Radiance (nW/cm²/sr)	Elvidge et al. (2021)
2	Roughness	Meter (standard deviation of elevation)	Amatulli et al. (2018)
3	Precipitation	Millimeter/year (annual mean)	Abatzoglou et al. (2018)
4	NPP	kg x C/m² (kilogram carbon per meter squared)	Running–Zhao (2021)
5	Coastal and non-coastal classification	Categorical	PODES 2014 (Statistic Indonesia 2014)
6	Topography	Categorical	PODES 2014 (Statistic Indonesia 2014)
7	Distance to city centre of ADM2 capital	Kilometer	PODES 2014 (Statistic Indonesia 2014)
8	Main income villagers	Categorical	PODES 2014 (Statistic Indonesia 2014)
9	Village location	Categorical (West and East Indonesia)	PODES 2014 (Statistic Indonesia 2014)

Note: PODES stands for village potential statistics (Potensi Desa).

Results and discussion

The analysis begins with logistic regression as an initial step to obtain the weights for IPW. Table 2 presents the logistic regression results, showing that all covariates used to construct the propensity score are significant at a p-value of < 0.01, indicating that the predictors strongly determine whether a village receives funds. Geographical and socioeconomic factors are strong predictors of treatment assignment, highlighting the potential for confounding factors. Without adjustment, the treatment and control groups would likely differ in baseline characteristics, violating the parallel trend assumption.

The logistic regression results align with the village funds policy design allocated for rural areas. The Ministry of Finance, Republic of Indonesia, allocates village funds based on two types of allocation: basic and affirmative. Basic allocation represents the minimum amount received by each village, calculated as 90% of the total fund divided equally among all villages in Indonesia. The remaining 10% is distributed as affirmative allocation, which considers population size, poverty level, and the geographical challenges of each village (Ministry of Finance 2015).

All rural villages (*desa*) receive the fund, as indicated by the positive coefficient of NPP; villages with high NPP tend to receive the fund. The logistic regression results strongly justify using propensity scores to construct weights for the IPW–DID approach. Furthermore, rural villages are generally far from city centres, consistent with the positive coefficient for distance to the city centre in the logistic regression output, indicating that more remote villages are more likely to receive funds.

Table 2

Logistic regression results for propensity score estimation

Treatment group	Coefficient	St. error	t-value	p-value
Roughness x distance to city centre of ADM2 capital	0.039***	0.003	11.88	0.000
Precipitation	−0.134***	0.013	−10.50	0.000
Net primary productivity	0.198***	0.004	47.46	0.000
Part of Indonesia	−1.017***	0.024	−42.25	0.000
Village terrain	0.141***	0.020	7.02	0.000
Coastal and non-coastal villages	0.387**	0.030	12.83	0.000
Main income source of villagers	−0.550***	0.006	−87.57	0.000
Distance to city centre (ADM2)	0.772***	0.014	53.77	0.000
NTL x roughness x distance	−0.025***	0.001	−27.88	0.000
Constant	0.683***	0.109	6.25	0.000

Mean dependent var	0.894	SD dependent var	0.308
Pseudo r-squared	0.380	Observations	156,288
Chi-square	21,485.295	Prob > chi-square	0.000
Akaike crit. (AIC)	65,539.626	Bayesian crit. (BIC)	65,639.220

Note: *** p<0.01, ** p<0.05, * p<0.1

The covariates related to geographical features align with the affirmative allocation formula, which accounts for the geographical difficulty index. For instance, the interaction between terrain roughness and distance to the city centre has a positive coefficient, indicating that villages with high roughness and located far from the city centre tend to receive the village fund. In contrast, the interaction coefficient between NTL, roughness, and distance to the city centre is positive. This finding reflects the fact that some urban villages, although not close to the ADM2 city centre, are located near the ADM3 capital.

Assessing covariate balance is a critical step before proceeding with estimation. This assessment ensures that the treatment and control groups are comparable in terms of key observable characteristics. When balance is achieved, systematic differences that could confound the analysis are minimized, resulting in a cleaner and more reliable estimation of the treatment effect. In contrast, observed differences in outcomes between the treatment and control groups may be attributed to baseline imbalances rather than to the treatment itself if adequate covariate balance is not established. Such imbalances can undermine the validity of the analysis by leading to violations of the parallel trend assumption, which is a foundational requirement of numerous causal inference methods (Abadie 2005).

Covariate balance can be assessed using the rule of thumb by examining the variance ratio and standardized differences. The literature suggests that the standardized difference should be below 0.25, and the variance ratio should lie between 0.5 and 2 (Rubin 2001, Stuart 2010). In the context of propensity score weighting, good covariate balance means that the distributions of covariates are similar between treatment and control groups after weighting. This comparability mimics what would be achieved through randomization in an experimental design, thereby strengthening the credibility of the resulting conclusions (Chesnaye et al. 2022).

Table 3

Covariate balance test summary

Variables	Standardized differences		Variance ratio	
	raw	weighted	raw	weighted
Roughness x distance	0.928	−0.240	2.293	0.916
Precipitation	0.105	0.042	0.505	0.605
Net primary productivity	0.762	−0.046	0.048	1.310
Part of Indonesia	−0.050	−0.081	0.974	0.961
Village terrain	−0.423	−0.085	2.420	1.189
Coastal and non-coastal villages	0.060	0.064	0.896	0.890
Main income source of villagers	−1.378	−0.036	0.171	1.042
Distance to city centre (ADM2)	0.996	0.020	1.550	0.940
NTL x roughness x distance to city centre	−1.003	0.234	3.354	0.829

Table 3 shows that after weighting, all covariates are balanced, indicating no systematic differences between the treatment and control groups. For example, precipitation has a standardized difference of 0.042, well below the 0.1 threshold, and a variance ratio of 0.605, close to the lower threshold of 0.5. By creating weights based on significant predictors and balancing characteristics between treated and control villages, the study achieves a more robust estimate of the causal effect of village funds (Wei et al. 2023).

Weighting improves the estimation results. Taken together, the results from Tables 4 and 5 strengthen the credibility of the findings. Table 4 shows that the treatment has a positive and significant effect both before and after weighting, with the weighted effect slightly smaller but still robust. Table 5 provides further support through a placebo test, where the false treatment effect is significant before weighting but disappears after weighting. This finding demonstrates that weighting effectively balances the groups and eliminates spurious pretreatment effects. These results indicate that the positive impact observed in Table 4 stems from the program itself rather than pre-existing differences, once selection bias is addressed.

The village fund allocation policy has a significant impact on the treated group. The coefficient of 0.076 indicates that treated villages experienced approximately 7.9[1]% higher outcomes compared to control villages. This value represents the average treatment effect for treated villages across the posttreatment period (2015–2021), relative to the pretreatment baseline. The result aligns with Hartojo et al. (2022), who found that the Village Fund policy positively affects rural economic growth, as proxied by night-time light intensity, in underdeveloped villages.

Table 4

DID regression results

Variables	Before weighting	After weighting
Treatment_status	0.0836***	0.0766***
	(0.0057)	(0.0192)
Village FE	Yes	Yes
Year FE	Yes	Yes
Within R^2	0.0004	0.0010
Observations	703,296	703,296

Notes: robust standard errors clustered at village level in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1

[1] The estimated coefficient of 0.076, expressed in natural logs, is transformed using Euler's number **e** as (**e**^{0.076}−1)\times 100, which equals about 7.9%.

Table 5

Placebo test results

Variables	Before weighting	After weighting
Placebo_status (2014)	−0.0966***	−0.0172
	(0.0065)	(0.0132)
Village FE	Yes	Yes
Year FE	Yes	Yes
Within R^2	0.0025	0.0002
Observations	156,288	156,288

Note: ***p < 0.01, **p < 0.05, *p < 0.1

Robustness testing

The basic assumption of DID analysis is the parallel trend assumption (PTA). This crucial assumption posits that, in the absence of treatment, the difference between the treatment and control groups would remain constant over time. Essentially, the PTA implies that the control group provides a valid counterfactual for the treated units, meaning that, had the intervention not occurred, the treated units would have followed trends parallel to those of the control group (Abadie 2005, Wei et al. 2023).

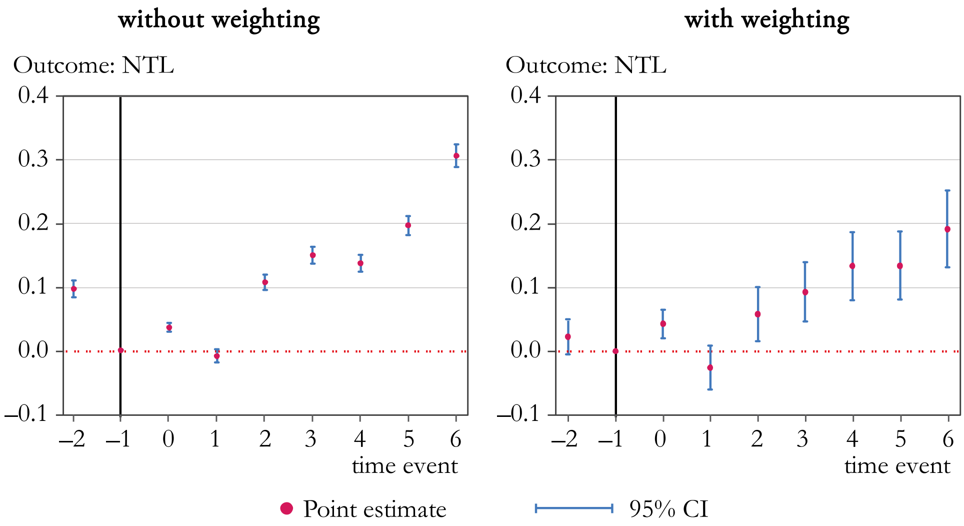
Figure 2 illustrates the visualization inspection of the PTA before and after weighting is applied. The first graph, showing unweighted trends, reveals problematic pretreatment patterns, with clear differences between the treatment and control groups before the 2015 Village Fund policy implementation. In contrast, IPW improves pretreatment parallel trends between 2013 and 2014, indicating that the weighting procedure effectively balances observable characteristics between treated and control villages. This is further supported by confidence intervals that cross zero, demonstrating that applying IPW addresses selection bias in the initial analysis (Carry et al. 2021).

After two years of policy implementation, the initially insignificant output reflects the lag in the benefits of the village fund. During the first years, most allocations were used for infrastructure development, including village roads, irrigation systems, water reservoirs, sports centres, and other health and economic facilities. This explains why the impact of the village fund became statistically significant starting in 2017 (lag 1).

Infrastructure projects require time for completion and integration into existing systems. In some cases, upgrades to rural roads were not aligned with production roads connecting fields to storage facilities or main rural roads, due to the limited funds received by villages at the beginning of the policy. Additionally, communities require time to adapt their economic activities to utilize new infrastructure. Complementary effects between different types of infrastructure – such as roads, irrigation, and markets – also take time to materialize and generate synergistic benefits (Hartojo et al. 2022).

Figure 2

Event study graph (all sample)



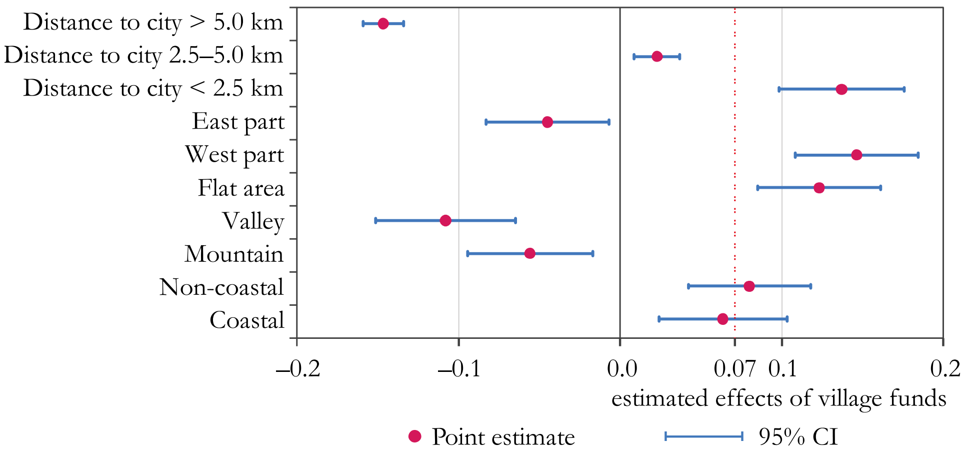
Heterogenous analysis

This section presents the heterogeneous analysis using subsamples based on village location and distance to the nearest city centre. As an archipelagic country, Indonesia exhibits diverse geographical characteristics, including distance to the city centre, region, village location, and coastal versus non-coastal status. These geographical conditions reveal notable heterogeneity between coastal and non-coastal areas in the analysis of the Village Fund policy in Indonesia. Figure 3 illustrates the estimated coefficients of the heterogeneous treatment effects of the policy across different geographical features.

Non-coastal areas benefit more from the Village Fund policy. Several factors explain this finding. First, in landlocked regions, villages have more complex economic activities and resources, allowing them to optimize the funds more effectively for development. Second, non-coastal areas face fewer geographical challenges compared to coastal regions, creating more stable and sustainable conditions for development. These findings contrast with Ravallion–Chen (2007), who found that rural development programs in China often benefited coastal areas more than inland regions.

Figure 3

Heterogeneous treatment effects



Geographical terrain strongly influences the impact of the policy. The analysis reveals a divergence in the Village Fund policy's effectiveness, highlighting that its success is closely tied to local geography. Consequently, the program cannot be considered universally successful; instead, it creates winners and losers depending on terrain. In flat areas, the policy performed as intended and was highly beneficial. Villages in these regions that received the funds experienced outcomes approximately 12.3% higher than the counterfactual trend observed in similar, untreated flat-area villages, indicating that the policy significantly elevated them above their expected trajectory.

However, policy implementation appears to have had unintended negative consequences in more challenging topographies. For villages in mountainous regions, the program was associated with a 5.7% decline in outcomes relative to the control group. This finding suggests that the policy not only failed to benefit certain villages but also caused treated villages in mountainous areas to fall behind the baseline trend of untreated villages. The adverse effect was even more pronounced in valley regions, where the policy was associated with an approximate 10.9% decline in outcomes relative to their counterfactual.

The findings suggest that while the policy propelled flat-area villages forward, it pushed villages in mountains and valleys significantly behind the expected path. This pattern highlights how geographical barriers can limit infrastructure improvements, market access, and service delivery, thereby reducing the effectiveness of policy interventions. The findings reinforce the need for tailored approaches in rural policy design that address unique geographic challenges to ensure inclusivity and maximize impacts.

Significant differences are also observed across regions, with Eastern and Western Indonesia experiencing different impacts. The results indicate that western regions benefited more positively, while the eastern regions experienced negative effects. In the more economically developed western regions, including islands such as Java and Sumatra, the policy had a strong and statistically significant positive effect. The marginal effect suggests that villages receiving the funds experienced substantially greater improvements in outcomes than similar untreated villages in the same region. This finding suggests that in areas with better infrastructure, more established markets, and higher administrative capacity, funds were effectively utilized to generate positive growth.

In contrast, the policy did not yield the same benefits in the less developed eastern part of Indonesia. The marginal effect of the policy in this region was slightly negative initially, but it was not statistically significant. This finding indicates that we cannot confidently assert any positive or negative impact of the program in the eastern regions. It suggests that village funds alone may be insufficient to stimulate growth in areas facing significant structural challenges. Essential complementary conditions, such as robust infrastructure and market access, may be lacking, limiting the productive use of the funds.

Notably, these results contrast with Hartojo et al. (2022), who, using monthly NTL data, found that since the implementation of the Village Fund, underdeveloped villages in East Indonesia – the most remote region – experienced faster growth than those in West and Central Indonesia. The average growth of night-time light intensity post-Village Fund was approximately 156% in East Indonesia, compared to 141% in Central Indonesia and 98% in West Indonesia. These results differ from the current study due to differences in the sample. The research analysed only underdeveloped villages, approximately 26,000 in total.

The heterogeneity analysis examines distance from the city centre, categorizing villages as under 2.5 km, between 2.5 and 5 km, and beyond 5 km. Villages closest to the city centre benefit more from improved infrastructure and market access, allowing them to utilize village funds more effectively. Conversely, the farther a village is from the city, the higher the costs of implementing development projects. For example, when villagers pave roads, renting machinery and purchasing inputs – often only available in urban areas – is more expensive.

Moreover, villages near cities may have higher administrative capacity and human capital, enabling more efficient fund utilization through spillover effects on administrative knowledge. These findings align with Asher–Novosad (2020), who show that villages closer to the city centre experienced greater economic gains from rural road construction programs in India.

Conclusions

Rural development became a key policy in Indonesia in 2015, with every village eligible to receive funds to enhance rural development outcomes. Since policy implementation, research has examined its impact using various outcome measures. However, to our knowledge, no study has examined its impact using a full sample of villages combined with novel data as a proxy for economic growth. Hartojo et al. (2022) only examined villages categorized as underdeveloped. This research provides findings on the village funds' impact on the rural economy, which uses night-time light data as a proxy, and the unavailability of yearly official statistical data at the village level.

Using the DID method with IPW, the findings indicate that the policy positively impacts rural economic development. Since the policy is not randomly assigned, standard DID could produce biased estimates due to violations of the PTA. Weighting helps balance the treatment and control groups during the prepolicy period.

In addition to the main results, the paper presents a heterogeneous analysis based on geographical location and distance of villages from the city centre. In landlocked and non-coastal regions, the policy has a significant impact. Landlocked villages possess more complex economic activities and resources, allowing them to utilize the funds more effectively for development. Furthermore, non-coastal areas face fewer geographical challenges than coastal regions, creating more stable and sustainable conditions for development.

Moreover, geographical disadvantages often outweigh policy impacts. Villages located in mountainous or valley areas, and those far from city centres, experience less benefit than villages in flat terrain close to urban centres. This pattern underscores how geographical barriers can limit infrastructure improvements, market access, and service delivery, thereby reducing policy effectiveness. These findings reinforce the need for tailored approaches in rural policy design that address unique geographic challenges to ensure inclusivity and maximize impact.

Even though the results are robust, this research has limitations. Several issues could inform future research aimed at examining the policy's impact more deeply. First, the study does not account for potential spatial spillover effects that may occur in small-unit analyses. Second, the research uses a binary treatment variable, whereas in practice, every village received some funding, which could support subsequent studies exploring continuous treatment effects.

Acknowledgment

The author thanks Lembaga Pengelola Dana Pendidikan (LPDP)/Indonesia Education Endowment Fund, Ministry of Finance, Republic of Indonesia, for the scholarship.

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