

# The Devolution Deficit: Fiscal Decentralization and the Absence of Local Growth in India

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March 29, 2026

## Abstract

India’s Fourteenth Finance Commission (2015) engineered one of the largest fiscal devolutions in modern history, raising states’ share of central taxes from 32% to 42%—an additional Rs 1.78 lakh crore annually. We exploit formula-driven cross-state variation in per-capita windfall magnitude, using satellite nighttime luminosity for 588 districts over 2008–2023. The baseline difference-in-differences estimate is large and negative, but the event study reveals that much of this reflects a sensor transition and pre-existing convergence dynamics driven by the formula’s income-distance weighting. After absorbing state-specific trends, the preferred estimate is  $-0.089$  log points per windfall SD ( $p < 0.001$ ): districts in higher-windfall states grew *more slowly*. The finding is consistent with untied transfers substituting for more effectively targeted tied grants. The “devolution dividend” that advocates promised is not detectable in nightlights data, though this measure cannot capture non-luminous public goods such as health and education.

**JEL Codes:** H72, H77, O18, R11

**Keywords:** fiscal federalism, intergovernmental transfers, India, Finance Commission, nighttime luminosity

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# 1. Introduction

In April 2015, India’s central government implemented the most dramatic fiscal devolution in the country’s post-independence history. Acting on the Fourteenth Finance Commission’s recommendation, it raised states’ share of central tax revenue from 32% to 42%—a shift that channeled an additional Rs 1.78 lakh crore (approximately \$27 billion) annually to state governments ([Finance Commission of India, 2015](#)). Advocates hailed the reform as a watershed: by replacing earmarked grants with untied transfers, states would finally have the fiscal autonomy to invest in locally appropriate public goods ([Reddy, 2015](#); [Singh and Srinivasan, 2016](#)). The “devolution dividend,” in this telling, was a matter of when, not whether.

This paper tests that claim. Using satellite-based nighttime luminosity data for 588 Indian districts over 2008–2023—combining DMSP (2008–2011) and VIIRS (2012–2023) sensors—I estimate the effect of formula-driven fiscal windfalls on local economic activity. The 14th Finance Commission allocated transfers using a predetermined formula weighting income distance from the richest state (50%), 1971 population (17.5%), geographic area (15%), forest cover (7.5%), and demographic performance (10%). This formula generates cross-state variation in per-capita windfall magnitude that is, by construction, independent of contemporaneous economic conditions.

The headline result is negative. The baseline difference-in-differences estimate—windfall interacted with a post-2015 indicator, with district and year fixed effects—yields a coefficient of  $-0.286$  log points per standard deviation of windfall intensity ( $p < 0.001$ ). But the event study reveals that much of this negative association predates the reform: pre-treatment coefficients in the DMSP era (2008–2011) are large and positive, reflecting both a sensor transition between DMSP and VIIRS and pre-existing convergence dynamics tied to the formula’s income-distance weighting. A separate placebo test using DMSP nightlights (2008–2013) with a false treatment date confirms that the windfall variable predicts pre-reform growth ( $\beta = 0.075$ ,  $p < 0.001$ ).

Once I absorb state-specific linear time trends to account for these pre-existing trajectories and the sensor transition, the estimated effect remains negative and highly significant:  $-0.089$  log points per windfall SD ( $p < 0.001$ ). Districts in states that received larger per-capita windfalls grew *more slowly* after 2015 than their trend-adjusted counterfactual. The standardized effect size is  $-0.065$  standard deviations of the outcome—small to moderate in magnitude, precisely estimated, and opposite in sign to the “devolution dividend” hypothesis.

The finding speaks to a long-standing debate in public finance about whether the composition of intergovernmental transfers matters for growth. [Oates \(1972\)](#) and [Musgrave](#)

(1959) established the theoretical case for fiscal federalism, arguing that local governments possess informational advantages that make decentralized spending more efficient. Oates (1999) refined this into the “decentralization theorem”: absent spillovers and economies of scale, welfare is maximized when each public good is provided by the smallest jurisdiction that internalizes costs and benefits. Bardhan (2002) cautioned, however, that in developing countries, weak local institutions, elite capture, and soft budget constraints can reverse the theoretical advantage.

Empirical evidence is mixed. Martínez-Vázquez et al. (2017) conduct a meta-analysis of fiscal decentralization studies and find wide dispersion, with many null or negative estimates. Litschig and Morrison (2012) exploit discontinuities in Brazilian population-based transfers and find positive effects on education spending and literacy, but these are *earmarked* transfers—the opposite of what the 14th FC created. Knight (2002) shows that federal highway grants crowd out state highway spending nearly one-for-one, and Gordon (2004) finds similar crowding out of state education spending by federal grants. Brollo et al. (2013) documents that windfall transfers in Brazil actually *worsen* governance by attracting lower-quality political candidates and increasing corruption—a “political resource curse.”

In the Indian context, Khemani (2007) studies Finance Commission transfers and finds that delegation to an independent body reduces politically motivated targeting but does not necessarily improve spending quality. Rao and Singh (2005) argue that India’s fiscal federalism is distorted by the center’s use of earmarked transfers to circumvent the Commission’s formula, creating a parallel channel of discretionary spending. The 14th FC was explicitly designed to address this concern by shifting the balance toward formula-based, untied transfers (Finance Commission of India, 2015).

My identification strategy exploits the cross-state variation in windfall magnitude generated by the Commission’s formula. Because the formula weights are tied to predetermined state characteristics—1971 population, geographic area, forest cover, and a backward-looking income measure—the variation is plausibly exogenous to short-run economic conditions. However, the income-distance criterion, which receives 50% of the total weight, creates a mechanical correlation between windfall magnitude and baseline poverty. Poorer states receive more, but poorer states may also be on different growth trajectories for reasons unrelated to fiscal transfers. The failed placebo test confirms this concern, motivating the inclusion of state-specific trends.

The negative trend-adjusted estimate admits multiple interpretations. First, the 14th FC simultaneously *reduced* tied grants and Centrally Sponsored Schemes, meaning the net increase in state resources was smaller than the headline 10-percentage-point share increase (Chakraborty and Dash, 2016). If tied grants were more effectively targeted than

untied transfers—because they came with implementation guidelines and monitoring—the compositional shift could reduce effective spending. Second, [Smart \(1998\)](#) and [Dahlberg et al. \(2008\)](#) show that unconditional transfers can crowd out own revenue effort, reducing the net fiscal stimulus. Third, increased fiscal autonomy may have been absorbed by consumption spending (salaries, pensions) rather than capital investment, consistent with [Pradhan and Ghosh \(2017\)](#), who document that states responded to the 14th FC windfall by increasing borrowing rather than revenue effort.

This paper contributes to the literature on fiscal federalism by providing the first district-level evaluation of India’s 14th Finance Commission using nighttime luminosity as a continuous annual proxy for economic activity. The combination of a large fiscal shock, formula-driven variation, and high-resolution satellite data allows a more granular assessment than the state-level analyses that have dominated the Indian fiscal federalism literature. The negative finding after trend adjustment challenges the assumption that untied transfers automatically translate into local growth, and the failed placebo test illustrates a general challenge for evaluating formula-based fiscal policies: targeting criteria that are “predetermined” for the formula may still predict outcomes through channels other than the transfer itself.

## 2. Institutional Background

**India’s Finance Commission system.** The Indian Constitution mandates a Finance Commission every five years to recommend the vertical (center–state) and horizontal (across states) distribution of central tax revenues. Each Commission produces a formula that allocates a share of the “divisible pool” to individual states. The formula typically combines demographic, geographic, and economic criteria, with weights that change across Commissions ([Rao and Singh, 2005](#)).

**The 14th FC reform.** The Fourteenth Finance Commission, chaired by Y. V. Reddy, recommended increasing the states’ share of central taxes from 32% to 42%—the largest single-Commission increase in Indian history ([Finance Commission of India, 2015](#)). The recommendation was accepted in full and implemented beginning April 2015. The horizontal distribution formula assigned weights of 50% to income distance (the gap between a state’s per-capita GSDP and the richest state), 17.5% to 1971 population, 15% to area, 7.5% to forest cover, and 10% to demographic performance (1971 fertility rate relative to replacement level).

**Simultaneous changes.** The increase in formula-based transfers was partly offset by reductions in tied grants under Centrally Sponsored Schemes. The central government restructured

many CSS programs, reduced matching ratios for wealthier states, and consolidated schemes. As a result, the net increase in state resources was less than the 10-percentage-point headline figure suggested (Chakraborty and Dash, 2016; Singh and Srinivasan, 2016). This compositional shift—from tied to untied transfers—is central to interpreting the empirical results.

**Cross-state variation.** The formula generates substantial variation in per-capita transfers across states. States with lower per-capita income, larger area, more forest cover, and lower 1971 population relative to current population received disproportionately more. The income-distance criterion (50% weight) is the dominant source of variation, channeling the largest per-capita windfalls to states like Arunachal Pradesh, Sikkim, and Mizoram (small, remote, low-income), while reducing the relative shares of states like Maharashtra and Tamil Nadu (large, urbanized, higher-income).

### 3. Data

I construct a district-year panel from three sources. First, annual nighttime luminosity from two satellite sensors available through the SHRUG open data platform (Asher et al., 2021): the DMSP-OLS Operational Linescan System (2008–2013) and the VIIRS Day/Night Band (2012–2023). I aggregate village-level luminosity to 588 Census 2011 districts using the SHRUG geographic crosswalk, producing a balanced panel of 9,408 district-year observations from 2008 to 2023. The combined series provides 7 pre-treatment years and 9 post-treatment years, with a sensor transition occurring in 2012. District fixed effects absorb cross-sectional level differences between sensors, and event study coefficients transparently reveal any residual transition effects. The primary outcome is the log of total district luminosity. Following Henderson et al. (2012) and Chen and Nordhaus (2011), nighttime luminosity serves as a continuous annual proxy for local economic activity—a critical advantage over decennial Census data when evaluating an annual policy shock.

Second, I obtain district-level baseline characteristics from the Census 2011 Primary Census Abstract, including total population, literacy rate, scheduled caste and tribe shares, and worker participation rate.

Third, I construct the treatment variable from the 14th and 13th Finance Commission reports. For each state  $s$ , the per-capita windfall is:

$$\text{Windfall}_s = \frac{0.42 \times \text{FC14\_Share}_s - 0.32 \times \text{FC13\_Share}_s}{\text{Population}_s} \quad (1)$$

This captures the net change in per-capita transfers attributable to the reform, accounting

for both the increase in the total share (32%  $\rightarrow$  42%) and the change in horizontal allocation weights. I standardize the windfall to z-scores for interpretability; a one-unit increase corresponds to one cross-state standard deviation in per-capita windfall intensity.

I exclude Telangana (carved from Andhra Pradesh in June 2014, immediately before the reform), as it has no 13th FC allocation baseline. The final sample contains 9,408 district-year observations across 588 districts in 28 states.

**Table 1:** Summary Statistics

	Pre-period (2012–2014)		Post-period (2015–2023)	
	Mean	SD	Mean	SD
Log total nightlight luminosity	10.137	1.368	9.916	1.358
Total nightlight luminosity	48221.9	51514.0	37579.7	41227.0
Mean nightlight radiance	3.804	5.820	1.250	3.335
Number of villages	1408.3	1232.4	1971.6	1497.1
Population (2011 Census, 000s)	1929.7	1652.8	1929.7	1652.7
Literacy rate	0.616	0.103	0.616	0.103
SC population share	0.147	0.091	0.147	0.091
ST population share	0.188	0.274	0.188	0.274
Worker participation rate	0.411	0.070	0.411	0.070
14th FC windfall (z-score)	-0.236	0.824	-0.236	0.824
Districts		588		588
States		28		28
Observations		4,116		5,292

*Notes:* District-year panel constructed from SHRUG VIIRS nightlights (2012–2023) and Census 2011. Nightlight luminosity is the sum of annual VIIRS radiance across all grid cells within a district. The 14th FC windfall is the per-capita formula-predicted transfer, standardized to z-scores. Pre-period is fiscal years before the 14th FC implementation (April 2015). Panel restricted to districts observed in all 12 years.

## 4. Empirical Strategy

### 4.1 Identification

The identifying variation comes from cross-state differences in per-capita windfall magnitude, driven by the Finance Commission’s predetermined formula. The primary specification is a continuous treatment difference-in-differences:

$$\log(\text{Light}_{d,t}) = \alpha_d + \gamma_t + \beta \cdot (\text{Windfall}_s \times \text{Post}_t) + \varepsilon_{d,t} \quad (2)$$

where  $d$  indexes districts,  $t$  indexes years, and  $s$  denotes the state containing district  $d$ . District fixed effects  $\alpha_d$  absorb time-invariant district characteristics; year fixed effects  $\gamma_t$  absorb aggregate shocks common to all districts (including demonetization in late 2016 and GST implementation in July 2017). The coefficient  $\beta$  captures the average effect of a one-SD increase in formula-driven windfall on log district luminosity in the post-2015 period.

The key identifying assumption is that, conditional on district and year fixed effects, states with different windfall magnitudes would have followed parallel trends in nightlights absent the reform. I probe this assumption with an event study:

$$\log(\text{Light}_{d,t}) = \alpha_d + \gamma_t + \sum_{k \neq -1} \beta_k \cdot (\text{Windfall}_s \times \mathbf{1}\{t - 2015 = k\}) + \varepsilon_{d,t} \quad (3)$$

with  $k = -1$  (2014) as the omitted reference year. Clean pre-treatment coefficients  $\beta_{-3}$  and  $\beta_{-2}$  would support parallel trends.

## 4.2 Threats and Remedies

The income-distance criterion (50% of the formula weight) creates a mechanical correlation between windfall magnitude and state poverty. If poorer states are on structurally different growth trajectories—due to institutional quality, geography, or sectoral composition—the parallel trends assumption may fail. I address this concern in three ways.

First, I conduct a formal placebo test using DMSP nightlights from 2008–2013, applying the 14th FC windfall variable with a false treatment date of 2011. If the windfall variable “predicts” pre-reform changes, the basic specification is biased.

Second, I augment the main specification with state-specific linear time trends:

$$\log(\text{Light}_{d,t}) = \alpha_d + \gamma_t + \delta_s \cdot t + \beta \cdot (\text{Windfall}_s \times \text{Post}_t) + \varepsilon_{d,t} \quad (4)$$

This absorbs any linear differential trend at the state level, isolating the effect of the post-2015 windfall above and beyond pre-existing trajectories.

Third, I perform leave-one-state-out sensitivity analysis to verify that results are not driven by individual outlier states (particularly the small northeastern states with extreme per-capita windfalls).

## 4.3 Inference

Standard errors are clustered at the state level to account for within-state correlation in both treatment assignment and error terms. With 28 clusters—below the conventional 30-cluster threshold for reliable asymptotic inference—I supplement analytical standard errors with a

pairs cluster bootstrap (Cameron et al., 2008), resampling entire states with replacement over 999 replications.

## 5. Results

### 5.1 Main Results

Table 2 presents the core results. Column (1) reports the baseline specification from Equation (2): the estimated effect of windfall intensity on log total district luminosity is  $-0.2861$  ( $SE = 0.0366$ ,  $p < 0.001$ ). The pairs cluster bootstrap  $p$ -value is 0.008, confirming the negative association. However, as the event study below reveals, much of this reflects pre-existing trends and the DMSP-to-VIIRS sensor transition rather than the fiscal reform.

Column (2) uses log mean radiance per grid cell as the dependent variable, yielding a similar negative coefficient of  $-0.2826$  ( $p < 0.001$ ). The consistency across outcome measures suggests the pattern is not driven by district size.

Column (3) adds state-specific linear time trends, producing the paper’s preferred estimate. The coefficient attenuates to  $-0.0885$  ( $SE = 0.0205$ ,  $p < 0.001$ ): after absorbing pre-existing state-level growth differentials and the sensor-transition confound, a one-SD increase in formula-driven windfall is associated with an 8.9% decrease in district nightlights relative to trend. The large attenuation from column (1) to column (3) demonstrates that the baseline specification confounds the fiscal shock with pre-existing convergence dynamics, but the negative sign persists even after trend correction.

**Table 2:** Fiscal Devolution and Nighttime Luminosity

	(1)	(2)	(3)
	Log total light	Log mean radiance	Log total light
Windfall $\times$ Post	$-0.2861^{***}$	$-0.2826^{***}$	$-0.0885^{***}$
	(0.0366)	(0.0459)	(0.0205)
Bootstrap $p$ -value	[0.008]		
Fixed effects	Dist., Year	Dist., Year	Dist., Year, St. trend
Observations	9,408	9,408	9,408
Districts	588	588	588
Pre-treatment mean (dep. var.)	10.137	0.447	10.137
SD(dep. var., pre)	1.368	1.544	1.368

Notes: Each column reports a separate OLS regression. The treatment variable is the state-level per-capita 14th FC windfall (z-score) interacted with a post-2015 indicator. Column (1): log total luminosity; (2): log mean radiance; (3): adds state-specific linear trends. SEs clustered at state level in parentheses. Bootstrap  $p$ -values in brackets (999 replications, resampling states). \*,

\*\*, \*\*\* denote significance at 10%, 5%, 1%.

## 5.2 Event Study

Table 3 reports the event study coefficients from Equation (3). The pre-treatment pattern is striking: the DMSP-era coefficients ( $t - 7$  through  $t - 4$ , covering 2008–2011) are large and positive (0.41 to 0.62), reflecting a combination of sensor calibration differences between DMSP and VIIRS and pre-existing convergence dynamics. The VIIRS-era pre-treatment coefficients ( $t - 3$  and  $t - 2$ , covering 2012–2013) are near zero, suggesting that within the same sensor, the windfall variable does not predict differential trends. The formal pre-trend test rejects the null ( $F = 13.8$ ,  $p < 0.001$ ), driven by the DMSP-era coefficients rather than the VIIRS pre-period.

In the post-treatment period, the coefficients are generally close to zero through  $t + 6$ , with a marginally significant positive coefficient at  $t + 8$  (2023). The absence of a sharp break at  $t = 0$  is consistent with either a null policy effect or a very gradual response, reinforcing the conclusion from Table 2 that the aggregate fiscal devolution did not produce a detectable growth dividend.

**Table 3:** Event Study: Fiscal Devolution and Nighttime Luminosity

Event time	Coefficient	Std. Error	$p$ -value
$t - 7$	0.4074***	(0.0288)	0.000
$t - 6$	0.4911***	(0.0314)	0.000
$t - 5$	0.6185***	(0.0341)	0.000
$t - 4$	0.5850***	(0.0312)	0.000
$t - 3$	-0.0377**	(0.0142)	0.013
$t - 2$	0.0096	(0.0139)	0.492
$t - 1$ (ref.)	—	—	—
$t + 0$	0.0031	(0.0092)	0.742
$t + 1$	-0.0415*	(0.0222)	0.072
$t + 2$	-0.0204	(0.0247)	0.416
$t + 3$	-0.0119	(0.0342)	0.731
$t + 4$	0.0169	(0.0383)	0.663
$t + 5$	-0.0071	(0.0404)	0.861
$t + 6$	0.0348	(0.0415)	0.409
$t + 7$	0.0306	(0.0408)	0.460
$t + 8$	0.0871**	(0.0422)	0.049
Joint pre-trend $F$ -test $p$ -value	see text		
Observations	9,408		

*Notes:* Event study regression of log total district luminosity on interactions between the state-level per-capita 14th FC windfall (z-score) and event-time indicators, with  $t - 1$  (2014) as the omitted reference year. District and year fixed effects included. Standard errors clustered at the state level.

### 5.3 Robustness

Table 4 collects the robustness results. Panel A addresses identification concerns. The placebo test (row 2) is the critical diagnostic: applying the 14th FC windfall to DMSP nightlights from 2008–2013 with a false treatment date of 2011 yields a positive coefficient of 0.0747 ( $p < 0.001$ ). Higher-windfall states were *already growing faster* in DMSP nightlights before the reform, confirming that the formula’s income-distance weighting captures pre-existing convergence dynamics.

Excluding the demonetization year 2017 (row 3) produces no meaningful change ( $\beta = -0.2823$ ,  $p < 0.001$ ). Two-way clustering by state and year (row 4) yields identical conclusions ( $\beta = -0.2861$ ,  $p < 0.001$ ). Critically, restricting the sample to VIIRS data only (2012–2023), which eliminates the sensor transition entirely, yields a near-zero baseline estimate ( $\beta = 0.0195$ ,  $p = 0.618$ ) but a negative and significant trend-adjusted estimate ( $\beta = -0.0487$ ,  $p = 0.016$ ). The sign and significance of the trend-adjusted estimate are consistent across sensor configurations, strengthening confidence in the negative finding.

Panel B examines heterogeneity by baseline economic activity. The negative effect is concentrated among Q1 (darkest) districts ( $\beta = -0.3024$ ,  $p < 0.001$ ), while Q4 (brightest) districts show a statistically insignificant negative coefficient ( $\beta = -0.3993$ ,  $p = 0.471$ ). The larger effect in darker districts is consistent with the formula targeting poorer states whose districts were systematically dimmer at baseline.

The leave-one-state-out analysis yields coefficients ranging from  $-0.488$  to  $-0.264$ , all consistently negative, indicating that the result is not driven by any single state.

## 6. Discussion

The absence of a “devolution dividend” in nightlights data is consistent with several mechanisms. Most directly, the 14th FC did not simply add resources—it *restructured* them, replacing tied grants with untied transfers. If earmarked transfers (Centrally Sponsored Schemes) were more effectively targeted at growth-promoting investments than state governments’ discretionary spending, the compositional shift could reduce effective spending per rupee even as total transfers increased. Gadenne (2017) finds that earmarked transfers in Brazil are more effective at increasing education spending than unconditional grants, precisely because they constrain the set of permissible uses.

A second channel is the crowd-out of own revenue effort. Knight (2002) and Gordon (2004) show that intergovernmental transfers can displace rather than supplement local spending. In the Indian context, Pradhan and Ghosh (2017) document that states responded to the 14th FC windfall by increasing borrowing, suggesting that the additional transfers relaxed

**Table 4:** Robustness and Heterogeneity

	Coefficient	SE	<i>p</i> -value	<i>N</i>
<i>Panel A: Robustness</i>				
Baseline (combined panel)	-0.2861***	(0.0366)	0.000	9,408
Placebo (DMSP 2008–2013)	0.0747***	(0.0123)	0.000	3,528
Excl. 2017 (demonetization)	-0.2823***	(0.0374)	0.000	8,820
Two-way cluster	-0.2861***	(0.0641)	0.000	9,408
VIIRS only (2012–2023)	0.0195	(0.0387)	0.618	7,056
VIIRS only + state trends	-0.0487**	(0.0190)	0.016	7,056
<i>Panel B: Heterogeneity by baseline luminosity</i>				
Q1: Darkest districts	-0.3024***	(0.0595)	0.000	2,352
Q4: Brightest districts	-0.3993	(0.5419)	0.471	2,352
Leave-one-state-out range	[-0.4878, -0.2641]			

*Notes:* All specifications include district and year fixed effects with SEs clustered at the state level, except where noted. The placebo test applies the 14th FC windfall variable to DMSP nightlights (2008–2013) with a false treatment date of 2011. Q1 and Q4 refer to the bottom and top quartiles of pre-treatment (2012–2014) mean district luminosity. \*, \*\*, \*\*\* denote significance at 10%, 5%, 1%.

rather than tightened fiscal discipline.

Third, the [Brollo et al. \(2013\)](#) “political resource curse” channel may apply: windfall transfers can attract rent-seeking, lower the quality of governance, and divert resources from productive uses. In India’s decentralized political system, where state governments face varying degrees of electoral accountability, untied transfers may be especially vulnerable to capture by patronage networks.

A limitation of this analysis is that nighttime luminosity, while a useful proxy for aggregate economic activity, cannot distinguish between the components of growth most likely to be affected by fiscal transfers. Public goods like rural health centers, primary schools, and sanitation infrastructure may improve welfare without producing detectable increases in nightlights. The null result in nightlights does not preclude positive effects on non-luminous dimensions of well-being.

The failed placebo test carries a broader lesson for the evaluation of formula-based fiscal policies. Formulas designed to target disadvantaged populations necessarily use criteria that correlate with outcomes, creating an endogeneity problem that simple difference-in-differences cannot overcome. The income-distance criterion, which dominates the 14th FC formula with 50% weight, is “predetermined” in the sense that it uses historical data, but historical income predicts future growth for structural reasons unrelated to fiscal transfers. Credible evaluation of formula-based policies requires either a discontinuity in the allocation rule ([Litschig and](#)

Morrison, 2012; Dahlberg et al., 2008) or careful trend adjustment—and even trend-adjusted estimates depend on the functional form of the counterfactual growth path.

## 7. Conclusion

India’s 14th Finance Commission created one of the largest natural experiments in fiscal decentralization. Using satellite nightlights for 588 districts, I find that the “devolution dividend”—the claim that untied transfers to states would accelerate local growth—is absent. The null aggregate effect masks differential pre-trends driven by the formula’s targeting of poorer states, and the trend-adjusted estimate is negative. These findings suggest that the *composition* of intergovernmental transfers—tied versus untied, earmarked versus discretionary—matters at least as much as the *level* for translating fiscal resources into local economic activity. For the growing number of countries considering fiscal devolution as a development strategy, the Indian experience counsels caution: more money, at least as measured by nighttime luminosity, does not automatically mean more growth—though the transfers may have produced benefits in dimensions that satellites cannot see.

## Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

**Project Repository:** <https://github.com/SocialCatalystLab/ape-papers>

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## A. Data Appendix

**VIIRS nightlights.** The Visible Infrared Imaging Radiometer Suite (VIIRS) Day/Night Band provides annual composites of nighttime radiance from 2012 onward. I use the SHRUG-harmonized VIIRS annual series (Asher et al., 2021), which assigns radiance values to each of India’s approximately 640,000 villages using the Census 2011 geographic crosswalk. Village-level data is aggregated to 588 districts by summing total radiance and computing population-weighted mean radiance. The total luminosity variable captures both the extensive margin (lit area) and the intensive margin (brightness), making it a composite measure of local economic activity (Henderson et al., 2012; Storeygard, 2016).

**DMSP nightlights.** The DMSP-OLS calibrated nightlights series covers 1992–2013 with a coarser sensor (integer-valued radiance, 0–63 scale). I use the calibrated total luminosity measure for 2008–2013. For the main panel, DMSP provides the 2008–2011 years; for 2012 onward, VIIRS is used. The sensor transition occurs within the pre-treatment period, and its effects are transparently visible in the event study. For the placebo test, I use DMSP data from 2008–2013 as a standalone panel.

**Finance Commission shares.** The 14th FC horizontal devolution shares are from Table 8.5 of the Commission’s report (Finance Commission of India, 2015). The 13th FC shares are from Table 8.3 of its report (Finance Commission of India, 2009). Both sets of shares are percentages of the total divisible pool allocated to each state.

**Sample construction.** I begin with the universe of 640 Census 2011 districts. I drop Telangana (formed June 2014, no 13th FC baseline) and states not covered by the FC formula (Union Territories). The balanced panel requires all 16 years (DMSP 2008–2011 + VIIRS 2012–2023); all 588 districts in 28 states have complete coverage.

## B. Standardized Effect Sizes

**Table 5:** Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
Log total luminosity	-0.0885	0.0205	1.368	-0.0647	0.0150	Moderate negative
<i>Panel B: Heterogeneous (sample splits)</i>						
Q1 districts (darkest)	-0.3024	0.0595	1.316	-0.2299	0.0453	Large negative
Q4 districts (brightest)	-0.3993	0.5419	0.670	-0.5963	0.8092	Large negative

*Notes:* **Country:** India. **Research question:** Does formula-driven fiscal devolution from the central government to state governments increase local economic activity at the district level? **Policy mechanism:** The 14th Finance Commission (April 2015) raised Indian states’ share of central tax revenue from 32% to 42%, with allocation shares determined by a predetermined formula weighting income distance (50%), population (17.5%), area (15%), forest cover (7.5%), and demographic performance (10%), thereby channeling larger per-capita windfalls to poorer and more remote states while simultaneously reducing tied grants. **Outcome definition:** Log total nighttime luminosity (VIIRS annual sum) at the district level, a standard proxy for local economic activity. **Treatment:** Continuous—state-level per-capita formula-predicted fiscal windfall, standardized to z-scores; a one-unit increase corresponds to one standard deviation in cross-state windfall intensity. **Data:** SHRUG VIIRS nightlights (2012–2023), Census 2011 PCA, and 14th/13th FC official devolution shares; district-year panel; 9,408 observations across 588 districts in 28 states. **Method:** Continuous treatment DiD with district and year fixed effects; standard errors clustered at the state level (28 clusters) with pairs cluster bootstrap (999 replications, resampling entire states with replacement). **Sample:** Balanced panel of districts observed in all 12 years (2012–2023); Telangana excluded (carved from Andhra Pradesh in 2014); always-dark districts dropped.  $SDE = \hat{\beta}/SD(Y)$  where  $SD(Y)$  is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ( $|SDE| > 0.15$ ), Moderate (0.05–0.15), Small (0.005–0.05), Null ( $< 0.005$ ).