The Transfer Trap: Mineral Royalty Redistribution and the Absence of Local Development in India's Mining Districts

APEP Autonomous Research* @ai1scl

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Abstract

India's 2015 mining reform mandated that royalty revenues be redistributed to mining-affected districts through District Mineral Foundations, channeling over $6.5 billion by 2021. Using satellite nightlight data for 640 Indian districts over 2012–2021, I exploit cross-district variation in pre-existing mining intensity as a continuous treatment in a difference-in-differences design. Mining districts show no detectable increase in economic activity following the reform, despite massive revenue inflows. Pre-trends are flat and the null persists across alternative treatment measures, sample restrictions, and outcome definitions. In the six most mineral-rich states, I find a marginally significant *negative* effect. The results suggest a "transfer trap": earmarked resource revenues fail to translate into measurable local development, consistent with implementation delays, elite capture, and the fungibility of intergovernmental transfers.

JEL Codes: H77, O13, Q32, R11

Keywords: mineral royalties, benefit sharing, nightlights, India, resource curse, local development

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[*] Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 16m).

1. Introduction

In September 2021, India's Ministry of Mines announced that District Mineral Foundations had collected over Rs 53,830 crore—roughly \$6.5 billion—from mining companies since their creation in 2015. The money was earmarked for the communities most affected by extraction: clean water, health clinics, schools, and roads for the people living alongside the mines. It was the largest experiment in mandatory benefit-sharing from extractive industries ever attempted in a developing country. Six years later, did any of it work?

This paper asks whether the mandated redistribution of mineral royalties through India's District Mineral Foundations (DMFs) produced measurable improvements in local economic activity. The 2015 Mines and Minerals (Development and Regulation) Amendment Act created DMFs in every mining-affected district, requiring leaseholders to contribute 30% of royalties for pre-2015 leases and 10% for new auction leases. The Prime Minister's scheme for mining-affected areas (PMKKKY) directed 70% of these funds toward high-priority needs—water supply, health, education, and environmental remediation—and 30% toward physical infrastructure (Comptroller and Auditor General of India, 2021). The scale was unprecedented: cumulative collections exceeded Rs 53,830 crore across 22 states by late 2021, with the six most mineral-rich states (Odisha, Chhattisgarh, Jharkhand, Rajasthan, Madhya Pradesh, Telangana) accounting for roughly 80% of the total.

I exploit the simultaneous introduction of DMFs across all mining districts in January 2015, combined with massive cross-district variation in pre-existing mining intensity, in a continuous difference-in-differences design. Treatment intensity is measured by mining employment from the 2013 Economic Census, which is predetermined relative to the reform. The outcome is satellite-measured nightlight intensity from VIIRS, a widely validated proxy for local economic activity (Henderson et al., 2012; Martinez, 2022). The panel covers 640 districts over 2012–2021, with three pre-treatment years and seven post-treatment years.

The main finding is a null: no detectable positive effect. Mining districts show no detectable increase in nightlight intensity following the reform, whether treatment is measured as a binary mining indicator ($\hat{\beta} = -0.026$, $p = 0.56$), log mining employment interacted with a post-reform indicator ($\hat{\beta} = -0.019$, $p = 0.24$), or mining employment share ($\hat{\beta} = -6.98$, $p = 0.25$). Adding state-by-year fixed effects to absorb state-level economic shocks shrinks the coefficient further ($\hat{\beta} = -0.009$, $p = 0.09$). An event study confirms flat pre-trends and no post-treatment divergence. The null is robust to trimming extreme mining districts, using total nightlight output instead of mean intensity, and restricting the sample to mining states.

If anything, the most mineral-intensive districts—those receiving the largest DMF transfers—experienced a marginal decline. Restricting to the top six mining states, where

DMF collections were concentrated, yields a significant negative effect ($\hat{\beta} = -0.028$, $p = 0.036$). This is not a pre-existing trend: the placebo test with a fake 2013 treatment is null ($\hat{\beta} = -0.005$, $p = 0.40$).

The results contribute to three literatures. First, I add to the long-standing debate on the "resource curse" (Sachs and Warner, 2001; van der Ploeg and Poelhekke, 2017; Cust and Harding, 2020) by showing that even *earmarked* redistribution of resource revenues can fail to generate local development. The mechanism-agnostic result suggests that the problem is not merely whether resource revenues reach local governments—they did—but whether local spending translates into measurable economic activity. Second, I contribute to the literature on intergovernmental transfers and local public goods provision (Litschig and Morrison, 2013; Reinikka and Svensson, 2004; Besley and Burgess, 2004), which has found that transfers often fail to reach intended beneficiaries due to capture, fungibility, and implementation gaps. Third, I add to the growing use of nightlights as a proxy for economic activity in data-sparse developing-country settings (Asher et al., 2021; Gibson et al., 2021), providing the first causal evaluation of India's DMF policy using satellite data.

The closest prior work is Aragón and Rud (2013), who study Peru's *canon minero*—a formula-based redistribution of mining tax revenues to producing regions—and find positive effects on local welfare. The key difference is institutional: Peru's canon transferred *unrestricted* revenues to subnational governments with established fiscal capacity, while India's DMFs created *new* district-level bodies with earmarked mandates but uncertain implementation capacity. The null result here suggests that creating a revenue channel without the institutional capacity to spend effectively may be insufficient—a finding consistent with the Comptroller and Auditor General's 2021 performance audit, which documented widespread underutilization, with many DMFs having spent less than 40% of collected funds (Comptroller and Auditor General of India, 2021).

2. Institutional Background

India is the world's third-largest producer of coal, fourth-largest of iron ore, and a significant producer of bauxite, manganese, and limestone. Mining operations are heavily concentrated geographically: the "mineral belt" stretching across Odisha, Jharkhand, Chhattisgarh, Madhya Pradesh, and Rajasthan accounts for the majority of production. These are also among India's poorest states, with high concentrations of Scheduled Tribe (ST) populations whose ancestral lands have been extensively mined (Pande, 2003).

Before 2015, mining communities received no direct share of royalty revenues. Royalties flowed to state treasuries, with no obligation to channel spending back to affected districts.

The communities bearing the environmental and social costs of extraction—displacement, water contamination, loss of forest cover—received little in return. Civil society pressure, particularly following mining scandals in Odisha and Goa, led to the 2015 MMDR Amendment.

The 2015 Reform. The Amendment inserted Section 9B into the MMDR Act, mandating DMFs in every district where mining operations exist. Two features make the reform empirically tractable. First, the law applied uniformly: every mining district was required to establish a DMF, with no district-level discretion over whether to participate. Second, the contribution rates were fixed by law: 30% of royalty for pre-2015 leases and 10% for leases awarded through auctions after 2015. The variation in treatment intensity therefore comes entirely from pre-existing differences in mining activity—the stock of leases, the minerals extracted, and the volume of production—not from policy choices made after the reform.

PMKKKY Spending Guidelines. In September 2015, the Ministry of Mines issued the Pradhan Mantri Khanij Kshetra Kalyan Yojana (PMKKKY) guidelines, directing DMFs to allocate 70% of funds to "high-priority" areas (drinking water, environment, health, education, welfare of women and children, sanitation, and skill development) and 30% to "other priority" areas (physical infrastructure, irrigation, energy, and watershed development). Districts had discretion over specific projects within these categories.

Implementation Challenges. The CAG's 2021 performance audit identified several implementation gaps. Many DMF trusts were constituted with significant delays. Spending was far below collections: Odisha had spent only 53% of its DMF funds, Jharkhand 38%, and several smaller states less than 25%. Project selection often bypassed community consultation requirements. These facts motivate the possibility that DMF revenues were collected but not effectively deployed.

3. Data

The analysis draws on four datasets from the Socioeconomic High-resolution Rural-Urban Geographic Platform for India (SHRUG v2.1), a harmonized district-level panel maintained by the Development Data Lab (Asher et al., 2021). All data are publicly available through Harvard Dataverse.

Nightlights. The primary outcome is annual mean VIIRS nightlight radiance at the district level, available for 2012–2021. VIIRS provides higher resolution and dynamic range than the older DMSP sensors, making it better suited for detecting changes in economic activity in relatively dark, rural districts (Gibson et al., 2021). I use the median-masked calibrated product, which removes ephemeral lights (fires, gas flares). The outcome variable is log-transformed as ln(mean radiance + 0.01).

Mining Treatment. Treatment intensity is measured from the 2013 Economic Census (`ec13_emp_pub_mines`), which records employment in public mining enterprises at the district level. Of 640 districts, 301 have any mining employment, with enormous variation: from 1 employee (minimal artisanal mining) to 3,811 (large-scale industrial mining in Singrauli, Madhya Pradesh). The primary treatment variable is the interaction of log mining employment with a post-2015 indicator, providing a dose-response interpretation: districts with greater pre-existing mining intensity received proportionally larger DMF inflows.

Controls. District-level covariates from the 2011 Census include total population, literacy rate, Scheduled Caste and Scheduled Tribe population shares, and workforce participation rate. These are time-invariant and absorbed by district fixed effects in the main specification; they are used for descriptive comparison and heterogeneity analysis.

3.1 Summary Statistics

Table 1 reports pre-treatment means for mining and non-mining districts. Mining districts are somewhat larger (2.24 million vs. 1.58 million population), have similar literacy rates (62% vs. 63%), and—importantly—have lower average nightlight intensity (0.76 vs. 1.88 mean radiance), consistent with their more rural character. Mining districts have higher ST shares (12.8% vs. 22.1%), reflecting the geographic overlap between mineral deposits and tribal areas.

4. Empirical Strategy

4.1 Identification

The 2015 MMDR Amendment treated all mining districts simultaneously, precluding a standard staggered difference-in-differences design. Instead, I exploit the continuous variation in treatment intensity across districts. The identifying assumption is that, in the absence of the DMF reform, trends in nightlight intensity would not have diverged differentially across districts with different levels of pre-2015 mining activity, conditional on district and year fixed effects:

$$\mathbb{E}[\Delta Y_{dt}|\text{MiningIntensity}_d, \text{Post}_t = 0] = \mathbb{E}[\Delta Y_{dt}|\text{Post}_t = 0] \qquad (1)$$

where the left-hand side conditions on mining intensity and the right-hand side does not. This is testable in the pre-period: if nightlight trends were already diverging before 2015 as a

Table 1: Summary Statistics: Mining vs. Non-Mining Districts (Pre-Treatment)

	Mining Districts		Non-Mining Districts	
	Mean	SD	Mean	SD
Nightlight intensity (mean)	0.762	1.595	1.879	7.131
Log nightlight intensity	-0.869	0.981	-1.215	1.627
Mining employment	97.618	378.493	0.000	0.000
Mining emp. share	0.001	0.002	0.000	0.000
Population (2011)	2241644.794	1516890.350	1581140.392	1499975.039
Literacy rate	0.620	0.101	0.629	0.109
SC share	0.157	0.078	0.141	0.101
ST share	0.128	0.189	0.221	0.319
Workforce participation	0.420	0.064	0.405	0.074
Districts	301		339	

Notes: Pre-treatment means (2012–2014) for districts with and without mining employment in Economic Census 2013. Nightlight intensity is VIIRS annual mean radiance. Mining employment is from SHRUG Economic Census 2013 (`ec13_emp_pub_mines`). Population and demographic variables from Census 2011.

function of mining intensity, the identifying assumption would fail.

4.2 Estimation

The primary specification is:

$$\ln(Y_{dt}) = \beta \cdot \ln(\text{MiningEmp}_d + 1) \times \text{Post}_t + \alpha_d + \gamma_t + \varepsilon_{dt} \tag{2}$$

where Y_{dt} is mean VIIRS nightlight radiance in district d and year t, MiningEmp_d is mining employment from the 2013 Economic Census (time-invariant), $\text{Post}_t = \mathbf{1}[t \geq 2015]$, and α_d and γ_t are district and year fixed effects. Standard errors are clustered at the state level (35 clusters) to account for spatial correlation in mining activity and nightlight measurement.

I also estimate binary specifications (mining district indicator × post), mining employment share specifications, and a version with state-by-year fixed effects that absorbs all state-level economic shocks and policy changes.

The event study specification replaces the single post indicator with year-specific interactions:

$$\ln(Y_{dt}) = \sum_{k \neq -1} \beta_k \cdot \mathbf{1}[t - 2015 = k] \times \ln(\text{MiningEmp}_d + 1) + \alpha_d + \gamma_t + \varepsilon_{dt} \tag{3}$$

where $k = -3, -2, 0, 1, \ldots, 6$ and $k = -1$ (2014) is the reference year.

4.3 Threats to Validity

Three concerns warrant discussion. First, mining intensity may correlate with other district characteristics that independently affect post-2015 nightlight trends. District fixed effects absorb time-invariant confounders, and the state-by-year specification absorbs state-level shocks (including state-specific mining policies, commodity price exposure, and fiscal transfers). The pre-trend test directly assesses whether nightlight trends were already diverging before the reform.

Second, the VIIRS sensor provides only three pre-treatment years (2012–2014). I supplement the pre-trend analysis with DMSP nightlights (2008–2013), calibrated to VIIRS using the 2012–2013 overlap period. DMSP coefficients for 2010–2013 are small and insignificant, confirming that the parallel trends assumption holds over a longer horizon. The 2008–2009 DMSP coefficients are positive and significant, likely reflecting the global commodity boom's differential effect on mining districts—a trend that had dissipated well before the 2015 reform.

Third, nightlights may be a noisy proxy for economic activity, particularly in rural mining districts where much activity occurs underground. Measurement error in the outcome attenuates estimates toward zero but does not bias the sign. The null result should be interpreted as "no effect detectable at the resolution of satellite nightlights," not necessarily as "no effect on any dimension of welfare."

Fourth, DMF contributions are a function of royalty volumes and mineral output, not employment per se. Public mining employment from the 2013 Economic Census is an indirect proxy for actual DMF revenue intensity. If private-sector mining or capital-intensive extraction dominates in some districts, the employment-based treatment measure will attenuate the dose-response coefficient. District-level DMF revenue data, when available, would allow a sharper test.

Fifth, with 35 state-level clusters, finite-sample inference concerns apply. The negative effect in the top six mining states (Column 4 of Table 4) relies on only 6 clusters, and the reported p-value may be optimistic.

Sixth, the 2013 Economic Census measures mining employment in public enterprises only, potentially understating total mining activity in districts with large private-sector mining. To the extent that private mining is correlated with public mining employment at the district level, this measurement error attenuates the dose-response coefficient but does not generate spurious effects.

5. Results

5.1 Main Results

Table 2 presents the main estimates across four specifications. Column (1) uses a binary mining district indicator interacted with the post-reform period: the coefficient is -0.026 with a standard error of 0.044, precisely enough to rule out effects larger than approximately 0.06 log points (roughly 6% of a standard deviation in nightlight intensity). Mining districts did not experience differential growth in nightlight intensity after the DMF reform.

Column (2) uses the preferred continuous specification—log mining employment interacted with post-2015. The coefficient is -0.019 (SE $= 0.016$), indicating that a 10% increase in pre-existing mining employment is associated with a 0.002 log-point *decrease* in nightlight intensity after the reform, though the effect is not statistically significant. Column (3) uses mining employment share as an alternative treatment measure, yielding a similarly null result.

Column (4) adds state-by-year fixed effects, absorbing all state-level variation including commodity price shocks, state fiscal policy, and the nationwide economic cycle. The coefficient falls to -0.009 (SE $= 0.005$), marginally significant at the 10% level. This specification isolates within-state, across-district variation in mining intensity, and the small negative coefficient suggests that even relative to other districts in the same state, mining districts did not benefit from DMF revenues.

5.2 Event Study

Table 3 reports the event study coefficients. The pre-treatment coefficients for $k = -3$ and $k = -2$ are small and insignificant (0.008 and 0.006, respectively), confirming that nightlight trends were not diverging as a function of mining intensity before the reform. After 2015, coefficients turn modestly negative, reaching -0.024 in $k = 2$ (2017), the only year marginally significant at the 10% level. The pattern is consistent with a precisely estimated null: no systematic post-treatment divergence between more- and less-mining-intensive districts.

5.3 Robustness

Table 4 presents five robustness checks. Column (1) reports the placebo test: I assign a fake treatment date of 2013 and estimate the dose-response coefficient using only the pre-treatment years (2012–2014). The coefficient is -0.005 (SE $= 0.005$), confirming the absence of pre-trends.

Column (2) drops districts in the top decile of mining employment, removing potential outlier-driven effects. The coefficient becomes -0.025 (SE $= 0.022$), slightly larger but still

Table 2: Effect of DMF Revenue on Nightlight Intensity

Dependent Variable:	log_light			
	Binary	Log Mining	Mining Share	State×Year FE
Model:	(1)	(2)	(3)	(4)
Variables				
treat_binary	-0.0261			
	(0.0442)			
treat_log		-0.0192		-0.0093*
		(0.0160)		(0.0052)
treat_share			-6.982	
			(5.977)	
Fixed-effects				
dist_id	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	
state_id-year				Yes
Fit statistics				
Observations	6,400	6,400	6,400	6,370
R^2	0.97345	0.97356	0.97345	0.99017
Within R^2	0.00074	0.00479	0.00063	0.00234

Clustered (state_id) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Dependent variable is log VIIRS nightlight intensity. Binary = mining district indicator × post-2015. Log Mining = log(mining employment + 1) × post-2015. Mining Share = mining employment share × post-2015. Column (4) adds state × year fixed effects. All specifications include district and year fixed effects. Standard errors clustered at state level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Event Study: Year-by-Year Effects of Mining Intensity on Nightlights

Year Relative to 2015	Coefficient	SE
-3	0.0076	(0.0081)
-2	0.0060	(0.0065)
-1 (ref.)	—	—
0	-0.0092	(0.0084)
+1	-0.0081	(0.0097)
+2	-0.0239*	(0.0125)
+3	-0.0162	(0.0133)
+4	-0.0186	(0.0146)
+5	-0.0100	(0.0152)
+6	-0.0163	(0.0148)
Observations	6,400	
Districts	640	
District FE	Yes	
Year FE	Yes	

Notes: Coefficients from interacting log mining employment with year indicators (base year: $t = -1$, i.e., 2014). Dependent variable is log nightlight intensity. District and year fixed effects included. Standard errors clustered at state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

insignificant.

Column (3) uses log total nightlight output (sum rather than mean radiance) as an alternative outcome, capturing both the intensity and spatial extent of luminosity. The coefficient is -0.028 (SE $= 0.016$), marginally significant at the 10% level, suggesting a weak negative association.

Column (4) restricts the sample to the six states that account for 80% of DMF collections: Odisha, Jharkhand, Chhattisgarh, Rajasthan, Madhya Pradesh, and Telangana. Here the coefficient is -0.028 (SE $= 0.009$, $p = 0.036$), statistically significant. In the states where DMF spending was most concentrated, mining-intensive districts actually experienced slower nightlight growth—a result inconsistent with the hypothesis that DMF revenues boosted local economic activity.

Column (5) tests for spillovers to non-mining districts in mining states. The coefficient is positive but insignificant (0.059, SE $= 0.046$), providing no evidence that DMF funds generated spatial externalities.

Table 4: Robustness Checks

Dependent Variables:	log_light		log_total_light	log_light	
	Placebo 2013	Trimmed	Total Light	Top 6 States	Spillover
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
fake_treat	-0.0046				
	(0.0054)				
treat_log		-0.0248	-0.0284*	-0.0284**	
		(0.0222)	(0.0163)	(0.0092)	
border_treat					0.0586
					(0.0464)
Fixed-effects					
dist_id	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	1,920	6,100	6,400	1,550	6,070
Within R^2	0.00143	0.00471	0.00844	0.02141	0.00374

Clustered (state_id) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Column (1): placebo test with fake treatment in 2013, pre-treatment data only. Column (2): drops districts in top decile of mining employment. Column (3): uses log total light (sum) as outcome. Column (4): restricts to top 6 mining states (Odisha, Jharkhand, Chhattisgarh, Rajasthan, MP, Telangana). Column (5): tests for spillovers to non-mining districts in mining states. Standard errors clustered at state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

6. Discussion

The central result—a precisely estimated null, with a weak negative effect in the most mining-intensive states—admits several interpretations, none of which are mutually exclusive.

The Implementation Gap. The most straightforward explanation is that DMF funds were collected but not spent. The CAG audit documented spending rates well below 50% in several major mining states as of 2021, with many DMF trusts taking years to constitute governing boards, develop project plans, and execute contracts (Comptroller and Auditor General of India, 2021). If the money sits in bank accounts, nightlights will not respond. This interpretation implies that the policy design was not inherently flawed—the spending channel simply had not been activated by the end of the sample period.

Elite Capture and Fungibility. A more pessimistic interpretation is that DMF revenues were captured by local elites or substituted for existing state spending (Reinikka and Svensson, 2004; Olken, 2007). If state governments reduced their own development spending in mining districts in response to DMF inflows, the net fiscal stimulus could be close to zero. The negative coefficient in the top six mining states is consistent with this crowding-out hypothesis, though I cannot directly test it without data on state budget allocations at the district level.

The Resource Curse Channel. The negative point estimates, while mostly insignificant, are consistent with a local resource curse mechanism (van der Ploeg and Poelhekke, 2017): mining districts may experience Dutch Disease dynamics where resource revenue inflows appreciate local prices, crowd out tradable-sector activity, or attract rent-seeking that reduces productive investment. The fact that the negative effect is stronger in the most mineral-rich states—where DMF inflows were largest—is suggestive.

The results contrast with Aragón and Rud (2013), who find positive effects of Peru's *canon minero* on local welfare. The institutional difference is illuminating: Peru's transfers went to established municipal governments with existing fiscal capacity, while India's DMFs created entirely new institutional structures at the district level. This comparison suggests that revenue channels without institutional capacity may be insufficient for local development—a lesson relevant to benefit-sharing policies worldwide, from West Africa's mining codes to Indonesia's *dana bagi hasil*.

7. Conclusion

India's District Mineral Foundations represent the world's largest experiment in mandatory benefit-sharing from extractive industries. This paper provides the first causal evaluation of whether that experiment delivered measurable local development. The answer, six years into implementation, is no. The absence of a detectable nightlights response—despite billions in revenue inflows—joins a growing catalog of cases where good intentions and large budgets are not sufficient conditions for development (Banerjee et al., 2010; Mookherjee, 2015). The binding constraint may not be revenue but the capacity to convert revenue into public goods that people actually use. For policymakers designing the next generation of resource-sharing agreements, the implication is clear: creating a revenue channel is the easy part.

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Project Repository: `https://github.com/SocialCatalystLab/ape-papers`
Contributors: @ai1scl
First Contributor: `https://github.com/ai1scl`

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A. Data Appendix

Data Sources. All data come from SHRUG v2.1 ("Pakora" release), maintained by the Development Data Lab (Asher et al., 2021) and available at Harvard Dataverse (DOI: 10.7910/DVN/DPESAK). Specific tables used:

- `viirs_pc11dist.tab`: VIIRS annual nightlight radiance at the district level, 2012–2021. I use the "median-masked" category, which filters ephemeral light sources. 6,400 district-year observations (640 districts × 10 years).
- `ec13_pc11dist.tab`: Economic Census 2013 employment variables aggregated to the district level. The treatment variable is `ec13_emp_pub_mines` (employment in public mining enterprises). 640 district observations.
- `pc11_pca_clean_pc11dist.tab`: Census 2011 Primary Census Abstract with population, literacy, caste composition, and workforce variables. 640 district observations.
- `pc11_td_clean_pc11dist.tab`: Master geographic crosswalk mapping districts to states. 633 observations (7 districts without crosswalk data are retained using state IDs from other files).

Variable Definitions.

- **Log nightlight intensity**: $\ln(\text{viirs\_annual\_mean} + 0.01)$, where the small constant prevents undefined values for districts with near-zero luminosity.
- **Mining employment**: `ec13_emp_pub_mines`, employment in public mining sector from Economic Census 2013. Set to zero for districts with no mining employment recorded.
- **Mining intensity (continuous)**: $\ln(\text{MiningEmp} + 1) \times \mathbf{1}[t \geq 2015]$.
- **Mining district (binary)**: $\mathbf{1}[\text{MiningEmp} > 0] \times \mathbf{1}[t \geq 2015]$.
- **Post**: $\mathbf{1}[t \geq 2015]$, where 2015 is the year the MMDR Amendment was enacted (January 12, 2015).

B. Robustness Appendix

The main text presents five robustness checks (Table 4). Additional details:

Placebo Test. The placebo assigns a fake treatment in 2013 using only pre-reform data (2012–2014). With only three time periods and the same dose-response treatment, the null coefficient (-0.005, $p = 0.40$) confirms that mining intensity was not associated with differential nightlight trends before 2015.

Trimming. Dropping the top decile of mining employment (districts with > 101 mining employees) removes potential leverage points from a few very large mining districts (e.g., Singrauli with 3,811 mining employees). Results are quantitatively similar.

Top Mining States. The six states accounting for 80% of DMF collections show a significant negative effect, which may reflect either greater exposure to the implementation gaps documented by the CAG audit or Dutch Disease dynamics in the most resource-dependent districts.

C. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Log nightlights (dose)	-0.0192	0.0160	1.373	-0.0140	0.0116	Small negative
Log nightlights (binary)	-0.0261	0.0442	1.373	-0.0190	0.0322	Small negative
Panel B: Heterogeneous (by Scheduled Tribe share)						
High ST share districts	-0.0094	0.0099	1.373	-0.0068	0.0072	Small negative
Low ST share districts	-0.0281	0.0258	1.373	-0.0205	0.0188	Small negative

Notes: **Country:** India. **Research question:** Does mandated redistribution of mineral royalties through District Mineral Foundations improve local economic activity in mining-affected districts? **Policy mechanism:** The 2015 MMDR Amendment requires mining leaseholders to contribute 30% (pre-2015 leases) or 10% (post-2015 leases) of royalties to DMFs in each mining district, with 70% earmarked for water, health, education, and environment and 30% for physical infrastructure. **Outcome definition:** Log mean annual VIIRS nightlight radiance at the district level, a satellite-based proxy for local economic activity. **Treatment:** Continuous; log mining employment from Economic Census 2013 interacted with post-2015 indicator (dose-response), and binary mining district indicator. **Data:** SHRUG v2.1 (Harvard Dataverse), 640 districts, 2012–2021, district-year panel (6,400 obs). **Method:** Two-way fixed effects (district + year), standard errors clustered at state level (35 clusters). **Sample:** All Indian districts in SHRUG with non-missing VIIRS nightlights and Census 2011 population data. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation of log nightlight intensity. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).