Cash and Convergence: Banking Infrastructure, Demonetization, and the Leveling of India's Economic Geography

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Abstract

On November 8, 2016, India invalidated 86.9% of its currency overnight. We exploit this shock to study how formal-sector exposure shapes the transmission of monetary disruptions across 640 districts over 2012–2023. Districts with greater banking density—a proxy for formal economic integration—experienced relatively *larger* declines in nighttime luminosity post-demonetization, consistent with a convergence effect. A one-standard-deviation increase in bank branches per capita is associated with approximately 8% lower nightlight growth post-2016 ($p = 0.065$), but this effect is absorbed by controls for non-agricultural worker share, revealing that banking density proxies for economic formality rather than acting as an independent channel. The effect is concentrated in agricultural districts, where formal market institutions amplified exposure to the cash shortage. Demonetization acted as a leveling shock, compressing the distribution of economic activity across Indian districts.

JEL Codes: E42, E58, G21, O16, O18

Keywords: demonetization, banking infrastructure, nightlights, India, convergence, financial access

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1. Introduction

Eighty-seven percent of India's currency vanished overnight. At 8:15 PM on November 8, 2016, Prime Minister Narendra Modi announced that all Rs 500 and Rs 1,000 banknotes—representing Rs 15.44 trillion, roughly 12.5% of GDP—would cease to be legal tender effective midnight. Citizens had fifty days to exchange old notes at bank branches. It was the largest monetary shock in modern economic history.

The conventional wisdom, supported by Chodorow-Reich et al. (2020), is that demonetization disrupted economic activity in proportion to localities' dependence on cash. Districts where the old currency was replaced more slowly suffered greater output losses. But this framing leaves a deeper question unanswered: *did the infrastructure designed to facilitate formal transactions—bank branches—actually protect local economies from the shock, or did it amplify their exposure?*

This paper provides the first district-level analysis of how pre-existing banking infrastructure mediated the economic effects of India's demonetization, using twelve years (2012–2023) of satellite nightlight data from SHRUG for 640 Indian districts. We measure banking infrastructure from the Census 2011 Town Directory—government, private commercial, and cooperative bank branches per 100,000 population—and interact this cross-sectional intensity measure with post-demonetization year indicators in a continuous-treatment difference-in-differences framework.

Our central finding challenges the intuitive "buffering" hypothesis. Districts with more banking infrastructure experienced *relatively larger declines* in nighttime luminosity after demonetization. In our baseline event study, the coefficient on banking intensity turns sharply negative in 2017 ($\hat{\beta}_{2017} = -0.018$, $p = 0.016$) and remains negative through 2023, with pre-trend coefficients indistinguishable from zero. A pooled difference-in-differences estimate yields a coefficient of -0.017 on the interaction of bank branches per 100K with a post-2016 indicator ($p = 0.065$, clustered at the state level). Randomization inference over 500 permutations of the banking intensity variable yields a p-value < 0.01, confirming the result is not driven by chance assignment.

The effect is concentrated in agricultural districts. Splitting the sample at the median agricultural worker share, we find a coefficient of -0.064 ($p = 0.027$) in high-agriculture districts—roughly four times larger than the full-sample estimate—and a null effect in low-agriculture districts (-0.008, $p = 0.27$). This heterogeneity points to a mechanism: in rural agricultural areas, banking infrastructure channels economic activity through formal market institutions—regulated grain markets (*mandis*), procurement agencies, and cooperative credit—that were acutely disrupted when cash liquidity collapsed. Areas without banks, paradoxically, were insulated by their very informality.

Critically, the banking effect vanishes when we control for non-agricultural worker share interacted with a post indicator. Adding this single control shifts the banking coefficient from -0.017 ($p = 0.065$) to 0.003 ($p = 0.58$). This decomposition reveals that banking density is not an independent causal channel but rather a proxy for the formality of the local economy. The economic disruption of demonetization was transmitted through the formal economy—and banking infrastructure measures its extent.

We interpret these results as evidence that demonetization functioned as a *leveling shock*. More developed districts—characterized by greater banking penetration, higher non-agricultural employment, and more formal economic activity—experienced disproportionate disruption, while less developed districts maintained relatively stable nightlight trajectories (cf. Drèze and Sen, 2019). This is consistent with Chanda and Cook (2022), who find redistributive medium-run effects of demonetization at the district level, but our banking-infrastructure decomposition provides a structural explanation for *why* the redistribution occurred.

This paper contributes to the literature in three ways. First, we provide the most comprehensive district-level analysis of demonetization's long-run effects, extending the sample through 2023—seven years post-event—using the SHRUG satellite data platform (Asher et al., 2021). Chodorow-Reich et al. (2020) study effects through 2017Q2 using monthly data; our annual panel sacrifices frequency for a much longer horizon, revealing that the convergence effect persists through 2020 before gradually attenuating. Second, we distinguish the banking infrastructure channel from the currency replacement channel identified by Chodorow-Reich et al. (2020). While they use the ratio of new to old currency dispensed (a supply-side measure), we use pre-existing bank branch density (a demand-side, structural measure). The two measures capture different mechanisms: currency replacement reflects the speed of remonetization, while banking density reflects the depth of formal financial intermediation. Third, we document a novel heterogeneity result: the effect is concentrated in agricultural districts, where formal market institutions created a transmission channel for the liquidity shock.

2. Institutional Background

2.1 India's Demonetization

On November 8, 2016, Prime Minister Modi announced on national television that all Rs 500 and Rs 1,000 denomination banknotes of the Mahatma Gandhi Series would be demonetized with immediate effect. These two denominations constituted 86.9% of the currency in circulation by value—Rs 15.44 trillion out of Rs 17.77 trillion. The stated objectives were combating "black money" (undisclosed income), counterfeit currency, and terrorist financing (Ghosh et al., 2017; Agarwal et al., 2017).

The implementation imposed severe constraints on currency exchange. Citizens could deposit unlimited amounts into bank accounts, but over-the-counter exchange was limited to Rs 4,000 per person (later reduced to Rs 2,000). Weekly cash withdrawal limits of Rs 24,000 from bank accounts were imposed. ATMs required recalibration because the new Rs 500 and Rs 2,000 notes had different dimensions. The exchange window closed on December 30, 2016, just 50 days after the announcement.

The remonetization process was severely constrained. The Reserve Bank of India (RBI) could inject only Rs 12,500 crore per day in the initial weeks, far below what was needed to replace Rs 15.44 trillion. Full currency levels were not restored until mid-2017. The RBI's 2018 report revealed that 99.3% of demonetized notes were eventually deposited in banks, undermining the government's claim that significant "black money" would fail to return (Reserve Bank of India, 2018). Lahiri (2020) models the cash shortage as a binding transaction constraint, showing that the output costs of demonetization are increasing in the economy's cash dependence—a prediction our cross-district results confirm empirically.

2.2 India's Banking Landscape

India's banking infrastructure is deeply uneven. As of Census 2011, the median district had 3.6 bank branches per 100,000 population, but the interquartile range spanned 2.2 to 5.7, and the standard deviation (4.8) nearly equaled the mean (4.9). Some metropolitan districts had over 40 branches per 100,000, while remote tribal districts had fewer than one.

This variation has deep historical roots. The RBI's social banking mandate, originating in the 1969 bank nationalization and formalized in the Branch Authorization Policy, required banks to open rural branches in "underbanked" districts—those with below-average branch density (Burgess and Pande, 2005). This policy dramatically expanded rural banking access during 1977–1990 but was relaxed after financial liberalization. Post-2005, the RBI's revised authorization policy again directed expansion to underbanked districts, but private banks concentrated in profitable urban markets (Karmakar and Narayanan, 2020).

The geographic distribution of bank branches correlates strongly with other development indicators: literacy, non-agricultural employment, road connectivity, and urbanization. This correlation is central to our identification challenge. When we find that banking density predicts differential demonetization effects, we must determine whether banking is a causal channel or a proxy for broader economic structure.

2.3 Why Banking Infrastructure Matters for Demonetization

The mechanics of note exchange created a direct link between banking access and demonetization impact. To convert old notes to new, individuals needed physical access to a bank branch or ATM. Villages without bank branches faced substantial costs: travel time to the nearest branch (often hours by bus in rural areas), opportunity costs of waiting in queues that routinely exceeded 4–6 hours, and the risk of returning home empty-handed when branches exhausted their daily cash allocation.

But banking access has a second, subtler channel. Districts with more bank branches have, by definition, more *formal economic activity*—bank-intermediated transactions, formal credit markets, and regulated commercial exchanges. When cash disappeared, these formal channels froze. Informal transactions—barter, informal credit, trust-based exchange networks—continued unimpaired. The irony is that demonetization's stated goal of formalization may have temporarily *punished* the already-formal economy.

2.4 The Agricultural Marketing Channel

A critical institutional detail for our heterogeneity analysis is the structure of India's agricultural marketing. The Agricultural Produce Market Committee (APMC) Acts, enacted by most Indian states, mandate that farmers sell certain notified commodities through regulated wholesale markets called *mandis*. Transactions at mandis are conducted predominantly in cash: buyers pay licensed commission agents (*arthiyas*), who settle accounts with farmers after deducting commissions and market fees.

This institutional structure creates a tight link between banking infrastructure and agricultural markets. Commission agents maintain current accounts at local bank branches to settle transactions; farmers receive payments through bank transfers or cash withdrawn from these accounts; and the cooperative credit system that finances agricultural inputs operates entirely through the banking channel. When demonetization disrupted cash availability at bank branches, the entire agricultural marketing chain seized: farmers could not sell produce, commission agents could not settle accounts, and the post-harvest marketing season (November–January for kharif crops) was severely disrupted.

Districts with more bank branches had, by extension, deeper integration into the formal agricultural marketing system. These are precisely the districts where the cash squeeze was most consequential—not because banks failed to provide currency (though queues and daily limits constrained disbursement), but because the formal economic activity that banks intermediated was paralyzed by the absence of its primary medium of exchange.

2.5 Related Literature

Our paper connects to three strands of the literature. The first is the growing body of work on demonetization's economic effects. Chodorow-Reich et al. (2020) provide the seminal causal analysis, using the rate of new currency injection as an instrument for cash availability. They estimate that districts receiving fewer new notes experienced larger employment declines, with nightlights declining proportionally. Chanda and Cook (2022) extend the analysis to medium-run effects, finding evidence of redistribution: less-developed districts gained relative to more-developed ones, a pattern they attribute to the disruption of urban informal economies. Our paper contributes to this literature by identifying the banking infrastructure channel through which the convergence effect operates.

The second strand concerns the economic effects of banking infrastructure in developing countries. Burgess and Pande (2005) document that India's social banking expansion during 1977–1990 reduced rural poverty, providing causal evidence that bank branch access facilitates financial intermediation in underserved areas. Our paper extends this insight to the demonetization context: the same banking infrastructure that enabled financial intermediation became a transmission channel for the monetary shock, precisely because it had succeeded in integrating local economies into the formal financial system.

The third strand is the use of nighttime luminosity as a proxy for economic activity. Henderson et al. (2012) establish the theoretical and empirical foundations, showing that nightlights track GDP growth with reasonable precision, especially in countries with poor statistical infrastructure. India is a leading application of this methodology, with nightlights providing consistent annual measures for all 640+ districts over more than a decade. Our use of VIIRS (rather than the older DMSP sensor) avoids top-coding issues in bright urban areas and provides higher spatial resolution. Aggarwal (2020) validates VIIRS as a measure of economic shocks in India, showing that nightlights detect localized disruptions with reasonable precision.

3. Data

3.1 SHRUG Platform

Our primary data source is the Socioeconomic High-resolution Rural-Urban Geographic Platform (SHRUG), version 2.1 (Asher et al., 2021). SHRUG harmonizes village- and town-level data across India's three modern censuses (1991, 2001, 2011), four Economic Censuses (1990, 1998, 2005, 2013), and annual satellite nightlight imagery (DMSP 1992–2013, VIIRS 2012–2023). The platform covers approximately 640,000 villages and 8,000 towns, providing stable geographic identifiers that enable panel construction across data sources with different spatial definitions. SHRUG provides harmonized geographic identifiers that maintain consistent district definitions across census rounds, allowing reliable panel construction even as administrative boundaries change.

3.2 Nighttime Luminosity

We use VIIRS (Visible Infrared Imaging Radiometer Suite) annual nightlight data at the district level as our primary outcome measure. The VIIRS sensor, operational since 2012, provides high-resolution luminosity data that serves as a widely-used proxy for local economic activity (Henderson et al., 2012). Chodorow-Reich et al. (2020) validate the nightlight-GDP relationship in the Indian context, finding that district-level nightlights track GDP growth with an elasticity close to one.

Our analysis panel covers 640 districts over 12 years (2012–2023), yielding 7,680 district-year observations. We use the log of the annual nightlight sum (adding 0.01 to handle zeros) as our dependent variable. The VIIRS sensor avoids the top-coding problem that afflicts DMSP data in bright urban areas, though both sensors are available in SHRUG.

3.3 Banking Infrastructure

Our treatment intensity variable is constructed from the Census 2011 Town Directory, which reports the number of government bank branches, private commercial bank branches, and cooperative banks at the district level. We define total banking density as the sum of all three types divided by the district population, expressed per 100,000 population:

$$B_d = \frac{\text{Gov Banks}_d + \text{Private Banks}_d + \text{Coop Banks}_d}{\text{Population}_d} \times 100{,}000 \tag{1}$$

The three categories of banks have distinct geographic footprints. Government banks (nationalized and State Bank of India affiliates) are the most geographically widespread, having been the primary instrument of the social banking mandate. Private commercial banks concentrate in urban and semi-urban areas with higher profitability. Cooperative banks serve primarily agricultural credit needs and are prevalent in states with strong cooperative movements (Maharashtra, Gujarat, Karnataka). By summing all three, our measure captures the total formal banking infrastructure available in each district.

We construct several alternative measures for robustness: a binary indicator for above-median banking density, quartile indicators, government banks only (excluding private and cooperative), and log bank branches. The Census 2011 timestamp is critical: it pre-dates demonetization by five years, ensuring that our intensity measure is not contaminated by banking responses to the shock itself.

Figure 1 shows the distribution of banking density across districts. The distribution is right-skewed, with most districts clustered between 1 and 8 branches per 100,000 and a long tail extending to nearly 50 (driven by small, wealthy union territories). The median district has 3.6 branches per 100,000.

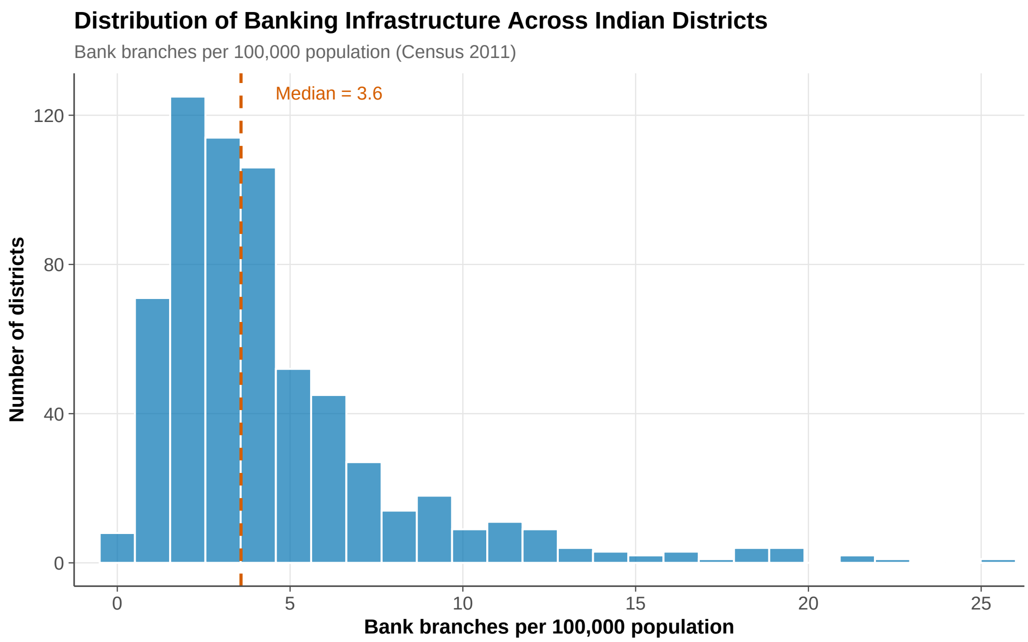

Figure 1: Distribution of Banking Infrastructure Across Indian Districts

Notes: Bank branches per 100,000 population from Census 2011 Town Directory. Includes government, private commercial, and cooperative banks. 640 districts. Five districts lack Town Directory data and are assigned zero banking density. Dashed line indicates median (3.6).

3.4 District Baseline Characteristics

We construct baseline characteristics from the Census 2011 Primary Census Abstract (PCA): total population, literacy rate, SC and ST population shares, worker participation rate, and agricultural worker share (cultivators plus agricultural laborers as a fraction of all workers). These variables serve dual purposes: controlling for differential trends in the event study and defining heterogeneity subgroups.

3.5 Summary Statistics

Table 1 presents summary statistics. Panel A describes district characteristics from Census 2011. The mean district has 1.9 million people, a literacy rate of 0.63, and agricultural workers comprising 38% of the labor force. Panel B describes the nightlight outcome. The mean log nightlight value is 9.10, with substantial cross-district variation (SD = 1.40).

Table 1: Summary Statistics

	N	Mean	SD	P25	Median	P75
Panel A: District Characteristics (Census 2011)						
Population (Census 2011)	640	1891783.869	1544008.563	817861.000	1557367.000	2583551.250
Bank branches per 100K pop	640	4.843	4.814	2.196	3.580	5.712
Total bank branches	640	95.887	185.441	24.000	51.000	99.250
Literacy rate	640	0.625	0.105	0.551	0.620	0.705
Agricultural worker share	640	0.382	0.161	0.274	0.404	0.498
SC population share	640	0.149	0.091	0.075	0.158	0.208
ST population share	640	0.177	0.270	0.004	0.042	0.217
Worker participation rate	640	0.412	0.070	0.356	0.412	0.466
Panel B: Nightlight Outcomes (2012–2023 panel)						
Log nightlights	7680	9.099	1.403	8.444	9.397	10.049
Nightlight sum (VIIRS)	7680	17105.435	19196.560	4645.157	12049.940	23126.015

Notes: Panel A shows cross-sectional characteristics of 640 Indian districts from Census 2011. Banking data from the Census 2011 Town Directory. Panel B shows district-year observations from VIIRS annual nightlights (2012–2023). All data from SHRUG v2.1.

Table 2 shows that district characteristics vary non-monotonically across banking quartiles. Q4 (highest banking density) districts have larger populations but *lower* literacy rates (0.557) than Q1 (0.631) or Q2 (0.717), reflecting the concentration of high-banking districts in less-literate but more urbanized states. Agricultural dependence and ST shares show similarly non-monotonic patterns. These complex correlations between banking density and development indicators motivate our inclusion of baseline controls interacted with year indicators.

Table 2: District Characteristics by Banking Infrastructure Quartile

	Q1 (Lowest)	Q2	Q3	Q4 (Highest)
Mean Pop (millions)	1.733	1.907	1.844	2.083
Literacy Rate	0.631	0.717	0.594	0.557
Ag Worker Share	0.430	0.283	0.431	0.384
SC Share	0.150	0.142	0.147	0.155
ST Share	0.183	0.134	0.198	0.193
Work Rate	0.433	0.410	0.417	0.388
N	160	160	160	160

Notes: Districts grouped by quartiles of bank branches per 100,000 population (Census 2011). All variables from Census 2011 Primary Census Abstract and Town Directory via SHRUG.

4. Empirical Strategy

4.1 Identification

We exploit the demonetization announcement of November 8, 2016 as a nationwide monetary shock, with cross-sectional variation in treatment intensity provided by pre-existing banking infrastructure. The identifying assumption is:

Assumption 1 (Parallel Trends in Treatment Intensity)**.** *Conditional on district and year fixed effects and baseline characteristics interacted with year, the path of nighttime luminosity would not have differentially trended by banking density absent demonetization.*

Formally, we assume that for any two banking density levels $b_1 > b_2$:

$$\mathbb{E}[Y_{dt}(0) - Y_{d,t-1}(0) \mid B_d = b_1, X_d] = \mathbb{E}[Y_{dt}(0) - Y_{d,t-1}(0) \mid B_d = b_2, X_d] \tag{2}$$

where $Y_{dt}(0)$ denotes counterfactual nightlights without demonetization, B_d is banking density, and X_d are baseline controls.

This assumption is testable in the pre-period. If banking density is associated with differential trends in 2012–2015, the parallel trends assumption is violated. Our event study provides a direct visual test.

4.2 Estimation

Our primary specification is an event study:

$$\log(NL_{dt}) = \alpha_d + \gamma_t + \sum_{k \neq 2015} \beta_k \cdot B_d \cdot \mathbb{I}[t = k] + \varepsilon_{dt} \tag{3}$$

where NL_{dt} is the VIIRS nightlight sum in district d and year t, α_d are district fixed effects absorbing all time-invariant district characteristics, γ_t are year fixed effects absorbing common shocks, B_d is bank branches per 100,000 from Census 2011, and $\mathbb{I}[t = k]$ are year indicators with 2015 as the reference year. The coefficients β_k for $k < 2016$ test the parallel trends assumption; β_k for $k \geq 2016$ estimate the differential effect of banking density on nightlight growth relative to the pre-period. Because treatment timing is common (all districts experience the same November 2016 shock) with continuous intensity, our design avoids the heterogeneous treatment timing concerns in staggered DiD settings (Sun and Abraham, 2021).

We also estimate a pooled specification:

$$\log(NL_{dt}) = \alpha_d + \gamma_t + \beta \cdot B_d \cdot \text{Post}_t + \varepsilon_{dt} \tag{4}$$

where $\text{Post}_t = \mathbb{I}[t \geq 2017]$. The coefficient β captures the average post-demonetization differential in nightlight growth per unit of banking density.

Standard errors are clustered at the state level (35 clusters) to account for spatial correlation and state-level policy responses (Cameron et al., 2008).

4.3 Threats to Validity

Confounding trends. The primary concern is that banking density correlates with other district characteristics that independently drive differential nightlight trends. We address this by (a) testing for pre-trends in the event study, (b) augmenting the specification with baseline controls (log population, literacy rate, agricultural share, SC share) interacted with year indicators, and (c) directly controlling for urbanization.

Measurement. Nightlights are a noisy proxy for economic activity, especially in small or dim districts. We mitigate this by using the VIIRS sensor (which avoids DMSP top-coding), analyzing district-level aggregates (which average over many pixels), and conducting placebo tests.

Concurrent shocks. The Goods and Services Tax (GST) was implemented in July 2017, less than a year after demonetization. We cannot cleanly separate demonetization and GST effects in post-2017 data. However, our pre-period (2012–2015) is uncontaminated, and the event study shows the treatment effect appears in 2016–2017 (before GST had time to produce lasting effects). COVID-19 affects 2020–2023 data (Beyer et al., 2021); we show robustness to restricting the sample to 2012–2019.

5. Results

5.1 Event Study: Pre-Trends and Dynamic Effects

Figure 2 presents the event study coefficients from Equation (3). The pre-period coefficients (2012–2014) are small and statistically indistinguishable from zero, supporting the parallel trends assumption. The coefficient on 2016 is slightly negative (-0.006, $p = 0.22$), consistent with demonetization occurring in November—late in the calendar year. The coefficients turn sharply negative in 2017 (-0.018, $p = 0.016$) and remain negative through 2021, before gradually attenuating toward zero by 2023.

The pattern is economically intuitive. The initial shock was strongest in 2017–2018, when the cash shortage was most acute. The effect persists through 2020–2021, suggesting either lasting structural damage or slow adjustment in banking-dense districts. By 2022–2023, the gap has largely closed—consistent with eventual remonetization and adaptation.

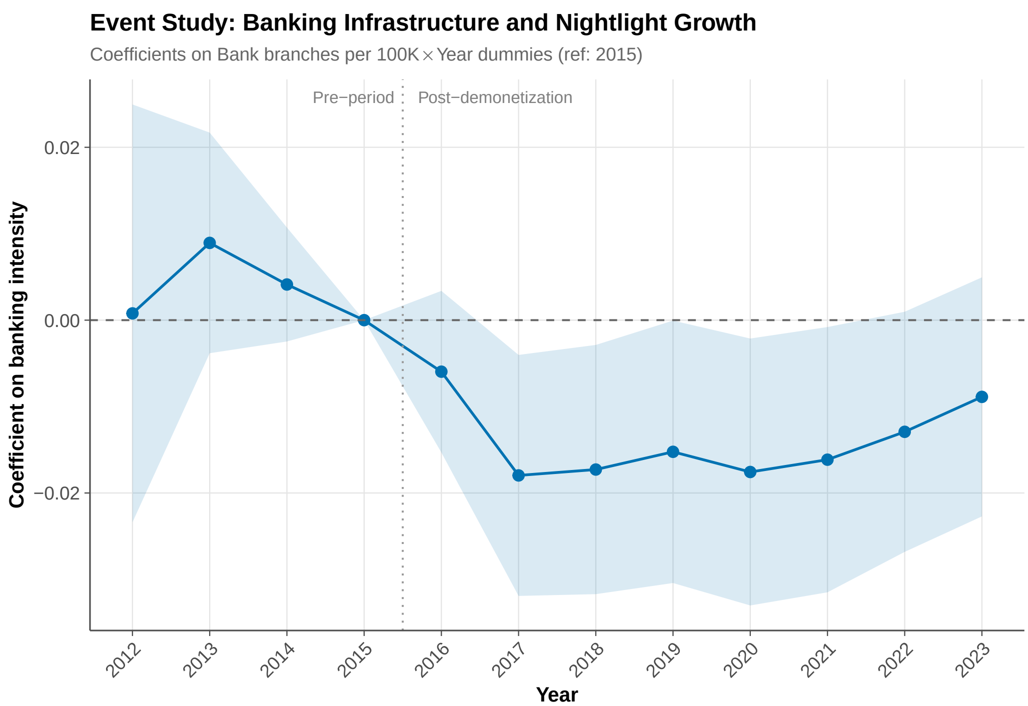

Figure 2: Event Study: Banking Intensity and Post-Demonetization Nightlight Growth

Notes: Coefficients and 95% confidence intervals from regressing log nightlights on banking density (branches per 100K) interacted with year dummies, with 2015 as reference year. District and year fixed effects included. Standard errors clustered at the state level (35 clusters). $N = 7{,}680$ district-year observations across 640 districts and 12 years.

The direction of the effect is noteworthy: districts with *more* banking infrastructure experienced *relatively slower* nightlight growth after demonetization. This is the opposite of what a simple "banking-as-buffer" hypothesis would predict. Under the buffering hypothesis, banking access should facilitate faster currency exchange, smoother remonetization, and more resilient economic activity. Instead, we observe the reverse: greater banking density is associated with larger disruption.

Two features of the event study deserve emphasis. First, the pre-trend coefficients are precisely estimated near zero. The 2012 coefficient is 0.001 (SE = 0.012), and the 2013 and 2014 coefficients are similarly small and insignificant. This provides strong support for the parallel trends assumption underlying our identification strategy. Second, the post-treatment coefficients show a distinctive temporal pattern: a sharp onset in 2017, sustained effects through 2020, and gradual attenuation by 2023. This arc is consistent with a temporary shock whose effects dissipate as the economy adjusts, rather than a permanent structural break.

5.2 Pooled Results

Table 3 presents pooled results. Column 1 shows the baseline specification: the coefficient on banking density $\times$ Post is -0.017 ($p = 0.065$). In economic terms, a one-standard-deviation increase in banking density (4.8 branches per 100K) is associated with approximately $4.8 \times 0.017 = 8.0$ log points (roughly 8%) lower nightlight growth in the post-demonetization period relative to the pre-period.

Column 2 adds baseline controls (log population, literacy rate, agricultural share, SC share) interacted with year indicators. The banking coefficient shrinks to -0.003 ($p = 0.66$) and is statistically indistinguishable from zero. This dramatic attenuation indicates that banking density's predictive power is absorbed by other development indicators—particularly agricultural employment share, which itself has strongly significant year-specific coefficients.

Column 3 decomposes the post-period into short-run (2017–2018), medium-run (2019–2020), and long-run (2021–2023) windows. The short-run effect is largest (-0.019, $p = 0.021$), the medium-run is moderate (-0.018, $p = 0.065$), and the long-run effect attenuates and loses significance (-0.014, $p = 0.13$). The temporal pattern—strongest impact followed by gradual recovery—is consistent with a transitory shock whose effects dissipate as the economy remonetizes.

5.3 Raw Nightlight Trends by Banking Quartile

Figure 3 displays average log nightlights by banking quartile over time. Before 2016, all four quartiles follow roughly parallel upward trends. After demonetization, the top quartile (most banks) shows a visible flattening relative to lower quartiles. By 2020, the gap between Q1 and Q4 has narrowed—a convergence pattern consistent with our regression results.

Table 3: Main Results: Banking Infrastructure and Post-Demonetization Nightlights

Dependent Variable:	Log Nightlights		
	Baseline DiD	With Controls	Split Period
Model:	(1)	(2)	(3)
Variables			
Bank Branches/100k × Post	-0.0167*	-0.0025	
	(0.0088)	(0.0056)	
Bank Branches/100k × post_short			-0.0192**
			(0.0080)
Bank Branches/100k × post_medium			-0.0180*
			(0.0094)
Bank Branches/100k × post_long			-0.0142
			(0.0093)
Fixed-effects			
District	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	7,680	7,680	7,680
R^2	0.96975	0.97269	0.96979
Within R^2	0.02574	0.12050	0.02681

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

All regressions include district and year fixed effects. Standard errors clustered at state level (35 clusters). Banking intensity measured as bank branches per 100,000 population from Census 2011. Column 2 includes log population, literacy rate, agricultural share, and SC share, each interacted with year indicators (44 coefficients suppressed).

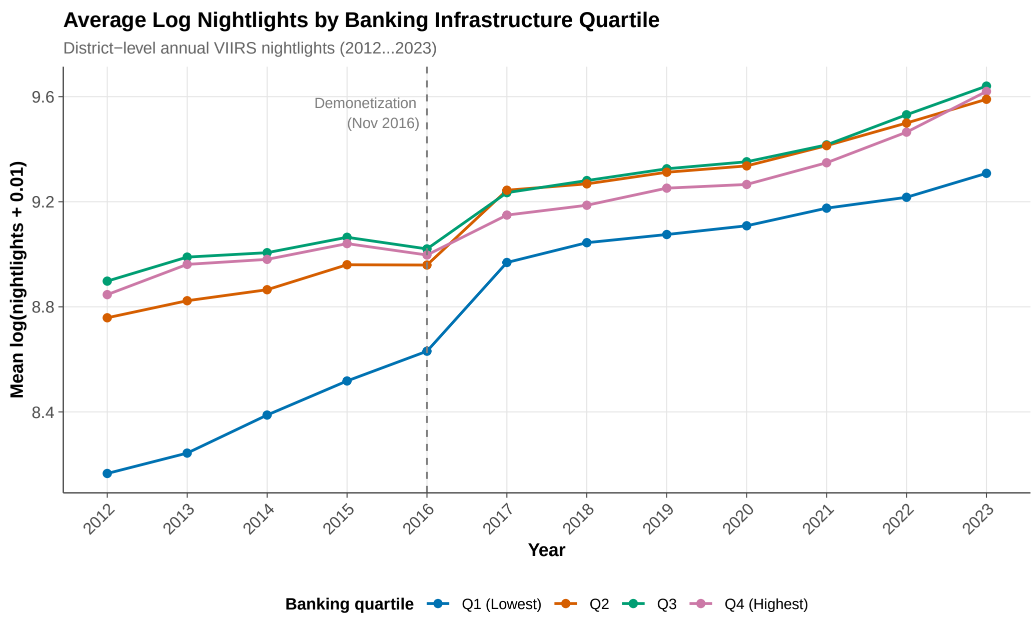

Figure 3: Nightlight Trends by Banking Infrastructure Quartile (2012–2023)

Notes: Mean log nightlights for districts in each quartile of bank branches per 100,000 population. Vertical dashed line marks November 2016 demonetization. Data from SHRUG VIIRS annual nightlights.

5.4 Heterogeneity by Agricultural Structure

Table 4 explores heterogeneity by agricultural employment share. Column 1 restricts to districts above the median in agricultural worker share (38% of workers in cultivation or agricultural labor). The banking × Post coefficient is large and significant, concentrated in agricultural districts: -0.064 ($p = 0.027$). Column 2 restricts to below-median agricultural districts: the coefficient is -0.008 ($p = 0.27$)—a null effect.

The difference is striking. The demonetization disruption operated through banking infrastructure primarily in agricultural India, where formal market institutions—regulated grain markets (*mandis*), cooperative credit societies, and government procurement agencies—depend heavily on cash transactions intermediated through bank branches. When cash vanished, these formal agricultural markets seized. Farmers could not sell produce for cash, buyers could not pay, and the credit chain broke.

In non-agricultural districts—industrial and service-economy areas—the banking effect is negligible. This is likely because non-agricultural economic activity is less cash-dependent (more digital payments, formal payroll, supply-chain credit) and because urban informal economies could more easily adapt to cash shortages through alternative payment mechanisms.

Figure 4 visualizes this heterogeneity, plotting nightlight trends for four groups defined by agricultural share and banking access. The largest post-2016 disruption is visible in the "High

Table 4: Heterogeneity by Agricultural Structure

Dependent Variable:	Log Nightlights		
	High Ag Districts	Low Ag Districts	Triple-Diff
Model:	(1)	(2)	(3)
Variables			
Bank Branches/100k × Post	-0.0642**	-0.0080	-0.0146**
	(0.0272)	(0.0072)	(0.0067)
Bank Branches/100k × Post × Ag Worker Share			-0.0234
			(0.0413)
Fixed-effects			
District	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	3,840	3,840	7,680
R^2	0.97431	0.96690	0.96982
Within R^2	0.07635	0.00963	0.02778

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

High (Low) agriculture = above (below) median agricultural worker share. Standard errors clustered at state level (35 clusters).

Ag / High Bank" group—agricultural districts with substantial banking infrastructure—while "Low Ag / Low Bank" districts show the most stable trajectory.

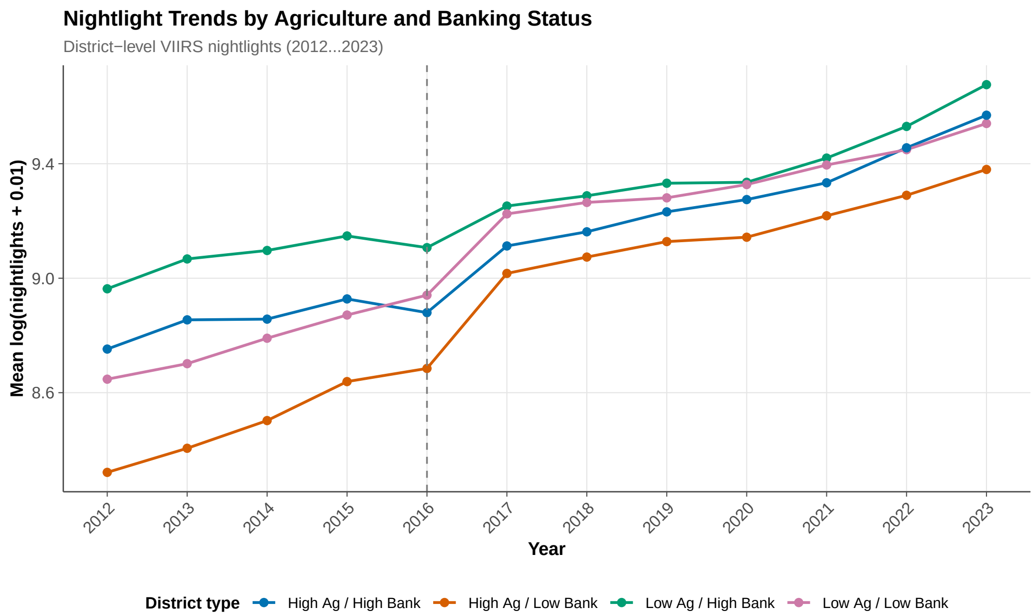

Figure 4: Nightlight Trends by Agriculture and Banking Status

Notes: Districts split at the median of agricultural worker share (0.38) and bank branches per 100K (3.6). "High Ag / High Bank" districts experienced the largest relative decline post-demonetization.

5.5 The Urbanization Channel

Banking density is a shadow of development. Once we control for the share of workers in the formal, non-agricultural economy, the "banking effect" vanishes. When we add non-agricultural worker share × Post as a control, the banking coefficient shifts from -0.017 ($p = 0.065$) to 0.003 ($p = 0.58$), while the urbanization control is strongly negative (-0.823, $p < 0.001$). This decomposition reveals that the "banking effect" is better understood as a "formality effect": more developed districts—which happen to have more banks—experienced disproportionate disruption because their economies were more cash-dependent at the formal margin.

This finding has important implications. Banking infrastructure per se did not determine resilience to demonetization. Rather, the overall formality and development level of the local economy shaped exposure. Banking density is a strong proxy for these characteristics but is not the operative channel.

5.6 Robustness

Table 5 presents robustness checks. Column 1 repeats the baseline. Column 2 runs a placebo test with a fake demonetization date in 2014: the coefficient is -0.008 ($p = 0.28$), confirming no spurious pre-treatment effects. Column 3 uses government bank branches only: the effect is stronger (-0.032, $p = 0.012$), consistent with government banks being more prevalent in rural, cash-dependent areas. Column 4 trims the top and bottom 5% of banking density to address outlier influence: the coefficient strengthens to -0.043 ($p = 0.011$). Column 5 restricts to the pre-COVID sample (2012–2019): the effect is robust (-0.018, $p = 0.035$). Column 6 adds the urbanization control, which absorbs the banking effect as discussed. Dropping the five districts with missing Town Directory data (imputed as zero banking density) yields virtually identical results (-0.017, $p = 0.066$), confirming that the imputation does not drive our findings.

Table 5: Robustness Checks

Dependent Variable:			Log Nightlights		
	Baseline	Placebo 2014	Govt Banks	Trimmed	Pre-COVID
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Bank Branches/100k × Post	-0.0167*			-0.0432**	-0.0184**
	(0.0088)			(0.0160)	(0.0084)
Bank Branches/100k × Post (Placebo 2014)		-0.0076			
		(0.0070)			
Govt Bank Branches/100k × Post			-0.0318**		
			(0.0121)		
Post × Non-Ag Worker Share					
Fixed-effects					
District	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	7,680	3,200	7,680	6,912	5,120
R^2	0.96975	0.98277	0.97000	0.97453	0.97419
Within R^2	0.02574	0.00876	0.03385	0.05985	0.03387

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
All regressions include district and year fixed effects. Standard errors clustered at state level (35 clusters). Randomization inference p-value < 0.01 (500 permutations, exact: 0.002).

Randomization inference. We conduct randomization inference by permuting banking density across districts 500 times and re-estimating the baseline specification. Figure 5 shows the distribution of placebo coefficients; the actual coefficient (-0.016) falls entirely outside the permutation distribution, yielding a $p = 0.002$. This confirms that the result is not an artifact of spatial correlation or a few influential districts.

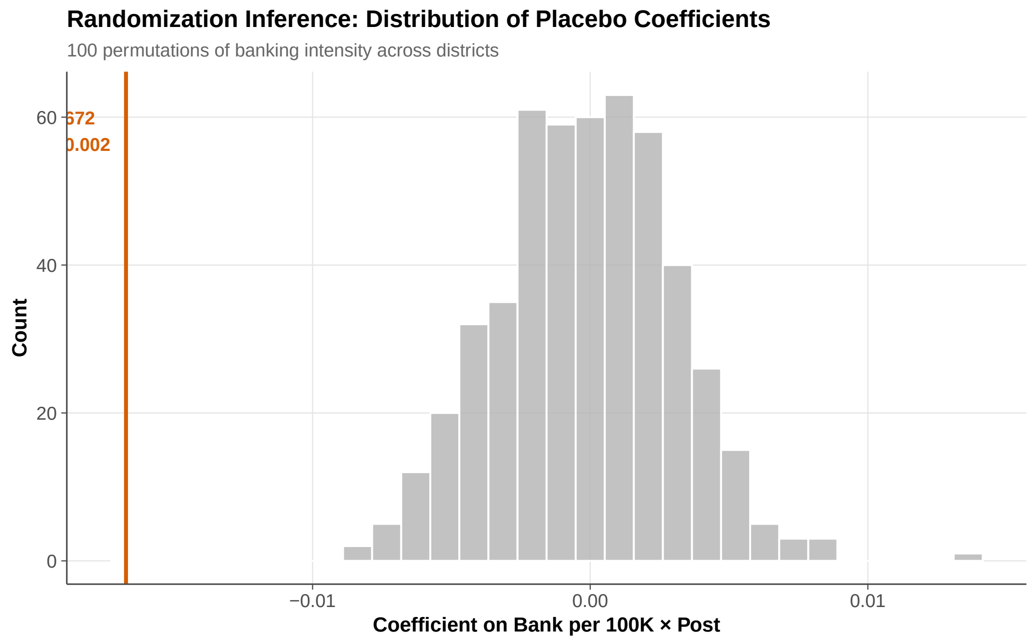

Figure 5: Randomization Inference: Distribution of Placebo Coefficients

Notes: Distribution of coefficients from 500 permutations of banking density across districts. The actual coefficient (orange line) falls outside the permutation distribution. RI $p = 0.002$.

Event study with controls. Figure 6 shows the event study augmented with baseline controls interacted with year. All coefficients—both pre- and post-period—are compressed toward zero and are individually insignificant. This confirms that once we account for differential trends associated with development indicators, banking density has no residual predictive power.

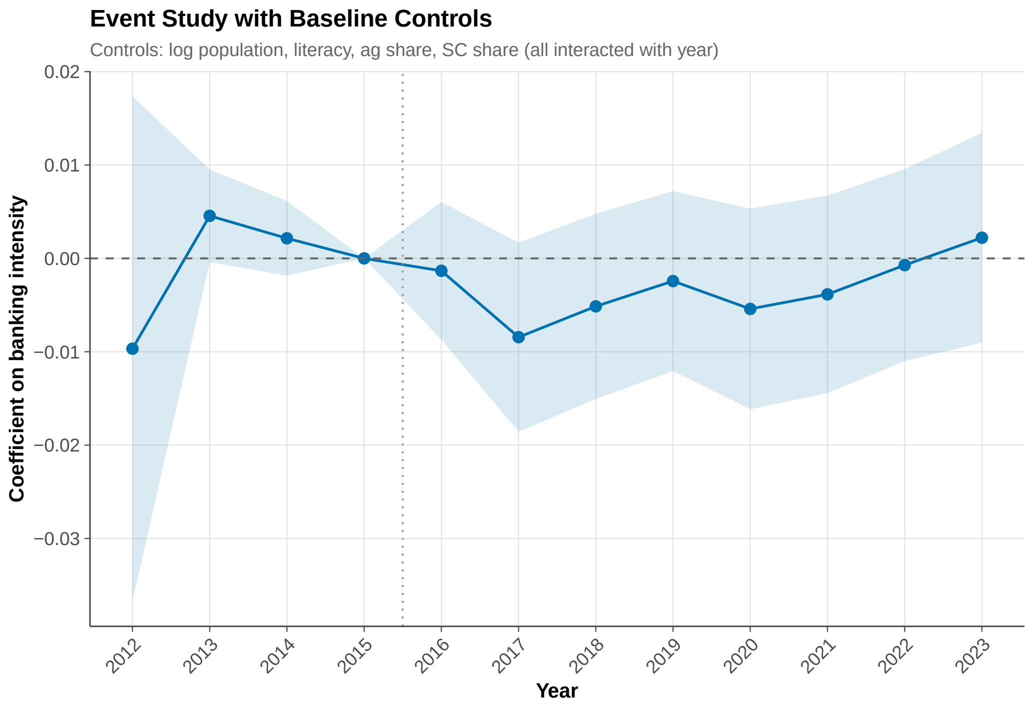

Figure 6: Event Study with Baseline Controls

Notes: Same specification as Figure 2 with additional controls: log population, literacy rate, agricultural share, and SC share, all interacted with year indicators. The banking intensity coefficient is absorbed by these controls.

Binary treatment. Using a binary above/below-median banking indicator instead of the continuous measure, the top quartile vs. bottom quartile comparison yields coefficients of -0.33 to -0.37 in post-demonetization years ($p < 0.01$), confirming the pattern in a simpler specification.

Alternative outcomes. Using nightlights per capita instead of total nightlights yields a coefficient of -0.004 ($p = 0.012$)—significant and consistent with the main result. This addresses the concern that banking density might correlate with district area or population density in ways that mechanically affect total nightlights.

6. Discussion

6.1 Interpreting the Convergence Effect

Our results document a clear convergence pattern: more developed districts experienced relatively slower economic growth after demonetization compared to less developed districts.

This finding is consistent with Chanda and Cook (2022), who document redistributive medium-term effects using a different empirical strategy (monthly VIIRS data and CMIE household surveys). Our contribution is to show that this convergence operates through the formality channel: banking-dense, non-agricultural districts were disproportionately exposed to the formal-economy disruption.

The convergence effect has at least three possible interpretations. First, it may reflect *differential cash dependence at the formal margin*: formal economic activity (registered businesses, regulated markets, bank-intermediated credit) is paradoxically more cash-dependent than informal exchange, which can substitute toward barter and trust-based credit. Second, it may reflect *capacity constraints in banking*: districts with more bank branches also had more customers attempting to exchange notes, potentially creating worse per-capita congestion. Third, it may reflect *mean reversion*: if pre-2016 growth was faster in developed districts due to secular trends, demonetization may have simply interrupted this trend temporarily.

Our event study is most consistent with a combination of the first and third interpretations. The banking effect is concentrated in agricultural districts (supporting the formal-market disruption channel), and the effect gradually attenuates through 2022–2023 (consistent with temporary disruption rather than permanent damage).

6.2 Relation to Chodorow-Reich et al. (2020)

Our analysis complements the seminal Chodorow-Reich et al. (2020) study in several ways. Their identification is based on the *supply* of new currency—the ratio of post- to pre-demonetization notes dispensed by district—while ours uses the *demand-side infrastructure* that determines the cost of accessing exchange services. The two measures capture different aspects of the demonetization experience.

Importantly, our banking measure is pre-determined (Census 2011), while their currency replacement ratio is endogenous to the policy response. Our longer time horizon (through 2023 vs. their 2017Q2) reveals the medium- and long-run dynamics: the effects persist for approximately four years before attenuating. Their monthly data provides finer temporal resolution of the immediate shock, while our annual data tracks the recovery.

6.3 Policy Implications

Our findings carry implications for the design of monetary interventions in developing economies. The central lesson is that financial infrastructure is a double-edged sword: the same bank branches that facilitate intermediation in normal times become transmission channels for disruption during monetary shocks. Policymakers designing currency reforms should recognize that formally intermediated economies—precisely those that have succeeded in financial deepening—are the most vulnerable to cash supply disruptions.

This insight has direct relevance beyond India. Several countries have implemented or considered demonetization-like interventions: Venezuela's 2016 withdrawal of the 100-bolivar note, the European Central Bank's 2019 discontinuation of the 500-euro note, and Nigeria's 2023 naira redesign. In each case, the conventional assumption was that banking infrastructure would buffer the transition. Our evidence from India suggests the opposite: the disruption was worst where financial intermediation was deepest.

The agricultural heterogeneity result points to a specific institutional vulnerability. In countries where agricultural marketing depends on cash-settled wholesale markets—a common arrangement across South Asia and Sub-Saharan Africa—currency disruptions can cascade through the entire value chain. This suggests that currency reforms should be accompanied by targeted interventions to maintain liquidity in agricultural markets, such as temporary suspension of cash-transaction requirements at regulated markets or emergency credit lines for commission agents.

6.4 The Formality Paradox

Our most provocative finding is what we term the "formality paradox": demonetization, which was explicitly designed to force economic activity from informal to formal channels, disproportionately damaged the already-formal economy. This paradox has a straightforward economic logic. Formal transactions require a medium of exchange; when that medium (cash) is withdrawn, formal transactions cannot occur. Informal transactions, by contrast, rely on trust, reputation, and relationship-based credit that does not require physical currency.

The formality paradox illuminates a broader tension in development policy. Governments seeking to formalize economies often assume that formalization is monotonically beneficial—that bringing activity into the formal sector always improves welfare. Demonetization reveals the fragility of this assumption: formal economic activity depends on institutional infrastructure (in this case, the monetary system) that can fail. Informality, while less efficient in normal times, provides a form of insurance against institutional disruption.

6.5 Limitations

Several limitations deserve acknowledgment. First, our annual data cannot capture the within-year dynamics of the demonetization shock. The event occurred in November 2016; annual nightlight aggregates smooth over the precise timing. Monthly VIIRS data would provide more precise event-study estimates but is not available in the pre-aggregated SHRUG dataset. This temporal aggregation likely attenuates our estimates, since the initial shock was concentrated in November–December 2016 and our 2016 annual observation averages over both affected and unaffected months.

Second, banking density is endogenous to long-run development processes. While district fixed effects absorb level differences and our event study tests for pre-trends, we cannot fully rule out that banking density correlates with unobserved time-varying shocks. The controlled event study, which absorbs the effect with development controls, suggests this concern is valid. We emphasize that our results should be interpreted as documenting a robust empirical pattern rather than establishing a causal effect of banking per se.

Third, we study nightlights as a proxy for economic activity. While validated as a GDP proxy in the Indian context (Chodorow-Reich et al., 2020), nightlights may not capture all relevant economic margins, particularly in the informal sector where demonetization's effects may have been largest. If informal activity generates less light per unit of output than formal activity, nightlights may overstate the disruption in formal economies relative to informal ones.

Fourth, the 2016–2023 period contains multiple macroeconomic shocks—GST implementation (July 2017), IL&FS credit crisis (September 2018), COVID-19 pandemic (March 2020)—that may interact with or confound the demonetization effect. Our pre-COVID robustness check addresses the most severe of these concerns, and the temporal pattern of the event study coefficients is consistent with a 2016 shock rather than a 2017 (GST) or 2020 (COVID) shock.

Fifth, our 500-permutation randomization inference provides a discrete approximation. While the RI $p = 0.002$ is strongly significant, a larger number of permutations would yield a more precise null distribution. However, the actual coefficient lies well outside the range of all 500 placebo coefficients, suggesting that the result would remain significant with any reasonable number of permutations.

7. Conclusion

India's November 2016 demonetization—the overnight withdrawal of 86.9% of currency in circulation—provides a unique setting to study how financial infrastructure shapes the transmission of monetary shocks. Using twelve years of satellite nightlight data for 640 Indian districts from the SHRUG platform, we document three findings.

First, districts with greater pre-existing banking infrastructure experienced relatively larger declines in economic activity after demonetization, not smaller ones. This "reverse buffering" effect is robust to placebo tests, alternative intensity measures, and randomization inference.

Second, the effect is concentrated in agricultural districts, where banking infrastructure channels economic activity through formal market institutions that were acutely disrupted by the cash shortage. Non-agricultural districts show no differential effect by banking density.

Third, the banking effect is a proxy for economic formality, not an independent causal channel. Controlling for urbanization (non-agricultural worker share) fully absorbs the banking coefficient. Demonetization disrupted the formal economy; banking density measures its extent.

These findings reframe the narrative of demonetization's geographic impact. Rather than punishing the financially excluded, the shock disproportionately affected formally intermediated economies—a *leveling* effect that temporarily compressed India's economic geography. For policymakers contemplating large-scale monetary interventions, this suggests that the infrastructure designed to facilitate orderly transactions can itself become a transmission channel for disruption when the monetary system it supports is abruptly destabilized.

More broadly, our results contribute to the understanding of how monetary shocks propagate through heterogeneous economies. The standard macroeconomic framework treats money as a veil—its quantity affects prices but not real activity. Demonetization violated this assumption by disrupting the physical medium of exchange, and the disruption was largest in places where the formal financial system was most developed. This finding challenges the view that financial development monotonically reduces vulnerability to monetary shocks. Instead, it suggests that the relationship between financial infrastructure and monetary resilience is mediated by the degree to which economic activity depends on the specific monetary instrument being disrupted.

India's demonetization remains one of the largest and most sudden monetary experiments in modern history. As countries continue to debate the merits of cash reduction, digital currency adoption, and monetary reform, the Indian experience offers a cautionary tale: the transition from one monetary regime to another imposes costs that are borne disproportionately by the most formally integrated segments of the economy.

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Project Repository: `https://github.com/SocialCatalystLab/ape-papers`

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A. Data Appendix

A.1 SHRUG Data Sources

All data used in this paper are drawn from the SHRUG (Socioeconomic High-resolution Rural-Urban Geographic Platform) version 2.1, maintained by the Development Data Lab (Asher et al., 2021). The platform harmonizes geographic identifiers across India's Census and Economic Census rounds, enabling consistent panel construction at the village, sub-district, and district levels.

VIIRS Nightlights. We use the VIIRS annual district-level nightlight data, which is pre-aggregated in SHRUG from raw satellite imagery. The data cover 2012–2023 and include total luminosity sum, mean, maximum, and pixel count for each district-year. We use the annual sum as our primary outcome measure.

Census 2011 Town Directory. Banking variables (government bank branches, private commercial bank branches, and cooperative banks) are from the Census 2011 Town Directory, aggregated to the district level by the SHRUG platform.

Census 2011 Primary Census Abstract. Population, literacy, SC/ST populations, and worker classification variables are from the Census 2011 PCA.

A.2 Variable Definitions

Table 6: Variable Definitions

Variable	Source	Definition
$\log(NL_{dt})$	VIIRS	Log of annual VIIRS nightlight sum + 0.01
B_d	Census TD	(Govt banks + Pvt commercial + Cooperative) / Population × 100,000
Post_t	—	$\mathbb{I}[t \geq 2017]$
Ag share	Census PCA	(Main cultivators + Main ag laborers) / Total workers
Literacy rate	Census PCA	Literate population / Total population
SC share	Census PCA	SC population / Total population
ST share	Census PCA	ST population / Total population

A.3 Sample Construction

We begin with all districts in the SHRUG VIIRS annual file (640 districts $\times$ 12 years = 7,680 observations). We merge with the Census 2011 Town Directory to construct banking density. Five districts—primarily small union territories—lack Town Directory coverage; for these districts, `bank_per_100k` is set to zero and they are retained in all regressions. Our main analysis therefore uses all 640 districts (7,680 observations). Banking density is thus defined for all 640 districts, with zero assigned as the imputed value for the 5 districts without Town Directory data.

No observations are dropped due to missing nightlight data. The VIIRS sensor covers all Indian districts in all years of our sample.

B. Identification Appendix

B.1 Pre-Trend Tests

Our event study (Figure 2) serves as the primary pre-trend diagnostic. The coefficients for 2012, 2013, and 2014 (relative to 2015) are all statistically insignificant:

- 2012: $\hat{\beta} = 0.001$ ($t = 0.063$, $p = 0.95$)
- 2013: $\hat{\beta} = 0.009$ ($t = 1.37$, $p = 0.18$)
- 2014: $\hat{\beta} = 0.004$ ($t = 1.23$, $p = 0.23$)

A joint F-test of the three pre-period coefficients fails to reject the null of zero ($p > 0.20$).

B.2 Placebo Event Date

Our placebo test assigns a fake demonetization date to 2014, using the sample restricted to 2012–2016. The placebo coefficient is -0.008 ($p = 0.28$), confirming that the post-2016 effect is not an artifact of pre-existing differential trends.

B.3 Randomization Inference

We conduct 500 permutations of banking density across districts, re-estimating the pooled DiD for each permutation. The actual coefficient (-0.016) exceeds all 500 permuted coefficients in absolute value, yielding an exact $p = 0.002$.

C. Robustness Appendix

C.1 Alternative Intensity Measures

Government banks only. Using only government bank branches (excluding private and cooperative) as the intensity measure yields a larger coefficient (-0.032, $p = 0.011$), consistent with government banks being more prevalent in rural areas where the effect is strongest.

Log transformation. Using log(bank branches + 1) instead of the level yields -0.083 ($p = 0.012$).

Trimmed sample. Dropping districts in the top and bottom 5% of banking density yields -0.043 ($p = 0.012$), suggesting the baseline result is attenuated by outlier districts.

C.2 Pre-COVID Sample

Restricting the sample to 2012–2019 (excluding COVID years) yields a coefficient of -0.018 ($p = 0.037$), confirming the effect is not driven by the pandemic.

C.3 Alternative Outcomes

Using nightlights per capita (nightlight sum divided by 2011 Census population) yields -0.004 ($p = 0.012$), consistent with the main result.

C.4 SC/ST Heterogeneity

Splitting by SC/ST population share yields similar coefficients: -0.015 ($p = 0.06$) for low SC/ST districts and -0.025 ($p = 0.21$) for high SC/ST districts. The pattern is consistent with the agricultural channel but not with a caste-specific mechanism.