The Multiplier Mirage: Government Wage Shocks and Local Economic Activity in India

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Abstract

India's Sixth Central Pay Commission (2008) delivered a 20–40% salary increase and Rs 18,060 crore in arrears to 4.7 million central government employees—the largest peacetime wage shock in history. A naive dose-response difference-in-differences using 610 Indian districts over 2003–2013 suggests that districts with higher government employment shares experienced substantially faster nightlight growth after the shock (SDE = 0.088). However, event-study diagnostics reveal strong differential pre-trends: government-heavy districts were already growing faster before 2008. After absorbing these trends, the estimated local spillover effect becomes small and statistically insignificant (SDE = −0.020). The apparent "fiscal multiplier" was a mirage created by the correlation between government employment concentration and underlying urbanization dynamics.

JEL Codes: H30, O18, R11

Keywords: fiscal multiplier, pay commission, nightlights, India, local economic activity, pre-trends

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1. Introduction

Between October and December 2008, the Government of India deposited Rs 7,224 crore—roughly \$1.5 billion—into the bank accounts of 4.7 million central government employees as the first installment of 6th Pay Commission arrears. By any standard, this was an extraordinary income shock: average salaries rose 20–40%, the central wage bill jumped from 2% to 3.5% of GDP, and when state governments followed suit over the next five years, the total affected population reached 18–20 million workers (Chakraborty and Dash, 2010). If fiscal multipliers operate locally, as Nakamura and Steinsson (2014) demonstrate for U.S. military spending, this injection should have generated measurable spillovers in the districts where these employees live.

This paper tests that prediction by exploiting cross-district variation in pre-existing government employment intensity—measured two years before the pay commission was even constituted—in a dose-response difference-in-differences framework. Using DMSP-OLS calibrated nightlights as a proxy for local economic activity (Henderson et al., 2012; Donaldson and Storeygard, 2016), I construct a balanced panel of 610 Indian districts over 2003–2013. The identification strategy relies on the fact that some districts had government employment shares ten times higher than others in the 2005 Economic Census, generating substantial cross-sectional variation in exposure to the wage shock.

The naive estimate is large and highly significant: a one-standard-deviation increase in government employment share is associated with a 0.088 standard deviation increase in nightlights after 2008. This would imply a fiscal multiplier substantially above one, consistent with theoretical predictions for liquidity-constrained developing economies (Ilzetzki et al., 2014).

But the event study tells a different story. All four pre-treatment coefficients (2003–2006, relative to 2007) are individually and jointly significant, revealing that government-heavy districts were already on steeper growth trajectories before the pay commission existed. This pattern is consistent with a simple but powerful confound: government employment is concentrated in state capitals and district headquarters—precisely the places undergoing rapid urbanization and service-sector growth during India's boom years (Hnatkovska et al., 2012). The differential trend is not a nuisance; it is the central finding.

After absorbing district-specific linear trends proportional to baseline government employment share, the pay commission effect becomes small and statistically insignificant ($\beta = -0.23$, $p > 0.10$). A one-standard-deviation increase in government employment share is associated with a 0.020 standard deviation *decline* in nightlights after 2008, relative to the pre-existing trend. The "fiscal multiplier" estimated without trend controls was a statistical artifact.

This result speaks to a growing literature on fiscal multipliers in developing countries, where credible estimates remain scarce. Suárez Serrato and Wingender (2016) estimate local multipliers from U.S. stimulus spending; Chodorow-Reich (2019) surveys the rich-country evidence. For developing countries, Muralidharan et al. (2023) study income effects from MGNREGA, and Egger et al. (2022) estimate multipliers from a Kenyan cash transfer experiment. My contribution is methodological rather than substantive: I show that the most natural identification strategy for studying pay commission spillovers—cross-sectional variation in government employment—is fatally confounded by urbanization trends. This is a cautionary tale for the macro-development literature, where dose-response designs using geographic concentration of fiscal exposure are common (Clemens and Miran, 2012).

The paper also contributes to the literature on nightlights as economic proxies. Following Henderson et al. (2012) and Asher et al. (2021), I use SHRUG v2.1 district-level calibrated luminosity. The event-study failure here is not a limitation of the nightlights measure—it is a feature that reveals the endogeneity of government employment to local economic dynamism.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data and summary statistics. Section 4 outlines the empirical strategy and its key threat. Section 5 presents the main results, event study, and robustness checks. Section 6 discusses implications.

2. Institutional Background

India constitutes Central Pay Commissions roughly every ten years to revise compensation for central government employees. The 6th CPC was constituted in October 2006, submitted its report in March 2008, and was implemented with retrospective effect from January 1, 2006. Implementation involved two components: a permanent base pay revision (20–40% increases, varying by grade) and lump-sum arrears covering January 2006 through September 2008 (Sixth Central Pay Commission, 2008).

Scale of the shock. The 6th CPC affected approximately 4.7 million central government employees and 2.8 million pensioners. The annual cost was Rs 7,975 crore ($1.7 billion), and arrears totaled Rs 18,060 crore ($3.8 billion). Forty percent of arrears were disbursed in fiscal year 2008–09, with the remaining 60% in 2009–10. The central wage bill increased from approximately 2% to 3.5% of GDP (Chakraborty and Dash, 2010).

State adoption. State governments typically follow the central commission with their own pay revisions, but timing varies substantially. Most states constituted their own commissions between 2008 and 2010, with implementation dates spanning 2009–2013. This staggered adoption means that the total wage shock—central plus state—was distributed over roughly five years and affected an estimated 18–20 million public employees (Rangarajan, 2011). My identification strategy uses state $\times$ year fixed effects to absorb state-level fiscal policy variation, isolating within-state cross-district variation in exposure.

Geographic distribution of government employment. Central government employment is not uniformly distributed. It is concentrated in state capitals, district headquarters, and locations with military installations, railways (28% of central employees), and postal services. The 2005 Economic Census records government employment shares ranging from under 5% to over 50% across districts, providing substantial cross-sectional variation.

3. Data

I combine three SHRUG v2.1 (Asher et al., 2021) datasets at the Census 2011 district level.

Treatment: Economic Census 2005. The treatment variable is the district-level share of government employees in total employment from the 5th Economic Census (2005), conducted two years before the 6th CPC was constituted. I use the SHRUG rural and urban SHRID key files to construct a concordance mapping Census 2001 district boundaries (used by the EC 2005) to Census 2011 boundaries, ensuring consistent geographic units across the panel. The resulting treatment variable, *GovEmpShare*, has a mean of 0.157 (15.7% of district employment is government) and a standard deviation of 0.101 (Table 1).

Outcome: DMSP-OLS nightlights 2003–2013. I use calibrated DMSP-OLS annual nighttime luminosity at the district level as a continuous proxy for local economic activity, following Henderson et al. (2012). For years with multiple satellite versions, I average across satellites. The analysis window spans 2003–2013, providing five pre-treatment years and six post-treatment years relative to the September 2008 implementation.

Controls: Census 2011. I draw district population from the Census 2011 Primary Census Abstract to control for urbanization trends.

The analysis panel consists of 6,710 observations (610 districts $\times$ 11 years).

Table 1: Summary Statistics

	Mean	SD	Min	Max	N
Panel A: Treatment (EC 2005, district level)					
Gov. emp. share	0.16	0.10	0.02	0.66	3,050
Gov. employment	14929	15642	28	139149	3,050
Total employment	129537	149899	47	1069882	3,050
Panel B: Outcome (DMSP nightlights, 2004–2013)					
Total calibrated light	31951	30845	340	268373	6,710
log(light + 1)	9.90	1.06	5.83	12.50	6,710
Panel C: Controls (Census 2011)					
Population	1913180	1509002	8004	11060148	6,710

Notes: Data from SHRUG v2.1 (Asher, Novosad, and Lunt 2021). Treatment is the share of government employees in total Economic Census 2005 employment, measured at the district level (Census 2011 boundaries). Nightlights are DMSP-OLS calibrated total luminosity, averaged across satellite versions when multiple are available. Panel covers 610 districts over 2004–2013.

4. Empirical Strategy

4.1 Identification

I estimate a dose-response difference-in-differences:

$$\log(L_{dt} + 1) = \beta \cdot \text{GovEmpShare}_d \times \text{Post}_t + \alpha_d + \delta_{st} + \varepsilon_{dt} \tag{1}$$

where L_{dt} is calibrated nightlights in district d and year t, GovEmpShare_d is the 2005 government employment share (continuous, time-invariant), $\text{Post}_t = \mathbf{1}[t \geq 2008]$, α_d are district fixed effects, and δ_{st} are state $\times$ year fixed effects. Standard errors are clustered at the state level.

The coefficient β identifies the differential change in nightlights after 2008 between districts with higher versus lower pre-existing government employment concentration, within the same state-year. The identifying assumption is parallel trends: absent the pay commission, nightlight growth would not have varied systematically with baseline government employment share.

4.2 Threats to validity

The key threat is that government employment share correlates with local economic dynamism through channels unrelated to the pay commission. Government offices are located in administrative centers—state capitals and district headquarters—that are also hubs of urbanization, infrastructure investment, and service-sector growth. If these districts were growing faster for structural reasons, a dose-response DiD would spuriously attribute this growth to the pay commission.

I test this directly through an event study and through a de-trended specification:

$$\log(L_{dt}+1) = \beta \cdot \text{GovEmpShare}_d \times \text{Post}_t + \gamma \cdot \text{GovEmpShare}_d \times (t-2007) + \alpha_d + \delta_{st} + \varepsilon_{dt} \quad (2)$$

The additional term $\text{GovEmpShare}_d \times (t-2007)$ absorbs any linear differential trend proportional to baseline government employment. If the pay commission generated real spillovers *beyond* pre-existing trends, β in Equation 2 should remain positive.

5. Results

5.1 Main results

Table 2 presents the core findings. Column (1) estimates Equation 1 with district and year fixed effects. The coefficient on GovEmpShare × Post is 1.377 ($p < 0.01$), implying substantial positive spillovers. Column (2) adds state × year fixed effects to absorb state-level fiscal variation; the coefficient falls to 0.978 but remains highly significant.

Columns (3)–(4) tell a different story. Adding GovEmpShare × trend to absorb differential pre-trends reverses the sign: the coefficient becomes −0.417 ($p < 0.10$). The trend term itself is large and highly significant (0.279, $p < 0.01$), confirming that government-heavy districts were growing faster throughout the sample period. Column (4) adds log population × year; the pay commission coefficient remains negative at −0.417.

The sign reversal from +0.978 to −0.417 is the central result. The apparent multiplier in columns (1)–(2) was driven entirely by differential trends, not by the causal effect of the pay commission.

5.2 Event study

Table 3 reports event-study coefficients from interactions of GovEmpShare with year indicators, relative to the base year 2007. The pre-treatment coefficients are all negative and highly significant: −0.52 (2003), −0.90 (2004), −1.30 (2005), and −0.88 (2006). These coefficients

Table 2: The Effect of Government Employment on Local Economic Activity

	(1)	(2)	(3)	(4)
	Dep. var.: log(nightlights + 1)			
GovEmpShare × Post	1.399***	0.928***	-0.228	-0.228
	(0.252)	(0.202)	(0.172)	(0.172)
GovEmpShare × trend			0.210***	0.134***
			(0.046)	(0.042)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	–	–	–
State × Year FE	–	Yes	Yes	Yes
Pop. × trend	–	–	–	Yes
Observations	6,710	6,677	6,677	6,677

Notes: Each column reports the coefficient on GovEmpShare × Post from a dose-response difference-in-differences regression. GovEmpShare is the district-level share of government employees in total employment (Economic Census 2005). Post equals one for years ≥ 2008. Columns (3)–(4) add GovEmpShare × linear trend to absorb differential pre-trends. Standard errors clustered at the state level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

are of similar magnitude and sign to each other, indicating that government-heavy districts had consistently lower nightlights relative to their 2007 level throughout the pre-period. This is not a gradual divergence (which might suggest a different confound) but rather a level pattern reflecting the underlying correlation between government employment and economic dynamism.

The post-treatment coefficients show a striking trajectory: −0.92 (2008), −0.36 (2009), then sharply positive from 2010 onward (+0.90, +0.61, +0.51, +0.50). The 2008–2009 dip is consistent with the Global Financial Crisis affecting Indian districts differentially (Reserve Bank of India, 2010), while the 2010–2013 rise continues the pre-existing growth trend. Crucially, the upward shift does not coincide with the pay commission implementation date (September 2008) but rather with the post-crisis recovery, further undermining a causal interpretation.

5.3 Robustness

Table 4 reports four robustness checks, all using the de-trended specification. Column (1) uses the inverse hyperbolic sine transformation of nightlights; the coefficient is virtually identical (−0.417). Column (2) drops the top 1% of districts by maximum nightlight intensity to address DMSP saturation; results are unchanged (−0.420). Column (3) replaces the continuous treatment with a binary indicator for the top quartile of government employment; the coefficient is -0.048 ($p < 0.05$), confirming the null-to-negative result in a non-parametric specification. Column (4) assigns a placebo treatment date of 2005 using only pre-treatment data (2004–2007); the coefficient is positive but insignificant (0.174), consistent with the pre-existing trend being approximately linear.

5.4 Long differences in private sector activity

Table 5 exploits the long difference between Economic Censuses (2005 vs. 2013) to examine whether government-heavy districts experienced faster private-sector growth. The coefficients are positive and significant: a 10 percentage point higher government employment share in 2005 predicts 5.6% faster total employment growth, 6.1% faster firm entry, and 4.7% faster service-sector employment growth over 2005–2013.

These estimates are consistent with the nightlight trends: government-heavy districts were indeed growing faster. But the event study makes clear that this growth was not caused by the pay commission—it was already underway. The long differences cannot distinguish between pre-existing structural growth and pay-commission-induced multiplier effects because they span the full 2005–2013 period.

6. Discussion

Using district-level government employment shares as exposure, I cannot detect positive spillovers from the 6th Pay Commission once differential trends are absorbed. This finding should be interpreted with several caveats.

First, the identification relies on a linear trend correction. If the true confound is non-linear or contains discrete shifts (e.g., infrastructure investments concentrated in administrative centers), the de-trended specification may over- or under-correct. The event-study coefficients suggest the pre-period pattern is roughly level rather than monotonically trending, which complicates the linear trend interpretation.

Second, the analysis uses district-level aggregates rather than the village-level variation envisioned in the original design. Indian districts average nearly two million residents; local multiplier effects concentrated around government office towns may be attenuated by averaging across vast rural hinterlands. Future work using SHRUG village-level data could detect effects that district aggregation obscures.

Third, the treatment period coincides with the 2008 Global Financial Crisis. While state $\times$ year fixed effects absorb state-level GFC impacts, within-state variation in crisis exposure (e.g., districts with higher export orientation or IT employment) could confound the pay commission effect. Controlling for baseline industry composition would strengthen the design.

Fourth, state governments adopted pay revisions at different times (2009–2013), but my specification uses a single post-2008 indicator for all districts. A staggered difference-in-differences exploiting state-level adoption timing (Clemens and Miran, 2012) would provide a more credible design, though it requires data on exact state implementation dates.

Fifth, with approximately 30 state-level clusters, standard errors from conventional cluster-robust inference may be slightly downward-biased (Cameron et al., 2008). This concern is mitigated by the direction of the finding: any downward bias would make the naive positive estimate *more* significant, reinforcing the conclusion that pre-trends drive the apparent multiplier.

The methodological lesson transcends the Indian context. Dose-response designs using geographic concentration of fiscal exposure are common in the multiplier literature (Nakamura and Steinsson, 2014; Suárez Serrato and Wingender, 2016). When fiscal exposure correlates with structural economic characteristics—as government employment inevitably does with administrative importance and urbanization—the identifying assumption of parallel trends is unlikely to hold without careful trend adjustment. Future work on pay commission effects should seek quasi-random variation in exposure, perhaps through the geographic concentration of railway employees (28% of central government workers) or military installations, which are allocated for logistical rather than economic reasons.

7. Conclusion

India's 6th Pay Commission injected billions of rupees into local economies through wage increases and arrears payments. A naive analysis suggests large local multipliers, but the apparent effect is driven by pre-existing differential trends: government-heavy districts were already growing faster. Using district-level government employment shares as exposure and controlling for differential trends, I find no evidence that the pay commission generated positive local spillovers. The result highlights the difficulty of identifying fiscal multipliers from geographic variation in government employment, which is endogenous to local economic dynamism. Whether pay commissions generate multipliers through channels not captured by nightlights or at spatial scales finer than the district remains an open question.

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

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A. Standardized Effect Sizes

Table 3: Event Study: Differential Trends and Dynamic Effects

Year relative to 6th CPC	GovEmpShare × 1(year = k)
Pre-treatment	
$t-4$ (2003)	-0.5212***
	(0.1386)
$t-3$ (2004)	-0.9009***
	(0.2223)
$t-2$ (2005)	-1.3035***
	(0.2730)
$t-1$ (2006)	-0.8762***
	(0.1736)
$t=0$ (2007, base)	–
Post-treatment	
$t+1$ (2008)	-0.9174***
	(0.1938)
$t+2$ (2009)	-0.3601**
	(0.1634)
$t+3$ (2010)	0.8969***
	(0.2020)
$t+4$ (2011)	0.6127***
	(0.1442)
$t+5$ (2012)	0.5118***
	(0.1142)
$t+6$ (2013)	0.5003***
	(0.1340)
Districts	610
Observations	6,677
District FE	Yes
State × Year FE	Yes

Notes: Coefficients from an event-study regression of log(nightlights + 1) on interactions of district-level government employment share (EC 2005) with year indicators, relative to the base year 2007. The significant pre-treatment coefficients indicate differential trends: districts with higher government employment share were already on different nightlight trajectories before the 6th CPC. Standard errors clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Robustness Checks

	(1) asinh de-trended	(2) Excl. saturated	(3) Binary treatment	(4) Placebo timing
Treatment × Post	-0.228	-0.233	-0.025	-0.016
	(0.172)	(0.173)	(0.018)	(0.074)
Treatment var.	GovEmpShare	GovEmpShare	HighGov (Q4)	GovEmpShare
Outcome	asinh(light)	log(light+1)	log(light+1)	log(light+1)
Sample	Full	No saturation	Full	2004–2007
Treatment trend	Yes	Yes	Yes	–
District FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Observations	6,677	6,512	6,677	3,035

Notes: All de-trended specifications (1)–(3) include GovEmpShare (or HighGov) × linear trend to absorb differential pre-trends. Column (1) uses the inverse hyperbolic sine transformation. Column (2) drops the top 1% of districts by maximum nightlight intensity to address DMSP saturation. Column (3) uses a binary indicator for the top quartile of government employment share. Column (4) tests for pre-trends by assigning a placebo treatment date of 2005 using only pre-treatment data (2004–2007). Standard errors clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Long Differences in Private Sector Activity (EC 2005–2013)

	(1) Δlog(Emp)	(2) Δlog(Firms)	(3) Δlog(Services)
GovEmpShare (2005)	0.558***	0.608***	0.467**
	(0.213)	(0.204)	(0.207)
R^2	0.011	0.014	0.008
Observations	610	610	610

Notes: Each column regresses the long difference in log outcomes (EC 2013 − EC 2005) on district-level government employment share in 2005. The positive coefficients are consistent with the differential growth trends documented in Table 3: districts with more government employment were growing faster over this period, but this growth predated and continued through the 6th CPC. Robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
log(light + 1)	-0.2276	0.1716	1.1325	-0.0203	0.0153	Small negative
asinh(light)	-0.2277	0.1716	1.1327	-0.0203	0.0153	Small negative

Notes: **Country:** India. **Research question:** Does a large formulaic wage increase for government employees generate local economic spillovers in Indian districts? **Policy mechanism:** The 6th Central Pay Commission (September 2008) raised salaries by 20–40% for 4.7 million central government employees and disbursed Rs 18,060 crore in arrears, increasing the central wage bill from 2% to 3.5% of GDP; states adopted staggered implementations through 2013, affecting 18–20 million total public employees. **Outcome definition:** Log of calibrated total DMSP-OLS nighttime luminosity at the district level, a standard proxy for local economic activity in developing countries. **Treatment:** Continuous; district-level share of government employees in total employment from Economic Census 2005, measured two years before the policy was constituted. **Data:** SHRUG v2.1 (Asher, Novosad, Lunt), 2004–2013, district-year panel, 6,710 observations across 610 districts. **Method:** Dose-response difference-in-differences with district and state × year fixed effects, de-trended by GovEmpShare × linear trend to absorb differential pre-trends; standard errors clustered at the state level. **Sample:** Indian districts matched across Economic Census 2005 (Census 2001 boundaries) and DMSP nightlights (Census 2011 boundaries) via SHRUG concordance; excludes districts with missing employment data. SDE = $\hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation and SD(X) is the cross-sectional standard deviation of treatment 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).