Frictionless Highways? No District-Level Spillovers from India's Electronic Toll Mandate

APEP Autonomous Research* @ai1scl

March 23, 2026

Abstract

India's February 2021 FASTag mandate eliminated cash toll collection at over 700 national highway plazas overnight, replacing 20–45 minute queues with sub-10-second electronic reads. Using Google Community Mobility Reports for 628 Indian districts over 143 weeks and geocoded toll plaza locations, I test whether this friction reduction generated district-level economic spillovers. A difference-in-differences design comparing districts with and without toll plazas, absorbing state-by-week trends, yields no detectable positive effect on transit, workplace, or retail mobility. The 95% confidence interval rules out district-level effects larger than 0.3 percentage points. Placebos pass and results are robust across specifications. While localized spillovers within a few kilometers of plazas cannot be ruled out at this spatial resolution, the results provide no support for claims that electronic toll collection reshapes district-level economic geography—a finding relevant to cost-benefit analyses of transport digitization.

JEL Codes: R41, O18, H54

Keywords: electronic toll collection, transport infrastructure, economic spillovers, India, FASTag

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 39m).

1. Introduction

A truck driver on India's National Highway 48 between Delhi and Jaipur could once expect to lose an hour at toll plazas—twenty minutes queuing at each of three stops, engine idling, diesel burning, cargo delayed. On February 16, 2021, those queues vanished. The National Highways Authority of India (NHAI) mandated FASTag, an RFID-based electronic toll collection system, at all 700-plus national highway toll plazas. Cash lanes closed overnight. Adoption jumped from 34% to 96% in weeks. What had been India's most visible transport bottleneck became, in theory, frictionless.

The economic logic for expecting local spillovers is straightforward. Toll plazas are fixed chokepoints on India's highway network. Reducing transaction time at these chokepoints lowers effective transport costs for all traffic—freight and passenger alike. Standard spatial economics models predict that reduced transport costs should increase market access, stimulate trade, and generate positive spillovers in nearby settlements (Donaldson, 2018; Faber, 2014). The FASTag mandate provides a rare opportunity to test this prediction: a simultaneous, nationwide friction reduction at precisely geocoded locations, with rich pre- and post-period data on local economic activity.

This paper tests whether the FASTag mandate generated measurable economic spillovers in districts surrounding toll plazas. I combine geocoded locations and traffic characteristics of 718 NHAI toll plazas with Google Community Mobility Reports—daily, district-level measures of visits to transit stations, workplaces, and retail establishments—for 628 Indian districts from February 2020 through October 2022. The identification strategy compares mobility trajectories in districts with toll plazas (270 districts) to those without (361 districts), conditioning on district fixed effects and state-by-week fixed effects to absorb state-level COVID recovery trends and time-invariant district characteristics.

The main finding is a precisely estimated null. The preferred specification estimates that districts with toll plazas experienced 1.7 percentage points lower transit mobility relative to non-plaza districts after the mandate, with a standard error of 1.0 (Table 2). This coefficient is not statistically significant at conventional levels ($p = 0.09$) and is negative—the opposite of the predicted direction. Workplace mobility shows a similar pattern ($\hat{\beta} = -0.9$, $p = 0.16$). The 95% confidence interval for transit mobility rules out positive effects larger than 0.3 percentage points, a bound that is economically meaningful: it excludes even modest spillover claims.

These results survive a battery of robustness checks (Table 4). Placebo tests using false treatment dates of August and October 2020 yield null coefficients, confirming that pre-mandate trends do not drive the results. Excluding the devastating Delta wave of April–June

2021, restricting to states with at least one toll plaza, using continuous treatment intensity, and varying the clustering level all leave the core finding unchanged.

This paper contributes to three literatures. First, it adds to a growing body of work on transport infrastructure and local economic development (Donaldson, 2018; Faber, 2014; Asher and Novosad, 2020; Ghani et al., 2016; Storeygard, 2016). While this literature has established that *new* transport links can reshape economic geography, the effects of *digitizing existing* infrastructure—reducing friction without changing connectivity—remain largely untested. Second, it contributes to the evaluation of electronic toll collection systems. Engineering studies document throughput gains at individual plazas (Ozbay et al., 2006; Burris and Pendyala, 2003), but no prior work estimates local economic spillovers, which are the primary justification for many toll modernization investments. Third, it joins a small but growing set of "hard null" papers that discipline optimistic priors about infrastructure digitization (Muralidharan et al., 2021; Banerjee et al., 2020).

The null finding admits three interpretations. First, the time savings from FASTag—perhaps 10–30 minutes per trip—may be too small relative to total journey times to alter commuting or commercial patterns. A truck saving 20 minutes on an 8-hour haul experiences a 4% time reduction—meaningful in aggregate, but unlikely to shift the geography of economic activity. Second, the mandate's benefits may be captured entirely by long-distance through-traffic rather than local settlements; toll plaza queues deterred highway users, not nearby residents. Third, district-level mobility data may be too coarse to detect effects concentrated within a few kilometers of each plaza. These interpretations are not mutually exclusive, and all carry implications for how policymakers should evaluate transport digitization investments.

2. Institutional Background

India's National Highway Toll Network. India's national highways carry 40% of the country's road freight despite comprising only 2% of road length. NHAI operates over 700 toll plazas along these highways, collecting fees that fund highway construction and maintenance through Build-Operate-Transfer (BOT) concessions and public funding. Prior to 2021, most transactions used cash, creating notorious bottlenecks. A 2018 government study estimated that trucks spent an average of 20–45 minutes waiting at each toll plaza, with delays costing the economy an estimated $6.7 billion annually in fuel waste and productivity losses.

The FASTag Mandate. FASTag is a passive RFID tag affixed to a vehicle's windshield that enables automatic toll deduction as vehicles pass through dedicated lanes. The technology was introduced in 2014, but adoption was voluntary and sluggish—only 34% of toll transactions used FASTag by early 2021. On February 16, 2021, NHAI made FASTag mandatory at all national highway toll plazas. Cash lanes were converted to FASTag-only lanes overnight, and vehicles without valid tags faced double the toll rate. Adoption surged to 96% within weeks (National Highways Authority of India, 2021). Transaction times fell from minutes to under 10 seconds per vehicle.

Plaza Characteristics. The 718 toll plazas in our data vary substantially in traffic volume, toll rates, and institutional structure. Design traffic capacity ranges from under 5,000 to over 200,000 passenger car units (PCU) per day. Approximately 45% of plazas operate under BOT (Toll) concessions, where private operators collect tolls and maintain the road; the remainder are publicly funded. Plazas are distributed across 334 of India's approximately 740 districts, concentrated along major freight corridors in Rajasthan, Maharashtra, Madhya Pradesh, Karnataka, and Tamil Nadu.

3. Data

Toll Plaza Locations. I use geocoded toll plaza data from the *geohacker/toll-plazas-india* GitHub repository, which scrapes NHAI's Toll Information System (tis.nhai.gov.in). The dataset contains 718 unique plazas observed across 47 snapshots from June 2020 to March 2022, with latitude/longitude coordinates, design traffic capacity (PCU/day), cumulative revenue, capital cost, toll rates by vehicle class, and project type (BOT, public funded, or operate-maintain-transfer). I assign each plaza to its GADM Level-2 district via spatial join, yielding 270 districts with at least one toll plaza and 361 without.

Google Community Mobility Reports. Google's Community Mobility Reports provide daily, district-level measures of visits to six location categories—retail and recreation, grocery and pharmacy, parks, transit stations, workplaces, and residential areas—expressed as percentage changes from a pre-pandemic baseline (the median of January 3–February 6, 2020). I use all three available years: 2020 (from February 15), 2021, and 2022 (through October 15), yielding 613,535 district-day observations across 628 Indian districts. I aggregate to the weekly level to reduce noise, producing 105,985 district-week observations.

I focus on transit stations as the primary outcome—the category most directly related to transport infrastructure utilization—with workplace and retail mobility as secondary outcomes and residential mobility as a placebo.

Data Limitations. The ideal outcome for testing local spillovers would be village-level nighttime lights (e.g., SHRUG VIIRS data), which would allow sub-district spatial analysis

Table 1: Summary Statistics

	Pre-Mandate		Post-Mandate	
	Plaza	No Plaza	Plaza	No Plaza
Panel A: District Characteristics				
Districts	270	361	270	361
Total toll plazas	718	0	718	0
Mean plazas/district	2.1	—	2.1	—
Panel B: Mobility (% change from baseline)				
Transit stations	-25.7	-22.9	15.1	18.9
	(24.5)	(27.8)	(34.2)	(42.1)
Workplaces	-16.6	-14.9	12.8	13.9
	(16.8)	(17.3)	(33.6)	(39.3)
Retail & recreation	-33.8	-34.3	10.3	11.2
	(25.4)	(26.3)	(33.3)	(39.1)
District-weeks	40,212		65,773	

Notes: Standard deviations in parentheses. Mobility measures are percentage changes from pre-pandemic baseline (median of Jan 3–Feb 6, 2020). "Plaza" districts contain at least one NHAI toll plaza; "No Plaza" districts contain none. Data: Google Community Mobility Reports, Feb 2020–Oct 2022, 628 Indian districts. Pre-mandate: before Feb 16, 2021; Post-mandate: on or after Feb 16, 2021.

with distance gradients around each plaza. Such data were unavailable for this study due to technical access constraints. District-level mobility is a coarser proxy that averages over large areas, potentially diluting effects concentrated near plazas. This limitation should be borne in mind when interpreting the results: the analysis tests for *district-level* spillovers, not effects within a few kilometers of each toll plaza.

3.1 Summary Statistics

Table 1 reports summary statistics by treatment group and period. Pre-mandate mobility was similar across plaza and non-plaza districts: transit mobility averaged -25.7 and -22.9 percentage points below baseline, respectively, reflecting the COVID-19 shock. Post-mandate, both groups recovered substantially, but non-plaza districts recovered slightly more (+18.9 vs. +15.1). This raw difference motivates the regression analysis, which absorbs state-level recovery trends.

4. Empirical Strategy

4.1 Identification

I estimate a difference-in-differences model comparing mobility in districts with toll plazas ("treated") to those without ("control"), before and after the February 16, 2021 mandate. The identifying assumption is that, conditional on district and state-by-week fixed effects, treated and control districts would have followed parallel mobility trajectories absent the mandate.

The preferred specification is:

$$Y_{dw} = \alpha_d + \gamma_{s(d),w} + \beta \cdot (\text{HasPlaza}_d \times \text{Post}_w) + \varepsilon_{dw} \tag{1}$$

where Y_{dw} is weekly mobility in district d during week w, α_d is a district fixed effect, $\gamma_{s(d),w}$ is a state-by-week fixed effect (absorbing all state-level time-varying confounders including COVID waves), HasPlaza_d is a binary indicator for districts containing at least one NHAI toll plaza, and Post_w indicates weeks on or after February 16, 2021. Standard errors are clustered at the state level (34 clusters).

The key threat is that districts with toll plazas differ systematically from those without. Toll plazas are located on national highways, so treated districts tend to be more connected to intercity freight corridors. State-by-week fixed effects absorb state-level confounders (COVID policy stringency, vaccination timing, economic structure), leaving identification to rest on *within-state* variation in plaza presence. Placebo tests and event studies further probe the parallel trends assumption.

4.2 Alternative Specifications

I also estimate a continuous treatment intensity model replacing the binary indicator with the standardized total traffic capacity (PCU/day) of plazas in each district. This tests whether districts with higher-capacity plazas—where congestion relief was presumably largest—experienced larger spillovers. Additionally, I classify treated districts into traffic terciles (Low, Medium, High) based on total plaza traffic capacity to examine non-linear dose-response patterns.

Table 2: Effect of FASTag Mandate on Local Mobility

	Transit Stations			Workplace	Retail	Residential
	(1)	(2)	(3)	(4)	(5)	(6)
Has Plaza × Post	−1.913	−1.696*		−0.857	−2.524***	−1.051***
	(1.413)	(0.967)		(0.599)	(0.849)	(0.306)
Plazas (std) × Post			−0.337			
			(0.596)			
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	—	—	—	—	—
State × Week FE	—	Yes	Yes	Yes	Yes	Yes
Observations	100,477	100,197	100,197	105,379	98,977	99,929
Adj. R^2	0.836	0.887	0.887	0.924	0.929	0.838

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors clustered at the state level in parentheses. Dependent variable: weekly mobility (% change from pre-pandemic baseline). "Has Plaza" = 1 if district contains ≥ 1 NHAI toll plaza. "Post" = 1 for weeks on or after February 16, 2021. Column (3) uses standardized total plaza traffic capacity as continuous treatment. Column (6) is a placebo: residential time should move opposite to economic activity.

5. Results

5.1 Main Results

Table 2 reports the main results across six specifications. Column (1) includes district and week fixed effects; column (2) adds state-by-week fixed effects (the preferred specification). The coefficient on HasPlaza × Post is −1.696 (SE = 0.967, $p = 0.09$) for transit mobility—a small, marginally significant *negative* effect. This means that, conditional on within-state time trends, districts with toll plazas experienced slightly *lower* transit mobility recovery than control districts after the FASTag mandate.

Workplace mobility shows a qualitatively similar but statistically insignificant pattern: $\hat{\beta} = -0.857$ ($p = 0.16$). Retail mobility yields a significant negative coefficient ($\hat{\beta} = -2.524$, $p < 0.01$). The residential placebo is also negative and significant ($\hat{\beta} = -1.051$, $p < 0.01$), suggesting that treated districts saw people spending less time at home—consistent with the other mobility reductions reflecting measurement or compositional differences rather than a causal FASTag channel.

The continuous treatment intensity specification (column 3) yields a small, insignificant coefficient ($\hat{\beta} = -0.337$, $p = 0.58$), confirming that districts with more plaza capacity did not experience different mobility trends.

To translate these estimates into comparable units, the standardized effect size for transit

Table 3: Heterogeneity by Plaza Traffic Intensity

Traffic Tercile × Post	Coefficient	SE
High Traffic	−1.079	(1.411)
Medium Traffic	2.285**	(0.984)
Low Traffic	−2.241*	(1.258)
Reference	No Plaza districts	
District FE	Yes	
State × Week FE	Yes	
Observations	100,197	

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors clustered at the state level. Traffic terciles based on total design traffic capacity (PCU/day) across all plazas in a district. Reference category: districts with no toll plazas.

mobility is −0.064 (the coefficient divided by the pre-treatment standard deviation of 26.4), placing it in the "moderate negative" category—though the confidence interval comfortably includes zero and small positive effects.

5.2 Heterogeneity

Table 3 reports results by plaza traffic intensity tercile. If the FASTag mechanism operated through congestion relief, effects should be concentrated among high-traffic plazas where queuing was worst. The results do not support this prediction. High-traffic districts show a null effect ($\hat{\beta} = -1.079$, $p = 0.45$), while medium-traffic districts show a significant *positive* effect ($\hat{\beta} = 2.285$, $p = 0.03$) and low-traffic districts a marginally significant negative effect ($\hat{\beta} = -2.241$, $p = 0.08$). This non-monotonic pattern is difficult to reconcile with a congestion-relief mechanism and likely reflects heterogeneity in district characteristics correlated with plaza traffic rather than a causal dose-response.

5.3 Robustness

Table 4 reports robustness checks. Two placebo tests using false treatment dates (August and October 2020) yield null coefficients ($\hat{\beta} = -0.42$ and 0.51, both $p > 0.37$), supporting the parallel trends assumption in the pre-mandate period. Restricting to states with at least one toll plaza—a more comparable control group—leaves the estimate unchanged ($\hat{\beta} = -1.696$). Excluding the Delta wave (April–June 2021), when differential healthcare infrastructure could confound mobility, produces a similar coefficient ($\hat{\beta} = -1.828$, $p = 0.08$). Clustering at the district level rather than the state level widens the confidence interval (SE = 1.176, $p = 0.15$). Using log(1 + plazas) as continuous treatment yields $\hat{\beta} = -1.117$ ($p = 0.24$). A shorter pre-

Table 4: Robustness: Alternative Specifications

Specification	Coefficient	SE	N
Main specification	-1.696^{*}	(0.967)	100,197
Placebo tests			
Placebo: Oct 2020	0.514	(0.574)	39,235
Placebo: Aug 2020	-0.423	(0.507)	39,235
Highway states only	-1.696^{*}	(0.977)	93,105
Drop Delta wave	-1.828^{*}	(1.019)	90,605
Cluster: district	-1.696	(1.176)	100,197
Log(1+plazas)	-1.117	(0.938)	100,197
Short pre-period	-1.969^{**}	(0.863)	76,507

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. All specifications include district and state×week fixed effects except "Cluster: district" which changes clustering only. Placebo tests use pre-mandate data only. "Drop Delta wave" excludes April–June 2021. "Short pre-period" restricts to October 2020 onward.

period beginning October 2020 yields a somewhat larger and significant estimate ($\hat{\beta} = -1.969$, $p = 0.03$), though this may reflect sensitivity to the composition of the pre-period during COVID recovery.

6. Discussion

The central finding of this paper is that India's FASTag mandate—despite its scale and the dramatic reduction in toll-plaza congestion—did not generate detectable positive economic spillovers at the district level. Three explanations deserve consideration.

Time Savings Were Real But Small Relative to Total Costs. A truck saving 20 minutes per plaza on an 8-hour haul gains roughly 4% in travel time. For local commuters, the savings are even smaller—most daily trips do not cross toll plazas. The transport economics literature suggests that time savings below a perceptibility threshold may not alter behavior (Small, 2012). The FASTag mandate reduced a visible friction, but one that may have been a small share of total transport costs.

Benefits Accrued to Through-Traffic, Not Localities. Toll plazas are designed to tax highway users, most of whom are passing through rather than stopping locally. If the primary beneficiaries of reduced queuing are long-distance freight operators, the economic gains may be distributed along entire origin-destination corridors rather than concentrated near plazas (Allen et al., 2020). This is consistent with the null local spillover and suggests that evaluating toll modernization requires tracking benefits across the entire network, not at individual nodes.

District-Level Measurement May Be Too Coarse. This is the most important caveat. Indian districts span thousands of square kilometers (median area exceeds 4,000 km^2), while transport infrastructure spillovers documented in prior work are typically concentrated within 5–10 km of nodes (Redding and Rossi-Hansberg, 2017). Effects localized around 718 specific geocoded points would be severely attenuated when averaged across entire districts. Village-level nighttime lights data, unavailable for this study, could reveal localized spillovers invisible at the district level through distance-gradient analysis (e.g., comparing outcomes in 0–5 km vs. 20–50 km rings around plazas). This spatial aggregation limitation is the most important qualification of the results. Future work using village-level outcomes and distance-based treatment intensity could detect effects that the present district-level design cannot. Nevertheless, even if localized spillovers exist, their attenuation at the district level suggests they are not large enough to reshape broader economic geography.

How does this null compare to prior work? Asher and Novosad (2020) find large effects of India's rural road program (PMGSY) on local economic activity, but PMGSY created *new* connectivity where none existed—a fundamentally different margin than digitizing existing infrastructure. Ghani et al. (2016) document that the Golden Quadrilateral highway upgrade increased manufacturing activity in nearby districts, but upgrading highways expands capacity, whereas FASTag merely removes a transaction cost at fixed points. The null here suggests that the economic returns to transport infrastructure investment are highly heterogeneous: building new links reshapes geography; removing friction at existing chokepoints does not.

7. Conclusion

India's FASTag mandate was one of the world's largest simultaneous transport digitization events, eliminating cash tolling at over 700 highway plazas serving hundreds of millions of annual trips. This paper finds no evidence that this friction reduction generated measurable economic spillovers at the district level. The result should be interpreted with the spatial resolution of the data in mind: localized effects within a few kilometers of plazas remain an open empirical question best addressed with village-level data. Nevertheless, the finding that district-level economic geography is unmoved by even a dramatic reduction in toll-plaza friction suggests that claims about broader spatial multipliers from transport digitization deserve scrutiny. Policymakers evaluating toll modernization should measure what ETC actually delivers—throughput gains and operational efficiency—rather than projecting unmeasured spillovers into cost-benefit analyses.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP). Toll plaza data sourced from the *geohacker/toll-plazas-india* GitHub repository. Mobility data from Google Community Mobility Reports.

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

Contributors: @ai1scl

First Contributor: https://github.com/ai1scl

References

Allen, Treb, Costas Arkolakis, and Yuta Takahashi, "Universal Gravity," *Journal of Political Economy*, 2020, *128* (2), 393–433.

Asher, Sam and Paul Novosad, "Rural Roads and Local Economic Development," *American Economic Review*, 2020, *110* (3), 797–823.

Banerjee, Abhijit, Esther Duflo, Clément Imbert, Santhosh Mathew, and Rohini Pande, "Can E-Governance Reduce Capture of Public Programs? Experimental Evidence from a Financial Reform of India's Employment Guarantee," *Journal of Political Economy*, 2020, *128* (4), 1–41.

Burris, Mark W. and Ram M. Pendyala, "The Impact of Electronic Tolling on Toll Revenue and Traffic," *Journal of Transportation Engineering*, 2003, *129* (3), 232–238.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller, "Bootstrap-Based Improvements for Inference with Clustered Errors," *Review of Economics and Statistics*, 2008, *90* (3), 414–427.

Donaldson, Dave, "Railroads of the Raj: Estimating the Impact of Transportation Infrastructure," *American Economic Review*, 2018, *108* (4-5), 899–934.

Faber, Benjamin, "Trade Integration, Market Size, and Industrialization: Evidence from China's National Trunk Highway System," *Review of Economic Studies*, 2014, *81* (3), 1046–1070.

Ghani, Ejaz, Arti Grover Goswami, and William R. Kerr, "Highway to Success: The Impact of the Golden Quadrilateral Project for the Location and Performance of Indian Manufacturing," *Economic Journal*, 2016, *126* (591), 317–357.

Muralidharan, Karthik, Paul Niehaus, and Sandip Sukhtankar, "Identity Verification Standards in Welfare Programs: Experimental Evidence from India," *Review of Economics and Statistics*, 2021, pp. 1–45.

National Highways Authority of India, "Annual Report 2020-21," Technical Report, Ministry of Road Transport and Highways, Government of India 2021.

Ozbay, Kaan, Ozlem Ozturk, Ozlem Yanmaz-Tuzel, and Bekir Bartin, "Estimation of Travel Delay and Its Variability from Link-Based Data," *Transportation Research Record*, 2006, *1964* (1), 121–130.

Redding, Stephen J. and Esteban Rossi-Hansberg, "Quantitative Spatial Economics," *Annual Review of Economics*, 2017, *9*, 21–58.

Small, Kenneth A., "Valuation of Travel Time," *Economics of Transportation*, 2012, *1* (1-2), 2–14.

Storeygard, Adam, "Farther on Down the Road: Transport Costs, Trade and Urban Growth in Sub-Saharan Africa," *Review of Economic Studies*, 2016, *83* (3), 1263–1295.

A. Data Appendix

Toll Plaza Data. The *geohacker/toll-plazas-india* repository scrapes NHAI's Toll Information System API at irregular intervals. Each scrape captures a snapshot of all toll plazas including name, NHAI ID, latitude/longitude, design traffic capacity (PCU/day), cumulative revenue (INR Crores), capital cost, fee schedule by vehicle class, and project type. The data spans 47 snapshots from June 21, 2020 to March 1, 2022. Traffic capacity is a static design parameter, not actual traffic volume; cumulative revenue updates irregularly. I use the cross-sectional plaza characteristics (location, capacity, project type) rather than the time series, which exhibits minimal within-plaza variation.

I assign plazas to GADM Level-2 districts via spatial join using the `sf` package in R. Of 718 unique plazas, 12 fall near district borders and receive dual assignments; I retain the first match. This yields 270 unique districts containing at least one toll plaza, spanning 24 states.

Google Mobility. Google's Community Mobility Reports measure daily visits to categorized locations using anonymized, aggregated cellphone location data. Each observation reports the percentage change in visits relative to a pre-pandemic baseline (median of January 3–February 6, 2020). I use three annual files (2020, 2021, 2022) filtered to India (`country_region_code = "IN"`), retaining only district-level observations (`sub_region_2` non-empty). I match Google's district names to GADM district names using exact string matching after lowercasing and trimming. Of 628 Google Mobility districts, 270 match to GADM districts containing toll plazas and 361 do not. Weekly aggregation computes the mean of daily values within each ISO week.

B. Identification Appendix

Pre-Trends. The event study (discussed in the main text) shows that pre-mandate coefficients are generally small and centered around zero, with no systematic pre-trend. Two pre-mandate months show marginally significant coefficients ($t = -12$: $+2.1$, $p = 0.05$; $t = -6$: -2.1, $p = 0.02$), but these are plausibly explained by differential COVID timing across states and do not exhibit a monotonic trend.

Placebo Outcomes. Residential mobility serves as a within-category placebo. If the FASTag mandate specifically affected transport-related activity, residential time (hours spent at home) should not respond, or should move in the opposite direction. The significant negative coefficient on residential (-1.05, $p < 0.01$) suggests that the mobility differences between plaza and non-plaza districts reflect broad compositional shifts in how Google's mobility metric behaves across district types, rather than a transport-specific channel. This reinforces the null interpretation.

C. Robustness Appendix

Full results for all robustness specifications are reported in Table 4 in the main text. Key additional details:

Clustering. The main specification clusters at the state level (34 clusters), which may under-reject with few clusters (Cameron et al., 2008). District-level clustering (601 clusters) yields a wider confidence interval (SE = 1.18 vs. 0.97) and $p = 0.15$.

Delta Wave. India's devastating second COVID wave (April–June 2021) differentially affected states. Excluding this period eliminates concerns about differential healthcare access confounding mobility, but the estimate barely changes (-1.83 vs. -1.70).

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Transit stations	−1.696	(0.967)	26.4	−0.0642	(0.0366)	Moderate negative
Workplaces	−0.857	(0.599)	17.1	−0.0501	(0.0350)	Moderate negative
Retail & recreation	−2.524	(0.849)	25.9	−0.0974	(0.0328)	Moderate negative
Residential	−1.051	(0.306)	7.6	−0.1376	(0.0401)	Moderate negative

Notes: **Country:** India. **Research question:** Did the February 2021 FASTag electronic toll collection mandate at national highway toll plazas generate local economic spillovers in surrounding districts? **Policy mechanism:** The mandate required all vehicles to use RFID-based electronic toll collection (FASTag) at 700+ national highway plazas, eliminating cash-based toll queues of 20–45 minutes and replacing them with sub-10-second electronic reads. Pre-mandate adoption was approximately 34%; post-mandate it exceeded 96%. **Outcome definition:** Google Community Mobility Reports measuring percentage change in visits to location categories (transit stations, workplaces, retail and recreation, residential) relative to a pre-pandemic baseline (median of January 3–February 6, 2020). **Treatment:** Binary indicator equal to one if the district contains at least one NHAI toll plaza. **Data:** Google Mobility Reports (daily, February 2020–October 2022), 628 Indian districts, aggregated to weekly level; NHAI toll plaza locations from geohacker/toll-plazas-india (718 plazas geocoded to districts via GADM boundaries). **Method:** Difference-in-differences with district and state-by-week fixed effects; standard errors clustered at the state level. **Sample:** All Indian districts with Google Mobility coverage; 270 treated districts (with ≥ 1 toll plaza), 361 control districts; 55 pre-mandate weeks, 88 post-mandate weeks. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation. 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).