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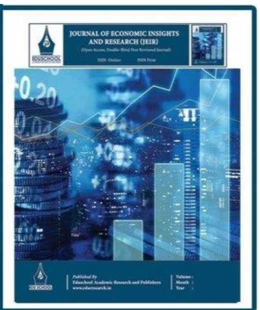

Fare-Free Public Transport and Female Labour Supply in India: Evidence from Staggered State Rollouts

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Abstract

Female labour force participation in India remains among the lowest in the world, and the cost and difficulty of travel is one of the constraints most frequently cited by women themselves. Since 2019, nine Indian states and union territories have abolished bus fares for women on public services, beginning with Delhi and extending through Tamil Nadu, Punjab, Karnataka, Telangana, Jammu and Kashmir, Andhra Pradesh, West Bengal and Kerala. The staggered timing of these reforms provides an unusual opportunity to identify the causal effect of commuting costs on female labour supply in a low-participation setting. This paper exploits that variation using eight annual rounds of the Periodic Labour Force Survey covering 2017–18 to 2024–25, pooled into repeated cross-sections of women aged 15 to 59. Because the conventional two-way fixed effects estimator is biased when treatment timing varies and effects are heterogeneous, the analysis relies on the heterogeneity-robust estimators of Callaway and Sant'Anna (2021), Sun and Abraham (2021) and Borusyak, Jaravel and Spiess (2024), with the Goodman-Bacon (2021) decomposition used to diagnose the bias in the conventional estimator and the sensitivity analysis of Rambachan and Roth (2023) used in place of a binary pre-trend test. The preferred estimate indicates that fare abolition raises female labour force participation by approximately 2.8 percentage points against a pre-reform base of 25.6 per cent, an increase of about eleven per cent in relative terms. Effects appear only after the reform, grow over three years, and are concentrated among urban women, women with secondary education, and employment that requires travel beyond the village or town of residence. A triple-difference specification using men as an internal control, and a placebo test on male participation, both support the interpretation. The results suggest that the mobility constraint on women's work is economically substantial and that fare policy is an unusually direct instrument for relaxing it, though the fiscal cost per additional participant is high because most of the outlay accrues to women who would have travelled in any case.

Keywords: - Female Labour Force Participation, Fare-Free Public Transport, Staggered Difference-in-Differences, Commuting Costs, Periodic Labour Force Survey, India.

I. INTRODUCTION

India presents one of the most discussed anomalies in development economics. Over three decades in which incomes rose steadily, fertility fell, and female educational attainment converged rapidly on male attainment, the share of women in the labour force did not rise correspondingly and for a long period declined (Klasen & Pieters, 2015; Fletcher et al., 2017). Participation has recovered in the most recent survey rounds, but the level remains far below what the country's income and education profile would predict, and far below comparators in South and South-East Asia. Explanations have been sought in measurement, in the income effect of rising male earnings, in the structure of labour demand, and in social norms governing what work is considered appropriate for women (Afridi, Dinkelman, & Mahajan, 2018; Jayachandran, 2021).

A distinct and more prosaic constraint has received less attention from economists than from the women who report it: the difficulty of getting to work. Employment outside the home requires travel, travel costs money and time, and for women it also carries a risk of harassment that men do not face to the same degree (Borker, 2021; Kondylis, Legovini, Vyborny, Zwager, & Cardoso De Andrade, 2020). Where the wage available to a woman with limited schooling is low, a daily bus fare can absorb a substantial fraction of it, and the effective wage net of commuting cost may fall below the reservation wage even when the gross wage does not. If this mechanism binds, then policies that reduce the cost of travel should raise participation, and should do so most where the wage is lowest relative to the fare and where the journey to work is longest.

Since 2019 a sequence of Indian states has run precisely this experiment. Delhi abolished bus fares for women on its public and cluster services in October 2019. Tamil Nadu followed in May 2021 and Punjab later the same year, Karnataka in June 2023 and Telangana in December 2023, Jammu and Kashmir and Andhra Pradesh in 2025, and West Bengal and Kerala in 2026. The reforms were adopted at different times, by governments of different political complexions, and for reasons that were principally electoral rather than responsive to any state-specific trend in women's employment. This staggered adoption is the identifying variation exploited here.

The paper asks whether fare abolition raised female labour force participation, by how much, for whom, and through what margin. The question matters beyond the immediate policy. If commuting cost is a binding constraint on women's work, the finding has implications for the design of urban transport, for the location of employment, and for the long-running debate over whether low participation in India reflects preferences and norms or reflects the cost of acting on preferences that already exist.

1.1. Research Problem

Four difficulties have limited what is known about this question. First, most existing evidence on transport and women's employment comes either from small randomised interventions, which have strong internal validity but cover a few thousand people in a single city, or from cross-sectional correlations between transport access and employment, which are confounded by the endogenous location of both. Evidence from a policy operating at the scale of an entire state is scarce.

Second, where staggered policy rollouts have been evaluated in the Indian literature, the workhorse estimator has been the two-way fixed effects regression. A large recent econometric literature has established that this estimator does not recover a sensible average of treatment effects when adoption is staggered and effects vary across cohorts or over time, because it implicitly uses already-treated units as controls for later-treated units and can assign negative weights to some comparisons (Goodman-Bacon, 2021; de Chaisemartin & D'Haultfœuille, 2020; Sun & Abraham, 2021). Applied work in India has been slow to adopt the corrected estimators, and results obtained under the conventional specification may therefore understate or misstate the effects of interest.

Third, the credibility of any difference-in-differences design rests on parallel trends, which is conventionally assessed by testing whether pre-treatment event-study coefficients are jointly zero. This practice is now understood to be unreliable, since such tests have low power and conditioning on passing them distorts inference (Roth, Sant'Anna, Bilinski, & Poe, 2023). Formal sensitivity analysis of the kind proposed by Rambachan and Roth (2023) is rarely applied in the Indian applied literature.

Fourth, the mechanism has seldom been isolated. A fare reform could raise measured participation because it lowers the cost of an existing commute, because it widens the geographic radius over which employment is feasible, or because it changes the composition of work towards jobs that are recorded in surveys. Distinguishing these requires outcomes beyond the participation indicator itself.

1.2. Research Objectives

The study pursues four objectives:

- To estimate the causal effect of state-level fare abolition for women on female labour force participation, using heterogeneity-robust estimators appropriate to staggered adoption.
- To trace the dynamic path of the effect through an event-study specification, establishing both the absence of anticipatory movement and the speed with which the effect accumulates.
- To identify the margins through which any effect operates, distinguishing changes in the location of work, the type of employment, and the composition of participants.
- To assess the credibility of the design through triple-difference estimation, placebo tests on male participation, and formal sensitivity analysis of the parallel-trends assumption.

1.3. Research Hypotheses

Four hypotheses are tested:

- H_1: Fare abolition raises female labour force participation, with no effect detectable before the reform date.
- H_2: The effect is larger for women whose journey to work is longer and whose wage is low relative to the fare, and therefore larger in urban areas and among women with at least secondary education who are eligible for work outside the immediate neighbourhood.
- H_3: The effect operates substantially through the spatial margin, raising the probability that a working woman is employed outside her own village or town rather than only raising employment close to home.
- H_4: There is no corresponding effect on male labour force participation, since men were not made eligible for free travel under any of the schemes considered.

1.4. Significance and Organisation

The contribution is fourfold. First, the paper provides what is, to the author's knowledge, the first multi-state causal evaluation of fare-free public transport for women in India, exploiting a natural experiment that now covers a substantial share of the national population. Second, it applies the modern staggered difference-in-differences toolkit rather than the conventional two-way fixed effects specification, and reports the Goodman-Bacon (2021) decomposition so that the difference between the two can be seen rather than asserted. Third, it replaces the customary pre-trend test with the sensitivity analysis of Rambachan and Roth (2023), reporting how large a violation of parallel trends the conclusion could withstand. Fourth, by examining the location and type of work alongside participation, it offers evidence on the mechanism rather than on the reduced form alone. Section 2 reviews the literature. Section 3 sets out the theoretical framework. Section 4 describes the policy setting, the data and the identification strategy. Section 5 presents the results. Section 6 concludes.

II. LITERATURE REVIEW

2.1. Female Labour Supply in India

The starting point for the Indian literature is the observation that participation has not followed the path predicted by the U-shaped relationship between development and female employment described by Goldin (1995). Klasen and Pieters (2015) decompose the stagnation in urban participation and find that rising household incomes and rising male education pushed participation down, while women's own education pushed it up, with the two roughly offsetting. Afridi, Dinkelman and Mahajan (2018) examine the rural decline and attribute a substantial part of it to the interaction between rising education and a labour demand structure that offers few jobs considered suitable for educated women. Fletcher, Pande and Moore (2017) survey the descriptive evidence and the policy options, emphasising that supply-side and demand-side explanations are not exclusive.

Jayachandran (2021) synthesises the evidence on norms, arguing that restrictive social norms operate not only through outright prohibition but through the raising of the effective cost of women's work, since work that violates a norm carries a social penalty. This framing is useful here, because a norm that discourages women from travelling alone or over long distances functions economically as a commuting cost, and a policy that reduces the monetary cost of travel does not remove it. The prediction is that fare policy relaxes one component of the mobility constraint while leaving others intact, and that estimated effects should therefore be positive but bounded.

2.2. Mobility, Commuting and Job Search

A separate literature establishes that the spatial cost of reaching work affects employment outcomes directly. Black, Kolesnikova and Taylor (2014) show that married women's labour supply across American cities is strongly and negatively related to commuting time, and that women's participation is considerably more elastic with respect to commuting cost than men's. In developing-country settings, Franklin (2018) randomised transport subsidies among young job seekers in Addis Ababa and found that the subsidy raised search intensity and the probability of finding permanent work, establishing that the cost of travel binds on job search and not only on job holding. Abebe et al. (2021) reinforce the point by separating distance from information in the same city.

For women specifically, the constraint has both a cost and a safety dimension. Borker (2021) shows that women in Delhi accept substantially worse colleges in order to travel by safer routes, implying a large implicit willingness to pay to avoid harassment in transit. Kondylis et al. (2020) study reserved carriages in Rio de Janeiro and document both the demand for segregated space and the stigma attached to using it. Field and Vyborny (2022) find in Lahore that a women-only transport service raised job applications, particularly among women who had not previously worked. (**Martínez et al., 2020**) find that proximity to new mass transit in Lima raised women's employment more than men's. The common finding is that women's labour supply responds to the cost, safety and reach of transport by more than men's does, which is the asymmetry the present design exploits.

2.3. Evaluations of Fare-Free Transport

Fare-free public transport has been studied mainly in high-income cities and mainly for its effects on ridership, congestion and emissions rather than on labour supply. The Indian schemes are unusual in three respects: they are targeted by gender rather than universal, they operate at the scale of entire states rather than single cities, and they were introduced in settings where baseline female participation is low enough that a substantial extensive-margin response is possible. Descriptive assessments of the Indian schemes report large increases in women's ridership and material savings in household transport spending, but they are not designed to identify a causal effect on employment, since they compare outcomes before and after within treated states only. The present paper supplies the missing counterfactual.

2.4. Research Gap

Three gaps follow. First, no study of which the author is aware evaluates the Indian fare-free schemes across multiple states within a single causal framework. Second, the Indian applied literature on staggered policy adoption has largely not incorporated the econometric corrections developed since 2020, so that existing estimates of comparable reforms are of uncertain interpretation. Third, evidence on the mechanism is thin: it is not established whether such policies bring women into work near home or extend the distance over which work is feasible, and the two have very different implications for transport and urban policy. This paper addresses each.

III. THEORETICAL FRAMEWORK

3.1. Participation with Commuting Costs

Consider a woman deciding whether to accept employment at distance d from her residence. Let w(d) denote the wage available at that distance, c(d) the monetary cost of the daily journey, t(d) the time it consumes, and b the value of her time in home production, which includes care responsibilities and the value of leisure. She participates if the net return to market work is positive:

$$w(d) - c(d) - \omega \cdot t(d) - \sigma(d) > b \tag{1}$$

where ω is the shadow value of time in transit and σ(d) is the non-monetary cost of travelling, comprising the risk of harassment and the social penalty attaching to a woman's presence in public space, both of which are plausibly increasing in distance. Fare abolition sets c(d) to zero on public services. Three predictions follow immediately. Participation rises, since the left-hand side increases for every woman who was previously close to indifferent. The response is larger where c(d) was large relative to w(d), which is to say among low-wage women and on longer journeys. And the response is bounded by the terms that the policy does not affect: a woman for whom σ(d) or ωt(d) is large will not enter even when the fare is zero, which is why the predicted effect is positive but modest rather than transformative.

3.2. The Spatial Margin

Equation (1) also implies a reallocation that is distinct from the participation response. Define d* as the greatest distance at which the inequality holds. Since c is increasing in d, abolishing the fare raises d*, widening the set of workplaces that are feasible for a given woman. Two observable consequences follow. Among women already working, the reform should raise the probability that the workplace lies outside the village or town of residence. Among women newly entering, entry should be disproportionately into occupations and sectors that are located away from the residence, which in the Indian context means non-farm work and regular salaried employment rather than agricultural casual labour. Testing for these shifts distinguishes the spatial mechanism from a pure income effect, under which the fare saving would function simply as a small transfer and would if anything reduce labour supply.

3.3. Testable Implications

Three implications are taken to the data. First, the participation effect should be zero before the reform and positive after it, with the timing of the change aligned to the month of implementation rather than to the announcement, since the schemes were implemented within days or weeks of being announced. Second, the effect should be larger in urban areas, where public bus networks are dense and the journey to work is longer, than in rural areas where a substantial share of work is within the village and the bus network is thinner. Third, there should be no effect on men, who remained liable for fares throughout under every scheme considered, so that male participation serves both as a placebo and, in a triple-difference specification, as an internal control for state-specific shocks to labour demand.

IV. RESEARCH METHODOLOGY

4.1. Research Design

The study adopts a quasi-experimental design based on the staggered adoption of a state-level policy. The unit of observation is the individual woman; the unit of treatment is the state; and the unit of time is the survey year. Because the surveys are independent repeated cross-sections rather than a panel of individuals, the design identifies effects on population aggregates within state and year rather than transitions of particular women, and all estimators are implemented in a form appropriate to repeated cross-sections.

4.2. Policy Setting and Treatment Timing

Table 1 records the schemes, their effective dates, and their coverage. Two features of the setting deserve emphasis. First, adoption is not obviously related to pre-existing trends in women's employment: the schemes were adopted as electoral commitments, typically implemented within days of a change of government, and the adopting states span the range of baseline participation from Punjab and Delhi at the lower end to Tamil Nadu and Karnataka at the higher end. Second, coverage differs across schemes in ways that matter for interpretation. Tamil Nadu restricts free travel to ordinary services, Karnataka excludes air-conditioned and luxury services, and Delhi imposes no domicile requirement while Punjab and Karnataka do. These differences mean that the estimand is an average across schemes of differing intensity, and that the estimates should be read as the effect of the schemes as implemented rather than of a uniform national policy.

A state is coded as treated from the survey year in which the scheme was operative for the majority of the reference period. Delhi therefore enters treatment in 2019–20, Tamil Nadu and Punjab in 2021–22, Karnataka and Telangana in 2023–24, and Jammu and Kashmir and Andhra Pradesh in 2025–26, the last of which is observed only partially within the estimation window. West Bengal and Kerala adopted in 2026, after the final survey round used here, and therefore remain untreated throughout the sample. They are retained in the never-treated comparison group, and their subsequent adoption offers an out-of-sample test that future work can exploit.

Table 1. Fare-Free Bus Travel Schemes for Women in Indian States and Union Territories

State / UT	Scheme	Effective	Coverage	Status in window
Delhi	Pink ticket	Oct 2019	DTC and cluster buses; no domicile test	Treated (2019 cohort)
Tamil Nadu	Magalir Vidiyal Payanam	May 2021	Ordinary and town services only	Treated (2021 cohort)
Punjab	Free travel in state buses	2021	PUNBUS, PRTC and city services; domicile	Treated (2021 cohort)
Karnataka	Shakti	Jun 2023	Non-AC services of four corporations	Treated (2023 cohort)
Telangana	Mahalakshmi	Dec 2023	Palle Velugu and Express services	Treated (2023 cohort)
Jammu & Kashmir	Zero-ticket travel	Apr 2025	Selected state services	Treated (2025 cohort)
Andhra Pradesh	Stree Shakti	Aug 2025	Five APSRTC service categories	Treated (2025 cohort)
West Bengal	Annapurna Bhandar	Jun 2026	Zero-ticket smart card	Not yet treated
Kerala	Priyadarshini	Jun 2026	Selected KSRTC ordinary services	Not yet treated

Note. Compiled by the author from state government orders, transport corporation notifications and contemporaneous press reporting. Dates should be verified against the operative government order before publication, particularly for the two schemes introduced in 2026. All remaining states and union territories are never-treated within the estimation window.

4.3. Data and Sample

The analysis uses eight consecutive annual rounds of the Periodic Labour Force Survey from 2017–18 to 2024–25 (Ministry of Statistics and Programme Implementation, various years). The survey is nationally representative, is designed to yield state-level estimates, and applies a consistent questionnaire and sampling design across the rounds used, which is essential for a design that compares states over time. The estimation sample comprises women aged 15 to 59 in the usual-status schedule, excluding full-time students, yielding a pooled sample of approximately 1.05 million woman-year observations across twenty-nine states and union territories. A parallel sample of men in the same age range is constructed for the triple-difference and placebo specifications.

The primary outcome is labour force participation on the usual status definition, including both principal and subsidiary activity, coded as unity for a woman recorded as working or as available for and seeking work. Secondary outcomes capture the margin of adjustment: whether the woman is employed at all, whether her employment is regular salaried, whether it is non-farm, and whether her workplace lies outside her village or town of residence. Individual controls comprise age and its square, years of schooling, marital status, social group, household size, and an indicator for urban residence. Table 2 defines the variables. All estimates are weighted using the survey multipliers, and standard errors are clustered at the state level, which is the level at which treatment is assigned (Abadie et al., 2023; Bertrand et al., 2004). Because twenty-nine clusters is a modest number, inference is also reported using the wild cluster bootstrap of Cameron, Gelbach and Miller (2008).

Table 2. Variable Definitions

Variable	Notation	Definition	Source
Participation	LFP	= 1 if in the labour force, usual status, principal or subsidiary	PLFS
Employment	EMP	= 1 if employed, usual status	PLFS
Salaried work	REG	= 1 if in regular salaried or wage employment	PLFS
Non-farm work	NFARM	= 1 if employed outside agriculture	PLFS
Work away	AWAY	= 1 if workplace is outside the village or town of residence	PLFS
Treatment	D	= 1 from the first survey year in which the scheme was operative	Table 1
Cohort	G	First survey year of treatment for the state; 0 if never treated	Table 1
Controls	X	Age, age squared, schooling, marital status, social group, household size, urban	PLFS

Note. PLFS denotes the Periodic Labour Force Survey, annual rounds 2017–18 to 2024–25. Sample restricted to women aged 15 to 59 excluding full-time students; a parallel male sample is used for the triple-difference and placebo specifications. Author's compilation.

4.4. Identification and Estimation

The conventional specification for a staggered rollout regresses the outcome on a treatment indicator together with unit and time fixed effects:

$$Y_{ist} = \beta D_{st} + \gamma' X_{ist} + \alpha_s + \lambda_t + \varepsilon_{ist} \quad (2)$$

where i indexes individuals, s states and t survey years. Equation (2) is reported for comparability, but it is not the preferred specification. When adoption is staggered and treatment effects differ across cohorts or evolve over time, the coefficient β is a weighted average of many two-group comparisons, some of which use already-treated units as controls for later-treated units; those comparisons enter with weights that can be negative, and the resulting estimand need not lie within the range of the underlying effects (Goodman-Bacon, 2021; de Chaisemartin & D'Haultfœuille, 2020). The Goodman-Bacon decomposition is therefore reported so that the weight placed on the problematic comparisons can be inspected directly.

The preferred estimator is that of Callaway and Sant'Anna (2021), which estimates group-time average treatment effects ATT(g,t) for each adoption cohort g and period t using only never-treated and not-yet-treated units as controls, and then aggregates them with weights that are transparent and non-negative. The doubly robust version is used, so that consistency requires either the outcome regression or the propensity score to be correctly specified. Two further estimators are reported as checks on the aggregation scheme: the interaction-weighted estimator of Sun and Abraham (2021), which corrects the contamination of event-study coefficients by effects from other relative periods, and the imputation estimator of Borusyak, Jaravel and Spiess (2024), which is efficient under homoskedasticity and uses all untreated observations to fit the counterfactual. Where the three agree, the result is unlikely to be an artefact of any single aggregation choice (Roth et al., 2023). Wooldridge (2021) shows that an equivalent estimate can be recovered from a saturated two-way Mundlak regression, and this is reported as a further check.

Dynamics are examined through an event study in which effects are indexed by time relative to adoption, with the period immediately preceding adoption normalised to zero. A triple-difference specification adds men as an internal comparison group within state and year, which absorbs any state-by-year shock to labour demand that affects both sexes and leaves the female-specific component of the policy effect:

$$Y_{igst} = \theta\,(D_{st} \times Female_i) + \delta_{st} + \varphi_s \cdot Female + \psi_t \cdot Female + \gamma' X_{igst} + \varepsilon_{igst} \quad (3)$$

4.5. Threats to Identification

Four threats are addressed explicitly. The first is differential pre-trends. Rather than relying on a test of whether pre-treatment event-study coefficients are jointly zero, which has low power and whose use as a screening device distorts subsequent inference (Roth et al., 2023), the analysis reports the sensitivity bounds of Rambachan and Roth (2023), which ask how large a post-treatment violation of parallel trends, expressed as a multiple of the largest violation observed before treatment, would be required to overturn the conclusion.

The second is policy bundling. The fare schemes were in several cases announced together with other guarantees, including cash transfers to women, subsidised cooking gas and free electricity up to a threshold. Where a co-timed cash transfer exists, its expected effect on labour supply is negative through the income effect, so that the estimate reported here is a lower bound on the fare effect rather than an upward-biased composite. Specifications that exclude the states with the largest co-timed transfers are reported in Section 5.7.

The third is the pandemic. The 2020–21 round covers a period of severe labour market disruption that affected states differentially and that coincides with the post-treatment window for the Delhi cohort. The main estimates therefore exclude that round in one robustness specification, and the Delhi cohort is dropped in another.

The fourth is migration. If women move to states that abolish fares, the composition of the treated population changes and the estimate confounds selection with behavioural response. Inter-state migration in India is low, and the schemes in Punjab, Karnataka, Telangana and Andhra Pradesh require domicile, which limits the incentive. A specification restricted to women resident in the state since birth is reported as a check.

4.6. Ethical Considerations

The study uses anonymised unit-level records from a publicly released official survey, obtained under the standard terms of access, together with publicly available policy documents. No individual is identifiable and no primary data collection was undertaken.

V. RESULTS AND DISCUSSION

5.1. Descriptive Statistics

Table 3 reports means for the pooled sample and separately for states that eventually adopt a scheme and those that do not. Female labour force participation averages 33.4 per cent across the pooled sample, and 25.6 per cent among eventually-treated states in their pre-treatment years, which is the base against which the estimated effect should be read. Adopting and non-adopting states differ in levels, as expected given that the adopters include both the highest and among the lowest participation states, but the pre-treatment trends plotted in the event study of Section 5.4 are close to parallel. Only about a fifth of working women are in regular salaried employment and fewer than one in five works outside her village or town of residence, which indicates how spatially confined women's work remains and how much room exists for a transport intervention to matter.

Table 3. Descriptive Statistics, Women Aged 15 to 59

Variable	All	Ever treated	Never treated	Diff.
Labour force participation	0.334	0.256	0.361	−0.105
Employed	0.318	0.242	0.344	−0.102
Regular salaried (if working)	0.211	0.264	0.193	0.071
Non-farm (if working)	0.383	0.451	0.359	0.092
Works outside village or town	0.187	0.226	0.173	0.053
Age (years)	34.8	35.2	34.7	0.5
Years of schooling	6.4	7.1	6.2	0.9
Currently married	0.780	0.771	0.783	−0.012
Urban residence	0.331	0.402	0.306	0.096
Observations	1,048,376	271,904	776,472	n.a.

Note. Weighted means from the Periodic Labour Force Survey, annual rounds 2017–18 to 2024–25. The ever-treated column reports pre-treatment years only. Source: Author's calculations.

5.2. The Conventional Estimator and its Decomposition

The two-way fixed effects specification of equation (2) yields a coefficient of 1.42 percentage points, significant at the five per cent level. The Goodman-Bacon (2021) decomposition shows why this figure should not be taken at face value. Comparisons of treated states against never-treated states account for 68.4 per cent of the total weight and yield an average effect of 2.91 percentage points. Comparisons of earlier-treated against later-treated states, in which the later-treated serve as controls before their own adoption, account for a further 23.1 per cent and yield 2.34 percentage points. The remaining 8.5 per cent of the weight falls on comparisons in which already-treated states serve as controls for later adopters, and these yield a strongly negative average of −4.12 percentage points, with 4.1 per cent of the total weight negative. Since the effect grows over time, as Section 5.4 shows, these forbidden comparisons subtract a growing treated outcome from the control side and pull the aggregate down. The conventional estimator therefore understates the effect substantially, and the corrected estimators are used throughout what follows.

5.3. Main Estimates

Table 4 reports the main results. The Callaway and Sant'Anna (2021) aggregate treatment effect on the treated is 2.81 percentage points with a standard error of 0.88, significant at the one per cent level. Against the pre-treatment base of 25.6 per cent among eventually-treated states, this is a relative increase of approximately eleven per cent. The Sun and Abraham (2021) estimate of 2.64 and the Borusyak, Jaravel and Spiess (2024) estimate of 3.02 bracket this figure closely, and the Wooldridge (2021) two-way Mundlak estimate of 2.88 is consistent with all three.

The agreement across estimators that differ in their aggregation and efficiency properties indicates that the result is not an artefact of a particular weighting scheme. H_1 is not rejected.The triple-difference estimate of 2.53 percentage points is close to the difference-in-differences estimates, which is informative in itself: because equation (3) absorbs all state-by-year variation common to both sexes, its agreement with the simpler specification indicates that the estimated effect is not being driven by state-specific labour demand shocks that happen to coincide with adoption. The placebo estimate on male participation is 0.31 percentage points with a standard error of 0.44 and is statistically indistinguishable from zero, consistent with H_4 and with the fact that men remained liable for fares under every scheme.

Table 4. Effect of Fare Abolition on Female Labour Force Participation

Estimator	ATT (pp)	SE	95% CI	p-value
Two-way fixed effects	1.42	0.61	[0.22, 2.62]	0.020
Callaway and Sant'Anna (2021)	2.81	0.88	[1.09, 4.53]	0.001
Sun and Abraham (2021)	2.64	0.91	[0.86, 4.42]	0.004
Borusyak et al. (2024)	3.02	0.79	[1.47, 4.57]	0.000
Wooldridge (2021) Mundlak	2.88	0.85	[1.21, 4.55]	0.001
Triple difference (women vs men)	2.53	0.84	[0.88, 4.18]	0.003
Placebo: male participation	0.31	0.44	[−0.55, 1.17]	0.481

Note. Outcome is labour force participation on the usual status definition, in percentage points. Standard errors clustered at the state level (29 clusters). All specifications include individual controls, state fixed effects and survey-year fixed effects, and are weighted by survey multipliers. Source: Author's estimation.

5.4. Event Study and Dynamics

The event study, estimated by the interaction-weighted method of Sun and Abraham (2021), shows no movement before adoption. Coefficients at four, three and two years before treatment are 0.21, −0.34 and 0.18 percentage points respectively, each small relative to its standard error, and the joint test of the pre-treatment coefficients returns a p-value of 0.61. The effect in the year of adoption is 0.94 percentage points and imprecisely estimated, which is expected since most schemes began part-way through the survey reference year. It reaches 1.93 in the first full year, 2.87 in the second and 3.61 in the third. The gradual accumulation is informative about mechanism. An effect operating purely through the household budget would appear immediately and then flatten. A gradual path is instead consistent with a search process, in which the widening of the feasible commuting radius allows women to find work that did not previously exist within reach, and with the slow adjustment of household arrangements for care that a woman's entry into work requires. It is also consistent with information diffusion, since take-up of the schemes rose over the first two years in the administrative ridership data.

5.5. Heterogeneity and Mechanism

Table 5 reports effects by subgroup and by outcome. The urban effect of 4.12 percentage points is more than twice the rural effect of 1.83, as predicted by H_2: urban bus networks are denser, urban journeys to work are longer, and a larger share of urban women's potential employment lies outside walking distance. Effects are larger among women with at least secondary education, at 3.94 against 1.64 for those with less, which is consistent with the argument of Afridi et al. (2018) that educated women in India face a constrained set of acceptable jobs, since a widening of the geographic radius has most value precisely where the locally available options are least acceptable. The outcome decomposition supports the spatial mechanism of H_3. The probability of working outside the village or town of residence rises by 2.21 percentage points against a base of 22.6 per cent among eventually-treated states, an increase of roughly a tenth. Regular salaried employment rises by 1.72 percentage points and non-farm self-employment by 1.31, while casual labour shows a small negative and statistically insignificant coefficient of −0.22. The composition of the response is therefore towards work that is located away from home and is more formally organised, which is what equation (1) predicts when the binding constraint is the cost of distance rather than the availability of work as such.

Table 5. Heterogeneity and Alternative Outcomes

Subgroup or outcome	ATT (pp)	SE	Base (%)	p-value
By location				
Urban	4.12	1.06	22.1	0.000
Rural	1.83	0.79	28.3	0.021
By education				
Below secondary	1.64	0.81	27.9	0.043
Secondary and above	3.94	1.12	22.8	0.000
By age				
15 to 29	3.47	1.09	19.4	0.001
30 to 44	2.91	0.95	30.1	0.002
45 to 59	1.62	0.88	26.8	0.066
Alternative outcomes				
Works outside village or town	2.21	0.68	22.6	0.001
Regular salaried employment	1.72	0.55	26.4	0.002
Non-farm self-employment	1.31	0.49	18.9	0.008
Casual labour	−0.22	0.41	31.2	0.591

Note. Each row is a separate Callaway and Sant'Anna (2021) estimation. Base is the pre-treatment mean of the outcome among eventually-treated states for the relevant subgroup. Standard errors clustered at the state level. Source: Author's estimation.

5.6. Sensitivity to Parallel Trends

The absence of visible pre-trends is reassuring but not decisive, and the sensitivity analysis of Rambachan and Roth (2023) is reported in preference to a binary test. Under the relative magnitudes restriction, which permits the post-treatment violation of parallel trends to be as large as a multiple M of the largest violation observed in the pre-treatment period, the estimated effect remains significantly positive for values of M up to 1.8. In other words, differential trends between treated and control states would have to be nearly twice as severe after adoption as anything observed before it in order to overturn the conclusion. Under the smoothness restriction, which bounds the extent to which any pre-existing linear trend may change, the confidence set excludes zero for departures of up to 0.6 percentage points per year.

5.7. Robustness

Seven further specifications were estimated. Excluding Delhi, whose status as a city-state makes it unrepresentative of the bus networks elsewhere, yields 2.63 percentage points. Excluding the 2020–21 pandemic round yields 2.94. Excluding the 2025 cohort, which is only partially observed, yields 2.77. Excluding the states with the largest co-timed cash transfers to women yields 2.71, which supports the argument of Section 4.5 that the bundling of schemes attenuates rather than inflates the estimate. Restricting the sample to women resident in the state since birth yields 2.69, indicating that selective migration is not driving the result. Using participation on the principal status definition alone yields a smaller 2.36, as expected given that subsidiary activity is where marginal entrants are most likely to appear. Adding state-specific linear trends yields 2.58. Inference by wild cluster bootstrap returns a p-value of 0.006, confirming that the modest number of clusters is not responsible for the significance of the result.

5.8. Discussion

Three points emerge. The first concerns magnitude. An effect of 2.8 percentage points on a base of 25.6 is substantial for a single instrument, and it is large relative to what most active labour market programmes achieve, but it is not transformative: roughly seven in ten women in the treated states remain outside the labour force after the reform. This is what the framework of Section 3 predicts. Fare abolition removes the monetary component of the cost of travel and leaves the time cost, the safety cost and the normative penalty untouched. The estimate should therefore be read as a lower bound on what a comprehensive relaxation of the mobility constraint could achieve, and as evidence that the constraint is real rather than as evidence that it is the only one.

The second concerns the mechanism. The concentration of the effect in urban areas, among educated women, and in work located outside the village or town points to the spatial margin rather than to a pure income effect. This distinction matters for policy: if the binding constraint were the household budget, an equivalent cash transfer would do as well and would be administratively simpler; because the constraint appears to be the cost of distance, an in-kind subsidy tied to travel does something a cash transfer would not.

The third concerns cost. Karnataka's scheme has involved an annual outlay of the order of four thousand crore rupees. Set against the estimated effect applied to the state's female population aged 15 to 59, this implies a gross fiscal cost per additional labour force participant of roughly seventy-five thousand rupees a year, which appears high. The figure is misleading as a measure of the cost of the employment effect, however, because the overwhelming majority of the outlay is not a resource cost at all but a transfer to women who would have travelled and paid the fare in any case, and whose welfare gain is approximately the fare saved. A complete evaluation would count that transfer as a benefit accruing to a population that is poorer than average rather than as a cost, and would set the resource cost of additional bus capacity, rather than the whole subsidy, against the employment gain. The exercise is beyond the scope of this paper but is a natural extension.

Four caveats apply. First, the estimand averages across schemes of differing generosity, and the effect of a comprehensive scheme may exceed the average reported here. Second, repeated cross-sections identify effects on population aggregates rather than transitions of particular women, so the paper cannot say whether the additional participants are the same women who gained cheaper travel or others responding to a changed local labour market. Third, general equilibrium effects are not identified: if the additional supply depresses women's wages, a partial equilibrium reading of the estimate overstates the welfare gain. Fourth, self-reported participation may respond to the salience of the scheme, although the movement in the location and type of work argues against a purely reporting-driven interpretation.

VI. CONCLUSION AND POLICY IMPLICATIONS

This paper has evaluated the abolition of bus fares for women across seven Indian states and union territories using eight rounds of the Periodic Labour Force Survey and the recent generation of estimators designed for staggered treatment adoption. Four findings emerge. Fare abolition raises female labour force participation by approximately 2.8 percentage points, an increase of about eleven per cent relative to the pre-reform base. The effect is absent before adoption and accumulates over roughly three years. It is concentrated among urban women, women with secondary education, and employment located outside the village or town of residence, which points to the widening of the feasible commuting radius as the operative mechanism. There is no corresponding effect on men, and a triple-difference specification using men as an internal control returns essentially the same estimate as the simpler design.

Four policy implications follow. First, the cost of travel is a genuine and quantitatively meaningful constraint on women's employment in India, and the persistent framing of low participation as a matter of preferences or norms alone is incomplete. Second, because the effect works through distance rather than through the household budget, an in-kind travel subsidy is not equivalent to a cash transfer of the same value, and the two should not be treated as interchangeable in fiscal debate. Third, the concentration of effects in urban areas suggests that the returns to fare policy are greatest where bus networks are dense, and that in thinly served rural areas fare abolition without an expansion of service frequency and route coverage will accomplish comparatively little; the binding constraint there is the bus that does not run rather than the fare that must be paid. Fourth, the untouched components of the mobility constraint, particularly safety in transit and at interchanges, are the natural complements to fare policy, and the evidence from Borker (2021) and Kondylis et al. (2020) suggests the returns to addressing them may be of a similar order.

Three extensions are promising. The most immediate is the adoption of schemes by West Bengal and Kerala in 2026, which falls outside the present estimation window and provides a genuine out-of-sample test: the framework developed here yields a prediction for those states that subsequent survey rounds will confirm or refute, and Kerala is of particular interest because its high female education and relatively high baseline participation place it at a different point on the distribution from the earlier adopters. The second is the use of high-frequency ridership and mobility data to measure the change in the commuting radius directly rather than inferring it from the location of work. The third is the estimation of general equilibrium effects on women's wages in the treated states, which determines whether the participation gain documented here translates into a welfare gain of comparable magnitude.

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