Cinderella No More: Night Shift Bans and Women's Employment in India

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1 Introduction

Compared to the global averages, Indian women are less likely to engage in income-generating work or actively seek employment. Globally, female labor force participation stands at 50%, whereas in India, only 28.52% of the total working age women participate in the labor force, and 27.44% are employed (MoSPI, 2022). Even women who find employment are mostly engaged in informal work. Globally, nearly 60% of employed women work in informal jobs (UNWomen, 2020). In India, 88% of women in the industrial sector and 71% in the service sector are engaged in informal employment (Bonnet et al., 2019). The literature points to different demand-side and supply-side factors that affect women's labor market outcomes in India.

On the supply side, Indian households often require that women prioritize household management or child/geriatric care over paid work (Sudarshan et al., 2014). Married women are subject to more rigid cultural restrictions on their mobility, and, therefore face greater challenges in participating in the workforce (Sudarshan and Bhattacharya, 2009). Stability in family income leads female family members to choose to drop out of, rather than join, the labor force (Andres et al., 2017). Agrawal et al. (2024) explain this mechanism as the 'income effect'– where women may be considered better off at home as their family income increases. Higher returns on home production relative to market production could reduce the participation in the labor force of better-educated women in rural areas (Afridi et al., 2018). Underestimation of women's contribution to the economy may be a consequence of measurement challenges of Indian official statistics, where unpaid family work may erroneously be classified as domestic duties (Hirway and Jose, 2011) [1].

[1]There are several reasons why the TUS-based rates are higher. First, the TUS has been able to capture multiple short-term jobs of people through its comprehensive reporting of all activities performed by men and women. The survey has also caught the simultaneous jobs, including non-SNA and SNA jobs performed at the same time. Second, there appears to be less bias in the reporting and identification of "work," since the responsibility of identifying "work" is not with the respondents. Third, the TUS has been able to net the subsistence work of people through comprehensive reporting of all activities.

Women's education levels exhibit a U-shaped relationship with their participation in the labor force (Klasen and Pieters, 2015). The downward-sloping part of the U-shaped curve can be explained by the stigmatized perception of low-skilled jobs as women attain intermediate levels of education (Goldin, 1994). The upward sloping curve may be a function of finding white-collar jobs as women attain secondary and further education levels. The education level of the family head is also negatively associated with a female family member's participation in the labor force (Klasen and Pieters, 2015).

The demand for women's labor is shaped by economic forces and institutional dynamics beyond household or individual constraints. It could potentially be affected by technological change (Weinberg, 2000), structural shifts from industrial jobs to jobs oriented to the service sector (Black and Spitz-Oener, 2010), employer's preference for flexibility in working hours (Goldin, 2014), political empowerment (Miller, 2008), and institutional changes (Neumark and Stock, 2006). Literature on the Indian labor market finds that the sectors that draw female workers may have expanded less, thereby depressing the demand for potential female labor (Chatterjee et al., 2015, Klasen and Pieters, 2015). Growing mechanization in agriculture and rising capital intensity in manufacturing sectors limit the demand for women's work because of limited access of women to education and skill development (Mehrotra and Parida, 2017). Political empowerment in the form of Panchayats with female leaders positively affects the demand for women's work and women's labor force participation (Bose and Das, 2014). Public works programs– such as National Rural Employment Guarantee Scheme– have a positive impact on the female labor force participation (Azam, 2012). Increased flexibility in the labor market, investment in infrastructure, and social spending lead to more jobs and increase the demand for women's work (Das et al., 2015).

In addition to these structural and economic forces, regulatory frameworks also play a crucial role in shaping labor demand, particularly for women. Montag (2011) uses a labor law flexibility index –developed by Besley and Burgess (2004)– to find that a one standard deviation increase in the labor regulation measure decreases the probability of a woman being economically active by 3% to 4%—the implied decrease in female labor force is between 15% and 18%. Existing studies primarily focus on how manufacturing-sector labor laws influence women's employment. Chakraborty and Mahajan (2023) study the effect of an increase in the firm size thresholds for the applicability of regulatory compliances under the Factories Act and the Industrial Disputes Act. The authors find that the share of female workers increases by 4.2% in the treated states, along with

Though this subsistence work (meaning production of own goods for self-consumption), including the collection of free goods such as fodder and fuel wood, is covered under the NSSO (2000) survey, it is frequently missed out when it is reported as "household work" (meaning non-SNA work). The biases that encourage women to underreport their SNA work or investigators to under-record SNA work are still present in the TUS; however, the extent of biases is likely to be minimal because activities are classified only after the survey is conducted.

a 5% and 15% rise in employment and output. The Factories Act contains multiple provisions that affect a firm's decision to employ women. For instance, Section 27 of the Factories Act prohibits women from working near cotton openers, Section 34 prescribes different maximum weights that can be lifted by men and women, Section 87 prohibits the employment of women in operations considered dangerous. Chakraborty and Mahajan (2023) capture the combined effects of all these different provisions associated with women in the Factories Act.

We study legal permissions given by some Indian states for women to work night shifts in service sector jobs. These permissions were administered through state amendments in the Shops and Establishments Acts. We use a natural experiment where some Indian states amended their respective Shops and Establishments Acts at different periods to permit women to be employed during the night shift.

The impact of night shift regulations on women's employment outcomes remains ambiguous. These regulations, which restrict women's employment during late hours, are implemented to improve workplace safety and reduce women's exposure to crime during 'riskier periods of the day'. However, they may also have unintended economic consequences. If employers perceive compliance as costly, they may adjust their hiring preferences or wages, potentially affecting women's labor market opportunities. Conversely, if women view these regulations as enhancing workplace safety, they may be more willing to supply labor during night shifts. This paper examines the effects of night shift regulations on women's employment outcomes, contributing to the broader debate on whether protective legislation expands or limits women's economic opportunities.

Several studies document the unintended consequences of protective legislations. Legal prohibitions on women for working night shifts in Taiwan were associated with a one percentage point decline in women's likelihood of being employed and 6.1% decline in their working hours (Zveglich and Rodgers, 2003). Job restrictions, including inaccessibility to certain jobs, in the Russian Federation explain 31% of the gender differential in wages (Ogloblin, 2005). Reforming a Japanese law that restricted women's overtime work to only 2 hours a day led to a 3.6 percentage points increase in the probability of women finding employment (Kato and Kodama, 2014). The authors believe that the effect highlights that removing overtime limitations on women's work would allow employers to adjust women's labor input in response to fluctuations in demand for output. Legal impediments to women's economic activities, like being unable to pursue a profession freely, are associated with 1.3% larger gaps in labor force participation for women (Gonzales et al., 2015). Laws limiting the working hours of women in the United States– such as laws mandating daily and weekly maximum hours of work– reduced their hours of work and the employment of immigrant women by 30% in most restrictive states (Landes, 1980). After the repeal of gender-specific weekly hour limits in U.S. industries, the probability of women exiting regulated industries fell by 1.9 percentage points (Fishback

et al., 2025).

In contrast, studies also highlight that these regulations may encourage women to find employment. Haddad and Kattan (2024) document an increase of 8% in the likelihood of being employed in U.S. states that passed night-work regulations in the nineteenth century. The authors believe that this increase is driven by a rise in female labor supply as the implementation of night work regulations made jobs more socially acceptable. On comparing the differential effects of the regulation on men and women, Goldin (1988) finds that both men and women's scheduled work hours declined by about 1.5 per week. Goldin (1988), therefore, suggests that maximum-hours laws may be a consequence of the workers demanding shorter working hours. The author reinterpreted the findings of Landes (1980) and argued that labor may have sought to build a coalition to reduce working hours through the legislation.

Regardless of their effect, most countries have reformed legal restrictions on women's work shifts. India remains one of the few countries in the world where legal restrictions on women's night-shift employment persist. The consequences of retaining such restrictions may be particularly serious for developing countries because they rely heavily on women's labor (Nataraj et al., 1998). India may be underusing the advantages of women's labor.

The novelty of our work is that we focus on the effect of service-sector labor regulations on the outcomes of women. Prior research has largely examined labor regulations in India's industrial and manufacturing sectors. To the best of our knowledge, the relationship between the employment of women and laws regulating employment in the service sector are not studied. This gap is significant, as the services sector employs 30% of India's workforce. (GoI, 2025).

We believe that night-shift regulations can affect the employment outcomes of women in important ways. Gupta (2021) capture one such mechanism. The authors find that after the 1991 trade liberalisation, establishments exposed to larger output tariff reductions reduced the share of female workers. An establishment experiencing a 10 percentage point reduction in output tariffs would reduce the female share of their workforce by 7 percent. Increased tariff reductions increased the number of shifts per worker–average tariff decline of 115 percentage points led to a 7 percent increase in shifts per worker. The authors believe that night-shift regulations for women would cap the number of shifts they can work, so employers may prefer to hire male workers whose shifts are not limited by law.

Gupta et al. (2025) analyze firm-level data from the Annual Survey of Industries to identify the effect of lifting restrictions on women's night-shift employment. They find that the reform led to a 13% increase in the number of female workers and a 3.5% increase in the share of female workers in large firms. They also find that the likelihood that a large firm would employ any women increased by 2.6 percentage points.

Our analysis differs from Gupta et al. (2025) in two ways. First, we focus on individual

outcomes, such as the sectoral composition of women's employment and the likelihood that a woman is employed in the services sector. By contrast, Gupta et al. (2025) study firm outcomes, such as the share and number of female employees. While their evidence shows how manufacturing firms respond to a change in the Factories Act (applicable to manufacturing firms), we show that similar reforms in the service sector, the Shops and Establishments Act, shifted women from manufacturing into services. Together, the two studies underscore that night-shift regulations shape women's employment outcomes across both manufacturing and services.

Using household survey data, we examine whether women in states that enacted the amendment are more likely to secure employment. We find that state amendments in the Shops and Establishments Acts did not alter the employment and unemployment levels for women, but influenced their employment composition. The amendment that allowed the employment of women in night shifts led to a 3.45 percentage point increase in the likelihood of women finding employment in the services sector and a 3.37 percentage point decline in the probability of women being employed in the manufacturing sector.

The amendment may have prompted a reallocation within the labor force, especially among women, who shifted from manufacturing employment into service sector roles. This was likely due to the relaxation of night work restrictions. The findings highlight how a gender-targeted legal reform can reshape employment structures more broadly, influencing women's employment outcomes.

The remainder of this paper is structured as follows. Section 2 provides a detailed review of the legal landscape governing women's night shift employment in India, highlighting key regulatory changes and their implications. Section 3 outlines the data sources and empirical strategy employed to estimate the effects of these legal reforms. Section 4 describes the empirical strategy used to estimate the effects of night shift regulations on women's employment outcomes. Section 5 presents the results, analyzing changes in women's labor force participation, employment patterns, sectoral shifts, and their employment contracts. We also identify heterogeneity of treatment effects based on period of exposure to treatment. Section 6 conducts robustness checks to validate the findings. Finally, Section 7 concludes by summarizing key insights, discussing policy implications, and suggesting avenues for future research.

2 Institutional Context

The Shops and Establishments Acts are enacted by Indian states to regulate the working hours and working conditions of establishments that provide trade, business or professional services. According to the Model Shops and Establishments Act, 2022, the definition of a shop is "any premises (to which the provisions of the Factories Act 1948 do not apply) where goods are sold, either by retail or wholesale or where services are rendered

to customers", and a commercial establishment is – "any premises (to which the provisions of the Factories Act 1948 do not apply) where any trade, business, manufacture, or any work in connection with, or incidental or ancillary thereto is carried on."

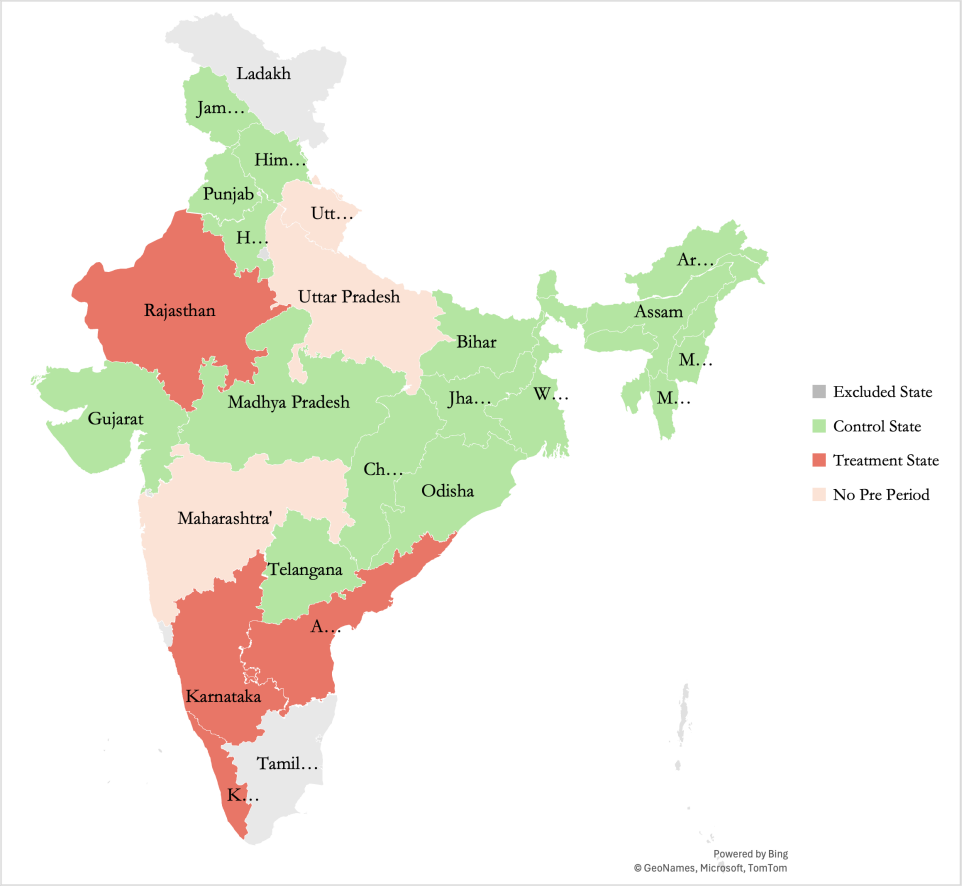


Figure 1: Treatment vs Control States based on Amendment of Shops and Establishments Act

Every Indian state has enacted its own Shops and Establishments Act and subsidiary regulations (rules) under the Act. Nine of these state laws forbid the employers regulated under the Act to "require or allow" women to work beyond a specific time (for instance 7 PM) in the evening. These are enacted in Madhya Pradesh, Assam, Tripura, West Bengal, Meghalaya, Odisha, Manipur, Nagaland, and Sikkim. Three states– Bihar, Chhattisgarh and Gujarat – prohibit the night-shift employment, but can make exceptions if the employer takes an inspector's permission. Five states –Haryana, Himachal Pradesh, Punjab, Jharkhand, and Telangana – only exempt certain kinds of enterprises like cinemas and theatres, information and technology firms or hospitals from women's night shift employment restrictions.

Seven Indian states allow employment of women if they comply with some conditions– Maharashtra, Uttarakhand, Kerala, Andhra Pradesh, Uttar Pradesh, Karnataka, and Rajasthan. We consider these states– Maharashtra, Uttarakhand, Kerala, Andhra Pradesh, Uttar Pradesh, Karnataka, and Rajasthan– as the states that introduced the treatment. Two states – Goa and Tamil Nadu – never had any night-shift employment restrictions

for women. We consider– Goa and Tamil Nadu– as already treated states.

3 Theoretical Framework

We consider an economy where firms operate in two sectors — Non-services (NS) and services (S). All the women in the model are assumed to be in the labor force and actively seeking work. Hiring a worker entails paying a wage w^f and, in some cases, incurring a compliance cost χ_S^f

A legal restriction that previously prohibited firms in the service sector from employing women beyond 7pm. This night-shift prohibition would impose a constraint on women's employment. The prohibition was relaxed in certain states through a policy reform, effectively removing the employment constraint. This reform would allow firms to respond to product demand fluctuations by employing women in evening shifts, if so needed Gupta (2021). However, the reform permits employment in the night shifts, conditional on the provision of specific safeguards (e.g., transport, leave). The requirement of these safeguards would also impose a compliance cost on the regulated firms. The effect of the reform would be ambiguous because:

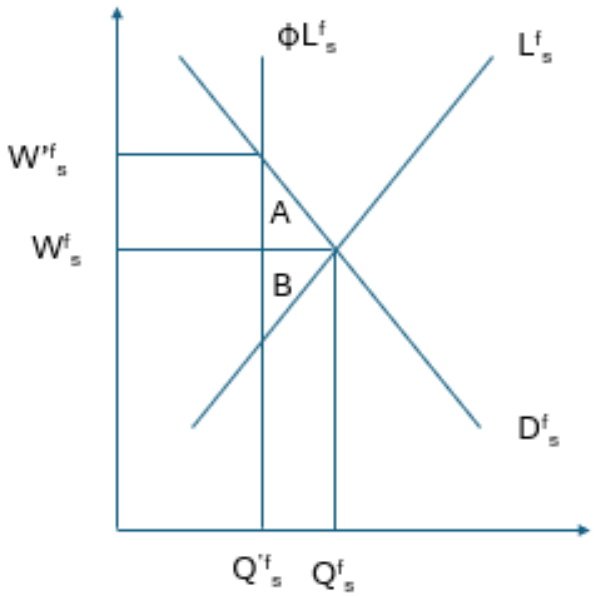


Figure 2: Theoretical Framework: Pre-TreatmentEquilibrium

- Before the reform (refer Figure 2), night-shift prohibition acts as a quantitative restriction: firms can only partially utilize the labor of women due to legal prohibitions on night work. We model this using a factor $\phi \in (0, 1)$, such that only a

fraction ϕ of the hired female labor can be effectively deployed. In the absence of the restriction, demand and supply curve would intersect at Q_s^f.

$$D_S^f(w^f) = a_S^f - b_S^f w^f = \phi \cdot L_S^f \quad \text{(Pre-reform)} \tag{1}$$

In the absence of the restriction, demand and supply would intersect at (Q_s^f, W_s^f), generating mutually beneficial employment opportunities. The areas A and B represent the gains from trade that are lost under the ban. Area A corresponds to the surplus forgone by firms, while area B represents the surplus forgone by workers. Together, these areas capture the deadweight loss created by the restriction.

- After the reform, the quantitative restriction is removed and replaced by a **compliance cost** χ_S^f incurred per female worker:

$$D_S^f(w^f) = a_S^f - b_S^f w^f - \chi_S^f = L_S^f \quad \text{(Post-reform)} \tag{2}$$

The net effect of the reform depends on the relationship between the removed constraint and the new compliance cost:

1. The compliance cost fully offsets the relaxation of the restriction. In this case, we expect no increase in women's employment in services.

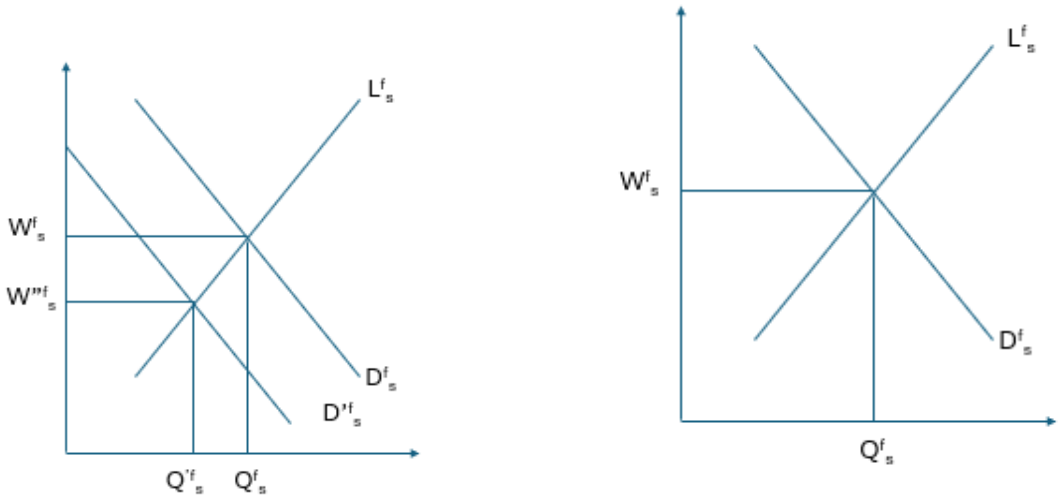
$$a_S^f < b_S^f w^f + \chi_S^f \tag{3}$$

2. The gain from relaxing the constraint dominates the cost. In this case, we expect a rise in women's employment in services.

$$a_S^f \geq b_S^f w^f + \chi_S^f \tag{4}$$

The removal of the hard constraint enables firms to more fully utilize the female workers they hire, thereby increasing the effective labor input per worker. However, this greater flexibility comes at a cost: the reform introduces compliance requirements that may limit firms' willingness to hire. Whether employment increases or not depends on the relative magnitude of these two effects.

Panel (a): Removing the quantitative constraint induces movement along the female labor supply curve, but the added compliance cost shifts the effective labor demand left by an equal magnitude. The two forces exactly offset in equilibrium, so women's employment in services is unchanged (though wages may adjust depending on slopes).



(a) Case 1: Offset effects; no net change in employment

(b) Case 2: Constraint removal dominates; employment rises

Figure 3: Theoretical framework—post-reform equilibria in services under two compliance-cost regimes.

Panel (b): In the second case, the compliance cost is not large enough to shift the demand curve substantially. The removal of the constraint dominates, leading to an outward shift in effective labor demand. In this scenario, equilibrium employment in services increases and lead to the equilibrium point.

We test these predictions directly in our empirical strategy by estimating the impact of the reform on women's sectoral employment outcomes and verifying whether the overall quantity of female labor absorbed by the services sector increases post-reform. Specifically, we explore whether the structure of the reform — the removal of a binding non-price constraint and its replacement with a compliance cost — translates into observable reallocation patterns in the labor market.

4 Data

We use the Periodic Labor Force Survey (PLFS) of India for employment and labor force statistics. The Periodic Labor Force Survey is a household survey conducted to measure the dynamics in labor force participation and employment status. It covers all of the Indian Union except some villages in Andaman and Nicobar Islands. The Periodic Labor Force Survey was initiated in 2017, therefore, our dataset spans over 5 years from June 2017 to June 2022.

The Periodic Labor Force Survey provides information on an individual's usual activity status, which refers to their activity during the 365 days prior to the survey date. This study focuses on the principal employment status of the female labor force, determined by the activity on which an individual spent the majority of their time during the same 365-day reference period. Additionally, data on working hours is available based on the

Current Weekly Status (CWS), which uses a reference period of 7 days before the survey date. An individual is classified as working (or employed) if they either worked for at least one hour on any day within those 7 days or had work available but did not perform it during that period.

The years of amendment of different State Shops and Establishments Acts are 2017, 2018 and 2020 (Anand and Kaur, 2022). The Gazette notifications of amendments to State Shops and Establishments Acts are available in Appendix 1. The amendments to the Shops and Establishments Act for Maharashtra, Uttar Pradesh, and Uttarakhand took place in 2017. PLFS didn't have pre-period data for these treatment states. As recommended by Callaway and Sant'Anna (2021), we therefore excluded these states from the analysis.

Amendment Year	Number of States	State Names
July 2017- June 2018	3	Uttarakhand, Maharashtra, Uttar Pradesh
July 2018 - June 2019	2	Kerala and Andhra Pradesh
July 2019 - June 2020	1	Rajasthan
July 2020 - June 2021	1	Karnataka

Table 1: Year of Amendment in the Shops and Establishments Acts of the Treatment States

Our dataset included 585,798 females, out of which 142,117 were working-age women participating in the labor force. Our sample consists of all working-age (willing to work) women across 5 years and 24 states. We estimate the likelihood that women who are willing and able to work find employment based on their principal employment status.

Employment Category	**Probability**
Unemployed	08%
Employed	92%
Non-Service Sectors	60%
Self-Employed	43%
Wage-Employed	17%
Service Sector	32%
Self-Employed	9%
Wage-Employed	23%

Table 2: Probability of a woman in the Labor Force finding a specific type of Employment

Out of the total number of women in the labor force (excluding the women employed in the manufacturing sector), nearly 8% women are unemployed. PLFS defines unemployed individuals as– "Persons, who, during the reference period, owing to lack of work, had not worked but either sought work through employment exchanges, intermediaries, friends or relatives or by making applications to prospective employers or expressed their willingness

or availability for work". The remaining 92% of women were either employed in the agriculture sector, the manufacturing sector, or the services sector (Refer table 2).

Within the sectoral employment, women could be self-employed or wage employed. PLFS defines self-employed individuals as –"persons engaged independently in a profession or trade on own-account or with one or a few partners". Self employed women could further be categorised into (i) Own Account Workers– who operate their enterprises on their own account or with one or a few partners and who during the reference period; (ii) Employers– who run their enterprise by hiring labour; and (iii) Helpers– who do not run the household enterprise on their own but assist the related person living in the same household in running the household enterprise.

PLFS also categorizes workers employed in a wage-employment into– (i) Casual workers – "getting in return wage according to the terms of the daily or periodic work contract is a casual wage labor"; and (ii) Salaried workers –"Persons working in return of a salary or wages on a regular basis and not on the basis of daily or periodic renewal of work contract".

5 Empirical Strategy

5.1 Baseline Identification Strategy

In order to identify the causal effect of female-specific labor regulation on women's employment, it is necessary to address various sources of endogeneity. States that have implemented female-specific labor regulations may exhibit time-invariant characteristics that lead to different levels of female employment compared to the states that have not. For instance, states adopting these regulations might have a more favorable legal environment for women overall. We observe a statistically significant difference between the average monthly expenditure on household consumption in the treatment and control states. This difference may indicate that the treatment states are more prosperous than the control states and, therefore, may house a greater demand for women's work.

Trends in the adoption of gender-specific labor regulations may have changed over time. Time-varying factors could simultaneously affect both the adoption of gender-specific labor regulations and labor market outcomes. For instance, during the COVID-19-induced lockdown in 2020, many workers returned to their home states, creating acute labor shortages for firms. In response to this constraint, firms may have sought to expand their available labor pool by hiring more women. To facilitate this, firms might have advocated for regulatory changes, such as the removal of night shift restrictions on women. If we fail to account for such time-varying shocks and responses, we risk attributing the effects of the lockdown to changes in night shift regulation, thereby biasing our estimates.

To deal with these sources of endogeneity, we include state and year-fixed effects

in our identification strategy. Given that individual-level characteristics can influence employment decisions, we conduct our analysis at the individual level, which allows us to control for exogenous characteristics of individuals. Given that our treatment is the amendment of the Shops Act, we compare the pretreatment and posttreatment differences in outcomes for a treated group (women in states that introduce an amendment in their night-shift provisions) and a control group (women in states that do not modify their night-shift provisions).

To estimate the average treatment effects of female-specific labor regulations, we rely on the following generalized difference-in-differences specification:

$$\mathrm{Y}_{ist} = \alpha + \lambda_t + \gamma_s + \beta \mathrm{Amendment}_{st} + \delta \mathrm{X}'_{ist} + \rho \mathrm{Z'}_{st} + \epsilon_{ist} \tag{5}$$

Y_{ist} is the probability that a female i is employed in a state s at time t. The main independent variable of interest Amendment is a state-time varying indicator variable that equals one for all state-years when the night-shift restriction was inactive. It assigns a value of zero to states that never adopted the labor law in question and to those that did so, but only for the years preceding its passage. This binary indicator allows us to classify states into treated and control states, as well as time-period into pre- and post-treatment for treated states. The terms γ_s and λ_t are state and time fixed effects respectively; X_{ist} is a list of individual level controls including age, marital status, and highest level of education attained, and their monthly household consumption expenditure. Z_{ist} is a list of state-time level controls including the per capita net state domestic products and the service sector gross value added of the included states. Finally, ϵ_{ist} is our error term.

Given that the variation in treatment occurs at the state level, we cluster standard errors at the level of the state (Bertrand et al., 2004). β measures the average treatment effect of the amendment to the Shops Act on the probability of a working-age woman in the labor force finding employment in states that introduced the amendment.

5.2 Dynamic Effects/ Event Study Design

We estimate the total number of workers and average working hours. We then estimate the Two Way Fixed Effects Difference in Differences (DiD) estimates to understand the effect of the amendment on outcomes for women in treatment states as compared to the states that did not introduce the amendment. A growing methods literature points out potential problems with two-way fixed effects (TWFE) DiD estimators when treatment is staggered in adoption and varies with time. In cases of staggered treatment timing, the coefficient β could be afflicted by bias because the coefficient is estimated using a weighted average of regressions that include the regressions where previously treated units are a control group for units that are treated later (Late Vs Early Regressions).

Such regressions are considered bad for they do not comply with the parallel trends assumption. To address these problems, we decompose the more complex TWFE DiD into simpler 2 x 2 sub-experiments. We identify the weight attached to the bad (Late Vs Early Regressions). Finally, we conduct a balanced event study aggregation to understand the dynamic effects of the treatment on states.

Our assumptions of exogeneity would fail if, in the absence of the amendment of the Shops Act, women's employment in the treatment and control states followed a different trend (potential violation of the parallel trends assumption). Proving the parallel trends assumption in a staggered treatment set-up is relatively challenging. Each group has different pre-treatment periods, making it harder to verify that all groups would have followed similar trends without treatment. Some groups may have different pre-treatment trends simply because they are treated later, leading to incorrect conclusions. Therefore, the literature recommends using the event study design to test for pre-trends.

We consider the dynamic impacts of the adoption of the three female-specific labor regulations on women's employment using the following event study specification:

$$Y_{ist} = \delta_i + \lambda_t + \gamma_s + \sum_{\tau=-3}^{\tau=3} \beta_\tau \text{Amendment}_{st}^{\tau} + \epsilon_{ist} \tag{6}$$

The main variable of interest, $\text{Amendment}_{s(t)}^{\tau}$, is an indicator variable that takes a value of one for states that amend the labor reforms, τ periods from the amendment, and zero otherwise. The year of the amendment is denoted as event time 0. This variable measures the total duration (in years) since the state implemented the specific labor law by the time the year t arrives. Our parameter of interest is β.

The event-study graphs from this methodology confirm our parallel trends assumption. Specifically, we test whether $\beta_\tau > 0$ for years $\tau \geq 0$ differ from zero. If the amendments increase the proportion of female workers, then β_τ should be positive for periods after the amendment.

The parallel trends assumption requires that, in the absence of treatment, treated and control groups would have followed the same trend. In graphical terms, this means that the pre-treatment coefficients (coefficient for years -2 and -1) should be statistically indistinguishable from zero. According to the graph, the near-zero, statistically insignificant coefficients in the pre-treatment periods indicate that there were no diverging trends between treated and control states before the policy change. The point estimates begin to increase and become more positive with time. Although confidence intervals are still wide and some overlap with zero, the upward trend suggests a potential positive impact of the amendment on women's salaried employment. This change after the amendment, contrasted with flat pre-treatment trends, supports the identification strategy.

6 Results

6.1 Labor Force Participation

Haddad and Kattan (2024) document that in the nineteenth-century United States, the introduction of night-shift regulations actually increased women's labor force participation. They argue that such laws were perceived as welfare-enhancing, making formal work more socially acceptable and encouraging more women to join the workforce. Motivated by this historical evidence, we begin by asking whether the removal of night-shift restrictions in India had a similar effect on women's decision to participate in the labor force.

Table 3 reports the estimates of Amendment_{st} on female labor force participation. Each column represents a different specification of fixed effects to test the robustness of results to alternate controls for unobserved heterogeneity. The first column controls for state and year fixed effects, the second column controls for state fixed effects and month-year fixed effects, the third column controls for year fixed effects and district-state fixed effects, and the fourth column controls for month-year as well as district-state fixed effects.

Table 3: TWFE of the Amendment on the Likelihood of Women being in Labor Force

	Probability of a Woman Participating in Labor Force					
	(1)	(2)	(3)	(4)	(5)	(6)
$Amendment_{st}$	-0.0019	-0.0039	-0.0040	-0.0061	-0.0119	-0.0130
	(0.0154)	(0.0164)	(0.0144)	(0.0156)	(0.0169)	(0.0175)
Dep Variable Mean	0.24	0.24	0.24	0.24	0.24	0.24
R Squared	0.0599	0.0604	0.0886	0.0890	0.888	0.1697
Observations	585,798	585,798	585,798	585,798	562,161,	562,161
State Fixed Effects	Yes	Yes	–	–	–	–
Year Fixed Effects	Yes	–	Yes	–	–	–
Month-Year Fixed Effects	No	Yes	No	Yes	Yes	Yes
District-State Fixed Effects	No	No	Yes	Yes	Yes	Yes
State-time Controls	No	No	No	No	Yes	Yes
Individual Controls	No	No	No	No	No	Yes

Notes: This table presents the Two-Way Fixed Effects (TWFE) estimates of the effect of the amendment on the probability of women participating in the labor force. The dependent variable is a binary indicator for labor force participation.The independent variable, $Amendment_{st}$, is a state-level policy change indicator that takes the value of 1 if the amendment was implemented in state s at time t, and 0 otherwise. Fixed effects specifications vary across columns. T-values are reported in parentheses. Standard errors are clustered at the state level. Statistical significance is denoted as *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Across all specifications, the coefficients are small in magnitude, ranging from –0.19

to –1.30 percentage points, and none are statistically significant. Given that the average labor force participation rate of women in the sample is 24%, these results indicate that the reform had no discernible effect on whether women entered or exited the labor force.

6.2 Employment

Having established that the law did not significantly alter women's decision to participate in the labor market, we next restrict attention to those women who are in the labor force and study whether the amendment affected their likelihood of being employed

Table 4 presents estimates from Two-Way Fixed Effects (TWFE) regressions that examine the impact of the amendment removing night-shift restrictions on the likelihood of employment of a working-age woman who is willing to participate in the economy.

Table 4: Estimated TWFE of the Amendment on the Likelihood of being Employed

	Probability of a Woman being					
	Employed					
	(1)	(2)	(3)	(4)	(5)	(6)
$Amendment_{st}$	-0.00622	-0.00736	-0.00647	-0.00795	-0.00825	-0.00266
	(0.00930)	(0.00949)	(0.00979)	(0.00982)	(0.00989)	(0.00861)
Adj. R^2	0.0546	0.0551	0.0763	0.0766	0.0766	0.259
Dep Variable Mean	0.920	0.920	0.920	0.920	0.920	0.920
Obs	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117
State Fixed Effects	Yes	Yes	–	–	–	–
Year Fixed Effects	Yes	–	Yes	–	–	–
Month-Year Fixed Effects	No	Yes	No	Yes	Yes	Yes
District-State Fixed Effects	No	No	Yes	Yes	Yes	Yes
State-time Controls	No	No	No	No	Yes	Yes
Individual Controls	No	No	No	No	No	Yes

Notes: This table presents the Two-Way Fixed Effects (TWFE) estimates of the effect of the amendment on the probability of women participating in the labor force. The dependent variable is a binary indicator for labor force participation.The independent variable, $Amendment_{st}$, is a state-level policy change indicator that takes the value of 1 if the amendment was implemented in state s at time t, and 0 otherwise. Fixed effects specifications vary across columns. T-values are reported in parentheses. Standard errors are clustered at the state level. Statistical significance is denoted as *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

In the employment panel (Columns 1–6), the coefficient varies between -0.06 percentage points and -0.03 percentage points across different specifications. The estimates are consistently small in magnitude and statistically insignificant, indicating that the amendment had no discernible impact on the likelihood of a woman being employed. The fact that the coefficient remains close to zero across all models suggests robustness of the null result, regardless of the specific fixed effects included.

6.3 Sectoral Employment

While overall employment probabilities remained unchanged, the sectoral panels reveal significant compositional shifts in women's employment across industries. On average, approximately 31.7% of women are employed in the service sector. The amendment is associated with a significant increase in the likelihood of women being employed in services, ranging between 2.30 percentage points and 3.39 percentage points. This translates to a 7.26% to 10.70% increase in the probability of a working-age woman being employed in the service sector in states that amended their Shops and Establishments Act.

In contrast, the coefficients on Non-Service employment are negative and statistically significant. On average, about 60.3% of women are not employed in service sector. The amendment is associated with a decline in the likelihood of women being employed in non - service sector, ranging between 2.92 percentage points and 3.30 percentage points. This corresponds to a 4.84% to 5.47% decline in the likelihood that a working-age woman is employed in non-service sectors such as manufacturing or agriculture, in states that implemented the reform.

Table 5: Estimated TWFE of the Amendment on Sectoral Employment

	Likelihood of a Woman Being Employed in											
	Non- Service						Service					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
$Amendment_{st}$	-0.0292*	-0.0315*	-0.0337**	-0.0372**	-0.0396***	-0.0330**	0.0230*	0.0241	0.0272**	0.0293**	0.0313**	0.0303**
	(0.0141)	(0.0159)	(0.0135)	(0.0145)	(0.0141)	(0.0121)	(0.0127)	(0.0145)	(0.0102)	(0.0118)	(0.0120)	(0.0113)
Adj. R^2	0.0929	0.0944	0.184	0.185	0.185	0.242	0.0564	0.0575	0.134	0.134	0.134	0.148
Dep Variable Mean	0.603	0.603	0.603	0.603	0.603	0.603	0.317	0.317	0.317	0.317	0.317	0.317
Obs	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117
State Fixed Effects	Yes	Yes	–	–	–	–	Yes	Yes	–	–	–	–
Year Fixed Effects	Yes	–	Yes	–	–	–	Yes	–	Yes	–	–	–
Month-Year Fixed Effects	No	Yes	No	Yes	Yes	Yes	No	Yes	No	Yes	Yes	Yes
District-State FE	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
State-time Controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Individual Controls	No	No	No	No	No	Yes	No	No	No	No	No	Yes

Notes: This table presents the Two-Way Fixed Effects (TWFE) estimates of the effect of the amendment on the probability of women participating in the labor force. The dependent variable is a binary indicator for labor force participation.The independent variable, $Amendment_{st}$, is a state-level policy change indicator that takes the value of 1 if the amendment was implemented in state s at time t, and 0 otherwise. Fixed effects specifications vary across columns. T-values are reported in parentheses. Standard errors are clustered at the state level. Statistical significance is denoted as *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Taken together, the results indicate that the removal of night-work restrictions did not expand women's aggregate employment, but instead reallocated female labor into the service sector.

6.4 Type of Employment

To complement the sectoral shifts shown in Table 6, Tables 7 and 8 examine the type of employment within Non-Services and Services, respectively.

In services, women experience gains primarily in wage employment. Out of the 31.7% participating women employed in the services sector, 22.8% of women are wage-employed and 8.9% are self-employed. The amendment is associated with a significant increase

in wage employment ranging between 2.9 and 3.4 percentage points, corresponding to a 13–15% increase in the probability of being wage-employed in services. The coefficients for self-employment in services are small and statistically insignificant, suggesting that there is no meaningful change in women's likelihood of being self-employed in the service sector.

Table 6: Estimated TWFE of Amendment on Type of Service Employment

	Probability of a Woman being											
	Wage- Employed						Self-Employed					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
$Amendment_{st}$	0.0291**	0.0304**	0.0315***	0.0337**	0.0336**	0.0319***	-0.00609	-0.00627	-0.00428	-0.00440	-0.00227	-0.00156
	(0.0123)	(0.0137)	(0.0108)	(0.0122)	(0.0122)	(0.0112)	(0.00433)	(0.00452)	(0.00433)	(0.00436)	(0.00431)	(0.00411)
Adj. R^2	0.0362	0.0370	0.0994	0.0999	0.0999	0.115	0.0347	0.0351	0.0484	0.0488	0.0489	0.0563
Dep Variable Mean	0.228	0.228	0.228	0.228	0.228	0.228	0.0891	0.0891	0.0891	0.0891	0.0891	0.0891
Obs	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117
State Fixed Effects	Yes	Yes	–	–	–	–	Yes	Yes	–	–	–	–
Year Fixed Effects	Yes	–	Yes	–	–	–	Yes	–	Yes	–	–	–
Month-Year Fixed Effects	No	Yes	No	Yes	Yes	Yes	No	Yes	No	Yes	Yes	Yes
District-State Fixed Effects	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
State-time Controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Individual Controls	No	No	No	No	No	Yes	No	No	No	No	No	Yes

Notes: This table presents the Two-Way Fixed Effects (TWFE) estimates of the effect of the amendment on the probability of women participating in the labor force. The dependent variable is a binary indicator for labor force participation.The independent variable, $Amendment_{st}$, is a state-level policy change indicator that takes the value of 1 if the amendment was implemented in state s at time t, and 0 otherwise. Fixed effects specifications vary across columns. T-values are reported in parentheses. Standard errors are clustered at the state level. Statistical significance is denoted as *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Within the 60.3% women employed in the Non- Service, 17.4% of women are wage-employed and 42.9% are self-employed. For wage-employed women in Non- Service sectors, coefficients are small and insignificant, indicating no systematic effect. By contrast, for self-employment, the coefficients are consistently negative, pointing toward a decline of about 0.26 to 0.33 percentage points in the likelihood of women being self-employed in Non-Service sectors.

Overall, the evidence suggests that the amendment induced a sectoral reallocation of women's employment from non-service activities into the service sector, with the gains concentrated in formal wage employment. The absence of an aggregate increase in women's employment, together with the decline in self-employment within non-service, indicates that the reform primarily enabled a transition away from non-service self-employment toward salaried positions in services.

Table 7: Estimated TWFE of Amendment on Type of Non-Service Employment

	Probability of a Woman being											
	Wage- Employed						Self-Employed					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
$Amendment_{st}$	-0.00304	-0.00412	0.000280	-0.00102	-0.000246	0.0000518	-0.0262	-0.0274	-0.0340*	-0.0362*	-0.0393*	-0.0331*
	(0.00985)	(0.00987)	(0.00813)	(0.00814)	(0.00809)	(0.00803)	(0.0215)	(0.0230)	(0.0190)	(0.0199)	(0.0200)	(0.0184)
Adj. R^2	0.067	0.068	0.113	0.114	0.114	0.125	0.114	0.115	0.203	0.203	0.203	0.246
Dep Variable Mean	0.174	0.174	0.174	0.174	0.174	0.174	0.429	0.429	0.429	0.429	0.429	0.429
Obs	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117
State Fixed Effects	Yes	Yes	–	–	–	–	Yes	Yes	–	–	–	–
Year Fixed Effects	Yes	–	Yes	–	–	–	Yes	–	Yes	–	–	–
Month-Year Fixed Effects	No	Yes	No	Yes	Yes	Yes	No	Yes	No	Yes	Yes	Yes
District-State Fixed Effects	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
State-time Controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Individual Controls	No	No	No	No	No	Yes	No	No	No	No	No	Yes

Notes: This table presents the Two-Way Fixed Effects (TWFE) estimates of the effect of the amendment on the probability of women participating in the labor force. The dependent variable is a binary indicator for labor force participation.The independent variable, $Amendment_{st}$, is a state-level policy change indicator that takes the value of 1 if the amendment was implemented in state s at time t, and 0 otherwise. Fixed effects specifications vary across columns. T-values are reported in parentheses. Standard errors are clustered at the state level. Statistical significance is denoted as *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

6.5 Heterogeneous Effects

6.5.1 Exposure to Treatment

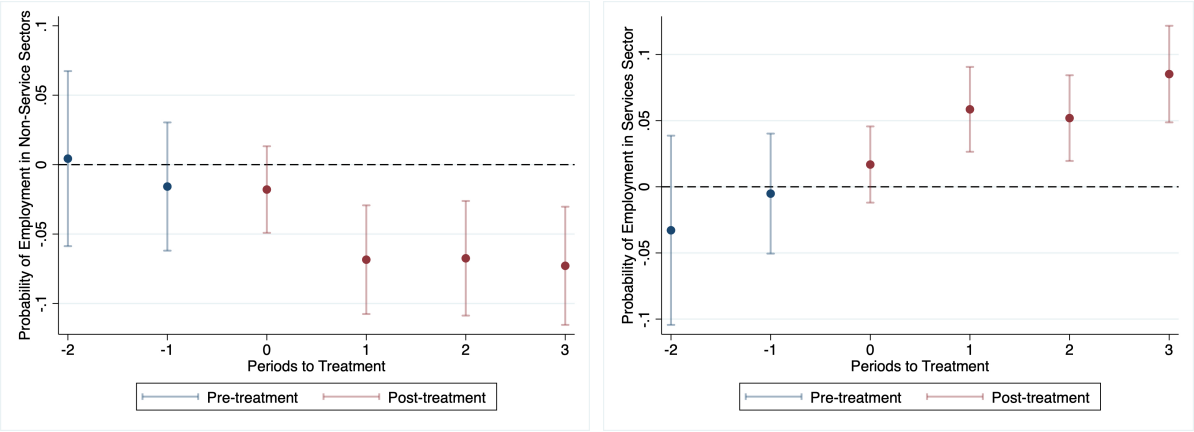
We use an event study design to examine how the effects of the amendment evolve with exposure to treatment. The estimates show no evidence of differential pre-trends. In the years preceding the amendment, the coefficients are small, statistically insignificant, and close to zero, suggesting that treated and control states followed parallel paths prior to reform. This lends credibility to our identification strategy.

The event study is estimated using the Callaway and Sant'Anna (2021) methodology, which corrects for the potential bias in two-way fixed effects estimators under staggered adoption. In particular, this estimator avoids comparisons where early-treated units serve as "controls" for later-treated units, which can otherwise bias estimates of dynamic treatment effects. As a result, the coefficients we present reflect treatment effects relative to the appropriate counterfactual and provide a more credible picture of how the reform reshaped women's employment trajectories over time.

Figure 2 plots the dynamic treatment effects of the amendment across sectors. **Panel (a): Non-Service Sector:** The pre-treatment coefficients are close to zero and statistically insignificant, indicating no differential pre-trends between treated and control states. After the amendment, the coefficients become consistently negative and statistically significant, implying a decline in women's probability of employment in non-service sectors. The magnitude grows over time, suggesting a persistent adjustment rather than a transitory dip.

Panel (b): Service Sector: Pre-treatment estimates also hover around zero, supporting the parallel-trends assumption. Post-treatment coefficients are positive and statistically significant, with the effect increasing in subsequent periods. This indicates a

Figure 4: Event Study Plot of Treatment Effect on Probability of being employed in



(a) Non Services Sector

(b) Services Sector

sustained rise in women's probability of employment in services following the reform.

Taken together, the policy appears to have reallocated female labor from non-service industries (e.g., manufacturing and agriculture) toward the service sector, rather than expanding overall women's employment.

6.5.2 District Safety

The impact of protective labor legislation depends critically on context. Haddad and Kattan (2024) show that in nineteenth-century America, the introduction of night-work regulations increased women's labor force participation because such laws were perceived as welfare-enhancing, making formal employment more socially acceptable. In contrast, our evidence from contemporary India suggests that the elimination of night-work restrictions does not uniformly expand women's employment opportunities. Instead, the effects are mediated by the local security environment.

Table 8 examines whether the effects of the amendment vary across districts with different levels of crimes against women, using quartiles of 2015 crime rates as a proxy for baseline safety. The results indicate that the impact of the reform is in fact heterogeneous and is mediated by the local security environment.

In districts with the lowest crime rates (Quartile 1), the amendment is associated with a significant decline of 5.1 percentage points in women's likelihood of being employed in non-services sectors, and a corresponding 4.2 percentage point increase in the probability of being employed in services. This suggests a strong reallocation of female labor into services where women perceive the reform as opening up new, safer opportunities.

In Quartile 2 districts, which also represent relatively safer environments, we again observe a significant negative effect on non-services employment and a positive effect on services. These patterns closely mirror the aggregate results and confirm that women are more likely to shift into services when crime levels are moderate to low.

Table 8: Heterogeneous Treatment Effects by District-Level Crimes Against Women (2015) Quartiles

	Probability of finding Employment based on Level of Crime in their District							
	Crime Quartile 1		Crime Quartile 2		Crime Quartile 3		Crime Quartile 4	
	Non-Services	Services	Non-Services	Services	Non-Services	Services	Non-Services	Services
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
$Amendment_{st}$	-0.0505***	0.0423***	-0.0279*	0.0270*	-0.0107	0.0265	-0.0393	0.0324
	(0 .0158)	(0.0136)	(0.0155)	(0.0143)	(0.0155)	(0.0217)	(0.0193)	(0.0256)
Dep Variable Mean	0.54	0.37	0.67	0.26	0.61	0.29	0.58	0.35
R Squared	0.1806	0.1508	0.1991	0.1351	0.1827	0.0879	0.1819	0.1517
Observations	33,398	33,398	32,667	32,667	33,130	33,130	32,864	32,864
Month-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

By contrast, in districts with higher crime rates (Quartiles 3 and 4), the coefficients are small in magnitude and statistically insignificant. This indicates that in areas where safety concerns are more pronounced, women do not take advantage of the reform to the same extent, and the amendment does not generate significant reallocation effects.

Taken together, these results deepen our main finding: the amendment did not expand aggregate female employment but facilitated sectoral reallocation into services. Importantly, the heterogeneous effects highlight that women's willingness to shift into service-sector night work depends critically on the local crime environment. Women appear to respond positively to legal reforms only where they can reasonably expect workplace and commuting safety, suggesting that institutional reforms alone may be insufficient without parallel improvements in public safety and enforcement.

7 Robustness Checks

We implement robustness checks for our results on the impact of the removal of night shift bans on the sectoral shifts in women's employment.

7.1 Bootstrapped Standard Errors

We bootstrap standard errors at the state level due to the relatively small number of clusters (24 states). Cross-sectional inference relies on a foundational assumption that the data are independent. Each observation is treated as a random draw from the same population, uncorrelated with the observation before or after it.

An important form of dependency arises in data with a group structure. Observations in the same group may be exposed to common shocks, unobserved factors, or policy treatments, which induce correlation in their error terms. These dependencies are termed the clustering problem because the observations within a group may not be independent. Cluster-robust standard errors allow arbitrary correlation of residuals within groups, while still assuming independence across groups. The effective sample size becomes the number

of groups, not the number of individual observations.

If the number of clusters is small, the true sampling variability may be underestimated. This is because with few clusters, the sample covariance of residuals within clusters is a noisy estimate of the population covariance. The variance estimator underestimates the true variability, and small standard errors cause false positives.

Table 9: Estimated TWFE of the Amendment on Sectoral Employment

	Likelihood of a Woman Being Employed in											
	Non-Service						Service					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
$Amendment_{st}$	-0.0292**	-0.0315*	-0.0337**	-0.0372**	-0.0396**	-0.0330**	0.0230	0.0241	0.0272**	0.0293**	0.0313**	0.0303**
	(0.0141)	(0.0159)	(0.0135)	(0.0145)	(0.0141)	(0.0121)	(0.0127)	(0.0145)	(0.0102)	(0.0118)	(0.0120)	(0.0113)
Adj. R^2	0.0929	0.0944	0.184	0.185	0.185	0.242	0.0564	0.0575	0.134	0.134	0.134	0.148
Dep Variable Mean	0.603	0.603	0.603	0.603	0.603	0.603	0.317	0.317	0.317	0.317	0.317	0.317
Obs	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117	1,42,117
State Fixed Effects	Yes	Yes	–	–	–	–	Yes	Yes	–	–	–	–
Year Fixed Effects	Yes	–	Yes	–	–	–	Yes	–	Yes	–	–	–
Month-Year Fixed Effects	No	Yes	No	Yes	Yes	Yes	No	Yes	No	Yes	Yes	Yes
District-State FE	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
State-time Controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Individual Controls	No	No	No	No	No	Yes	No	No	No	No	No	Yes

Notes: This table presents the Two-Way Fixed Effects (TWFE) estimates of the effect of the amendment on the probability of women participating in the labor force. The dependent variable is a binary indicator for labor force participation. The independent variable, $Amendment_{st}$, is a state-level policy change indicator that takes the value of 1 if the amendment was implemented in state s at time t, and 0 otherwise. Fixed effects specifications vary across columns. T-values are reported in parentheses. Standard errors are clustered at the state level. Statistical significance is denoted as *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Bootstrap standard errors simulate the sampling distribution of the estimator by re-sampling the data many times and re-estimating the model. There are many ways to bootstrap standard errors. One of the ways, the parametric bootstrap, is to keep the values of the dependent variable fixed and draw from the distribution of residuals. This creates a new estimate of the dependent variable based on the predicted value. Wild-cluster bootstrap involves randomly flipping residuals with certain weights to simulate variability, rather than resampling the data. This maintains clustered dependence structure, avoids small sample bias, and gives valid inference with very few treated clusters.

In this section, we report regression results with cluster-robust standard errors in parentheses and p-values obtained from wild cluster bootstrap tests clustered at the state level. The bootstrap-based inference does not materially alter the findings. However, the level of statistical significance is occasionally attenuated. In some cases coefficients that were significant at the 1% level under conventional inference remain significant only at the 5% level, and a few coefficients that appeared significant at the 10% level lose significance once the wild cluster bootstrap is applied.

This pattern is consistent with the expectation that conventional cluster-robust methods can somewhat understate the true sampling variability when the number of clusters is modest. The substantive interpretation of the results is unchanged: the direction and magnitude of the estimated coefficients are stable, and the main inferences hold, albeit with slightly more conservative significance thresholds.

7.2 Assignment of Placebo treatments

As an additional robustness check, we first assign fake treatment dates to randomly selected control states. From the pool of states that never amended their Shops and Establishments Acts, we randomly draw a set of states equal in number to the actual treated states. Each of these placebo states is then assigned a treatment year drawn from the distribution of actual amendment years (2017–2020). We re-estimate our baseline regression under this placebo assignment and repeat the procedure many times to generate a distribution of placebo treatment effects.

In the second exercise, we retain the actual set of treated states but assign them placebo treatment dates that predate the true amendments by two to three years. This design serves as a pre-trend falsification test: if significant effects appear under these fake treatment dates, it would suggest that our estimated effects are contaminated by spurious pre-trends. Conversely, finding coefficients close to zero in these placebo regressions supports the validity of the parallel trends assumption underlying our identification.

The third exercise involves assigning the actual treatment dates of the reforming states to randomly selected control states. For instance, if Maharashtra amended its Act in 2017, a randomly chosen control state is "treated" in 2017. We then re-estimate the difference-in-differences specification under this assignment. If our results were merely capturing common time shocks rather than the reform itself, we would expect to observe significant effects even in these placebo regressions.

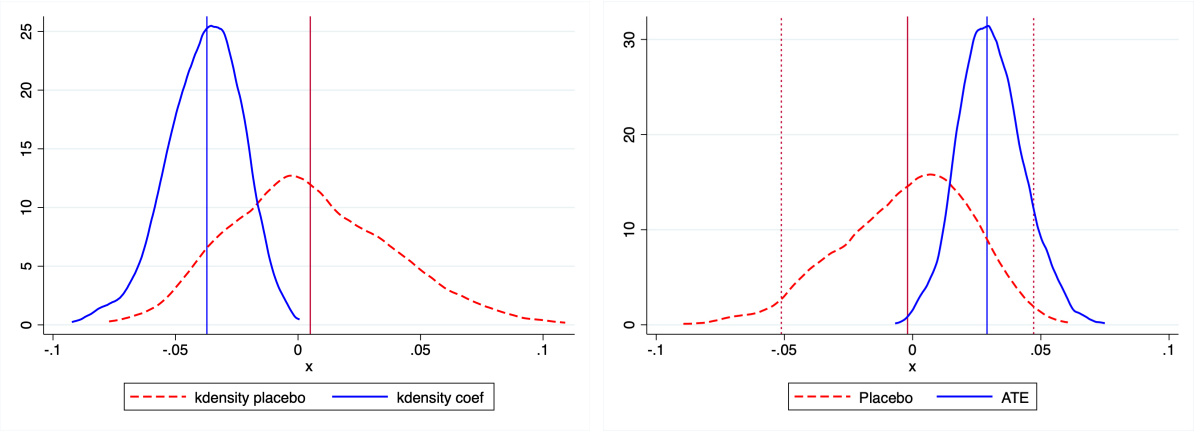
Taken together, these placebo exercises help assess whether our main results are driven by chance correlations or broader time shocks. The expectation is that the placebo effects will center around zero, while the actual treatment effect lies outside the distribution of placebo estimates. This strengthens our confidence that the estimated effects reflect the causal impact of the reform.

Figure 5 shows the distribution of placebo coefficients when fake treatment dates are randomly assigned to control states. The red vertical line is the mean placebo effect (close to zero), and the red dotted lines are the 95% confidence intervals from this placebo distribution. The actual treatment effects (represented by a blue line) for manufacturing (–0.0372) and services (0.0292) are fairly away from the placebo coefficients.

To plot figure 6, control states are assigned the real treatment dates of the reforming states. The placebo distributions concentrate tightly around zero. The placebo distributions are centered at zero, so the exercises still confirm that the "average fake effect" is not very different from zero, unlike the actual treatment effect.

In figure 7 the same treated states are used, but fake treatment years are assigned. In this design, the placebo estimates lie closest to the true effects. This is unsurprising: because the underlying assumptions of this placebo most closely resemble the actual empirical setting—holding fixed the treated units and their industrial structure, and only

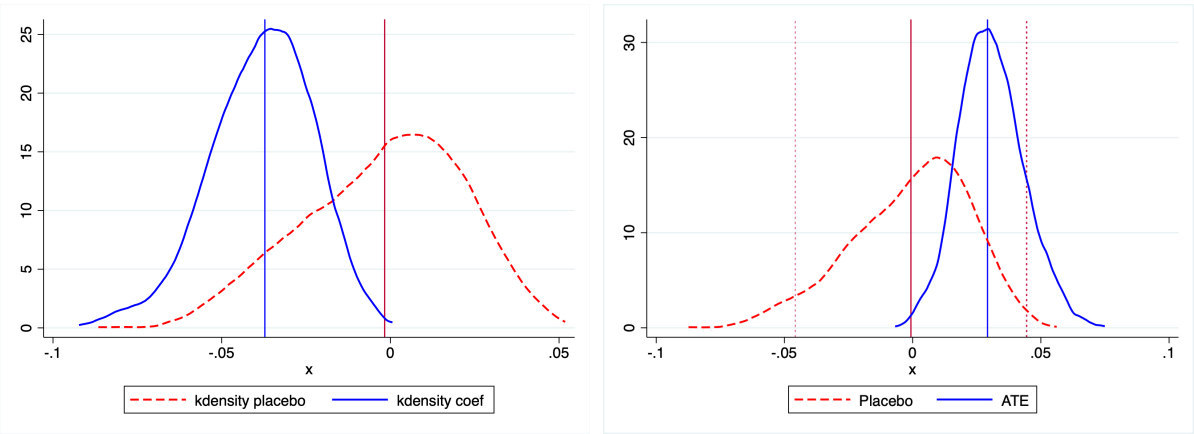
Figure 5: Distribution of Placebo (Dates and States) Treatments and Actual Treatment



(a) Non-Services Sectors

(b) Services Sectors

Figure 6: Distribution of Placebo (States) Treatments and Actual Treatment



(a) Non-Services Sectors

(b) Services Sectors

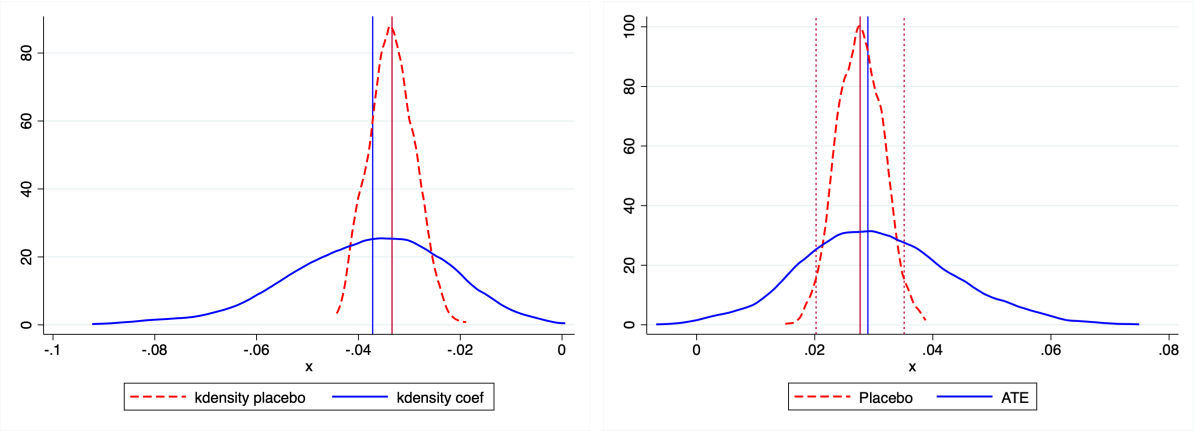
perturbing the timing of reform—the resulting distribution of placebo coefficients naturally converges toward the actual estimates. In manufacturing, the placebo distribution is shifted in the negative direction, while in services, the placebo distribution is shifted upward. This pattern underscores that the closer the placebo scenario is to the actual assignment mechanism, the closer the estimates approach the real results.

Across all placebo exercises, the estimated coefficients for manufacturing and services lie within the confidence intervals of the placebo distributions. This indicates that, while the placebo effects themselves are centered at zero, we cannot statistically rule out that the observed effects arise by chance.

7.2.1 Sun & Abraham

To further assess the robustness of our findings, we implement the interaction-weighted event-study estimator proposed by Sun and Abraham (2021). This method addresses the bias that arises in staggered difference-in-differences settings when treatment effects

Figure 7: Distribution of Placebo (Dates) Treatments and Actual Treatment

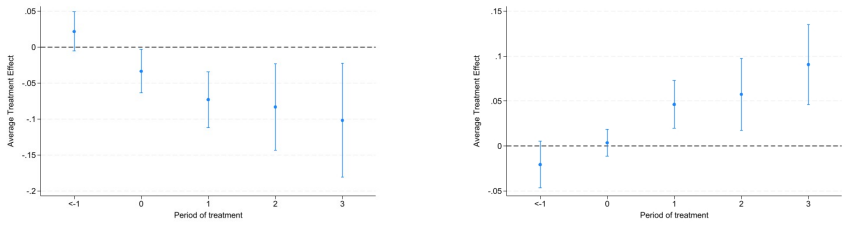


(a) Non-Services Sectors

(b) Services Sectors

are heterogeneous across cohorts. By plotting dynamic treatment effects relative to the year of reform, we are able to verify the parallel trends assumption and examine how the impact of the reform evolves over time across sectors.

Figure 8: Event-study plots of treatment effects on probability of female employment by sector



(a) Non-Service Sectors

(b) Services Sector

Estimates use the Sun and Abraham (2021) interaction-weighted estimator with state and year fixed effects.

Figure 8 presents event-study plots of the effect of the reform on women's employment in services and non-services. The dynamics show a clear divergence: in services, the coefficients turn positive after treatment and increase over time, while in non-services they decline and remain negative. Pre-treatment coefficients are close to zero, providing support for the parallel trends assumption.

Taken together, the Sun and Abraham (2021) estimates reinforce our main findings: the reform did not generate overall employment gains but reallocated women's labor from non-service activities toward wage employment in the service sector.

Notes: The table reports average treatment effects (ATT) on women's employment in each sector, computed as the mean of event-time coefficients for years 0–2 relative to the pre-treatment period. Estimates follow the interaction-weighted estimator of Sun and Abraham (2021). All specifications include state and year fixed effects. Standard errors, clustered at the state level, are shown in parentheses. Statistical significance is denoted as *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table 10: Sectoral Average Treatment Effects (ATT) by Estimator

	Sun & Abraham		Callaway & Santanna		TWFE	
	Non-Service	Service	Non-Service	Service	Non-Service	Service
ATT	-0.0728***	0.0495***	-.05001***	0.0393***	-0.0622***	0.0447***
	(0.0260)	(0.0155)	(0.0150)	(0.0119)	(.0190)	(0.0159)
Observations	2,464	2,464	2,450	2,450	2,464	2,464
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

8 Takeaways

Policies are also a product of context. The context can change, but the rules may not. Night-shift regulations, for example, made working more socially acceptable for women in the 19th-century United States of America (Haddad and Kattan, 2024). But in 20th-century India, the same rules have often limited women's choice to work. Early evidence from my ongoing research suggests that removing these restrictions in the services sector raised women's chances of being employed in the sector by about 9.6

Existing evidence on the effect of night-shift regulations remains ambiguous. If women view these regulations as enhancing workplace safety, they may be more willing to work. However, if firms see compliance as burdensome, they may adjust hiring or wages in ways that limit women's job options (Rodgers, 2012) (Landes, 1980). Our results help clarify this ambiguity in the Indian context.

We find no evidence that the amendment changed women's overall participation in the labor force, nor their aggregate likelihood of being employed. Instead, the effects are concentrated in sectoral and occupational reallocations. Women moved out of manufacturing and into services, where wage employment rose. The transition was not driven by greater entry into self-employment but rather by a shift toward more formal, employer-linked work in services. These findings underscore that the removal of gender-specific restrictions did not expand the total pool of working women, but it did reshape where and how women work.

The effects of policies that apply to one gender are often mistaken for effects of gender itself. If fewer women work night shifts, it might appear to reflect personal preference or constraint, when in fact it may be the consequence of a rule that systemically restricts women's labor market opportunities. My research aims to separate the effects of gender from the effects of gender-specific rules, showing how institutions and regulations interact with the labor market.

In India today, more than 330 million working-age women are not employed—a number nearly equal to the entire population of the United States. Even modest improvements

in policy design could help unlock this vast, unrealised potential. My work will be dedicated to tapping this unrealised potential, by identifying and evaluating policies that shape women's labor market opportunities, and by showing how reforms can promote both equity and efficiency.

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