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From Dusk Till Dawn: The Impact of Lifting Night Shift Bans on Female Employment

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Abstract

We examine whether lifting a ban on the employment of female workers at night can spur firm demand for female labor. Different states in India have amended their labor regulations to remove a prohibition on the employment of female workers on night shifts in factories. Using firm-level panel data and a dynamic difference-in-differences estimator we find that following the regulatory relaxation, large firms significantly increased both the share and number of female workers. These effects are driven by larger firms operating in export-oriented industries and tighter labor markets. Our findings demonstrate that removing gender-discriminatory regulations can expand female employment and improve firm flexibility in hiring.

JEL Codes: J08, J16, J23, J78, K31

Keywords: Female employment, night shift, gender, regulation

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1 Introduction

Globally, females have less than two-thirds of the legal rights that males have in the workplace (World Bank, 2024). Discriminatory workplace legislation spans multiple dimensions: many females around the world do not have the right to work in specific sectors or at particular times of the day, many are denied access to parental leave and to safe workplaces, and many more are not protected from discrimination in pay or promotions. Even where males and females have equal legal rights on paper, only 40 percent of countries have established the institutional mechanisms necessary to enforce these rights (World Bank, 2024). The presence of these legal barriers leads to lower economic participation of females and is also associated with lower levels of economic growth and development (Hyland *et al.*, 2020).

One class of discriminatory legislation prohibits females from working in jobs that are considered "unsafe". At least 20 countries prohibit females from working at night, while 45 countries prohibit females from working in sectors that are considered "unsafe" by lawmakers, including sectors such as mining, construction, and some parts of the manufacturing sector (World Bank, 2024). Although these laws were perhaps intended to be well-meaning, they assume a lack of agency on the part of females, as well as the inability of employers to provide safe workplaces in which all employees can be productive. These forms of "paternalistic discrimination" (Buchmann *et al.*, 2023) can contribute to gender gaps in employment outcomes, including the reduced participation of females in the labor force. By constraining the demand for labor by employers, they can also reduce the productivity of firms, preventing them from hiring the best workers available for the job.

In this context, we ask two questions. First, do laws intended to benefit females by preventing them from working in unsafe conditions constrain the demand for their labor? Second, under what conditions will this constraint be the most binding, and which types of firms are most likely to expand the employment of females when the constraint is removed? If females themselves consider night work to be harmful, then it is not clear that a law banning their employment in night shifts will be binding. On the other hand, if females are ready and willing to work night shifts, and if firms respond to the removal of the ban by increasing their employment of females, then the ban can be understood to be a binding constraint on firm demand. Even where females are willing to work at night, firms may not demand their labor because of the fixed costs associated with employing females. For instance, firms with females employees are required to provide some basic workplace amenities, such as redressal mechanisms to address workplace sexual harassment, separate gendered toilets, and transport facilities for females, among others. Some firms may be more willing and able to absorb these fixed costs of hiring females, compared to others.

We examine these questions in the context of the manufacturing sector of a large developing country, India. We exploit the fact that some states in India eliminated the ban on female employment in night shifts in different years between 2014 and 2017, and implement a difference-in-differences estimator to estimate the impact of the ban on a number of outcomes, including share of female workers, female workers, male workers, wages and output. Our results are robust to the use of dynamic difference-in-differences estimators that allow for heterogeneous treatment effects over time and also allow for construction of correct counterfactual when the treatment is staggered.

We further examine the conditions under which the removal of the constraint on the demand for female labor can have positive impacts on female employment in manufacturing firms. Using a triple difference estimator, we compare the effects of the removal of the ban on large firms with small firms, on the assumption that larger firms find it relatively more cost-effective on a per-worker basis to incur the fixed costs associated with hiring female workers (Chakraborty & Mahajan, 2023). Larger firms are also more likely to have a night shift as compared to smaller firms. We also explore whether the treatment effects are stronger for firms that have female workers at the baseline. Such firms are more likely to have the required infrastructure to employ females after the removal of ban. We also compare outcomes in firms operating in more competitive product markets with those operating in less competitive markets. We measure competitiveness by the share of production that is exported. Finally, we compare firms in tight labor markets with those in labor markets with higher unemployment, to see if the impact of removing the ban on the employment of female workers at night depends on the extent to which additional labor is available to be employed without a relatively sharp increase in wages.

We find that the removal of the ban on female ability to work at night did lead to an increase in total employment of females in factories in treated states. These results are driven by large firms (those with at least 250 employees). We find an increase in the following outcomes in large firms: percentage of females hired as a share of the total workers employed by the firm (by 3.5 percent), total number of females hired (by 13 percent), and the number of firms who hire at least one female (by 6.5 percent). We find no significant impact on the employment of males, suggesting that firms have increased the total number of workers they employ, rather than substituting females for males. Firms that already employed females before the regulatory change were more likely to increase the use of female workers after the prohibition was removed than firms that did not employ any female workers before the ban. We do not find a significant change in total firm output or profits because of an increase in the number of females. This is expected because the share of female employees is only around 10% of the total workforce at the firm level. Hence, even a significant increase in the female share of employment might not translate into a significant change in output, at least in the short-run. We also find a slight reduction in capital expenditure by firms, which can also lead to no change in total output, as labor substitutes for capital.

We also find evidence of heterogeneous treatment effects along other dimensions. Firms in export-oriented industrial sectors are more likely to increase the employment of females at both intensive and extensive margins. These firms operate in a more competitive product market which makes them more responsive to the removal of constraints on the demand for labor. Finally, we find that the impact of the removal of the ban was most significant in labor markets that had an above-median level of unemployment, suggesting that firms were more likely to hire additional female workers in labor markets when it was less likely to result in an increase in wage costs. This is reinforced by the fact that we do not observe any impact of the removal of the constraint on the wages of either males or females.

Our study contributes to a large literature on the determinants of female labor force participation (see Heath *et al.* (2024) for a review). Much of the literature has focused on constraints on female labor supply; however, recent studies have focused on constraints on the demand for female labor by employers (Deshpande & Singh, 2024). One strand of the literature has examined the impact of labor laws aimed at protecting workers and improving working conditions on employment (see Betcherman (2015) for a review). One example of such policies is provision of paid maternity and parental leave, which have been extensively evaluated across the world with mixed effects on female employment and wages (see Canaan *et al.* (2022) and Del Rey *et al.* (2021) for recent reviews). Additionally, several studies have found that even gender-neutral labor regulation such as minimum wage and employment protection legislation have sometimes disproportionately hurt female labor force participation (Montenegro & Pagés, 2004; Feliciano, 1998; Del Carpio *et al.*, 2015; Fang & Lin, 2015; Suryahadi *et al.*, 2003). As we show in our study, more explicitly gender-discriminatory laws also constrain growth in female labor force participation.

To our knowledge, this is the first study analysing the removal of a law that prohibits females from working at night. In another setting, Zveglich & Rodgers (2003) find that restrictions on female working hours in Taiwan reduce their overall employment and hours worked. While some studies have found that the implementation of anti-discrimination legislation leads to an improvement in the employment and pay outcomes of females (Amin & Islam, 2015; Zabalza & Tzannatos, 1985; Eberts & Stone, 1985), at least one study has documented the potential adverse effect of introducing anti-discrimination laws on female employment, as they can increase the cost to firms of hiring females (Neumark & Stock, 2006). We provide additional evidence on the adverse effects of discriminatory legislation on female employment outcomes in the largest labor market in the world.

Our study also describes a specific pathway through which gender inequality in law can affect economic development. Literature focused on cross-country analyses has found that countries with less gender discrimination in laws tend to have better economic outcomes (Hyland *et al.*, 2020; Morrison *et al.*, 2007; Gonzales *et al.*, 2015; Fernández, 2014). Our paper provides an example of one such discriminatory legislation that constrains firms from hiring females who are able and willing to take on productive work, and suggests that removing such distortionary regulation could close the economic gap between less developed and more developed countries.

In Section 2 we discuss the legal context for the ban on female employment in night shifts in the manufacturing sector, and provide a theoretical framework to understand the impact of the removal of the ban. In Section 3, we describe the data we use in this analysis and the empirical strategy. In Section 4 we analyse the results. Section 5 concludes.

2 Background and context

2.1 Global background

Historically, female participation in night shifts was restricted to protect them from exploitation by employers in factories. After some European countries banned night shift work for females in the early 20th century, the International Labor Organization (ILO) passed the Night Work (Women) Convention in 1919 which banned night work for females in industrial work, including mining, manufacturing, construction, and maintenance. The Convention was ratified by 165 countries. Amendments in 1934 and 1948 allowed countries to permit night shift work in case of "emergencies".

The health consequences of night shift work include an increased risk of occupational accidents (Vedaa *et al.*, 2019; Wagstaff & Lie, 2011; Kecklund & Axelsson, 2016; Jayachitra & Jagannarayan, 2021), higher rates of cardiovascular diseases such as diabetes and blood pressure (Leger *et al.*, 2018; Kecklund & Axelsson, 2016), disturbed circadian rhythms or sleep cycles (Costa, 1996), emotional and mood disorders due to disturbance of sleep (Medic *et al.*, 2017), depression (Angerer *et al.*, 2017), and some forms of cancer (Kecklund & Axelsson, 2016). Specifically for females, Megdal *et al.* (2005) conducted a systematic review of the literature on the association between night work and breast cancer. They find evidence for an increased risk of breast cancer, possibly due to exposure to light at night and subsequent suppression of melatonin. Fernandez *et al.* (2020) also find that night shift workers were more likely to require fertility treatments.

The evidence thus suggests that there are potential health consequences of night shift work; however, most of them are common to both males and females. They are also exacerbated when firms overwork their employees by making them work long day and night shifts, without sufficient gaps between them. As gender attitudes became more progressive, the ILO Convention came to be seen as discriminatory against females. In 1990, it was amended to be gender-neutral and to specify the conditions under which night shift work could be safe for both males and females. By then, 72 out of the 165 original signatories had already introduced amendments that allowed females to work at night. Most of these signatories were high-income, developed countries with relatively progressive gender norms.

In other countries, discriminatory laws remain in place to restrict the ability of females to work at night (Politakis, 2001), potentially limiting female access to employment opportunities and reinforcing many harmful gender stereotypes (Farley, 1996).

2.2 Legal context in India

India, as a signatory to ILO conventions, explicitly banned night shift work for females in the manufacturing sector through the Factories Act of 1948, a federal legislation. The Act allowed females to work only between the hours of 6 am and 7 pm. Similar legislation was passed to prevent females from working at night in the non-manufacturing commercial sector through the Shops and Establishments Act, 1948. However, since labor legislation is a subject on which both national and state governments can implement legislation, states have the authority to make laws and regulations that exempt specific firms or sectors from these federal restrictions (Anand & Kaur, 2022).

States first began to amend state-specific Shops and Establishments Acts and related regulations to allow females to work at night in non-manufacturing commercial firms. These changes coincided with the boom in call centers that had a high demand for female workers at night. Regulatory change was much slower for the manufacturing sector, possibly due to the greater physical risks involved with manufacturing work and the stronger gender norms about the suitability of such work for females.

In 2000, the Madras High Court (in the state of Tamil Nadu) struck down Section 66(1)(b) of the Factories Act, which prohibited females from working at night in factories, holding that it violated the constitutional right to equality since it deprived willing females of the opportunity to work.[1] This was soon followed by a similar judgment by the Andhra High Court (in the state of Andhra Pradesh) in 2001.[2] More recently, the Gujarat High Court in 2013 also ruled that the restriction on females working at night in factories was unconstitutional.[3]

Following these judgments, some states began to permit females to work at night in factories. These permissions were typically granted in one of two ways. One was through *legislative amendments* of the Factories Acts by states, allowing factories to employ females at night. The second was through *executive exemptions*, whereby state governments issued notifications and regulations allowing females to work at night in factories that meet certain conditions, or issued exemptions to specific firms that apply for permission to the state's Labor Bureau.

In both cases, the state typically defines certain conditions that employers must meet to receive an exemption allowing them to employ females on night shifts in factories. Some of these conditions include provisions that were outlined in the Madras High Court judgment of

[1]Vasantha R. vs Union Of India (Uoi) And Others. Retrieved from https://indiankanoon.org/doc/715470/.

[2]Triveni K.S. And Ors. vs Union Of India (UoI) And Others. Retrieved from https://indiankanoon.org/doc/432677/

[3]Mahila Utkarsh Trust vs Union Of India. Retrieved from https://indiankanoon.org/doc/78049057/.

2001: establishing mechanisms to prevent sexual harassment of females, employing females in groups, creating separate work sheds, cafeterias, sanitation and medical facilities for males and females, providing transport, offering paid menstrual leave, and ensuring a gap of at least twelve hours between successive shifts.

Till 2020, no Indian state gave females complete freedom to work in factories at night. However, at least 11 states have amended their state-specific legislation or regulations to allow exemptions to some factories and sectors. We include 7 of these states in our analysis: Andhra Pradesh, Assam, Haryana, Himachal Pradesh, Maharashtra, Punjab, and Uttar Pradesh. Of the other 4 states, Goa and Karnataka implemented changes in 2019, which is after the period of analysis in this study. The timing of Tamil Nadu's regulatory changes is uncertain, since reports suggest that females were employed on night shifts in manufacturing units in Tiruppur as early as 2000, after the Madras High Court judgment allowing females to work at night was passed (Vas, 2000; Communist Party of India (Marxist-Leninist) Liberation, 2014).[4] Consequently, we exclude Tamil Nadu from our analysis since it is likely to be an 'always-treated' state. We also exclude Madhya Pradesh, which implemented changes to restrictions on night shift work for women through an amendment, but also implemented major changes to its labor laws during the same time period. Table 1 summarizes the actions taken by the 7 states considered treated in our analysis between 2014 and 2017. Any state that has not made a modification to its Factories Act through an executive or legislative change continues to follow the historical legal position of a complete ban on females working night shifts in manufacturing firms.

Since states have adopted varying approaches (legislative vs executive) for allowing factories to employ women at night, the intensity of treatment may differ across those listed in Table 1. In this paper, we focus on the comparison between these treated states, all of

[4]The government further issued clarifications regarding conditions that must be maintained for women to work in night shifts in 2017, acknowledging a previous government order in 2013 that amended the legislative provision pertaining to night shift work for women. Tamil Nadu government notification retrieved from https://gmgvellore.wordpress.com/wp-content/uploads/2020/06/factories-work-amendment-for-employing-female-worker-in-night-shift.pdf.

which allowed females to work night shifts, and all other control states, where no factories could employ females in night shifts. Our results are robust to excluding states that allowed women to work at night through regulatory exemptions provided to specific factories, and not through legislation.

2.3 Theoretical framework

There are potential benefits to firms of being able to employ female workers at night. First, if firms face constraints in hiring labor, access to a bigger pool of workers can allow them to increase total employment, without paying higher wages. Second, female workers could even be used to replace male workers, especially if they can be hired at lower wages. Even if their wages are not lower, female workers may be less disruptive at work and less likely to unionize (Ratnam & Jain, 2002). Finally, other firm outcomes can also change. For instance, firms may be able to increase profits through the more intensive use of capital (Lanfranchi *et al.*, 2002; Foss, 2012). If capital and labor are substitutes, firms can reduce their dependence on capital, without affecting output. Wages may also be affected: an increase in female demand can put upward pressure on female wages. However, if the labor market is characterized by unemployment then the female wage rate may not actually increase.

Withdrawing the law prohibiting females from working at night could, therefore, have an impact on the employment of males and females, the wages of males and females, and firm output and profits. We discuss the likely effects on each outcome below.

Female workers: If the ban is a binding constraint on hiring females, then we would expect that withdrawing the law will lead to an increase in the employment of female workers in factories in the treated states. However, employing female workers involves some fixed costs. As described in the previous section, these costs may include – providing female-only toilets, rest facilities and transport, and investing in the necessary institutional mechanisms to tackle workplace sexual harassment. Given these costs, firms may invest in hiring female workers only if the fixed costs per worker are sufficiently low. Large firms, with greater resources and existing night shifts, can spread these costs over more employees and are thus more likely to hire additional females or any females at all. Such firms are also more likely to introduce or intensify the use of night shifts in their factories. Similarly, firms that already employ some female workers may have already invested in the fixed costs associated with hiring female workers (Chakraborty & Mahajan, 2023), and hence may find it easier to respond to the removal of the ban by deploying female workers at night.

Male workers: The impact on the employment of male workers will depend on the extent to which male and female workers are substitutes, and the extent of the gains from adding night shifts. If male and female workers are substitutes for one another, there can be a decline in male employment, especially in large firms, in states where the ban is withdrawn. However, if firms are now able to move to a multiple-shift production system, they may realize sufficiently large increases in output and profits that allow them to maintain or even increase their level of male employment, though by less than the rise in female employment.

Output and profits: If firms face a shortage of male labor when the ban on female labor at night is in place, then they may not be able to organize their workers into multiple shifts. The removal of this constraint could allow them to move to a multi-shift system of production, including night shifts, which could allow them to increase both output and profits. If the firm is already operating night shifts, then firms may be able to substitute between male and female labor to hire more suitable workers for the job, which could increase firm-level output and profits. However, if the overall female share in the firm's total labor force is low (which is true in the Indian context), we may not find a significant impact on output and profits even if there is an increase in the share of female employment. Additionally, if firms substitute labor for capital, output may remain unchanged.

Wages: We are unlikely to observe effects on wages within the firm as a result of the change in the regulations since within-firm wages are likely to be sticky over the short-to-medium term, as in our study. However, female wages in formal employment could change, depending on existing labor market conditions and the extent of the increase in demand for female labor with the removal of the ban.

It is not obvious that an increase in demand for female labor will translate into an increase in wages. For instance, if there is no unemployment, an increase in the demand for female labor could lead to an increase in female wages. On the other hand, if there is unemployment because, say, the minimum wage is set above the market clearing wage, female wages might not be affected even as the demand for female labor increases. The impact on male wages will additionally depend on whether the demand for male labor increases or not.

Heterogeneity in treatment effects: As we have already discussed, a key dimension along which we may observe heterogeneity in treatment effects is firm size. This is because bigger firms are best placed to absorb the fixed costs of employing women on night shifts. Similarly, firms that already employ female workers will have already invested in the necessary infrastructure required to employ females and are therefore more likely to expand female night shift employment once the ban is lifted. We also expect to see the largest impact on firms in those settings where the ban was a binding constraint. For instance, firms that export a large share of their output typically operate in more competitive markets and are under greater pressure to decrease costs and increase productivity. We may expect such firms to respond more strongly to the removal of the ban than firms which operate in less competitive output markets since they have strong incentives to hire the most productive workers, regardless of gender. Conversely, firms in tight labor markets with low unemployment may lack the financial flexibility to expand hiring and add shifts, since doing so could push up wages.

In the sections that follow, we examine the impact of the removal of the ban on employing females in night shifts on these outcomes. We also examine heterogeneity in treatment effects on these outcomes by different mediators that we discuss above, including firm size, whether the firm already employs female workers, whether the firm belongs to an export-oriented industry, and the extent of unemployment in the local labor market.

3 Data and methods

In this section, we first describe the establishment-level data on manufacturing enterprises used in the main analysis and compare the states which implemented the reform to those that continued to impose restrictions on employing females on night shifts. We next describe the empirical strategy we use to estimate the impact of the removal of the ban on firm-level outcomes including female employment.

3.1 Data descriptives

We use data from the Annual Survey of Industries (ASI), a nationally representative survey of registered manufacturing establishments in India.[5] These establishments are registered under the Factories Act in India.[6] The survey consists of two components: an annual census of all establishments which employ at least 100 workers or are located in the six least developed states of India, and a stratified sample of one-third of all establishments which employ fewer than 100 workers. Firms in the stratified sample are typically surveyed once every three years.

We use ASI data from 2009-10 to 2018-19. States started to remove restrictions on

[5] The unit of observation in the ASI is an establishment or a manufacturing plant rather than a firm. However, for the sake of exposition, we are going to use the terms establishment and firm interchangeably. Existing evidence shows that multi-plant establishments constitute only 5% of all manufacturing establishments with sales of at least $30 million in India (Chakrabati & Tomar, 2022). Since multi-establishment firms are generally bigger in size, the proportion of multi-establishment firms among all firms in India is likely to be much smaller.

[6] The Act is applicable to firms with 10 or more workers when the firm uses electric fuel power, and to firms that employ 20 or more workers when the firm does not use electric power without power. However, around 25% of firms in the ASI data have fewer than 10 employees. This is because the economic census of firms which forms the basis of the sampling frame is only updated once every 5 years. Also, some previously registered firms may not have de-registered after downsizing.

night-shift work for females from 2014 onwards, with several states making regulatory changes in 2017 (Table 1). These seven states comprise our treated states. States not listed in this table comprise control states, with the following exceptions: Tamil Nadu, where the timing of treatment is unclear, and Jharkhand, Madhya Pradesh and Rajasthan, which implemented major changes to their labor laws in this same period. We drop these states from our analysis. Our final sample consists of 23 states and 6 Union Territories (UTs).[7]

We drop years prior to 2009 because of changes to the survey questionnaire that make it difficult to compare data collected before 2009 to data collected after. However, we show that our results are robust to using data from 2000. We exclude data from 2019-20 since this was affected by the COVID-19 pandemic.

The ASI collects information on a host of firm-level employment characteristics such as number of workers, supervisors, permanent employees and contract workers, their days of work, and total wage expenditure incurred by the firm.[8] However, gender-disaggregated data is only collected for workers and worker man-days. In addition, the survey also provides data on output, input use, fixed capital, and credit. We deflate the variables expressed in nominal terms, such as wages, by using the Consumer Price Index (CPI) with a base year of 2004. Output is deflated by two-digit industry-specific Wholesale Price Index (WPI) with 2004 as the base year.

Table 2 shows the summary statistics of key pre-treatment variables across states that eased the night shift restrictions (treatment states) versus states that did not (control states). For states with night shift amendments, the pre-treatment period includes the years prior to the year in which the regulation was amended in that state. For states without amendments, the pre-treatment period includes the years prior to 2014, which is when the first state-level night shift amendment in our sample took place. Values of pre-treatment variables are

[7]The state of Telangana separated from the state of Andhra Pradesh in 2014. For consistency, we treat Andhra Pradesh and Telangana as a single, undivided state in our sample.

[8]Workers are employees engaged in manufacturing tasks. Supervisors are employees not directly involved in manufacturing tasks but are responsible for overall management and supervision. Workers and supervisors together comprise permanent employees. Contract workers are manufacturing workers hired on contractual terms by the establishment, and ineligible for the benefits and job security available to permanent workers.

calculated using the most recent pre-treatment year for firms in states with amendments to minimize measurement error, but we also confirm that our results are robust to calculating the average value of variables across the entire pre-treatment period from 2009-13 for all states, with or without amendments.

Panel A shows that firms operating in treatment states were typically smaller, with fewer manufacturing workers. They have a smaller share of female workers as well, and employed them for fewer days in the pre-treatment period.

Panels B and C show that firms in treated states also had fewer permanent employees, lower output and labor productivity, but a slightly higher share of exports. Daily wage rates were similar across firms in both treated and control states. As discussed earlier, we hypothesize that larger firms are more likely to benefit from the removal of the restriction on female ability to work night shifts. As we see, the data suggest that treated states were not more likely to have larger firms (as measured by the total number of permanent employees); in fact, their firms were smaller on average.

Panel D reports the unemployment rate calculated using data from the 2011-12 Employment and Unemployment Schedule of the National Sample Surveys. It shows that the unemployment rate was approximately 50 percent *lower* in treated states than control states. This implies that treated states did not implement the reform in order to create more jobs in an environment with high unemployment.

Nonetheless, it is still possible that the states that implemented the reforms did so for reasons that are correlated with the outcomes of interest, making the adoption of the reform process endogenous. We address this concern to our empirical strategy in the next section.

3.2 Empirical methodology

We use a difference-in-differences framework to compare firm-level outcomes in states that eased the night shift restrictions (treatment states) to outcomes in states that maintained the status quo of preventing females from working in factories at night (control states). Our main outcome variables of interest are the percentage of female workers employed by a firm as a share of all manufacturing workers, the number of female and male workers in a firm, and whether a firm employs any female worker at all.

We estimate the following equation:

$$y_{fist} = \beta_1 Post_{st} \times NightShift_s + \gamma_f + \psi_{it} + \epsilon_{fist} \tag{1}$$

where y_{fist} is the outcome for firm f operating in industry i within state s at time t. $NightShift_s$ takes a value of 1 if the state s had no restrictions on night shift work for females in any year during our sample period, and 0 otherwise. $Post_{st}$ takes a value of 1 in all the years when the state s has no restriction on night time work and 0 otherwise. We control for time-invariant characteristics of the firm through firm fixed effects (γ_f). We control for macroeconomic shocks that affect all firms in a given industry and year with industry-year fixed effects (ψ_{it}). These are important controls since there were industry-level policy changes during this period.[9] We cluster the error term, ϵ_{fist}, by state since the treatment occurs at the level of the state. Our coefficient of interest is β_1 which captures the average outcome differences in firms operating in treatment versus control states after the implementation of the reform compared to before the implementation.

As discussed before, some states that allowed firms to employ female workers at night also mandated them to provide specific amenities and infrastructure, such as separate toilets for females and transportation facilities. This could lead to a differential effect of the policy on larger versus smaller firms. The fixed costs of these infrastructural investments per worker will be more feasible to absorb for larger firms, which are more productive and profitable. Hence, larger firms will find it easier to make these investments and hire females. Moreover, larger firms are more likely to already employ females and, therefore, might already have the required infrastructure in place (Chakraborty & Mahajan, 2023). This implies that the

[9] For instance, the Government of India started Merchandise Exports from India Scheme (MEIS) in 2015. It also started providing tax incentives for firms in certain sectors after 2016 to increase employment.

treatment is more likely to have an impact on larger firms compared to smaller firms. We test for this by adding an interaction term to equation 1 as shown below:

$$
\begin{aligned}
y_{fist} = \beta_1\, Post_{st} \times NightShift_s + \beta_2\, Post_{st} \times NightShift_s \times Large_f \\
+ Post_{st} \times Large_f + Large_f \times \delta_t + \gamma_f + \psi_{it} + \epsilon_{fist}\,.
\end{aligned}
\tag{2}
$$

where $Large_f$ takes a value of 1 if firm f employs at least 250 permanent employees in the most recent year in which the firm appears in our data (since ASI is an unbalanced panel), before the policy change occurs in a state. For firms in control states, we assign the value of $Large_f$ based on the number of permanent employees they have in 2013 – the year before any firm in our time period of study was treated – or whichever is the most recent year before 2013 when it appears in the data. We later demonstrate the robustness of our results to different thresholds of firm size apart from 250.[10] We also control for the interactions of firm size with year fixed effects (δ_t) – an important control given the other regulatory changes in the country like Maternity Benefits and Prevention of Sexual Harassment legislations.

We also estimate heterogeneity in firm response along the following dimensions measured before the implementation of the policy: i) whether the firm employs any female manufacturing worker and number of female workers employed because a firm that already hires female workers will face fewer costs in complying with the regulations required to hire them in night shifts; ii) whether the firm operates in an export-oriented sector because such firms are likely to be more responsive to policy changes; and iii) state-level unemployment rate (overall and for females) which proxies for local labor market conditions which can affect the ease with which the firm can hire additional workers.

Our key identifying assumption is that at the firm level the policy change was exogenous to unobserved time-varying characteristics of the firm that we do not already control for.

[10]We take threshold of 250 for defining large firms since this is the accepted international threshold (OECD).

This is likely to be the case as it seems implausible that a single firm could have influenced a sudden change in a policy that was in place for many decades. We are also assured of our assumption by a lack of pre-trends in the outcome variable before the implementation of the policy. We estimate the pre-trends and the year-wise effects of the policy with the following event-study specification:

$$y_{fist} = \sum_{\tau=-3,\tau\neq-1}^{\tau=4} \beta_\tau NightShift_s^\tau + \gamma_f + \psi_{it} + \epsilon_{fist} \tag{3}$$

where $NightShift_s^\tau$ is an indicator variable that takes a value of one for a treated state, s, τ periods after it implemented the regulatory change, and zero otherwise. β_τ capture the average differences in outcomes between firms in treated states versus control states τ periods after the reform as compared to the year before the reform (base year). We also estimate equation 3 for the sample of large and small firms separately.

Recent advances in the difference-in-differences estimation literature have shown that two-way fixed effects coefficients can be biased in settings where the treatment is staggered (Goodman-Bacon, 2021). In particular, the two-way fixed effects regressions can include forbidden comparisons where the states that changed were treated earlier are used as a comparison group for states that were treated later. Such comparisons might be problematic if the treatment effects vary over time or when they are heterogeneous across treated states. To address this concern, we show the robustness of our results to the dynamic treatment effects estimator by Sun & Abraham (2021).[11]

4 Results

In this section, we first present estimates of the impact of the relaxation of restrictions on employing female workers at night in factories on firm-level outcomes such as female

[11] We prefer this estimator since this is relevant for staggered treatments where treated units do not become untreated over time. Additionally, it allows for direct inclusion of other controls like industry by time fixed effects which are important in our context.

employment. We also present estimates separately for large and small firms, since large firms are not only more likely to have night shifts but are also more likely to be able to meet the infrastructural requirements necessary to hire females at night.

4.1 Impact on female employment

Table 3 shows the estimates for equation 1. The outcomes are the share of female workers out of total workers in columns 1-2, the number of female workers in columns 3-4, the number of male workers in columns 5-6 and whether any female worker is employed in an establishment in columns 7-8. The first column for every outcome includes firm and year fixed effects. The second column additionally includes industry-by-year fixed effects to control for any industry-specific changes in outcomes over time. This is our preferred specification due to industry-specific policy changes that may have been implemented at different points in time. When considering the sample of all firms (Panel A), the relaxation of night shift restrictions does not have an effect on female employment in any of the specifications. However, estimates in Panel B for equation 2 show that the treatment effect varies significantly with firm size. In larger firms (firms with at least 250 permanent employees), both the share of female employees (column 2) and the number of female employees (column 4) increases relatively more by 0.47 percentage points and 5.6 workers as compared to smaller firms, respectively, with both coefficients significant at the 10% and 5% level, respectively. This represents a relative increase of 11.7% over the mean female employment in large firms. We also find a relative increase in the probability that a large firm hires any female workers, by 2.1 percentage points, which is significant at the 1% level (column 8). The number of male workers, however, do not change significantly in the large firms, when compared to small firms (column 6).

To estimate the treatment effect of the change in regulation for large firms, we estimate the same specification in equation 1 on a sample of large firms only (Table 3, Panel C). We find that the share of female workers increases by 0.39 percentage points or 3.5 percent of the baseline mean (column 2 of Panel C) in large firms, with this result significant at the

10% level. We also observe an increase of 6.1 in the number of female workers (column 4 of Panel C), a 13 percent increase over the mean, and this estimate is significant at the 5% level. Finally, large firms are 2.6 percentage points (6.5 percent over mean) more likely to employ any female workers after the regulatory change, significant at the 1% level (column 8). This evidence indicates that only large firms had the financial capacity to meet the infrastructural requirements mandated by the law to employ females workers at night. Again, there is no significant impact on the number of male workers (column 6).

These results will reflect the true treatment effect only if the treated and control states would have witnessed a similar change in employment outcomes in the absence of the policy change. To evaluate whether this is likely to hold, we estimate equation 3 and plot the year-wise average differences in outcomes between the treatment and control states, relative to the year before the reform. Figure 1 shows the estimates for the share of female workers as a percentage of total workers, number of female workers, number of male workers and whether a female worker is employed across the four panels. We show the results separately for all firms and large firms in each panel. For the sample consisting of all firms, the treatment effects are small and insignificant. Only the probability of a firm hiring any female worker increases significantly 4 years after the implementation of the reform. In contrast, for large firms we see that all measures of female worker employment increase after the reform. In fact, there is a slight positive trend in effect sizes after the implementation of reforms, which is expected as it might take firms some time to invest in the infrastructure required to employ female workers at night. On the other hand, there is no change in the number of male workers. Reassuringly, the event study estimates show absence of pre-trends as suggested by insignificant coefficients before the implementation of the reform for the share of female workers (Panel a) and whether any female worker is employed (Panel d). The difference-in-differences coefficients are significantly negative in the pre-treatment period for both the number of female and male workers employed in large firms. Therefore, we consider the share of female workers and whether a female worker is employed by the firm as our primary outcomes.

We also estimate the dynamic estimators proposed in the literature to address the potential bias in the two-way fixed effects estimates due to the staggered roll-out of the reforms across states. In particular, we use the estimator proposed by Sun & Abraham (2021). Figure A.1 in the Appendix plots the coefficients for the sample of large firms. For all the outcomes of interest, the event study estimates obtained using the alternative estimator are qualitatively similar to the two-way fixed effects estimator. We report the main estimates in the Appendix in Table A.1. The results are similar to those obtained using our main specification, which are reported in Table 3.

4.2 Robustness of main results

We implement a number of robustness checks for our main results on the impact of the treatment on the employment of females.

First, we estimate a more flexible relationship between firm size and the impact of the treatment instead of taking a cutoff of 250 permanent employees. In Table A.2, we allow the treatment effect of the regulatory change to depend on a quadratic function of firm size. We note a positive coefficient on the treatment indicator interacted with pre-treatment firm size and a negative coefficient on the square of firm size, both of which are significantly different from zero at the 1% level for female workers, male workers and the probability that a firm employs any female workers. These suggest that the treatment effects are concave in firm size. However, for the share of female workers and the probability of employing any woman, the coefficient on the squared term is close to zero. We also test that our results are robust to alternative definitions large firms, defining them as those with at least 50, 100, 150, 250, and 300 as the total number of permanent employees. Figure 2 plots the estimates for varying definitions of large firms. We find that the coefficient on the percentage of female workers, the number of female workers, and whether the firm hires any female workers increases in magnitude as the cutoff to define large firms increases. The estimate for male workers remains very close to 0 for all definitions.

Second, in Table A.3 we show robustness to alternative definitions of the dependent variables when estimating the effects for large firms. We show that our estimated treatment effects of the regulatory change on the absolute number of male and female workers are robust to log-transforming the outcome variables. Our preferred specification continues to be the non-transformed outcome because of the potential selection issues that arise with log transformations of variables with a large number of 0 values (Chen & Roth, 2024; Mullahy & Norton, 2022). We also check if there is any increase in total number of workdays after the removal of night shifts, separately for female and male workers. Columns 2 and 3 of Panel B show that while the number of workdays of female employees increases, there is no significant change in the number of workdays for men. There is also a 3.3% increase in the share of workdays by female employees (Column 1).

Third, we bootstrap standard errors at the level of state due to the relatively small number of clusters (we have 29 clusters, including 23 states and 6 Union Territories). Since the treatment occurs at the state level, correlation between residuals within a state might lead to biased standard errors. In Table A.4, we bootstrap the standard errors at the state level and find that our coefficients, in fact, become more significant as compared to the main results.

Fourth, we show that our results are robust to dropping the states of Haryana and Uttar Pradesh from our analysis, which implemented the change in regulations through an exemption process. Since individual firms had to apply for an exemption, firms may be differentially selecting into treatment based on characteristics that are correlated with our outcomes of interest, causing biases in our estimates. Table A.5 shows that even after removing these states from our sample, large firms increase their female workforce both on the intensive and extensive margins.

Fifth, we explore the sensitivity of our results to the introduction of the Maternity Benefits Amendment Act(Government of India, 2017; Bose & Chatterjee, 2024). India enacted the Maternity Benefits Amendment Act (MBAA) in 2017 across the country. It mandated all firms with at least 10 employees to provide 26 weeks of maternity leave for all female employees. It also mandated that firms with at least 50 employees make provision for creches. We do not believe this is a concern for our study since we rely on state-level variation in treatment for our analysis, while the impact of the maternity benefits legislation affected all states. However, it might still be the case that the ability and willingness of firms to hire more female workers in response to changes in night work regulations was muted after the introduction of the maternity benefits legislation. This would imply that our main estimates might be a lower bound of the true treatment effects. We restrict our sample till 2016 in Table A.6, Panel A, to exclude the years after the passage of this Act. We find that the results remain qualitatively unchanged. Additionally, we also test for robustness to extending the ASI panel data to the first year of availability 2000 (Panel B). Again, our findings remain the same.

Finally, in our analysis, we define firm size as the number of permanent employees in a firm in the year preceding the reform in a particular state. This implies that the firm sizes in the treated states are defined based on different years since the year of treatment varies. To ensure this does not confound our estimates, we redefine the firm size variable to be the number of permanent employees in a firm in 2013 – the year before the first state enacted the reform. If a firm is not surveyed in 2013 (since our data is an unbalanced panel) then we take the firm size in the most recent year prior to 2013 when the firm was surveyed. Table A.6, Panel C, shows that our results remain unchanged.

4.3 Heterogeneous effects of the reform

We next examine heterogeneity in treatment effects based on firm, industry and state-level characteristics that potentially affect whether the prohibition on hiring female workers at night was likely to be a binding constraint on firm demand for female labor. For these specifications, we focus only on large firms with at least 250 employees since these firms change female employment in response to the reform. We consider the following characteristics: whether a firm employed any female worker before the regulatory change, the share of exports in the total output, and state-level unemployment rate (overall and for females).

Baseline employment of females: We first examine whether firms who had some female workers at baseline are more likely to employ more females after the reform as compared to firms that had no female workers at the baseline. We do not directly observe whether firms have night shifts or not, or whether they introduced night shifts after the change in regulations. However, we expect that firms which already employ females are more likely to introduce night shifts in response to the treatment since they might already have in place the mandated infrastructure required to employ females at night. Table 4 shows that this is indeed the case. In Panel A, the coefficients on the interaction of the treatment indicator ($Post_{st} \times NightShift_s$) and an indicator for a firm with a positive number of pre-treatment female workers are positive and significantly different from 0. They are also substantially larger than those for firms that had no female workers in the pre-treatment period, both for the share and the number of female workers. A test for equality of the coefficients on the two interaction terms is rejected at the 5% level for female workers and at 20% for the share of female workers. In Panel B, we present the coefficients on the interaction of the treatment indicator with the number of pre-treatment female workers. These are all insignificantly different from 0, suggesting that there is a sharp jump in treatment effects at 0 female workers, but not beyond the extensive margin. Taken together, these results suggest that if policymakers encourage firms to employ at least some females, this can have a cascading effect on overall female employment in the firm when regulations are eased.

Share of exports Next, we examine whether industry-level characteristics matter for the effectiveness of the reform. In particular, we compare firms in industries where an above-median share of total output is exported with firms in industries where a below-median share of output is exported. A large literature finds that export-oriented firms are more agile and productive than other firms (see Harrison & Rodríguez-Clare (2010) for a review). We expect that export-oriented firms operate in more competitive product markets, which will make them more likely to want to hire the best workers for the job and be less resistant to employing female workers. We test for heterogeneity by industry-level export shares, rather than firm-level export shares since these are more likely to be exogenous to the firm's choices. However, our results are also robust to using firm-level export shares instead (results available on request).

Table 5 shows that the treatment effects are concentrated among firms in export-oriented industries (Panel A). We reject that the coefficients on the treatment indicators are equal for firms in above-median and below-median export-oriented industries for share of female workers and number of female workers at the 1% level and for whether firms hire any female workers at all at the 10% level. In Panel B, we demonstrate that the treatment effect increases with the industry-wide share of total output exported, with the coefficients on the interaction term being positive and significantly different from 0 for all outcomes except the increase in probability of hiring any woman.

A possible confounding factor can be that exporting firms typically employ more females at the baseline (Rocha & Piermartini, 2023). This is true in our data as well: firms in industries with an above-median export share have a larger share of female workers at 14.32% than firms in industries with a below-median export share at 7.28%. Hence, it is possible that higher export ratio is a proxy for having more female employees at the baseline, which we have already found to be an important mediator of the impact of the reform. In Appendix Table A.7, we show that even after controlling for having at least one female employee at the baseline, the above-median exporting firm's response to the reforms is comparatively larger than the response of the below-median exporter. This reinforces our claim that the reform was particularly effective for firms in industries which are more competitive.

Local labor market conditions Finally, we investigate the impact of local labor market conditions on the effect of the reforms on large firms. In the presence of unemployment –

which could be due to binding minimum wages – an increase in the demand for female labor as a result of the change in regulation may not increase wages for female labor. This is because the binding minimum wage is already above the market clearing wage rate. Thus, firms in states with higher unemployment may have a greater incentive to hire female labor since the increase in wages will be limited, relative to firms operating in states with lower unemployment. Additionally, greater unemployment would also allow firms to quickly hire female workers since surplus labor is likely to be available in such states.

We find suggestive evidence that the reforms had the largest impact in states that had higher overall unemployment rates and female-specific unemployment rates during the pre-treatment period (Table 6). We show that the number of female workers increases by 0.11 and 0.030 for every percentage point increase in the baseline level of overall and female unemployment rates, respectively, with these coefficients significant at the 5% level (column 2, Panel A and Panel B). The coefficient on the share of female workers is positive but not significantly different from 0.

Thus, we find that large firms that are relatively export-intensive and which already employed female workers before the reform, increase their demand for female workers after the relaxation of the night shift regulation. This response may reflect both lower hiring costs and higher demand for female workers in these industries. Additionally, firms in states with higher unemployment rates were better able to hire more women at lower cost following the policy change.

4.4 Other outcomes

In this section we examine how lifting the night shift ban affected additional outcomes such as wages, output and profits.

First, we examine the impact on the total wage bill of the firm and the per-worker wage rates for both males and females. In Appendix Table A.8, we estimate the impact of the reform on both male and female wage bills and wages. Unsurprisingly, we find that the female wage bill for the average large firm rises by ₹279,500 (approximately US $3,300) because of its increased demand for female labor, an increase of 15% over the control mean (column 1, Panel B). This coefficient is significantly different from 0 at the 1% level. The overall wage bill also increases but this coefficient is statistically insignificant as the share of female workers in the workforce continues to be small, even after the change in the policy. The share of wages as a proportion of total expenditure also increases (column 7 in Panel B) and this coefficient is statistically significant at the 10% level.

We do not observe any significant effects on female or male daily wages, either in the sample of all firms or in the sample of large firms (Columns 4-6 of Table A.8). These estimations include firm fixed effects, and wages paid by a firm are likely to be sticky in the short-to-medium term. We drop firm fixed effects from the estimating equation to see whether male and female wages respond to the changes in average firm demand for female labor. This may be the case if, for example, changes in wages are driven by firms in specific industries which respond strongly to the policy. Column 1 of Table A.9 shows that for larger firms, the female wage rates actually fall, instead of rising. This suggests that the increase in demand for women may also have had a positive impact on the supply of labor. These results are also consistent with our previous result that the biggest increases in employment for female labor took place in labor markets with relatively high levels of female unemployment.

Finally, we consider the effects of the reform on other important firm characteristics and report the estimates in Appendix Table A.10. The total number of workers employed by a large firm increases by 9 (Column 1, Panel B), though the standard errors are high and the coefficient is insignificantly different from 0.[12] Output and profits, however, remain unchanged (columns 2-3 of Panel B, Table A.10). These findings suggest that while the number of female employees increases after the reform, the absolute increase is not large enough to change firm's output and profits. Further, column 4 shows that there is a reduction in capital stock

[12]We also examine the effects on all permanent employees, as well as all permanent and contract employees. We do not find any significant increase in total employees using any definition of total labor use. These results are available on request.

by 4.5%, albeit not statistically significant. This shows that firms could be using labor to substitute for capital investment, resulting in output remaining unchanged.

5 Conclusion

Regulations which constrain the demand of firms for female workers can significantly depress female employment. This can adversely affect female economic empowerment, even if the regulation was intended to protect female welfare in the first place. We find that in India, a developing country with a strikingly low rate of female labor force participation, paternalistic laws that prevent females from working at night effectively constrain the demand for female labor. In states where such laws are removed or modified, firms respond by employing more female workers in manufacturing jobs.

This, however, is driven by large firms i.e., those with at least 250 employees and among these for firms that already employ at least one female manufacturing worker before the treatment. This suggests that there are fixed costs to employing females in factories; for smaller firms, it may not be cost-effective to employ females due to the high per-worker costs of providing female-friendly workplace amenities and infrastructure mandated by the law in order to employ females at night. We also find that the impact of regulatory change is larger for bigger firms in nimble export-oriented industries, which tend to operate in more competitive product markets and are relatively more constrained in hiring labor. Firms are also more responsive to regulatory change in states with higher levels of unemployment since they can employ more female workers without a significant impact on the wage rate.

Our results show that while gender-regressive laws like bans on night work do constrain firm demand for female labor, a pertinent issue is the high cost of employing females. This arises because firms need to invest in additional workplace amenities to comply with the modified provisions of the law. Since this is expensive for small firms, the burden of employing females in manufacturing necessarily falls on larger firms who can afford to do so. Unless these costs are also reduced through further reform, significant increases in female employment in manufacturing will continue to depend on the growth of labor demand in large firms.

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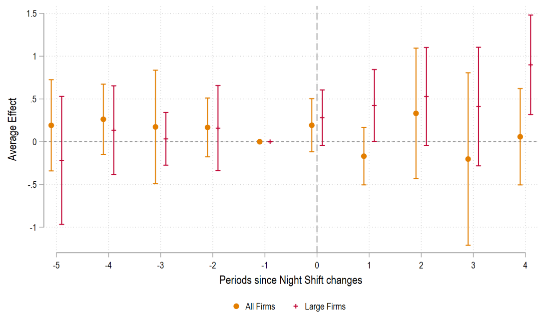
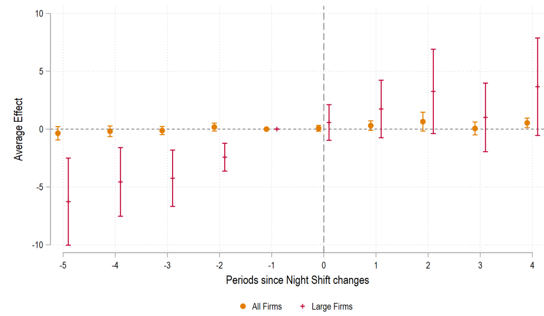
Figure 1: Event study estimates for the impact of night shift changes on the employment of female workers

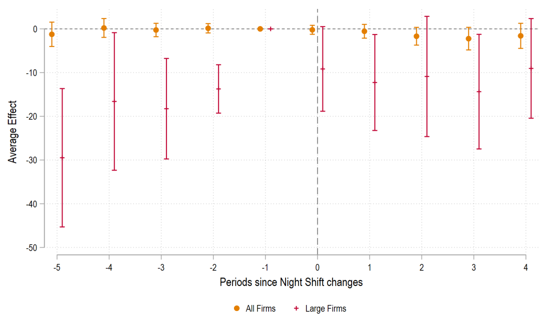
(a) Share of female workers



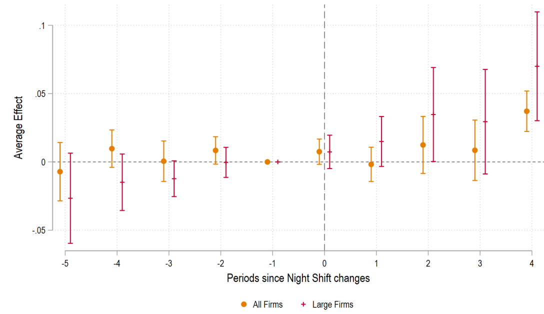
(b) Female workers



(c) Male workers



(d) Extensive margin

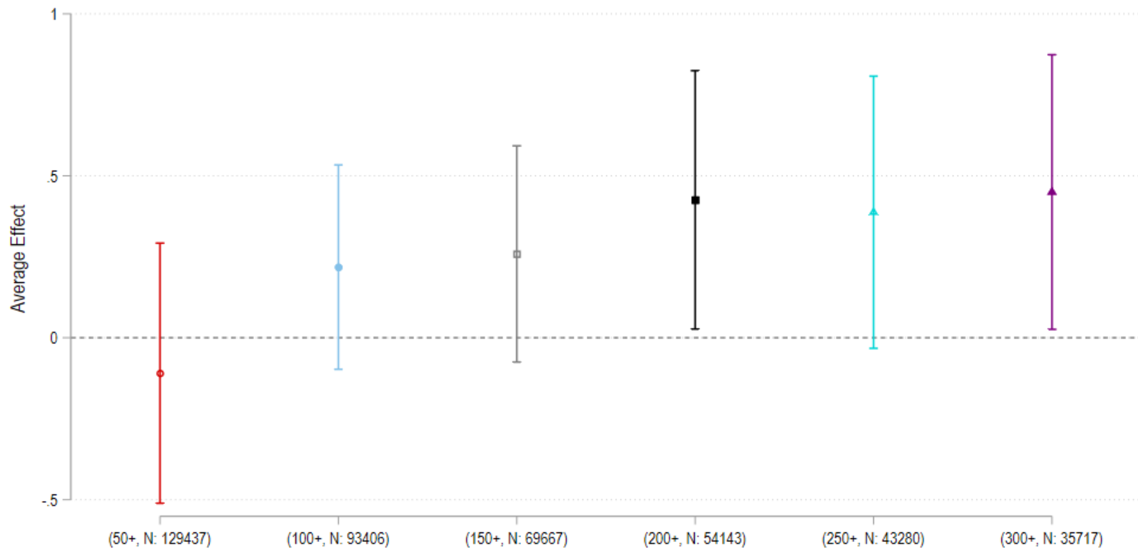


Notes: These figures show the impact on workers of firms in a state after that state allowed the employment of women at night. The regulatory change was made in the year 0. The figures plot the estimated effect of the regulatory change in each year, relative to the year before the change was made. For control states, the pre-treatment period is before 2014, when the first regulatory change is implemented. The sample comprises either all firms or large firms, which are firms that employ at least 250 employees. Share of female workers is defined as the number of female workers in the firm divided by the sum of male and female workers, and extensive margin is measured by a binary variable which is 1 whenever the firm has at least one female worker and 0 otherwise. For all figures, we show the results from the OLS estimation of equation 3 with firm and industry-year fixed effects. The bars show the 95% confidence interval for the estimates.

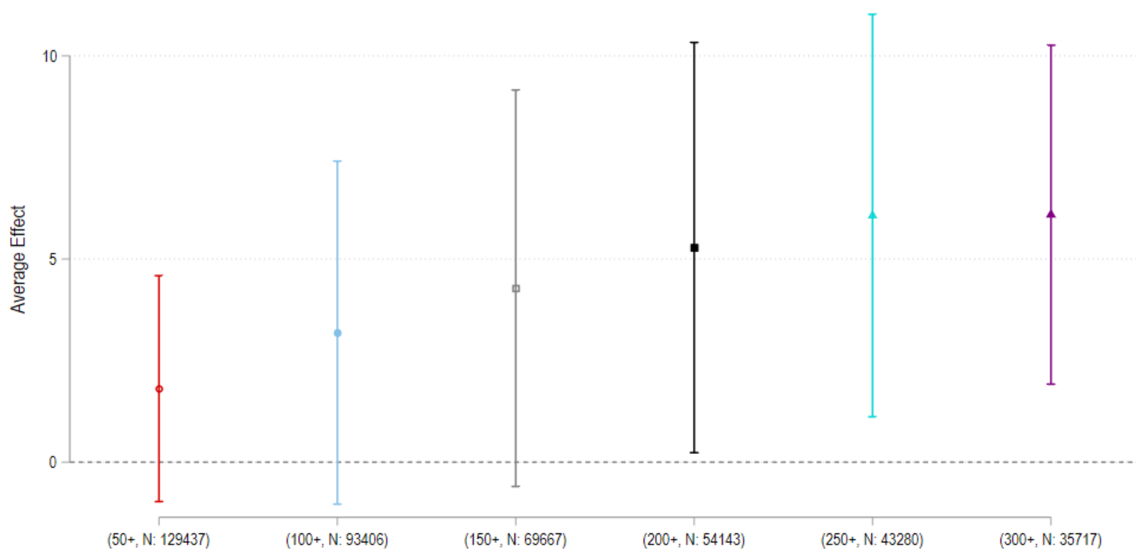
Source: Data on firms is from a panel version of the Annual Survey of Industries from 2009 to 2018.

Figure 2: Treatment effects for firms of different sizes

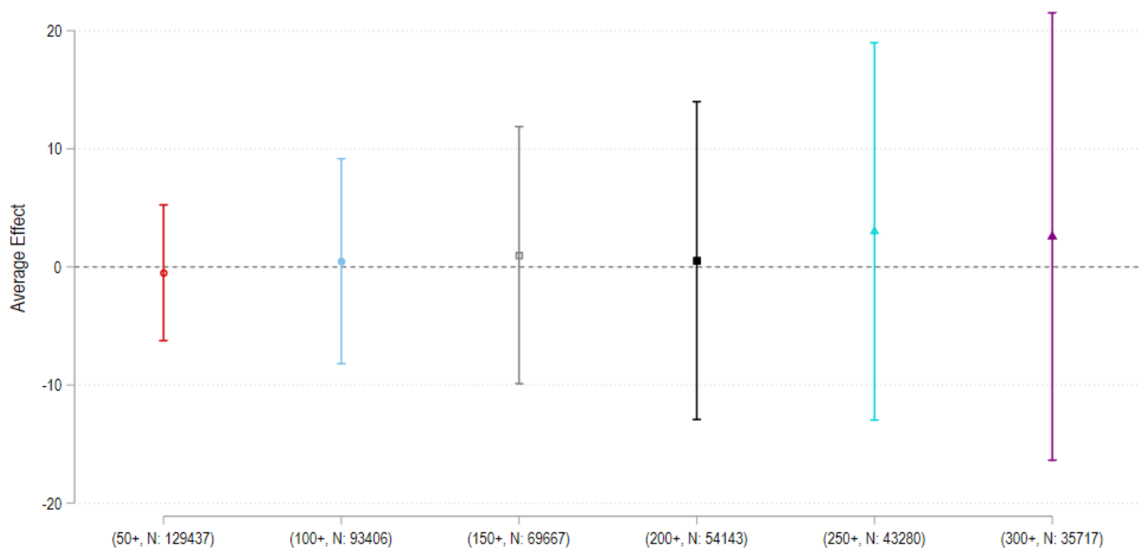
(a) Share of female workers



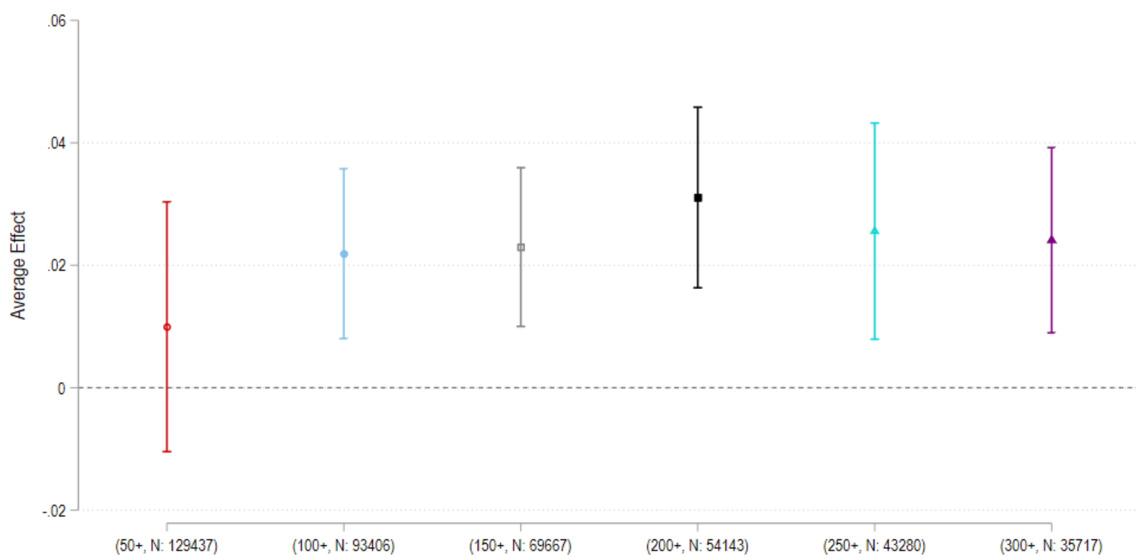
(b) Female workers



(c) Male workers



(d) Extensive margin



Notes: These figures show the impact on workers of firms in a state after that state allowed the employment of women at night. The figure plots the estimated average effect of the night shift changes for firms of different sizes: with permanent employees of at least 50, 100, 150, 200, 250, and 300, respectively. Share of female workers is defined as the number of female workers in the firm divided by the sum of male and female workers, and extensive margin is measured by a binary variable which is 1 whenever the firm has at least one female worker and 0 otherwise. The estimation includes firm and industry-year fixed effects. The bars show the 95% confidence interval for the estimates.

Source: Data on firms from a panel version of the Annual Survey of Industries from 2009 to 2018.

Table 1: Timing and nature of night shift changes across states (till 2018)

State name	**Night shift year**	**Type of change**	**Source**
Punjab	2014	Legislative	Newspaper article
Andhra Pradesh	2015	Executive	Newspaper article
Maharashtra	2015	Legislative	Amendment
Assam	2016	Executive	Amendment
Haryana	2017	Executive	Article and Application form
Himachal Pradesh	2017	Executive	Government notification
Uttar Pradesh	2017	Legislative	Gazette notification

Notes: This table reports the year in which states made amendments to the regulation of night shift work for women in factories. We report the year of the amendment, whether it was carried out using an executive order or a legislative change, and the source for the information. We exclude 2 states that also implemented changes to night shift legislation due to the following reasons: Tamil Nadu is excluded because the timing of its regulatory changes is uncertain, as reports suggest that females were employed on night shifts in manufacturing units in Tiruppur as early as 2000, after the Madras High Court judgment allowing females to work at night was passed (Vas, 2000; Communist Party of India (Marxist-Leninist) Liberation, 2014); and Madhya Pradesh is excluded as it implemented changes to restrictions on night shift work for women through an amendment, but also implemented other major changes to its labor laws during the same time period. Goa and Karnataka implemented night shift changes in 2019, which is after the period of analysis in this study, and so they remain untreated during our analysis.

Table 2: Summary statistics for states with and without night shift changes

	(1)	(2)	(3)	(4)	(5)
Variable	Mean: Untreated	Mean: Treated	Difference	SE	p-value
Panel A: Main dependent variables					
Total manufacturing workers	26.44	22.13	-4.314	0.309	0.000
Share of female workers (%)	10.45	7.983	-2.470	0.200	0.000
Share of female workdays (%)	10.45	7.935	-2.518	0.200	0.000
Share of firms with female workers (%)	24.48	22.02	-2.455	0.396	0.000
Panel B: Other firm variables					
Total permanent employees	35.25	29.78	-5.468	0.494	0.000
Female wage rate (per workday)	112.1	113.9	1.770	1.820	0.331
Male wage rate (per workday)	139.6	142.3	2.695	6.201	0.664
Wage rate (per workday)	130.1	133.7	3.605	5.595	0.519
Share of exported products (%)	2.417	3.421	1.004	0.129	0.000
Panel C: Firm productivity					
Total output (Millions)	115.4	97.80	-17.59	2.589	0.000
Total profit (Millions)	10.43	8.049	-2.379	0.409	0.000
Capital stock (Millions)	26.04	23.49	-2.550	0.772	0.001
Panel D: State characteristics					
Unemployment rate	0.0463	0.0239	-0.0224	0.013	0.096
Female unemployment rate	0.0870	0.0385	-0.0485	0.025	0.067

Notes: This table presents summary statistics for all firms. We use unique observations for every firm in the pre-treatment period by taking each firm's most recent observation prior to the year of treatment for treated states and before the first year of treatment for untreated states. The share of female workers is defined as total female workers in permanent employment out of total male and female workers. Share of female workdays are calculated as the number of female workdays divided by the sum of male and female workdays. Total permanent employees includes total manufacturing workers, supervisors, and other employees. Wages are deflated using the Consumer Price Index (CPI) with 2004 as the base year. Output is deflated by two-digit industry-specific Wholesale Price Index (WPI) with 2004 as the base year.
Source: Data on firms is from a panel version of the Annual Survey of Industries from 2009 to 2018. State-level unemployment rate is calculated from the 68th round of the NSS Employment-Unemployment Survey (2011-12).

Table 3: Impact of night shift changes on the employment of female workers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Share Female		Female Workers		Male Workers		Extensive Margin	
Panel A: All firms								
Post × NightShift	-0.1845	-0.0831	0.4064	0.4703	-1.0741	-0.6012	0.0019	0.0032
	(0.3108)	(0.2083)	(0.3538)	(0.3826)	(1.4356)	(1.1476)	(0.0127)	(0.0085)
Observations	292835	292835	292835	292835	292835	292835	292835	292835
Mean	8.518	8.518	5.783	5.783	43.397	43.397	0.236	0.236
Panel B: Heterogeneity by firm size								
Post × NightShift	-0.197	-0.080	0.006	0.091	-0.587	-0.294	0.001	0.002
	(0.326)	(0.212)	(0.167)	(0.216)	(0.855)	(0.648)	(0.013)	(0.008)
Post × NightShift × Large	0.557	0.472*	5.565	5.635*	-5.863	-2.741	0.021**	0.021***
	(0.371)	(0.249)	(3.334)	(2.936)	(9.283)	(7.932)	(0.010)	(0.007)
Observations	265617	265612	265617	265612	265617	265612	265617	265612
Mean	8.518	8.518	5.780	5.781	43.367	43.369	0.236	0.236
Panel C: Large firms only								
Post × NightShift	0.360	0.388*	5.571	6.069**	-6.450	2.984	0.022*	0.026***
	(0.295)	(0.204)	(3.387)	(2.407)	(9.808)	(7.756)	(0.011)	(0.009)
Observations	43422	43263	43422	43263	43422	43263	43422	43263
Mean	11.233	11.187	48.202	48.156	325.796	326.524	0.401	0.401
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Industry-Year FE	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The table shows the impact on female workers in firms in a state after the state allowed female workers to work on night shifts. *Post × NightShift* is 1 for states that implemented the amendments to night shift regulations in the year when they implement the change and all years after that, and 0 otherwise. Large firms are firms that employ 250 workers or more. The dependent variable in columns (1)-(2) is the number of female workers in the firm divided by the sum of male and female workers, for columns (3)-(4) and (5)-(6) it is the number of female and male workers respectively, and in columns (7)-(8) it is a binary variable which is 1 whenever the firm has at least one female worker and 0 otherwise. Mean refers to the mean of the dependent variable before the first treatment year. The estimation includes firm fixed effects in all columns, years in odd-numbered columns, and industry-year fixed effects in even-numbered columns. In Panel B, we also control for the interaction between firm size and post treatment, as well as the interaction between firm size and treated states. Each column reports the effective number of observations after incorporating the included fixed effects. Standard errors in parentheses are clustered at the state level. ***, **, * show significance at 1%, 5% and 10%, respectively.
Source: Data on firms from a panel version of the Annual Survey of Industries from 2009 to 2018.

Table 4: Impact of night shift changes: Heterogeneity by baseline female employment

	(1) Share Female	(2) Female Workers	(3) Male Workers
Panel A: Large firms, heterogeneity by baseline female workers (binary)			
Post × NightShift × Baseline Female Workers = 0	0.089	1.380	-3.256
	(0.162)	(0.883)	(7.037)
Post × NightShift × Baseline Female Workers > 0	0.651*	10.390**	9.084
	(0.351)	(4.116)	(8.836)
Test for Equality (p-value)	0.161	0.043	0.054
Observations	43252	43252	43252
R-Squared	0.941	0.902	0.864
Mean	11.187	48.156	326.524
Panel B: Large firms, heterogeneity by baseline female workers (continuous)			
Post × NightShift	0.324	2.157	0.227
	(0.310)	(1.834)	(7.027)
Post × NightShift × Baseline Female Workers	-0.001	0.054	0.054
	(0.004)	(0.032)	(0.048)
Observations	43252	43252	43252
R-Squared	0.941	0.904	0.864
Mean	11.187	48.156	326.524
Firm FE	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes

Notes: The table shows the impact on female workers in firms in a state after the state allowed female workers to work on night shifts. *Post × NightShift* is 1 for states that implemented the amendments to night shift regulations in the year when they implement the change and all years after that, and 0 otherwise. The table reports heterogeneity by baseline female workers in the firm, defined as above/below the median for large firms in Panel A, and the baseline number in Panel B. The median for baseline number of female workers is 1. Large firms are firms that hire 250 workers or more. The dependent variable in column (1) is the number of female workers in the firm divided by the sum of male and female workers, for columns (2) and (3) it is the number of female and male workers respectively. Mean refers to the mean of the dependent variable before the first treatment year. The estimation includes fixed effects for individual firms and industry-year. Each column reports the effective number of observations after incorporating the included fixed effects. We also control for the interaction between the baseline female employment and post treatment, as well as the interaction between baseline female employment and treated states. Standard errors in parentheses are clustered at the state level. ***, **, * show significance at 1%, 5% and 10%, respectively.
Source: Data on firms from a panel version of the Annual Survey of Industries from 2009 to 2018.

Table 5: Impact of night shift changes on the employment of female workers: Heterogeneity by baseline export share at industry level in firms

	(1) Share Female	(2) Female Workers	(3) Male Workers	(4) Extensive
Panel A: Large firms, heterogeneity by baseline share of exported products at industry level				
Post × NightShift × Below Median	-0.792**	-2.061	-10.214	0.008
	(0.315)	(1.562)	(9.724)	(0.016)
Post × NightShift × Above Median	0.587**	7.448**	5.178	0.028***
	(0.235)	(2.850)	(8.276)	(0.009)
Test for Equality	0.001	0.009	0.131	0.251
Observations	43258	43258	43258	43258
R-Squared	0.941	0.902	0.864	0.788
Mean	11.184	48.144	326.525	0.401
Panel B: Large firms, heterogeneity by continuous export share at industry level				
Post × NightShift	-0.175	0.221	-8.019	0.023*
	(0.281)	(3.163)	(6.895)	(0.012)
Post × NightShift × Share Export	0.085**	0.868***	1.598***	0.000
	(0.034)	(0.295)	(0.480)	(0.001)
Observations	43258	43258	43258	43258
R-Squared	0.941	0.902	0.864	0.788
Mean	11.184	48.144	326.525	0.401
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes

Notes: The table shows the impact on female workers in firms in a state after the state allowed hiring of women in night shifts. *Post* × *NightShift* is 1 for states that implemented the amendments to night shift regulations in the year when they implement the change and all years after that, and 0 otherwise. The table reports heterogeneity by the baseline share of exported products at the industry level. The median value for the baseline share of exported products is 2.8%. The baseline share of female workers for firms in industries with above median export share is 14.32 and for firms in industries with below median export share is 7.28. Large firms are firms that hire 250 workers or more. The dependent variable in column (1) is the number of female workers in the firm divided by the sum of male and female workers, for columns (2) and (3) it is the number of female and male workers respectively, and in column (4) it is a binary variable which is 1 whenever the firm has at least one female worker and 0 otherwise. Mean refers to the mean of the dependent variable before the first treatment year. The estimation includes fixed effects for individual firms and industry-year. We also control for the interaction between the baseline export share and post treatment, as well as the interaction between baseline export share and treated states. Each column reports the effective number of observations after incorporating the included fixed effects. Standard errors in parentheses are clustered at the state level. ***, **, * show significance at 1%, 5% and 10%, respectively.
Source: Data on firms from a panel version of the Annual Survey of Industries from 2009 to 2018.

Table 6: Impact of night shift changes on the employment of female workers: Heterogeneity by unemployment rate

	(1) Share Female	(2) Female Workers	(3) Male Workers	(4) Extensive
Panel A: Large firms, heterogeneity by overall unemployment (NSS)				
Post × NightShift	-0.272	-16.093*	-18.035	-0.001
	(0.626)	(8.232)	(13.428)	(0.018)
Post × NightShift × Unemployment Rate	0.348	10.944**	10.232	0.013
	(0.353)	(4.296)	(7.000)	(0.011)
Observations	43263	43263	43263	43263
R-Squared	0.941	0.902	0.864	0.788
Mean	11.1871	48.1565	326.5238	0.4006
Panel B: Large firms, heterogeneity by female unemployment (NSS)				
Post × NightShift	-0.045	-4.237	0.981	-0.007
	(0.291)	(3.730)	(10.142)	(0.010)
Post × NightShift × Female Unemployment Rate	0.149	3.083**	0.452	0.011**
	(0.101)	(1.488)	(2.208)	(0.004)
Observations	43263	43263	43263	43263
R-Squared	0.941	0.902	0.864	0.788
Mean	11.1871	48.1565	326.5238	0.4006
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes

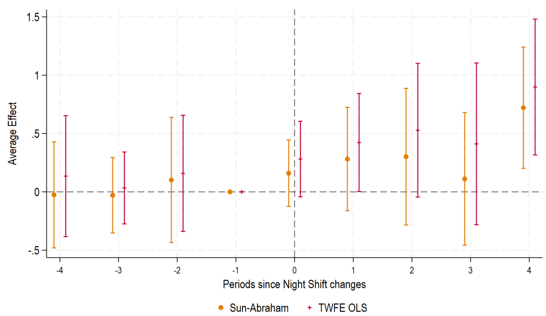
Notes: The table shows the impact on female workers in firms in a state after the state allowed hiring of women in night shifts. *Post × NightShift* is 1 for states that implemented the amendments to night shift regulations in the year when they implement the change and all years after that, and 0 otherwise. The table reports heterogeneity by a state's baseline unemployment rate from NSS 2011-12, overall unemployment rate in Panel A and female unemployment rate in Panel B. The median overall unemployment rate across states is 2.3 and median female unemployment rate is 3.5. Large firms are firms that hire 250 workers or more. The dependent variable in column (1) is the number of female workers in the firm divided by the sum of male and female workers, for columns (2) and (3) it is the number of female and male workers respectively, and in column (4) it is a binary variable which is 1 whenever the firm has at least one female worker and 0 otherwise. Mean refers to the mean of the dependent variable before the first treatment year. The estimation includes fixed effects for individual firms and industry-year. Each column reports the effective number of observations after incorporating the included fixed effects. We also control for the interaction between the unemployment rate and post treatment, as well as the interaction between unemployment rate and treated states. Standard errors in parentheses are clustered at the state level. ***, **, * show significance at 1%, 5% and 10%, respectively.

Source: Data on firms from a panel version of the Annual Survey of Industries from 2009 to 2018. State-level unemployment rate calculated from the 68th round of the NSS Employment-Unemployment Survey (2011-12).

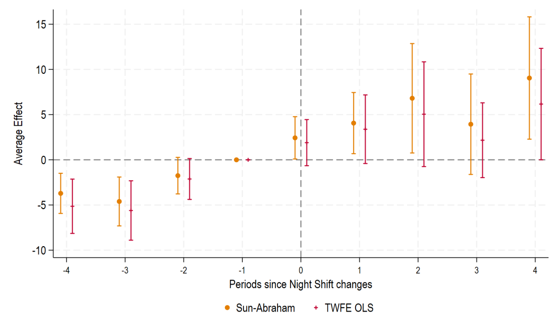
A Appendix Figures and Tables

Figure A.1: Event study estimates for the impact of night shift changes on the employment of female workers: Large firms (Sun and Abraham)

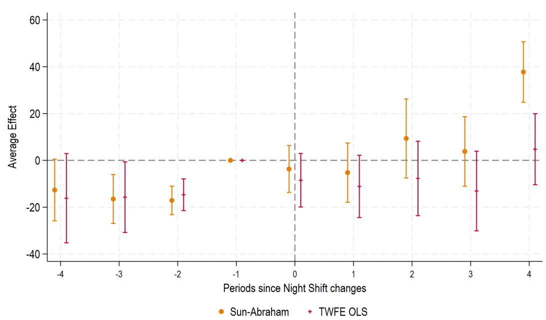
(a) Share of female workers



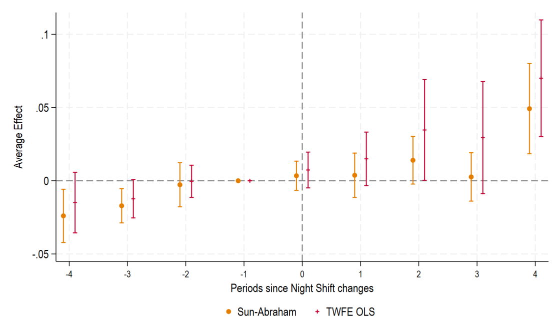
(b) Female workers



(c) Male workers



(d) Extensive margin



Notes: These figures show the impact on workers of large firms in a state after that state allowed the employment of women at night. The regulatory change was implemented in year 0. The figure plots the estimated effect of night shift changes relative to the year before the change was implemented. For control states, the pre-treatment period is before 2014, when the first regulatory change was implemented. The sample comprises large firms, which are firms that employ at least 250 permanent employees. Share of female workers is defined as the number of female workers in the firm divided by the sum of male and female workers, and extensive margin is measured by a binary variable which is 1 whenever the firm has at least one female worker and 0 otherwise. For all figures, we show the results from two different estimators: the standard two-way fixed effects OLS, and the event study framework from Sun & Abraham (2021). The estimation includes firm and industry-year fixed effects. The bars show the 95% confidence interval for the estimates.

Source: Data on firms is from a panel version of the Annual Survey of Industries from 2009 to 2018.

Table A.1: Dynamic ATT estimates (Sun & Abraham, 2021) for impact of Night Shift changes on the employment of female workers

	(1) Share Female	(2) Female Workers	(3) Male Workers	(4) Extensive
	Large firms			
Post × NightShift	0.314	5.251**	8.395	0.015*
	(0.194)	(2.318)	(6.139)	(0.007)
Observations	43263	43263	43263	43263
Adjusted R-Squared	0.931	0.884	0.839	0.750
Mean	11.187	48.156	326.524	0.401
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes

Notes: The table shows the impact on female workers in firms in a state after the state allowed hiring of women in night shifts, using the Robust Staggered DID Estimator proposed by Sun & Abraham (2021). *Post* × *NightShift* is 1 for states that implemented the amendments to night shift regulations in the year when they implement the change and all years after that, and 0 otherwise. The sample is large firms with more than 250 workers. The dependent variable in column (1) is the number of female workers in the firm divided by the sum of male and female workers, for columns (2) and (3) it is the number of female and male workers respectively, and in column (4) it is a binary variable which is 1 whenever the firm has at least one female worker and 0 otherwise. Mean refers to the mean of the dependent variable before the first treatment year. The estimation includes Fixed Effects for individual firms and years. Each column reports the effective number of observations after incorporating the included fixed effects. Standard errors in parentheses are clustered at the state level. ***, **, * show significance at 1%, 5% and 10%, respectively.
Source: Data on firms from a panel version of the Annual Survey of Industries from 2009 to 2018.

Table A.2: Impact of night shift changes on the employment of female workers: Heterogeneity by baseline firm size

	(1) Share Female	(2) Female Workers	(3) Male Workers	(4) Extensive
	Heterogeneity by continuous baseline size			
Post × NightShift	-0.091	-0.144	-1.737	0.000
	(0.214)	(0.385)	(1.053)	(0.009)
Post × NightShift × Baseline Firm Size	0.066**	0.927***	1.582**	0.005***
	(0.024)	(0.202)	(0.643)	(0.001)
Post × NightShift × Baseline Firm Size Square	-0.001*	-0.014***	-0.035***	-0.000***
	(0.001)	(0.004)	(0.012)	(0.000)
Observations	265629	265629	265629	265629
R-Squared	0.863	0.884	0.909	0.782
Mean	8.5181	5.7828	43.3987	0.2364
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes

Notes: The table shows the impact on female workers in firms in a state after the state allowed hiring of women in night shifts. *Post × NightShift* is 1 for states that implemented the amendments to night shift regulations in the year when they implement the change and all years after that, and 0 otherwise. The table reports heterogeneity by baseline firm size, defined as the size of the firm in the latest year it is observed prior to the treatment. The dependent variable in column (1) is the number of female workers in the firm divided by the sum of male and female workers, for columns (2) and (3) it is the number of female and male workers respectively, and in column (4) it is a binary variable which is 1 whenever the firm has at least one female worker and 0 otherwise. Mean refers to the mean of the dependent variable before the first treatment year. The estimation includes fixed effects for individual firms and industry-year. Each column reports the effective number of observations after incorporating the included fixed effects. Standard errors in parentheses are clustered at the state level. ***, **, * show significance at 1%, 5% and 10%, respectively.
Source: Data on firms from a panel version of the Annual Survey of Industries from 2009 to 2018.

Table A.3: Impact of night shift changes on the employment of female workers: Robustness to outcome variable definition

	(1) Share Female	(2) Female Workers	(3) Male Workers
Panel A: Log transformation			
Post $\times$ NightShift	0.0614**	0.1263***	0.0369
	(0.0232)	(0.0357)	(0.0342)
Observations	43263	43263	43263
Mean	11.187	48.156	326.524
Panel B: Workdays in firm			
Post $\times$ NightShift	0.378*	2163.804**	692.015
	(0.202)	(853.025)	(2701.385)
Observations	43263	43263	43263
Mean	11.436	1.4e+04	1.0e+05
Firm FE	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes

Notes: The table shows the impact on female workers in firms in a state after the state allowed hiring of women in night shifts. *Post* $\times$ *NightShift* is 1 for states that implemented the amendments to night shift regulations in the year when they implement the change and all years after that, and 0 otherwise. Large firms are firms that hire 250 workers or more. In Panel A, we take the log transformation of the variables after adding a value of 1 to deal with zeros. In Panel B, we use the number of workdays in the firm instead of workers, with the share of female workdays and total female and male workdays as outcomes. For Panel A, the dependent variable in column 1 is the number of female workers in the firm divided by the sum of male and female workers, for columns 2 and 3, it is the number of female and male workers respectively. Mean refers to the mean of the dependent variable before the first treatment year. The estimation includes fixed effects for individual firms and industry-year fixed effects. Each column reports the effective number of observations after incorporating the included fixed effects. Standard errors in parentheses are clustered at the state level. ***, **, * show significance at 1%, 5% and 10%, respectively.

Source: Data on firms from a panel version of the Annual Survey of Industries from 2009 to 2018.

Table A.4: Impact of night shift changes on the employment of female workers (Bootstrappped)

	(1) Share Female	(2) Female Workers	(3) Male Workers	(4) Extensive
Panel A: Large firms				
Post × NightShift	0.388***	6.069***	2.984	0.0255***
	(0.135)	(0.950)	(2.868)	(0.00599)
Observations	43263	43263	43263	43263
Mean	11.187	48.156	326.524	0.401
States	26	26	26	26
Panel B: All firms, heterogeneity by firm size				
Post × NightShift	-0.0800	0.0909	-0.294	0.00235
	(0.182)	(0.109)	(0.310)	(0.00478)
Post × NightShift × Large	0.472**	5.635***	-2.741	0.0208***
	(0.230)	(0.847)	(2.832)	(0.00698)
Observations	265612	265612	265612	265612
Mean	8.518	5.781	43.369	0.236
States	28	28	28	28
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes

Notes: The table shows the impact on female workers in firms in a state after the state allowed hiring of women in night shifts. *Post* × *NightShift* is 1 for states that implemented the amendments to night shift regulations in the year when they implement the change and all years after that, and 0 otherwise. Large firms here are firms that hire 250 workers or more. The dependent variable in column (1) is the number of female workers in the firm divided by the sum of male and female workers, for columns (2) and (3) it is the number of female and male workers respectively, and in column (4) it is a binary variable which is 1 whenever the firm has at least one female worker and 0 otherwise. Mean refers to the mean of the dependent variable before the first treatment year. The estimation includes Fixed Effects for individual firms and industry-year. For Panel B, we also include fixed effects for the interaction between firm size and years, as well as the interaction between firm size and treated states. Standard errors in parentheses are clustered at the state level and bootstrapped with state-level sampling and 100 replications. ***, **, * show significance at 1%, 5% and 10%, respectively.
Source: Data on firms from a panel version of the Annual Survey of Industries from 2009 to 2018.

Table A.5: Impact of night shift changes on the employment of female workers: Removing states with exemption applications

	(1) Share Female	(2) Female Workers	(3) Male Workers	(4) Extensive
Panel A: Large firms				
Post × NightShift	0.411**	5.533***	3.200	0.0297***
	(0.185)	(1.082)	(3.272)	(0.00703)
Observations	35910	35910	35910	35910
Mean	13.026	56.019	324.769	0.428
States	24	24	24	24
Panel B: All firms, heterogeneity by firm size				
Post × NightShift	-0.0459	0.135	-0.699**	0.00628
	(0.225)	(0.156)	(0.341)	(0.00537)
Post × NightShift × Large	0.587**	5.528***	-3.066	0.0238***
	(0.266)	(1.221)	(3.014)	(0.00810)
Observations	217949	217949	217949	217949
Mean	10.119	6.676	42.203	0.268
States	26	26	26	26
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes

Notes: The table shows the impact on female workers in firms in a state after the state allowed hiring of women in night shifts. *Post × NightShift* is 1 for states that implemented the amendments to night shift regulations in the year when they implement the change and all years after that, and 0 otherwise. Large firms here are firms that hire 250 workers or more. As a robustness check for the main results, we exclude two states which allowed factories to hire women in night shifts but only after applying for an exemption (Haryana and West Bengal). The dependent variable in column (1) is the number of female workers in the firm divided by the sum of male and female workers, for columns (2) and (3) it is the number of female and male workers respectively, and in column (4) it is a binary variable which is 1 whenever the firm has at least one female worker and 0 otherwise. Mean refers to the mean of the dependent variable before the first treatment year. The estimation includes Fixed Effects for individual firms and industry-year. For Panel B, we also include fixed effects for the interaction between firm size and years, as well as the interaction between firm size and treated states. Standard errors in parentheses are clustered at the state level and bootstrapped with state-level sampling and 100 replications. ***, **, * show significance at 1%, 5% and 10%, respectively.
Source: Data on firms from a panel version of the Annual Survey of Industries from 2009 to 2018.

Table A.6: Impact of night shift changes on the employment of female workers: Robustness to years used in analysis

	(1) Share Female	(2) Female Workers	(3) Male Workers	(4) Extensive
	Panel A: Keeping years till 2016			
Post × NightShift	0.3764*	3.1876**	1.2797	0.0316**
	(0.2197)	(1.4242)	(7.5177)	(0.0115)
Observations	34375	34375	34375	34375
Mean	11.866	51.767	328.360	0.407
	Panel B: Keeping years 2000-2018			
Post × NightShift	0.596**	7.532***	4.998	0.033***
	(0.264)	(2.541)	(9.270)	(0.011)
Observations	58571	58571	58571	58571
Mean	11.344	47.173	317.217	0.382
	Panel C: Firm size defined using years till 2013			
Post × NightShift	0.311*	4.795**	-0.663	0.024**
	(0.177)	(2.231)	(6.870)	(0.009)
Observations	42239	42239	42239	42239
Mean	11.468	49.712	332.071	0.403
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes

Notes: The table shows the impact on female workers in firms in a state after the state allowed hiring of women in night shifts. *Post* × *NightShift* is 1 for states that implemented the amendments to night shift regulations in the year when they implement the change and all years after that, and 0 otherwise. Large firms are firms that hire 250 workers or more. In Panel A, we only keep years till 2016 in the analysis, and consequently, only states which had amendments till 2016 are treated. This is to test for robustness against the introduction of the Maternity Benefits (Amendment) Act in 2017. In Panel B, we keep all years for ASI (2000-2018). In Panel C, we define firm size by using years till 2013 for all states, instead of taking the latest pre-treatment year for treated states. The dependent variable in column 1 is the number of female workers in the firm divided by the sum of male and female workers, for columns 2 and 3, it is the number of female and male workers respectively, and in columns 4 it is a binary variable which is 1 whenever the firm has at least one female worker and 0 otherwise. Mean refers to the mean of the dependent variable before the first treatment year. The estimation includes fixed effects for individual firms and industry-year fixed effects. Each column reports the effective number of observations after incorporating the included fixed effects. Standard errors in parentheses are clustered at the state level. ***, **, * show significance at 1%, 5% and 10%, respectively.

Source: ASI (2009 to 2018) in Panels A and C. ASI (2000 to 2018) in Panel B.

Table A.7: Impact of night shift changes on the employment of female workers: Heterogeneity by baseline export share at industry level in firms and baseline female employment

	(1) Share Female	(2) Female Workers	(3) Male Workers	(4) Extensive
Post × NightShift	-0.973***	-5.283*	-14.540	0.005
	(0.327)	(2.601)	(9.575)	(0.020)
Post × NightShift × Above Median Export Share	1.295***	8.132**	13.671	0.020
	(0.373)	(3.038)	(9.825)	(0.017)
Post × NightShift × Baseline Female Employment > 0	0.473	8.451**	11.486*	0.008
	(0.383)	(4.088)	(6.026)	(0.024)
Test for Equality (p-value)	0.132	0.910	0.842	0.709
Observations	43247	43247	43247	43247
R-Squared	0.941	0.902	0.864	0.790
Mean	11.184	48.144	326.525	0.401
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes

Notes: The table shows the impact on female workers in firms in a state after the state allowed hiring of women in night shifts. *Post × NightShift* is 1 for states that implemented the amendments to night shift regulations in the year when they implement the change and all years after that, and 0 otherwise. The table reports heterogeneity by baseline share of exported products at the industry level. The median value for the baseline share of exported products is 2.8%. The baseline share of female workers for firms in industries with above median export share is 14.32 and for firms in industries with below median export share is 7.28. Large firms are firms that hire 250 workers or more. The dependent variable in column (1) is the number of female workers in the firm divided by the sum of male and female workers, for columns (2) and (3) it is the number of female and male workers respectively, and in column (4) it is a binary variable which is 1 whenever the firm has at least one female worker and 0 otherwise. Mean refers to the mean of the dependent variable before the first treatment year. The estimation includes fixed effects for individual firms and industry-year. We also control for the interaction between the baseline export share and post treatment, as well as the interaction between baseline export share and treated states. Each column reports the effective number of observations after incorporating the included fixed effects. Standard errors in parentheses are clustered at the state level. ***, **, * show significance at 1%, 5% and 10%, respectively.
Source: Data on firms from a panel version of the Annual Survey of Industries from 2009 to 2018.

Table A.8: Impact of Night Shift changes on wages in firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total Wage Bill			Wage Rate			Labour Share of Expenditure
	Female	Male	Overall	Female	Male	Overall	
Panel A: All firms, heterogeneity by firm size							
Post $\times$ NightShift	-0.039	-0.004	0.029	-2.429	4.033	2.756	-9.854
	(0.109)	(0.413)	(0.492)	(5.640)	(3.257)	(3.279)	(6.654)
Post $\times$ NightShift $\times$ Large	2.486**	2.551	3.228	-10.731	-1.058	-0.862	11.003
	(1.117)	(6.375)	(7.225)	(13.766)	(4.654)	(2.202)	(8.868)
Observations	265612	265612	265612	67921	263698	265611	265610
R-Squared	0.847	0.913	0.911	0.195	0.591	0.594	0.265
Mean	2.069	26.286	28.872	131.262	149.828	139.188	6.917
Panel B: Large firms							
Post $\times$ NightShift	2.795***	8.131	10.030	-15.338	3.119	0.980	0.031
	(0.758)	(6.351)	(6.796)	(9.647)	(4.494)	(2.337)	(0.648)
Observations	43263	43263	43263	17725	43169	43263	43261
R-Squared	0.854	0.883	0.877	0.571	0.586	0.628	0.305
Mean	19.197	245.017	272.286	207.083	236.600	205.573	9.308
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the impact on wages paid in firms in a state after the state allowed hiring of women in night shifts. *Post* $\times$ *NightShift* is 1 for states that implemented the amendments to night shift regulations in the year when they implement the change and all years after that, and 0 otherwise. The categories of firm sizes are: Micro (Less than 10 workers), Small (10-50 workers), Medium (50-250 workers), and Large (250+ workers). The dependent variable in columns (1) and (2) is the total wages paid to female and male workers respectively, in hundred thousands of rupees. The dependent variable in columns (3) and (4) is the wage rate paid to female and male workers respectively, defined as total female/male wages divided by total number of work days done by female/male workers. The dependent variable for column (5) is the overall wage rate for workers, defined as the total wages paid to workers divided by the total workdays. Mean refers to the mean of the dependent variable before the first treatment year. The estimation includes Fixed Effects for individual firms and industry-year. For Panel A, we also control for the interaction between firm size and post treatment, as well as the interaction between firm size and treated states. Each column reports the effective number of observations after incorporating the included fixed effects. Standard errors in parentheses are clustered at the state level. ***, **, * show significance at 1%, 5% and 10%, respectively.
Source: Data on firms from a panel version of the Annual Survey of Industries from 2009 to 2018.

Table A.9: Impact of Night Shift changes on wage rate in firms without firm FE

	(1)	(2)	(3)
	Wage Rate: Without Firm FE		
	Female	Male	Overall
Panel A: All firms, heterogeneity by firm size			
Post $\times$ NightShift	-1.580	13.675	11.549
	(2.936)	(10.255)	(10.171)
Post $\times$ NightShift $\times$ Large	-21.230**	-16.383	-14.870*
	(9.459)	(9.764)	(7.985)
Observations	72288	258837	260478
R-Squared	0.024	0.011	0.009
Mean	128.719	149.884	139.321
Panel B: Large firms			
Post $\times$ NightShift	-31.942**	-3.392	-5.401
	(12.210)	(4.498)	(3.335)
Observations	17823	42639	42728
R-Squared	0.290	0.230	0.230
Mean	210.202	237.389	206.160
State FE	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes

Notes: The table shows the impact on wages paid in firms in a state after the state allowed hiring of women in night shifts. *Post* $\times$ *NightShift* is 1 for states that implemented the amendments to night shift regulations in the year when they implement the change and all years after that, and 0 otherwise. The categories of firm sizes are: Micro (Less than 10 workers), Small (10-50 workers), Medium (50-250 workers), and Large (250+ workers). The dependent variable in column (1)-(3) is the wage rate (defined as total wages paid divided by number of workdays) paid to female workers, male workers, and all workers respectively. Mean refers to the mean of the dependent variable before the first treatment year. The estimation includes Fixed Effects for individual firms and industry-year. For Panel A, we also control for the interaction between firm size and post treatment, as well as the interaction between firm size and treated states. Each column reports the effective number of observations after incorporating the included fixed effects. Standard errors in parentheses are clustered at the state level. ***, **, * show significance at 1%, 5% and 10%, respectively.
Source: Data on firms from a panel version of the Annual Survey of Industries from 2009 to 2018.

Table A.10: Impact of night shift changes on other firm outcomes

	(1) Total Workers	(2) Total Output	(3) Total Profit	(4) Capital Stock
Panel A: All firms, heterogeneity by firm size				
Post × NightShift	-0.203	-0.007	0.373	-0.028
	(0.692)	(0.023)	(0.256)	(0.041)
Post × NightShift × Large	2.893	-0.031	-0.856	-0.009
	(9.595)	(0.051)	(0.559)	(0.068)
Observations	265612	265612	265612	265609
R-Squared	0.912	0.947	0.563	0.897
Mean	49.149	17.205	11.251	15.143
Panel B: Large firms				
Post × NightShift	9.054	-0.021	-0.371	-0.045
	(9.127)	(0.035)	(0.545)	(0.038)
Observations	43263	43263	43263	43261
R-Squared	0.861	0.888	0.522	0.876
Mean	374.680	20.534	12.203	18.750
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes

Notes: The table shows the impact on production in firms in a state after the state allowed hiring of women in night shifts. *Post × NightShift* is 1 for states that implemented the amendments to night shift regulations in the year when they implement the change and all years after that, and 0 otherwise. The categories of firm sizes are: Micro (Less than 10 workers), Small (10-50 workers), Medium (50-250 workers), and Large (250+ workers). The dependent variable in column (1) is the total number of workers, which is the sum of male and female workers. For column (2) we use the firm's total output as the outcome, with a log transformation taken after adding a value of 1 to deal with zeros. For column (3), the dependent variable is the inverse sine hyperbolic transformation of the firm's profit (to deal with negative values). The dependent variable for column (4) is the log of the firm's closing value of fixed capital (with 1 added). Mean refers to the mean of the transformed dependent variable before the first treatment year. The estimation includes Fixed Effects for individual firms and industry-year. For Panel A, we also control for the interaction between firm size and post treatment, as well as the interaction between firm size and treated states. Each column reports the effective number of observations after incorporating the included fixed effects. Standard errors in parentheses are clustered at the state level. ***, **, * show significance at 1%, 5% and 10%, respectively.
Source: Data on firms from a panel version of the Annual Survey of Industries from 2009 to 2018.