Equal Access, Unequal Safety:

Women's Night Work and Crime in India [†]

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Abstract

Can expanding women's access to work compromise their safety? We study Indian states' removal of factory night-work bans using data from 2008–2024. Female employment rises by 23 percent and recorded crimes against women by 22 percent, with no corresponding increase in unrelated crimes. Recorded domestic and non-domestic violence both increase. Survey-reported sexual violence by non-partners also increases, while the rise in recorded domestic abuse may reflect greater reporting. Reforms expanding women's economic opportunities can thus have unintended safety consequences.

Keywords: female employment; protective labor regulation; violence against women; crime reporting; India.

JEL codes: J16; J78; K42.

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1 Introduction

"Protective" labor laws remain widespread: the World Bank's 2024 report identifies 77 countries that restrict women's access to night work or particular industries, including 20 that prohibit women from working at night (World Bank, 2024). These laws constrain women's economic opportunities in the name of protecting them. However, repealing them can expand access to employment without addressing the safety risks invoked to justify the restrictions. Can removing policies that discriminate against women in the labor market have unintended consequences for their safety?

We study this question in India, where harassment in public spaces is a widespread concern for women, especially in urban areas (Beaman et al., 2026). We study state-level reforms that gradually relaxed restrictions on women's night work in Indian factories. Section 66(1)(b) of the Factories Act, 1948, limited the hours during which women could work, effectively excluding them from night shifts. Several states subsequently relaxed this restriction, expanding women's access to factory jobs and allowing employers greater flexibility in organizing production. These reforms therefore allow us to examine whether removing a legal barrier to women's employment increases their economic opportunities while also affecting their safety.

The relationship between employment and violence is theoretically ambiguous, and its effects may differ within and outside the household. Outside the household, new work and travel patterns may increase women's exposure to potential perpetrators, especially at night (Borker, 2021; Chakraborty et al., 2018). In Becker's model of crime, potential perpetrators weigh the benefits of an offense against its expected costs, including the probability of detection and punishment. Greater exposure may reduce the effort required to find a potential victim, while travel through poorly monitored areas or at late hours may lower the perceived probability of detection. Violence by non-partners may therefore increase if expanded employment opportunities are not accompanied by effective protection. Formal safeguards can counteract these mechanisms, but their deterrent effect depends on implementation and enforcement.

Within the household, improved earnings opportunities may strengthen women's bargaining position and make it easier to leave an abusive relationship (Cesur et al., 2025). Employment

outside the home may also reduce time spent with an abusive partner. Conversely, changes in economic roles may generate conflict or provoke backlash from partners seeking to maintain control (Aizer, 2010; Amaral et al., 2025; Anderberg et al., 2016; Chin, 2012). The net effect on partner violence is therefore uncertain.

An empirical assessment must also account for reporting. Employment can provide the resources and autonomy needed to disclose abuse or approach the police. Therefore, recorded crime can rise because women experience more violence or because more existing violence becomes visible, or both. The first is a safety cost of the reform. The second reflects women's greater ability to seek help. Distinguishing these possibilities is important to evaluate the trade-off between opportunity and safety. We combine data on employment and police-recorded crime with household survey data, using the latter to investigate the mechanisms.

Our data cover the period 2008–2024. Ten states in the estimation sample relaxed the restriction between 2014 and 2023. We use this staggered adoption in an event-study design that allows treatment effects to differ across adoption cohorts (Sun & Abraham, 2021).

We report three main findings. First, the restriction was binding for women's factory employment. Its removal increases female employment by about 23 percent. The male employment coefficient is small and statistically insignificant. The employment response appears in the reform year and persists over the following years. Household survey data show a larger increase in recent work, in any sector, among urban women in reforming states.

Second, recorded crimes against women increase by 3.67 cases per 100,000 population, approximately 22 percent. The increase includes 1.64 cases of non-domestic violence and 2.03 domestic offenses. By contrast, non-gender violent crimes and property and economic crimes show no corresponding response. Within reforming states, the increase in non-domestic offenses is larger in districts where more women already worked in factories before the reform, and in more urban and less agricultural districts, although part of these differences predates the reform. This geographic pattern is in line with the response being stronger in areas more exposed to the economic opportunities opened by the reform.

Third, survey evidence suggests different mechanisms behind the rise in recorded domestic and non-domestic offenses. Survey-reported intimate partner violence does not increase sig-

nificantly, consistent with a reporting explanation for domestic offenses. This interpretation remains tentative, as the disclosure measure neither increases significantly in the triple-difference comparison nor distinguishes partner from non-partner violence. By contrast, survey-reported sexual violence by someone other than a husband or boyfriend increases, with larger point estimates in several groups plausibly affected by expanded work opportunities. The reform therefore coincides with increased non-partner violence, while the relative roles of incidence and reporting in domestic offenses remain less clear.

We provide evidence in line with the key assumption of the estimation strategy, i.e., that, without the reform, outcomes in treated and comparison areas would have followed similar trends. Moreover, the lack of change for other crime categories suggests that a general change in crime recording is unlikely to explain our findings. We also assess sensitivity to alternative estimators, wild cluster bootstrap inference, the exclusion of 2020, individual states, and the exclusion of comparison districts that border reforming states.

1.1 Contribution and related literature

The paper's main contribution is to examine the potential unintended safety consequences of a reform intended to improve women's economic opportunities. Night-work restrictions were justified as protection for women, yet the literature has mainly studied their effects on employment. We provide evidence on the safety concern that motivated them. The paper therefore connects two dimensions of gender inequality that are often studied separately: legal restrictions on access to employment and exposure to violence. The question is particularly consequential where legal change outpaces improvements in the conditions that allow women to work safely.

Our estimates capture the effects of relaxing night-work restrictions on factory employment and violence against women. The employment data do not identify workers' shifts, and the crime data do not record when offenses occur. Employment changes may also extend beyond night shifts: as Gupta et al. (2025) note, allowing women to work at night can facilitate production across multiple shifts, increasing demand for labor throughout the day. We therefore cannot establish whether the employment changes are concentrated in night shifts or whether the

additional violence occurs at night. Greater exposure during nighttime work and travel is a plausible mechanism (Sur, 2015; World Bank, 2023), but our estimates do not isolate its contribution.

This contribution is closely related to the literature on protective labor regulation. Hyland et al. (2020) and World Bank (2024) document persistent differences in women's legal economic rights. Historical studies show that the employment effects of protective laws depend on the restriction and the setting. Goldin (1988) finds no adverse effect of maximum-hours legislation on women's manufacturing employment share, while Kattan and Haddad (2025) find that some regulations concerning working conditions and night work increased women's employment in the United States. Other work documents employment costs of working-hours restrictions and gains from relaxing them in Taiwan and Japan (Kato & Kodama, 2014; Zveglich & van der Meulen Rodgers, 2003). Recent studies examine the political economy of protective legislation and the consequences of repealing restrictions on women's workweeks (Doepke et al., 2025; Fishback et al., 2026).

The closest study is Gupta et al. (2025), who find that relaxing night-shift bans in Indian states increased female employment in large factories. Relative to their analysis, we follow the reforms through 2024, which adds the states that reformed after 2018 and several more post-reform years, and we find that female employment rises across all factories.[1] Kaur et al. (2026) study the parallel relaxation of night-work restrictions for women in shops and service establishments after 2017 and find that it shifted women's employment toward services without changing its level. Several states amended both laws, and our crime estimates are robust to controlling for these amendments (Section 4.5). We examine a potential unintended consequence of these reforms: greater exposure to violence. Our contribution is thus to show that expanding women's economic rights can have consequences for their safety, informing debates in the many countries that continue to restrict women's access to work.

[1]Gupta et al. (2025) use the establishment panel of the ASI for 2009–2018 and seven states that reformed between 2014 and 2017, and they report effects concentrated in establishments with at least 250 workers. We use state-by-industry aggregates built from the ASI for 2009–2024, which also reflect the entry and exit of establishments, and our baseline estimator allows effects to differ across adoption cohorts. Our longer panel also covers the states that reformed after their sample period ends, namely Uttarakhand, Goa, Orissa, and Karnataka.

The second literature studies employment, economic resources, and violence against women. Aizer (2010) and Anderberg et al. (2016) show how relative labor-market opportunities shape domestic violence, while Chin (2012) emphasizes changes in exposure within the household. In India, Amaral et al. (2015) examine recorded gender-based violence following the expansion of public employment, and Amaral (2017) studies reforms to women's inheritance rights. Our setting concerns access to private factory employment, which may change both household relationships and women's work and travel patterns. Examining domestic offenses alongside other offenses against women allows us to consider these potentially different responses.

Our setting also connects to evidence that harassment and fear of violence shape women's education, employment, and travel decisions (Borker, 2021; Chakraborty et al., 2018; Siddique, 2022). Research on public transport and workplace harassment documents the costs of unsafe environments and the responses of workers and employers (Adams-Prassl et al., 2024; Aguilar et al., 2021; Folke & Rickne, 2022; Kondylis et al., 2025). Doleac and Sanders (2015) demonstrate that ambient light affects criminal activity, and Amaral et al. (2025) evaluate policing against sexual harassment in Indian public spaces. Whether expanding women's access to work in turn raises their exposure to harassment and violence has received less attention. This is the question we address.

The third literature concerns the interpretation of reported crime. In India, Iyer et al. (2012) show that greater political representation can increase recorded crimes against women through reporting. Studies of female officers and women police stations likewise demonstrate the importance of access to law enforcement (Amaral et al., 2021; Jassal, 2020; Miller & Segal, 2019). Survey evidence documents substantial underreporting to formal sources (Palermo et al., 2014), although more private survey methods do not necessarily elicit higher reports of violence (Agüero & Frisancho, 2022). We provide evidence from a labor-market reform that can alter these reporting incentives even though it does not primarily reform the police.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting and data. Section 3 presents the empirical strategy. Section 4 reports the employment and crime estimates, district heterogeneity, and evidence on incidence and reporting. Section 5 concludes.

2 Institutional Background and Data

2.1 Institutional background

During our study period, the Factories Act, 1948, regulated working conditions in registered manufacturing establishments, generally covering factories with at least ten workers using power or twenty workers without power. Section 66(1)(b) restricted women's employment to 6 a.m.–7 p.m.; states could vary these limits, but the original provision preserved a prohibition between 10 p.m. and 5 a.m. (Government of India, 1948). Such restrictions reflect a broader tension between protection from the risks of night work and equal employment opportunities (International Labour Office, 2004). The state amendments we study followed court rulings that found the prohibition discriminatory, easing concerns that their adoption reflected broader policy shifts in favor of women that could independently affect employment and violence.

State reforms relaxed the restriction while typically requiring written consent, security, female supervision, or minimum numbers of women per shift. The intervention therefore combines expanded legal access with employer obligations. Our estimates capture this package as implemented, including any effects of its safeguards.[2]

India has 28 states and 8 union territories. We exclude three states.[3] We further exclude Assam from the crime analysis.[4] The final sample contains 32 states and union territories, ten of which reform: Punjab (2014), Madhya Pradesh and Maharashtra (2016), Himachal Pradesh and Haryana (2017), Uttarakhand (2019), Orissa (2020), Uttar Pradesh (2022), and Goa and

[2]The conditions largely reproduce those set by the Madras High Court in *Vasantha R. v. Union of India* (2000), which struck down Section 66(1)(b). Beyond consent and security, they typically require employer-provided transport between home and factory, adequate lighting inside the factory and along the routes women use, and compliance with the Sexual Harassment of Women at Workplace (Prevention, Prohibition and Redressal) Act, 2013. Some states add CCTV surveillance, GPS tracking of vehicles, or regular reporting to the factory inspector.

[3]Tamil Nadu relaxed the restriction before our panel begins. Andhra Pradesh and Telangana split in 2014, during our panel, and Telangana inherited Andhra Pradesh's regime.

[4]Assam authorized night shifts for women in October 2016. We exclude it because of discontinuities in policing and crime registration from 2022 (National Crime Records Bureau, 2022, 2023). It is treated from 2016 in the factory employment analysis, and our crime results are robust to including it (Appendix Table A.9).

Karnataka (2023). Appendix Table A.1 lists amendment dates and annual treatment assignments; Appendix Figure A.1 maps adoption.

2.2 Data

The Annual Survey of Industries (ASI), 2009–2024, supplies employment by sex. It covers factories registered under the Factories Act, so the employment outcomes refer to the establishments to which the restriction applied. We use its sampling weights to construct a state-by-industry panel: log female employment and log male employment among workers employed directly by factories. These outcomes allow us to distinguish additional female hiring from changes in workforce composition.

District-level National Crime Records Bureau (NCRB) data cover 2008–2024. Our main aggregate combines rape and attempted rape, kidnapping and abduction of women, insult to modesty, and cruelty by a husband or his relatives. We distinguish domestic offenses (the last category) from non-domestic violence (the remaining categories). This classification concerns offense types and does not identify incident location, time of day, or the relationship between victim and offender. Outcomes are cases per 100,000 population. Appendix Table A.2 provides the exact definitions of these categories.

State-level mortality data provide complementary outcomes: female homicide (2014–2024), female suicides attributed to marriage-related problems, love affairs, or family problems (2014–2024), and accidental deaths (2012–2023). The NCRB's *Accidental Deaths and Suicides in India* distinguishes natural forces from other accidental causes for women and men.

We then use survey data on recent employment, violence, and disclosure based on two rounds of the National Family Health Survey: NFHS IV (2015–2016) and NFHS V (2019–2021). We construct outcome-specific samples according to module eligibility requirements and use survey weights throughout. The employment measure includes all sectors, while disclosure captures whether respondents told anyone about the violence they experienced, including relatives or friends.

To examine heterogeneity across local economic conditions, we construct a district-level measure of women's factory employment from the 2013 Economic Census microdata: women

hired in manufacturing establishments with ten or more workers, excluding household and handloom units, as a share of total district employment. We complement it with three measures from the 2011 Census: the urban share of households, the share of workers employed outside cultivation, agricultural labor, and household industry, and the female literacy rate. All four measures are defined before the earliest reform in our sample, and we split districts at the median of each.

Appendix Table A.3 reports descriptive statistics. Treated and control states are comparable along many characteristics, although, at baseline, treated areas have larger factory sectors. Appendix Figures A.2 and A.3 document geographic variation in women's factory employment and crimes against women, respectively. Figure 2 compares crimes against women in treated and control areas by year relative to reform. These descriptive patterns suggest an increase in gender-based violence in treated areas following the removal of night-work bans.

3 Empirical Strategy

We compare changes in reforming states with changes in states that do not reform during the study period, using the interaction-weighted event-study estimator of Sun and Abraham (2021). The unit is a district-year for crime and a state-by-industry-year cell for employment. Let G_s denote state s's first treatment year and $k = t - G_s$ denote event time. The district-level specification is

$$Y_{dst} = \alpha_d + \delta_t + \sum_{g \in \mathcal{G}} \sum_{\substack{k \in \mathcal{K} \\ k \neq -1}} \theta_{gk} \mathbf{1}\{G_s = g\} \mathbf{1}\{t - g = k\} + \varepsilon_{dst}, \tag{1}$$

where Y_{dst} is the outcome, α_d and δ_t are district and year fixed effects, and $\mathcal{G}$ indexes adoption cohorts. Employment models replace the district and year fixed effects with state-by-industry and industry-by-year fixed effects. Industry-by-year fixed effects absorb national shocks to individual industries, so the employment estimates compare the same industry across reforming and comparison states and do not reflect differences in industrial composition. Estimators with year rather than industry-by-year fixed effects give similar results (Section 4.5). The estimator

aggregates cohort-specific coefficients by event time, allowing effects to vary across cohorts. The reference is the year before reform. We display event times -5 to $+5$, pooling earlier and later observations in the endpoints.

Tables report post-reform averages and, for the offense decomposition, separate effects in the first two years ($k = 0, 1$) and subsequently ($k = 2, \ldots, 5$, including the final pooled bin). Different horizons can reflect different cohort composition because recent adopters contribute fewer post-reform years.

This design estimates responses to the reform. A causal interpretation requires that, absent reform, outcomes in treated and comparison states would have followed parallel trends, that anticipation does not affect the reference period, and that reforms do not generate spillovers to the comparison group. The main threat is that adoption coincides with other state-level changes affecting employment or crime. Although fixed effects absorb persistent differences across locations and common year shocks, they do not account for state-specific shocks coinciding with reform. National reforms such as the Criminal Law (Amendment) Act, 2013, and the Sexual Harassment of Women at Workplace Act, 2013 are absorbed by year fixed effects. A state policy for women would bias the estimates only if it coincided with reform in each adopting state. The heterogeneity analysis exploiting within-state variation, in Section 4.3, absorbs any such policy. Moreover, we assess these concerns using pre-reform event-study estimates, comparison crimes, sample exclusions, and the alternative estimators and robustness checks described in Section 4.5. We cluster standard errors by state and report wild cluster bootstrap inference in Appendix Table A.8.

We complement the main analysis with a stacked design that compares districts above and below the median of baseline district characteristics within the same state and year, using state-by-year fixed effects that absorb state-level shocks coinciding with reform (Section 4.3). A two-wave triple-difference design for NFHS outcomes helps distinguish incidence from reporting (Section 4.4).

4 Results

4.1 Factory employment

Table 1, column (1), shows that log female employment rises by 0.211, implying an increase of approximately 23 percent. The male employment estimate is small, positive, and statistically insignificant. The increase thus reflects additional female employment, with no detectable reduction in male employment. The absence of a male response also suggests that the estimates do not reflect a general expansion of manufacturing in reforming states. This is in line with Gupta et al. (2025), who find a 13 percent increase in female employment in large factories and no significant change in male employment.

Figure 1 shows no pre-reform coefficient for female employment significant at the 5 percent level. Female employment increases by roughly 23 percent in the first two years and 24 percent thereafter, while male employment shows no significant change at any horizon.

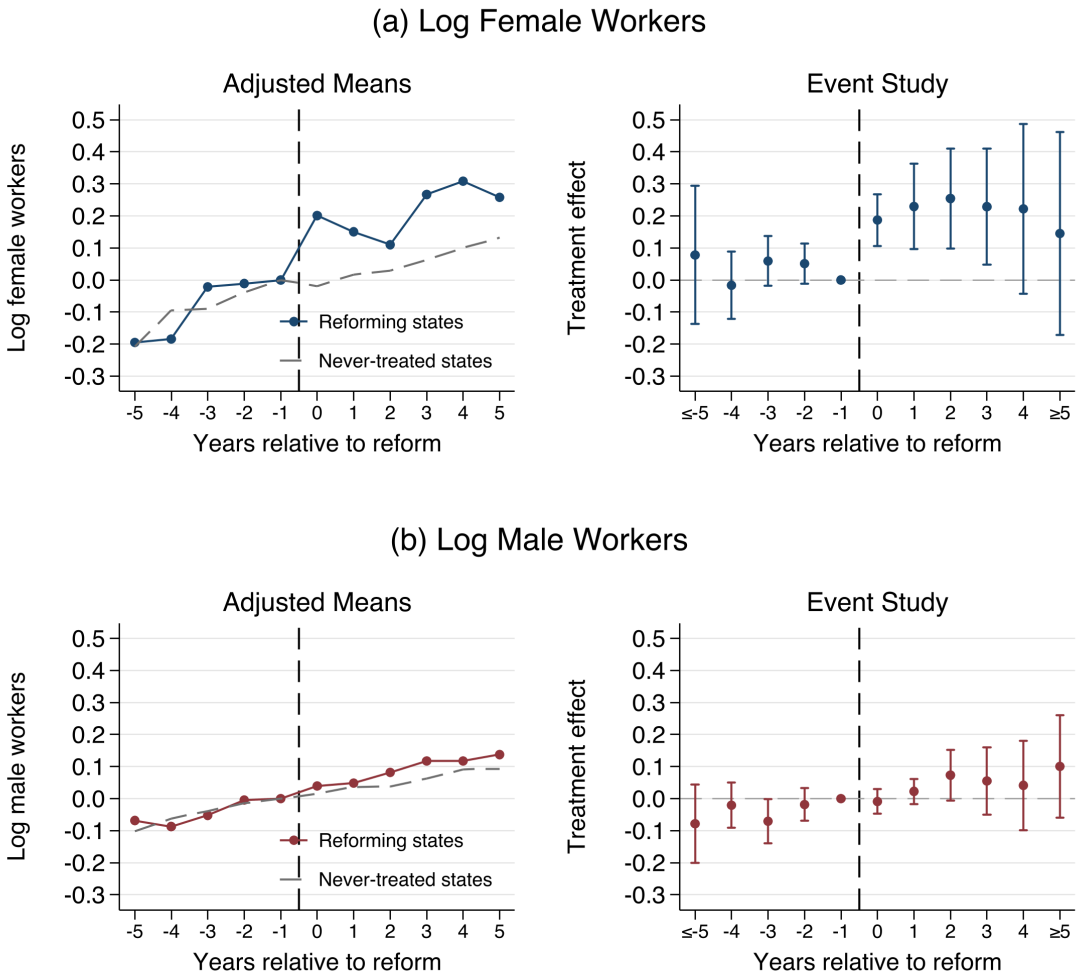
These estimates measure the number of workers in registered factories, and they leave room for the reform to change women's work in other ways. The ASI does not record shifts, so women already employed may have moved to later hours without any change in headcount. The reform may also have affected work outside registered factories. Evidence from the NFHS, presented in the next section, also points to a broader employment response: in reforming states, recent employment rose by 8 percentage points more among urban women than among rural women, relative to the same urban-rural difference in comparison states (Table 3, Column 1). Because this measure covers work in any sector, it captures employment changes beyond registered factories. The crime estimates below therefore measure the overall effect of the reform, including changes in employment, working hours, and work-related travel.

Table 1: Effect of Night Ban Removal on Factory Employment and Crime against Women

	Factory employment		Crimes against women per 100,000		
	Log female workers	Log male workers	Total	Non-domestic violence	Domestic violence
	(1)	(2)	(3)	(4)	(5)
Ban Removal	0.211**	0.047	3.668***	1.640***	2.028***
	(0.088)	(0.044)	(0.844)	(0.598)	(0.407)
Pre-trend avg.	0.043	-0.047	-0.655	-0.220	-0.435
	(0.053)	(0.037)	(0.845)	(0.482)	(0.443)
Mean dep. var.	5.352	7.915	16.343	9.653	6.690
Observations	7,960	9,698	9,359	9,359	9,359

Notes: The table reports post-reform averages from the interaction-weighted event study of Sun and Abraham (2021). Columns (1) and (2) measure the log of female and male workers employed directly by factories in state-by-industry (two-digit NIC, manufacturing) cells for 2009–2024, built with ASI weights, with state-by-industry and industry-by-year fixed effects. Columns (3) to (5) measure district crimes against women per 100,000 population for 2008–2024, with district and year fixed effects. Column (3) is the total (rape, attempt to rape, kidnapping and abduction of women, insult to modesty, and cruelty by husband and relatives), column (4) non-domestic violence (rape, attempted rape, kidnapping and abduction of women, and insult to modesty), and column (5) domestic violence (cruelty by husband and relatives). Pre-trend avg. is the mean of the event-time coefficients -5 to -2, relative to the year before reform. Standard errors are clustered by state and reported in parentheses. Asterisks denote $p < 0.10$ (*), $p < 0.05$ (**), and $p < 0.01$ (***). Sources: ASI 2009–2024; NCRB 2008–2024; Census of India 2011.

Figure 1: Night Ban Removal and Factory Employment



Notes: In panel (a) the outcome is the log of female workers and in panel (b) the log of male workers employed directly by factories, in state-by-industry (two-digit NIC, manufacturing) cells built with ASI weights. The left graphs plot adjusted means by year relative to reform for the cohorts observed over the full window (reforms up to 2019). Each cohort forms a stack with the never-treated states over the five years before and after adoption, cells are held fixed over the stack window, outcomes are net of industry-by-year effects estimated on never-treated cells, and means are expressed relative to the year before reform. The right graphs report the event studies behind Table 1, columns (1) and (2), estimated on all cohorts with the interaction-weighted estimator of Sun and Abraham (2021) with state-by-industry and industry-by-year fixed effects. Event times outside $[-5, +5]$ are pooled into the endpoints. Bars are 95 percent confidence intervals from standard errors clustered by state. Source: ASI 2009–2024.

4.2 Recorded crime against women

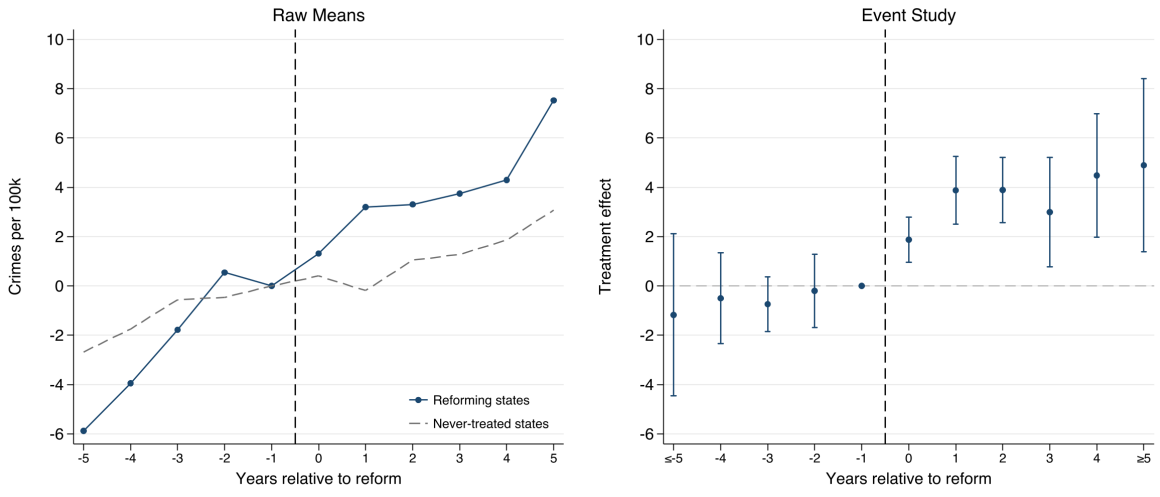
Having established that the reforms increased women's employment, we turn to their effects on recorded crimes against women. Recorded offenses increase by 3.67 cases per 100,000 population, equivalent to 22 percent of the reported mean (Table 1, column 3). This increase spans both non-domestic and domestic offenses, which rise by 1.64 and 2.03 cases per 100,000 population, respectively (columns 4 and 5), or approximately 17 and 30 percent of their means.

Both margins discussed in the introduction therefore respond.

Figure 2 shows no significant pre-reform coefficients. Recorded crime rises by approximately 1.9 cases per 100,000 in the reform year, increases further over the next two years, and remains elevated.

Appendix Table A.5 decomposes the response. In the first two years, rape and attempted rape rise by about 13 percent of their mean, kidnapping and abduction by 19 percent, and insult to modesty by 20 percent. Kidnapping and abduction remain significant at later horizons; the other estimates become less precise. Cruelty by husbands or relatives rises by 1.30 cases per 100,000 initially and 2.39 subsequently, approximately 19 and 36 percent of its mean. By contrast, non-gender violent crimes and property and economic crimes show no detectable response (Appendix Figure A.4), weighing against a general increase in crime recording. The crime response covers all women in reforming districts, whereas the employment response concerns factory workers. More than half of the increase consists of domestic offenses, and the crime data do not identify whether victims are employed, so the response is unlikely to reflect victimization of newly hired workers alone. Section 4.4 examines the role of reporting.

Figure 2: Night Ban Removal and Crimes against Women



Notes: The outcome is total crimes against women per 100,000 population in districts. The left graph plots raw means by year relative to reform for the cohorts observed over the full window (reforms up to 2019). Each cohort forms a stack with the never-treated states over the five years before and after adoption, and means are expressed relative to the year before reform. The right graph reports the event study behind Table 1, column (3), estimated on all cohorts with the interaction-weighted estimator of Sun and Abraham (2021) with district and year fixed effects. Event times outside $[-5, +5]$ are pooled into the endpoints. Bars are 95 percent confidence intervals from standard errors clustered by state. Sources: NCRB 2008–2024; Census of India 2011.

4.3 District exposure to the reform

If the crime response is connected to newly opened work opportunities, it should vary with local exposure to the affected activities. Table 2 splits districts at the median of four baseline characteristics: women's factory employment, the urban share of households, the non-agricultural share of workers, and female literacy (Section 2.2). For each half, we report the main event-study estimate. We test the difference between halves with a stacked difference-in-differences that compares districts above and below the median within the same state and year. Each adoption cohort is compared with districts remaining untreated within its event window. District-by-stack, state-by-year-by-stack, and above-median-by-year-by-stack fixed effects absorb persistent local differences, all state-year shocks, and national changes in the gap between the two halves. Appendix Figure A.5 reports the event studies for each half.

For each district characteristic, total recorded crimes against women rise in both groups after the reform, with larger increases in above-median districts. Most of this difference comes from non-domestic offenses: within the same state and year, the increase in above-median districts exceeds that in below-median districts by 1.1 to 1.5 recorded non-domestic offenses per 100,000 (Column 6). The corresponding gap for domestic offenses is smaller and close to zero when districts are split by women's factory employment or urban population share (Column 9). The event studies show a similar pattern: the two groups diverge after the reform primarily in recorded non-domestic offenses. Taken together, the increases are larger in districts where the reform plausibly had greater reach: those that are more urban, depend less on agriculture, have higher education levels, or have more women working in factories. Because these comparisons are within the same state and year, the pattern is difficult to explain solely by contemporaneous changes that affected all districts in a state equally.

Table 2: Effect of Night Ban Removal on Crime against Women by District Characteristics

	Total			Non-domestic violence			Domestic violence		
	Below	Above	Diff.	Below	Above	Diff.	Below	Above	Diff.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Women in factories									
Ban Removal	3.437***	4.037***	1.275	1.368**	1.912***	1.248*	2.068***	2.126***	0.027
	(0.762)	(0.921)	(1.006)	(0.687)	(0.592)	(0.691)	(0.310)	(0.512)	(0.530)
Pre-trend avg.	-0.797	-0.395	-1.000	0.039	-0.429	-1.315**	-0.836**	0.033	0.315
	(0.805)	(1.042)	(0.830)	(0.572)	(0.480)	(0.627)	(0.380)	(0.599)	(0.494)
Observations	4,680	4,679	23,940	4,680	4,679	23,940	4,680	4,679	23,940
Panel B. Urban household share									
Ban Removal	3.054***	4.220***	2.064*	1.040*	2.183***	1.511*	2.013***	2.037***	0.553
	(0.631)	(0.978)	(1.181)	(0.584)	(0.660)	(0.836)	(0.281)	(0.551)	(0.511)
Pre-trend avg.	-0.540	-0.641	-1.413	0.147	-0.452	-1.129*	-0.687*	-0.189	-0.284
	(0.698)	(1.231)	(1.036)	(0.466)	(0.610)	(0.600)	(0.352)	(0.680)	(0.557)
Observations	4,688	4,671	23,940	4,688	4,671	23,940	4,688	4,671	23,940
Panel C. Non-agricultural workers									
Ban Removal	2.577***	4.761***	2.149**	0.736	2.457***	1.097	1.842***	2.304***	1.052***
	(0.707)	(1.209)	(1.030)	(0.557)	(0.739)	(0.870)	(0.274)	(0.668)	(0.382)
Pre-trend avg.	-0.285	-1.199	-2.173***	0.019	-0.539	-1.078**	-0.304	-0.660	-1.095**
	(0.782)	(1.130)	(0.775)	(0.498)	(0.635)	(0.488)	(0.387)	(0.603)	(0.462)
Observations	4,699	4,660	23,940	4,699	4,660	23,940	4,699	4,660	23,940
Panel D. Female literacy									
Ban Removal	3.057***	4.287***	2.491**	1.208*	2.138***	1.142***	1.848***	2.149***	1.349*
	(0.731)	(1.071)	(1.094)	(0.630)	(0.658)	(0.364)	(0.325)	(0.634)	(0.809)
Pre-trend avg.	0.201	-1.543	-1.183	0.703	-0.961	-0.741	-0.502	-0.582	-0.441
	(1.183)	(1.163)	(1.038)	(0.662)	(0.670)	(0.516)	(0.614)	(0.571)	(0.638)
Observations	4,695	4,664	23,940	4,695	4,664	23,940	4,695	4,664	23,940

Notes: The table reports the effect of night ban removal on crimes against women in districts below and above the median of four baseline characteristics. Panel A: women employed in manufacturing establishments with ten or more workers, excluding household and handloom units, as a share of district employment (2013 Economic Census). Panel B: urban households as a share of households. Panel C: workers outside cultivation, agricultural labor, and household industry as a share of main and marginal workers. Panel D: literate women as a share of women. Panels B to D use the 2011 Census. Medians are taken over districts. Columns (1)–(3) report total crimes against women, columns (4)–(6) non-domestic violence, and columns (7)–(9) domestic violence (cruelty by husband or relatives), all per 100,000 population. Below and Above report the average post-reform effect ($k = 0$ to $k = 5$) from the interaction-weighted estimator of Sun and Abraham (2021) with district and year fixed effects, estimated separately in each half. Diff. reports the interaction of reform with an above-median indicator from a stacked difference-in-differences in which each adoption cohort forms a stack with districts untreated during its event window; models include district-by-stack, state-by-year-by-stack, and above-median-by-year-by-stack fixed effects. Pre-trend avg. is the mean of the event-time -5 to -2 coefficients from the corresponding event study. Standard errors are clustered by state and reported in parentheses. Asterisks denote $p < 0.10$ (*), $p < 0.05$ (**), and $p < 0.01$ (***). Sources: NCRB 2008–2024; Census of India 2011; Economic Census 2013.

4.4 Incidence and reporting

Employment opportunities might change both exposure to violence and the costs of disclosure, making police records alone insufficient to distinguish incidence from reporting (Amaral et al.,

2021; Iyer et al., 2012; Palermo et al., 2014). To this end, we combine them with survey and mortality evidence. Survey reports of violence do not depend on contact with the police. An increase in survey-reported violence would therefore point to incidence, while an increase in disclosure without it would point to reporting.

Survey reports. Six states reformed between NFHS IV and V: Madhya Pradesh, Maharashtra, Himachal Pradesh, Haryana, Uttarakhand, and Orissa.[5] Since factories are concentrated in urban areas,[6] we expect urban women to be more exposed to the employment opportunities opened by the reform. With two survey rounds and a limited number of reforming states, urban-rural differences in expected exposure provide an additional source of variation. We therefore use a triple-difference design that interacts urban residence with reform status and the post-reform period:

$$\begin{aligned} Y_{idst} = \beta_1(T_s \times P_t \times U_{ist}) + \beta_2(T_s \times P_t) + \beta_3(T_s \times U_{ist}) \\ + \beta_4(P_t \times U_{ist}) + \lambda U_{ist} + X'_{ist}\gamma + \alpha_d + \delta_t + \varepsilon_{idst}. \end{aligned} \tag{2}$$

Here T_s indicates reforming states, P_t NFHS V, and U_{ist} urban residence. Models include district and wave fixed effects, the lower-order interactions, and controls for age, household size, children, religion, and education; district effects subsume state effects. We use survey weights and cluster by state. The triple difference β_1 compares urban–rural changes in reforming and comparison states. Identification requires these differences to have evolved similarly absent reform (Olden & Møen, 2022). The coefficient β_2 is the treated–comparison change for rural women; the urban change is $\beta_1 + \beta_2$.

Table 3 shows an 8.1-percentage-point triple-difference increase in recent employment, equivalent to 26 percent of its mean (sample sizes vary across columns by NFHS eligibility). This covers work in any sector. Columns (2)–(3) show no evidence of increased partner violence. Columns (4)–(7) instead indicate increased non-partner violence, particularly among groups plausibly more affected by reform, i.e., never-married, young, or currently working women.

[5]Punjab reformed before NFHS IV and is excluded. Goa, Karnataka, and Uttar Pradesh reformed after NFHS V and remain untreated in this comparison.

[6]Manufacturing accounted for nearly three times the share of women's employment in urban areas as in rural areas in 2012, 28.7 versus 9.8 percent (Ministry of Statistics and Programme Implementation, 2021).

NFHS records only the person who forced the respondent the first time, so the outcome equals one when that person was neither a husband nor a boyfriend.[7] Finally, Column (8) shows that disclosure to anyone rises by 5.7 percentage points for the rural reference group, significant at the 5 percent level. The additional urban response is noisy and not significant.

Table 3: Effect of Night Ban Removal on Women's Outcomes (NFHS IV/V)

		Partner violence		Non-partner: Forced sex				
	Worked recently (past 12 months)	Sexual IPV	Other IPV	Total	Never-married	Age < 23	Currently working	Disclosed violence to anyone
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post × Urban	0.081***	0.005	-0.016	0.008**	0.048***	0.032***	0.014*	0.002
	(0.025)	(0.017)	(0.019)	(0.003)	(0.013)	(0.009)	(0.007)	(0.040)
Treated × Post	-0.016	0.020	0.016	0.001	-0.005	-0.001	0.003	0.057**
	(0.028)	(0.025)	(0.041)	(0.002)	(0.010)	(0.006)	(0.003)	(0.025)
Mean dep. var.	0.310	0.063	0.294	0.006	0.013	0.009	0.009	0.137
Observations	193,550	106,895	106,895	125,329	18,921	23,264	32,827	35,537
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports the effect of the ban removal on women's outcomes using NFHS IV and NFHS V. In column (1) the outcome is an indicator equal to 1 if a woman is currently working or has worked in the past twelve months. In columns (2) and (3) the outcome is an indicator equal to 1 if the respondent reports having experienced any sexual IPV, or any other form of IPV (emotional, physical), respectively. In columns (4) to (7) the outcome is an indicator equal to 1 if the respondent reports ever having been forced to perform unwanted sexual acts and the person who did so the first time was not a current or former husband or boyfriend: column (4) covers all respondents to the domestic violence module, and columns (5) to (7) restrict to never-married women, women under 23, and women currently working. In column (8) the outcome is an indicator equal to 1 if the respondent has disclosed violence she experienced to anyone (partner, family members, relatives, friends, police, and others). Appendix Table A.4 reports the survey questions. Punjab, which reformed before NFHS IV, and Assam, as in the crime analysis, are excluded, as are Orissa's NFHS V interviews before its October 2020 reform. All outcomes are estimated using NFHS survey weights. The model includes district and wave fixed effects and individual controls (age, household size, number of children, religion dummies for Hindu, Muslim and Christian, and years of education). Standard errors are clustered by state and reported in parentheses. Asterisks denote $p < 0.10$ (*), $p < 0.05$ (**), and $p < 0.01$ (***). Sources: NFHS IV and NFHS V.

Mortality. Deaths provide another way to assess whether violence increased, since their recording does not depend on victims coming forward. They are therefore less sensitive to changes in disclosure, although under-recording and misclassification remain possible (Drèze & Khera, 2000; Ferraz & Schiavon, 2022; Sekhri & Storeygard, 2014). Appendix Table A.6 shows no increase in female homicide or in suicides attributed to marriage-related problems, love affairs, or family problems. The suicide estimates are less precise and do not rule out

[7]The estimates are similar when every report is counted (0.015, SE 0.006, Column 4) or when women whose first perpetrator was a husband are excluded (0.008, SE 0.003).

meaningful increases. The family-problems series declines before the reform, so its estimate is less informative.

We also examine accidental deaths, as some violent deaths may be recorded as accidents (Amaral, 2017; Singh et al., 2003). Appendix Table A.7 reports results for women by cause, with no statistically significant post-reform effects. Deaths from unnatural causes also decline before the reform (Column 1). These categories cover a wide range of events, including traffic accidents, so the results offer only limited evidence on concealed violence and say little about non-lethal abuse.

Overall, the findings suggest different explanations for the rise in recorded domestic and non-domestic offenses. For non-domestic violence, survey reports of sexual violence by non-partners, which do not depend on contact with the police, also rise, pointing to increased incidence. For domestic violence, the absence of a detectable increase in survey-reported partner violence or lethal outcomes suggests that greater reporting may contribute to the rise in recorded offenses. This interpretation remains tentative, as the disclosure measure neither increases significantly in the triple-difference comparison nor distinguishes partner from non-partner violence.

4.5 Robustness

The main findings are similar across alternative difference-in-differences estimators (Appendix Figure A.6). The Callaway–Sant'Anna estimates (Callaway & Sant'Anna, 2021) imply an increase of 0.20 in log female employment and 3.76 recorded crimes per 100,000 population, both close to the baseline estimates. With 1,000 state-level wild cluster bootstrap replications, both estimates remain statistically significant at the 5 percent level (Appendix Table A.8). Appendix Figures A.7 and A.8 also examine sensitivity to excluding 2020 and dropping one state at a time.

We examine possible cross-border spillovers in two ways: by adding an indicator for never-treated districts bordering a reformed state, and by excluding those districts altogether. The estimated post-reform increases in crime are 4.20 and 3.96 cases per 100,000 population, respectively, compared with 3.67 in the baseline specification (Appendix Figure A.9). The result therefore does not depend on including these border districts in the comparison group. Finally,

the crime estimate is similar when Assam is included, when we control for state amendments to the Shops and Establishments Acts that allowed women to work at night in shops and offices (Kaur et al., 2026), and when we exclude comparison states that amended these Acts during the sample period (Appendix Table A.9).

5 Conclusion

The relaxation of factory night-work bans in India increased women's factory employment by approximately 23 percent and recorded crimes against women by 22 percent. Non-domestic crime responses were larger in districts where more women already worked in factories and in more urban and less agricultural districts. Expanding women's access to work thus brought substantial employment gains alongside an increase in recorded offenses against them.

The evidence suggests different explanations for domestic and non-domestic violence. For domestic violence, the absence of a detectable increase in survey-reported partner violence or lethal outcomes is consistent with increased reporting contributing to the rise in recorded offenses. This interpretation remains tentative: the disclosure measure neither increases significantly in the triple-difference comparison nor distinguishes partner from non-partner violence. For non-domestic violence, the rise in survey-reported sexual violence by non-partners points to an increase in actual incidence.

These findings highlight the importance of safety alongside equal access to employment. The reforms we study included formal safeguards, but our analysis cannot establish how effective those safeguards were or which protections would work best. Nor does it establish that reinstating restrictions would improve women's welfare. It shows that evaluating employment reforms requires looking beyond job gains to whether women can safely take advantage of the opportunities those reforms create.

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Online Appendix

A. Setting and Data

Table A.1: Removal of the Night-Work Ban by State

State	Amendment date	Treatment year	Instrument
Punjab	February 2014	2014	Notification
Maharashtra	December 2015	2016	Act
Madhya Pradesh	June 2016	2016	Notification
Himachal Pradesh	May 2017	2017	Notification
Haryana	August 2017	2017	Notification
Uttarakhand	November 2018	2019	Notification
Orissa	October 2020	2020	Ordinance
Uttar Pradesh	May 2022	2022	Notification
Goa	April 2023	2023	Act
Karnataka	August 2023	2023	Act
Assam	October 2016	2016	Notification

Notes: Amendment date is the month and year of the state notification/amendment relaxing Section 66(1)(b) of the Factories Act, 1948. Treatment year is the year coded as the onset of treatment in the analysis; instruments issued in November or December are coded to the following year. Assam is treated in the factory employment analysis and in the crime robustness check of Appendix Table A.9. Source: state government notifications, ordinances and amendment Acts.

Figure A.1: Treatment Adoption by Year

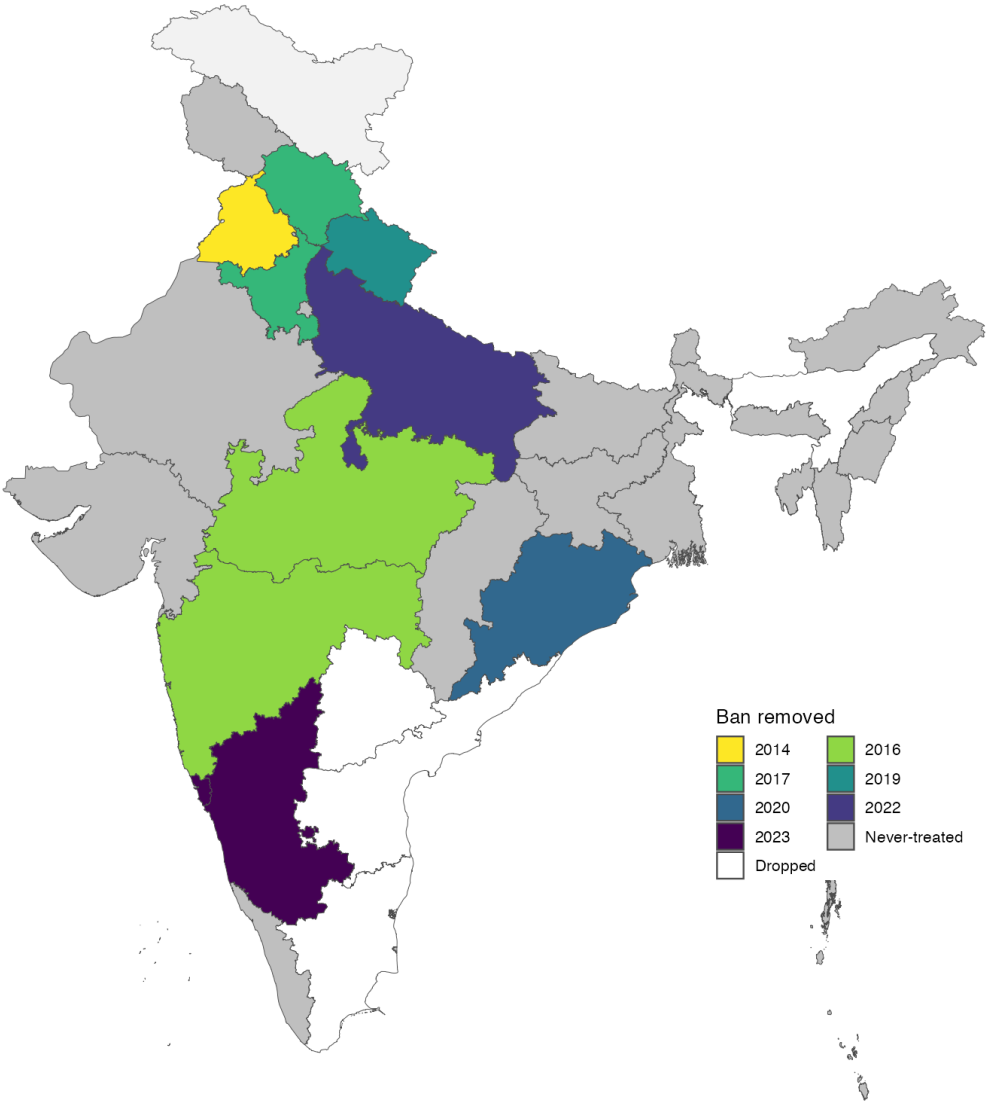


Table A.2: Definition of Crime Categories

NCRB category	IPC / Statute	Definition	Coverage	Level
A. Crimes against women				
Rape	Sec. 376	Sexual intercourse by a man with a woman against her will; without her consent; with her consent obtained by putting her, or any person she is interested in, in fear of death or hurt; with her consent given in the belief that the man is her lawfully wedded husband; with her consent when, owing to unsoundness of mind or intoxication, she is unable to understand the nature and consequences of that to which she consents; or with or without her consent when she is below the statutory age. Includes attempt to commit rape; the series is harmonized across the 2012 POCSO reclassification.	2008–2024	District
Kidnapping & Abduction of Women	Sec. 363–369	Kidnapping from lawful guardianship: taking or enticing a minor (under eighteen years if female) or a person of unsound mind out of the keeping of the lawful guardian without that guardian's consent. Abduction: by force compelling, or by any deceitful means inducing, a woman to go from any place. Includes kidnapping or abduction of women to compel marriage, for ransom, or for other unlawful purposes.	2008–2024	District
Insult to the Modesty of Women	Sec. 509	Whoever, intending to insult the modesty of a woman, utters any word, makes any sound or gesture, or exhibits any object, intending that such word or sound be heard, or that such gesture or object be seen, by the woman, or intrudes upon her privacy.	2008–2024	District
Cruelty by Husband or his Relatives	Sec. 498A	Subjecting a woman to cruelty by her husband or his relatives, where cruelty means any wilful conduct of such a nature as is likely to drive the woman to commit suicide or to cause grave injury or danger to life, limb, or health (whether mental or physical); or harassment of the woman with a view to coercing her or any person related to her to meet an unlawful demand for property or valuable security, or on account of failure to meet such demand.	2008–2024	District
B. Non-gender violent crimes				
Murder	Sec. 302	Culpable homicide is murder where the act causing death is done with the intention of causing death; or with the intention of causing such bodily injury as the offender knows to be likely to cause death; or with the intention of causing bodily injury sufficient in the ordinary course of nature to cause death; or with the knowledge that the act is so imminently dangerous that it must in all probability cause death.	2008–2024	District
Attempt to Commit Murder	Sec. 307	Whoever does any act with such intention or knowledge, and under such circumstances, that if he by that act caused death he would be guilty of murder.	2008–2024	District
Culpable Homicide not amounting to Murder	Sec. 304	Causing death by doing an act with the intention of causing death, or of causing such bodily injury as is likely to cause death, or with the knowledge that the act is likely to cause death, but without the special circumstances that make the act murder.	2008–2024	District
Arson	Sec. 435, 436	Committing mischief by fire or any explosive substance intending, or knowing it to be likely, to cause damage to property (Sec. 435), or to cause the destruction of a building ordinarily used as a place of worship, as a human dwelling, or for the custody of property (Sec. 436).	2008–2024	District
C. Property / economic crimes				
Robbery	Sec. 392–394	Theft or extortion becomes robbery where the offender, in order to commit it, voluntarily causes or attempts to cause death, hurt, or wrongful restraint, or fear of instant death, hurt, or wrongful restraint.	2008–2024	District

Continued on next page

Table A.2 (continued)

NCRB category	IPC / Statute	Definition	Coverage	Level
Burglary	Sec. 445–452	House-breaking: entering, or quitting after committing an offense, a house or part of a house used as a human dwelling by any of the means specified in the Code; includes house-breaking by night (lurking house-trespass committed after sunset and before sunrise).	2008–2024	District
Dacoity	Sec. 391, 395	Robbery committed, or attempted, conjointly by five or more persons, counting all persons present and aiding in the commission.	2008–2024	District
Criminal Breach of Trust	Sec. 405–409	Being in any manner entrusted with property, or with dominion over property, dishonestly misappropriating or converting it to one's own use, or dishonestly using or disposing of it in violation of any direction of law or legal contract governing the trust.	2008–2024	District
Cheating	Sec. 415, 420	By deceiving any person, fraudulently or dishonestly inducing the person to deliver property or to consent to its retention, or intentionally inducing the person to do or omit an act which he would not otherwise do and which causes, or is likely to cause, harm to him in body, mind, reputation, or property.	2008–2024	District
D. Mortality: female homicide, suicide, and deaths by cause				
Female Homicide	Sec. 302	Murder victims recorded as female in the NCRB "Murder Victims by Sex" tables; a subset of murder defined by the sex of the victim rather than a separate offense.	2014–2024	State
Female Suicide (marriage-related, love affairs, family problems)		Suicides by women recorded in the Accidental Deaths and Suicides in India (ADSI) reports and attributed to marriage-related causes (non-settlement of marriage, dowry, extra-marital affairs, divorce, and other marriage-related causes), love affairs, or family problems.	2014–2024	State
Female Deaths from Unnatural Causes		Accidental deaths of women recorded in the NCRB *Accidental Deaths and Suicides in India* volumes under causes other than the forces of nature: traffic accidents, drowning, falls, poisoning, electrocution, accidental fire, accidental explosion, factory and machine accidents, collapse of structure, suffocation, sudden deaths, deaths of women during pregnancy, consumption of illicit liquor, firearm, mines and quarry disasters, stampedes, and residual and unrecorded causes. This broad category may contain misclassified violent deaths, but it does not directly identify them. The same series is collected for men, who were never covered by the night-work provision.	2012–2023	State
Female Deaths from Forces of Nature		Accidental deaths of women attributed to natural forces: avalanche, cyclone, earthquake, epidemic, exposure to cold, flood, forest fire, heat and sun stroke, landslide, lightning, torrential rain, tornado, tsunami, and other natural causes. This series serves as a comparison outcome; the corresponding series is also collected for men.	2012–2023	State

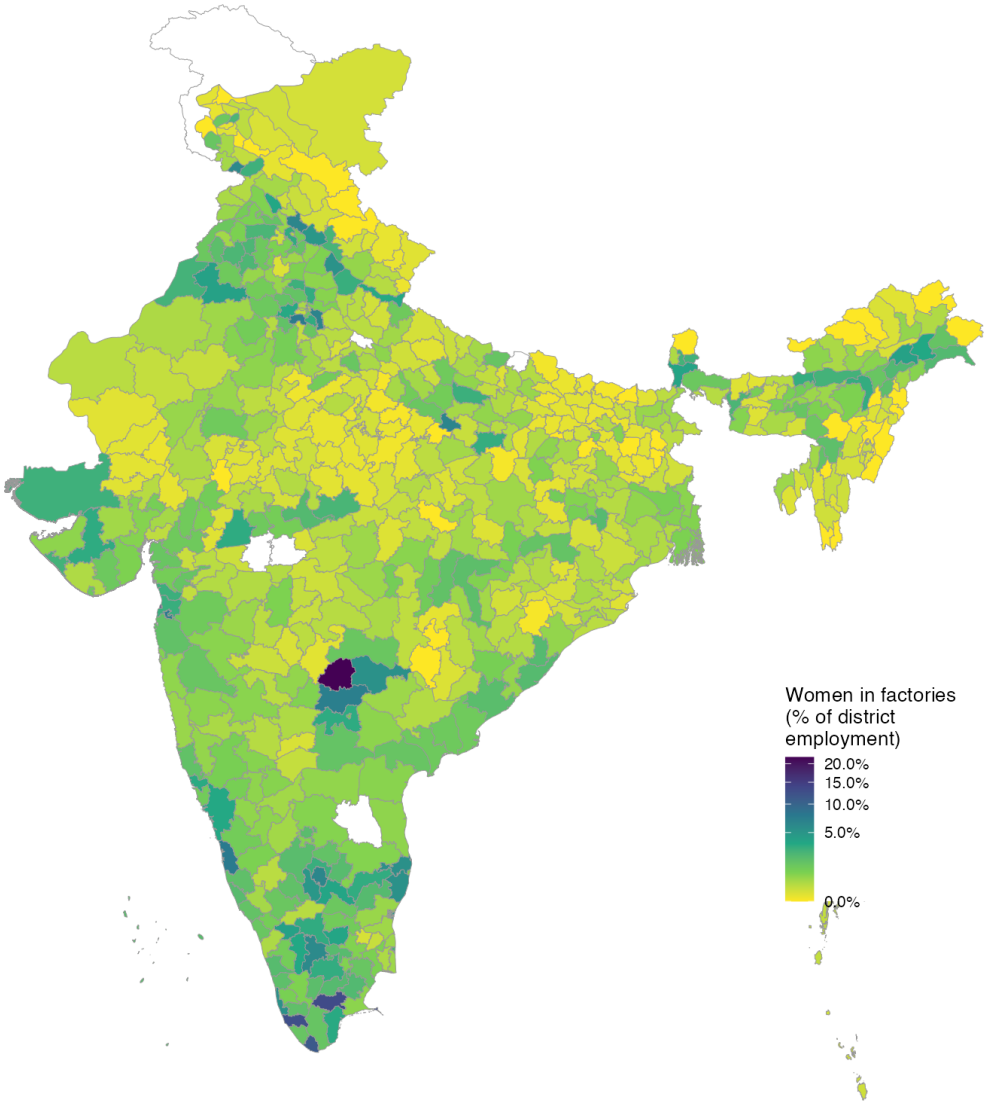
Source: NCRB.

Table A.3: Descriptive Statistics

	Treated states		Never-treated	Diff
	Pre	Post	Mean (SD)	(Pre − NT)
	(1)	(2)	(3)	(4)
Panel A. Crime per 100,000 population				
Crimes Against Women	12.54 (7.61)	23.00 (13.04)	16.38 (14.17)	-2.45
Non-domestic violence	6.95 (4.63)	14.38 (8.10)	9.38 (8.24)	-1.05
Rape (incl. attempt)	2.92 (2.11)	6.34 (3.81)	4.54 (4.86)	-0.82
Kidnapping	3.47 (3.20)	7.51 (5.27)	4.40 (4.77)	-0.31
Insult to modesty	0.56 (1.09)	0.53 (0.84)	0.45 (1.31)	0.08
Domestic violence	5.59 (4.36)	8.62 (6.83)	7.00 (8.83)	-1.40
Non-Gender Violent Crimes	6.59 (3.63)	7.01 (4.65)	8.52 (19.64)	-1.17
Property/Economic Crimes	17.14 (12.16)	21.60 (16.09)	19.46 (27.90)	-1.82
Panel B. Mortality and suicide per 100,000 population				
Female homicide victims	0.813 (0.324)	0.699 (0.265)	0.705 (0.488)	0.102
Female suicides, marriage-related	0.317 (0.222)	0.441 (0.311)	0.262 (0.306)	0.041
Female suicides, love affairs	0.215 (0.166)	0.188 (0.123)	0.290 (0.314)	-0.063
Female suicides, family problems	0.971 (0.632)	1.319 (0.852)	1.116 (1.190)	-0.114
Female deaths, un-natural causes	14.57 (6.40)	16.99 (7.30)	11.70 (8.97)	2.14
Female deaths, forces of nature	0.724 (0.893)	0.396 (0.466)	0.440 (0.637)	0.168
Panel C. Annual Survey of Industries				
Log female workers	5.234 (2.219)	5.515 (2.183)	4.460 (2.210)	0.833
Log male workers	7.816 (2.018)	8.057 (2.031)	6.319 (2.389)	1.478**
Panel D. District-level baseline moderators (pre-shock)				
Women in factories (% of district employment)	0.844 (1.273)	–	0.743 (1.495)	0.101
Urban households (% of households)	25.15 (16.98)	–	27.79 (23.36)	-2.64
Non-agricultural workers (% of workers)	36.42 (17.84)	–	41.24 (23.34)	-4.82
Female literacy (% of women)	55.23 (10.87)	–	53.92 (14.33)	1.31
Panel E. NFHS DDD outcomes (NFHS-4 vs NFHS-5)				
Worked recently (past 12 months)	0.336 (0.472)	0.357 (0.479)	0.272 (0.445)	0.060
Sexual IPV	0.055 (0.228)	0.053 (0.225)	0.069 (0.254)	-0.020
Other IPV	0.277 (0.448)	0.267 (0.443)	0.293 (0.455)	-0.030
Forced sex (non-partner)	0.006 (0.079)	0.008 (0.086)	0.006 (0.079)	-0.001
Disclosed violence to anyone	0.114 (0.318)	0.152 (0.359)	0.140 (0.347)	-0.033*
Urban	0.376 (0.484)	0.353 (0.478)	0.329 (0.470)	0.040
Age	30.09 (9.75)	30.75 (9.75)	29.96 (9.87)	0.27
Household size	5.58 (2.52)	5.36 (2.29)	5.60 (2.50)	-0.14
Number of children	1.67 (1.48)	1.61 (1.40)	1.74 (1.60)	-0.09
Years of education	7.30 (5.02)	8.08 (4.96)	6.92 (5.19)	0.61
Hindu	0.861 (0.346)	0.874 (0.332)	0.771 (0.420)	0.088
Muslim	0.083 (0.276)	0.076 (0.265)	0.183 (0.387)	-0.097**
Christian	0.012 (0.109)	0.007 (0.086)	0.029 (0.169)	-0.017

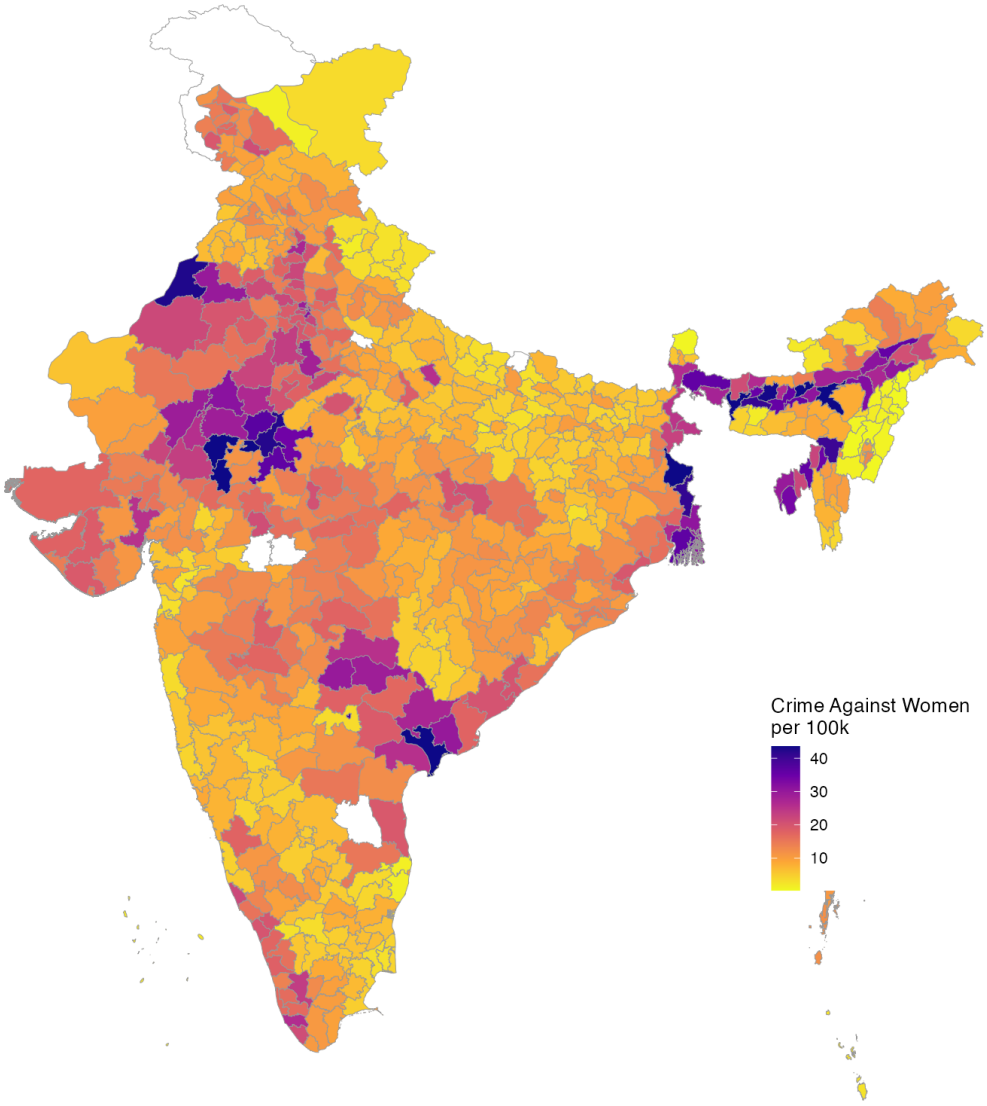
Notes: Columns (1) and (2) report mean (SD) for treated states in the pre-treatment and post-treatment periods, respectively. Column (3) reports the mean (SD) for never-treated states pooled across all sample years. Column (4) reports the difference between the treated pre-treatment mean and the never-treated mean net of year fixed effects with standard errors clustered by state. Panel A reports crime rates per 100,000 population at the district-year level. Panel B reports the state-year mortality and suicide outcomes used as incidence anchors in Tables A.6 and A.7: female homicide covers 2014–2024, the ADSI suicide series 2014–2024 and the ADSI accidental-death series 2012–2023. Accidental deaths are expressed per 100,000 of the own-sex population and the remaining rows per 100,000 of the 2011 Census state population. Panel C reports ASI labor outcomes in state-by-industry (two-digit NIC, manufacturing) cells for 2009–2024 using ASI weights. Panel D reports cross-sectional pre-shock moderators at the district level used in Table 2, over all districts. Panel E reports outcomes from NFHS IV (Pre) and NFHS V (Post) with the never-treated column pooling both waves. Asterisks denote $p < 0.10$ (*), $p < 0.05$ (**), and $p < 0.01$ (***). Sources: NCRB 2008–2024, ADSI 2012–2024, ASI 2009–2024, Census of India 2011, Economic Census 2013, NFHS IV (2015–2016) and NFHS V (2019–2021).

Figure A.2: Baseline Women's Factory Employment by District



Notes: Women hired in manufacturing establishments with ten or more workers, excluding household and handloom units, as a share of total district employment. Source: Economic Census 2013.

Figure A.3: Pre-Treatment Crime against Women per 100k by District



Sources: NCRB 2008–2024, Census of India 2011.

Table A.4: NFHS Survey Questions

Outcome	Column	Question	Respondents
Worked recently	(1)	"Aside from your own housework, have you done any work in the last seven days?"; "As you know, some women take up jobs for which they are paid in cash or kind. Others sell things, have a small business or work on the family farm or in the family business. In the last seven days, have you done any of these things or any other work?"; "Although you did not work in the last seven days, do you have any job or business from which you were absent for leave, illness, vacation, maternity leave or any other such reason?"; "Have you done any work in the last 12 months?" Equal to 1 if the answer to any of these is yes.	Women in households selected for the state module

Continued on next page

Table A.4 (continued)

Outcome	Column	Question	Respondents
Sexual IPV	(2)	"(Does/did) your (last) husband ever do any of the following things to you: physically force you to have sexual intercourse with him even when you did not want to? physically force you to perform any other sexual acts you did not want to? force you with threats or in any other way to perform sexual acts you did not want to?" Equal to 1 if the answer to any item is yes.	Ever-married women in the domestic violence module
Other IPV	(3)	"(Does/did) your (last) husband ever: say or do something to humiliate you in front of others? threaten to hurt or harm you or someone close to you? insult you or make you feel bad about yourself?" and "(Does/did) your (last) husband ever do any of the following things to you: push you, shake you, or throw something at you? twist your arm or pull your hair? slap you? punch you with his fist or with something that could hurt you? kick you, drag you or beat you up? try to choke you or burn you on purpose? threaten or attack you with a knife, gun, or any other weapon?" Equal to 1 if the answer to any item is yes.	Ever-married women in the domestic violence module
Non-partner Violence	(4)–(7)	Ever-married women: "Now I want to ask you about things that may have been done to you by someone other than (your/any) husband. At any time in your life, as a child or as an adult, has anyone ever forced you in any way to have sexual intercourse or perform any other sexual acts when you did not want to?" Never-married women: "At any time in your life, as a child or as an adult, has anyone ever forced you in any way to have sexual intercourse or perform any other sexual acts when you did not want to?" Equal to 1 if the person who did this the first time was not a current or former husband or boyfriend.	All women in the domestic violence module
Disclosed violence	(8)	"Thinking about what you yourself have experienced among the different things we have been talking about, have you ever tried to seek help?" and "From whom have you sought help?" (own family, husband's family, current or former husband, current or former boyfriend, friend, neighbor, religious leader, doctor, police, lawyer, social service organization, other). Equal to 1 if the respondent sought help from anyone.	Women in the domestic violence module who report any violence

Notes: Wording from the NFHS IV Woman's Questionnaire. Column numbers refer to Table 3.

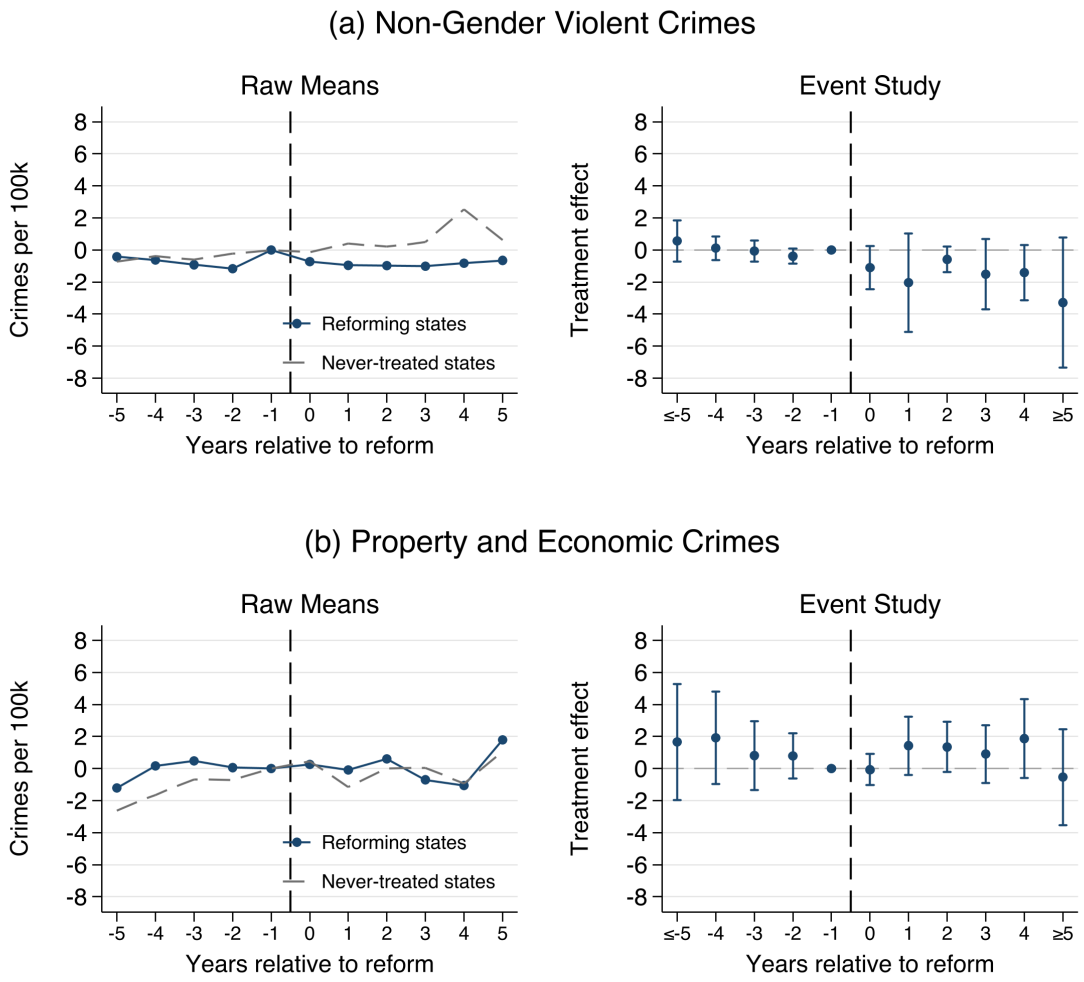
B. Additional Results

Table A.5: Effect of Night Ban Removal on Crime against Women by Offense

	Non-domestic violence						Domestic violence	
	Rape		Kidnap & abduction		Insult to modesty		Cruelty (husb./rel.)	
	Short run	Long run	Short run	Long run	Short run	Long run	Short run	Long run
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ban Removal	0.546***	0.061	0.917***	1.457**	0.112**	0.155	1.302***	2.391***
	(0.202)	(0.579)	(0.202)	(0.581)	(0.045)	(0.131)	(0.318)	(0.545)
Mean dep. var.	4.167		4.935		0.550		6.690	
Observations	9,359		9,359		9,359		9,359	

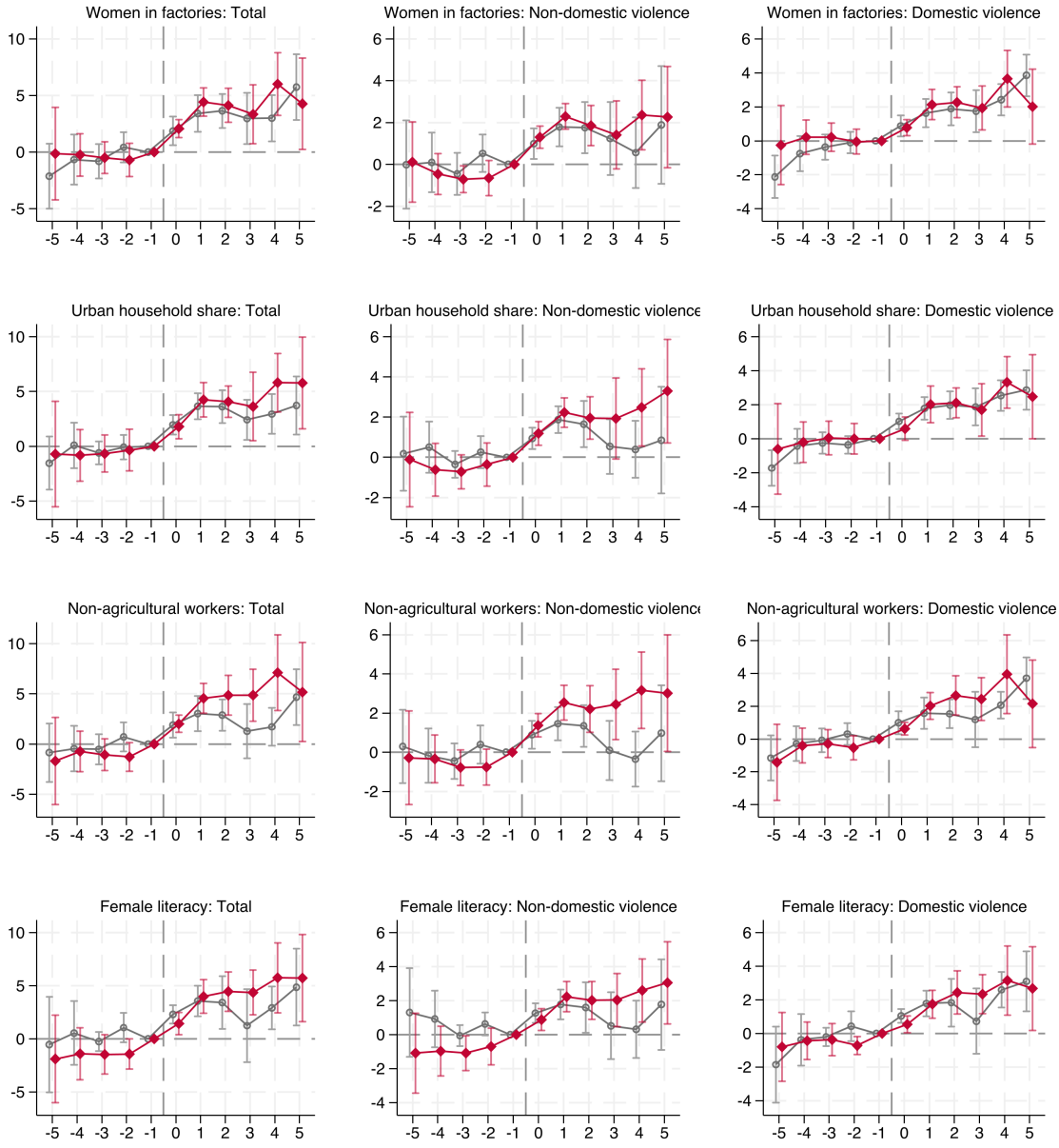
Notes: The table reports event-study estimates of the effect of the removal of the night ban by type of crime. The short run is the average of the coefficients for the year of the amendment and the year that follows ($k = 0$ and $k = 1$); the long run is the average of the coefficients for the remaining years of the event window ($k = 2$ to $k = 5$). Columns (1)–(6) report non-domestic offenses, and columns (7) and (8) report domestic offenses (cruelty by husband or relatives). The grouping is based on offense codes rather than observed incident location. All outcomes are expressed per 100,000 population. The model uses an interaction-weighted estimator proposed by Sun and Abraham (2021) with district and year fixed effects. Standard errors are clustered by state and reported in parentheses. Asterisks denote $p < 0.10$ (*), $p < 0.05$ (**), and $p < 0.01$ (***). Sources: NCRB 2008–2024, Census of India 2011.

Figure A.4: Night Ban Removal and Placebo Crimes per 100k



Notes: In panel (a) the outcome is non-gender violent crimes (murder, attempt to murder, culpable homicide, and arson) and in panel (b) property and economic crimes (robbery, burglary, cheating, criminal breach of trust, and dacoity), per 100,000 population in districts. The left graphs plot raw means by year relative to reform for the cohorts observed over the full window (reforms up to 2019). Each cohort forms a stack with the never-treated states over the five years before and after adoption, and means are expressed relative to the year before reform. The right graphs report event studies estimated on all cohorts with the interaction-weighted estimator of Sun and Abraham (2021) with district and year fixed effects. Event times outside $[-5, +5]$ are pooled into the endpoints. Bars are 95 percent confidence intervals from standard errors clustered by state. Sources: NCRB 2008–2024; Census of India 2011.

Figure A.5: Night Ban Removal and Crime against Women by District Median Split



Notes: Event-study estimates of the effect of night ban removal on crimes against women per 100,000 population, by district median split of the four baseline characteristics in Table 2. Grey hollow circles: districts below the median; red diamonds: districts above the median. Rows: women in factories, urban household share, non-agricultural workers, and female literacy. Columns: total crimes against women, non-domestic violence, and domestic violence. Estimates from the interaction-weighted estimator of Sun and Abraham (2021) with district and year fixed effects, estimated separately in each half. The reference is the year before reform, and event times outside $[-5, +5]$ are pooled into the endpoints. Bars are 95 percent confidence intervals from standard errors clustered by state. Sources: NCRB 2008–2024; Census of India 2011; Economic Census 2013.

Table A.6: Effect of Night Ban Removal on Female Deaths per 100k

	Female	Female suicides		
	homicide	Marriage-related	Love affairs	Family problems
	(1)	(2)	(3)	(4)
Ban Removal	-0.057	0.046	0.012	0.182
	(0.043)	(0.037)	(0.035)	(0.117)
Pre-trend avg.	-0.082	0.016	-0.005	-0.548***
	(0.101)	(0.040)	(0.048)	(0.151)
Mean dep. var.	0.748	0.388	0.199	1.170
Observations	341	342	342	342
Years	2014–24	2014–24	2014–24	2014–24
State & Year FE	Yes	Yes	Yes	Yes

Notes: The table reports post-reform averages from state-level interaction-weighted event studies. Column (1) measures female homicide for 2014–2024. Columns (2) to (4) measure female suicides by recorded cause for 2014–2024: marriage-related problems (non-settlement of marriage, dowry, extra-marital affairs, divorce, and other marriage-related causes), love affairs, and family problems. Rates are per 100,000 of the 2011 Census state population. Models include state and year fixed effects. Pre-trend avg. is the mean of the event-time coefficients -5 to -2, relative to the year before reform. Standard errors are clustered by state and reported in parentheses. Asterisks denote $p < 0.10$ (*), $p < 0.05$ (**), and $p < 0.01$ (***). Sources: NCRB Crime in India; ADSI; Census of India 2011.

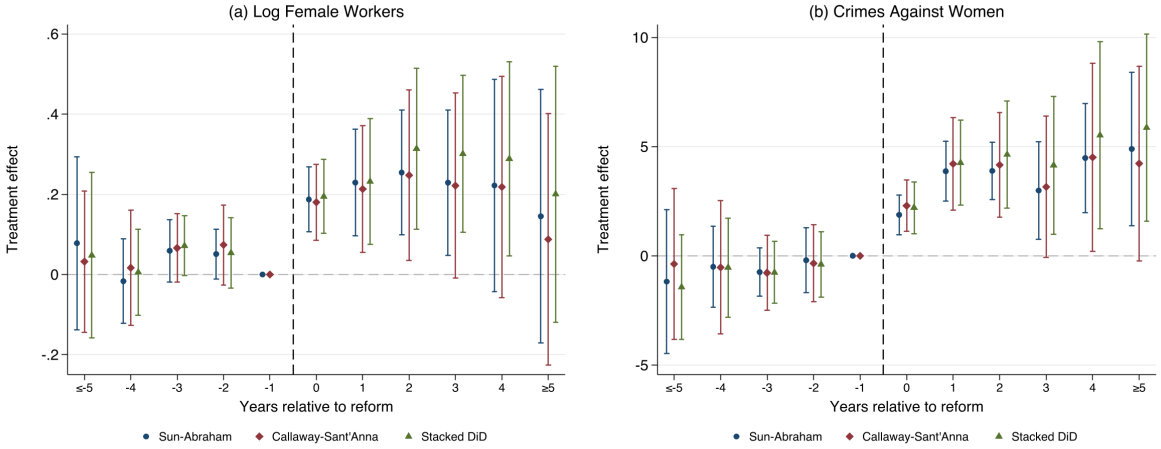
Table A.7: Effect of Night Ban Removal on Women's Accidental Deaths per 100k

	Un-natural causes	Forces of nature
	(1)	(2)
Ban Removal	0.654	-0.022
	(0.736)	(0.088)
Pre-trend avg.	-0.670*	-0.053
	(0.351)	(0.104)
Mean dep. var.	15.640	0.579
Observations	376	376
Years	2012–23	2012–23
State & Year FE	Yes	Yes

Notes: The table reports post-reform averages from state-level interaction-weighted event studies for 2012–2023. Column (1) comprises women's accidental deaths from causes other than natural forces; column (2) comprises women's deaths attributed to forces of nature. Rates are per 100,000 of the female population. Models include state and year fixed effects. Pre-trend avg. is the mean of the event-time coefficients -5 to -2, relative to the year before reform. Standard errors are clustered by state and reported in parentheses. Asterisks denote $p < 0.10$ (*), $p < 0.05$ (**), and $p < 0.01$ (***). Sources: NCRB ADSI, 2012–2023; Census of India 2011.

C. Robustness

Figure A.6: Alternative DiD Estimators



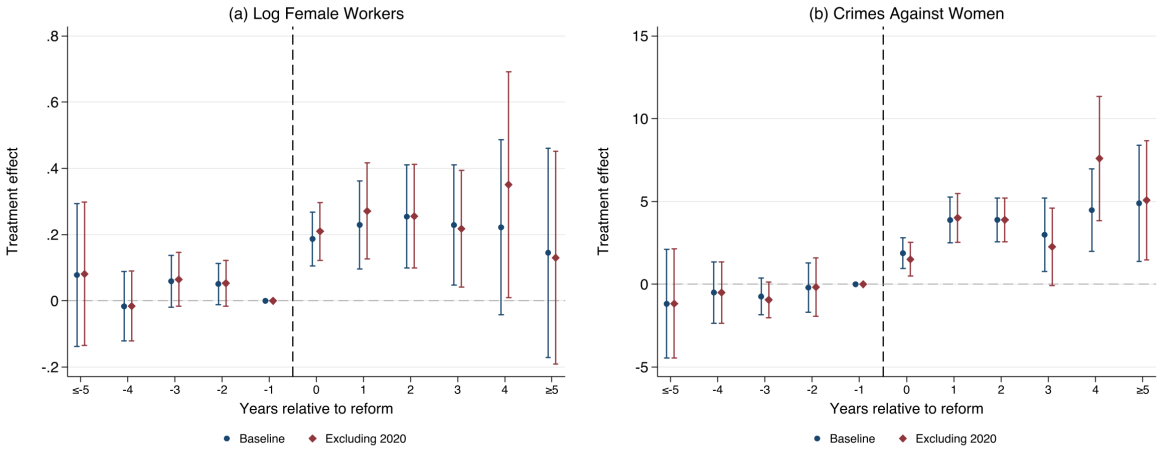
Notes: The figure reports event studies of the removal of the night ban from three estimators: the interaction-weighted estimator of Sun and Abraham (2021), the estimator of Callaway and Sant'Anna (2021) with never-treated comparison units, and a stacked difference-in-differences. In panel (a) the outcome is the log of female workers employed directly by factories in state-by-industry (two-digit NIC, manufacturing) cells for 2009–2024, built with ASI weights; in panel (b) it is total crimes against women per 100,000 population in districts (rape, attempt to rape, kidnapping and abduction of women, insult to modesty, and cruelty by husband and relatives). In panel (a) the Sun and Abraham estimates include state-by-industry and industry-by-year fixed effects, and the other two estimators include state-by-industry and year fixed effects; in panel (b) all three include district and year fixed effects. Event times outside $[-5, +5]$ are pooled into the endpoints. Bars are 95 percent confidence intervals from standard errors clustered by state. Sources: ASI 2009–2024; NCRB 2008–2024; Census of India 2011.

Table A.8: Wild Cluster Bootstrap Inference

	Log female workers	Crimes against women per 100k
	(1)	(2)
Ban Removal	0.195	3.760
	(0.094)	(1.380)
95% CI (pointwise)	[0.011, 0.378]	[1.055, 6.464]
Pre-trend avg.	0.047	-0.505
	(0.054)	(1.152)
Mean dep. var.	5.352	16.343
Observations	7,980	9,359

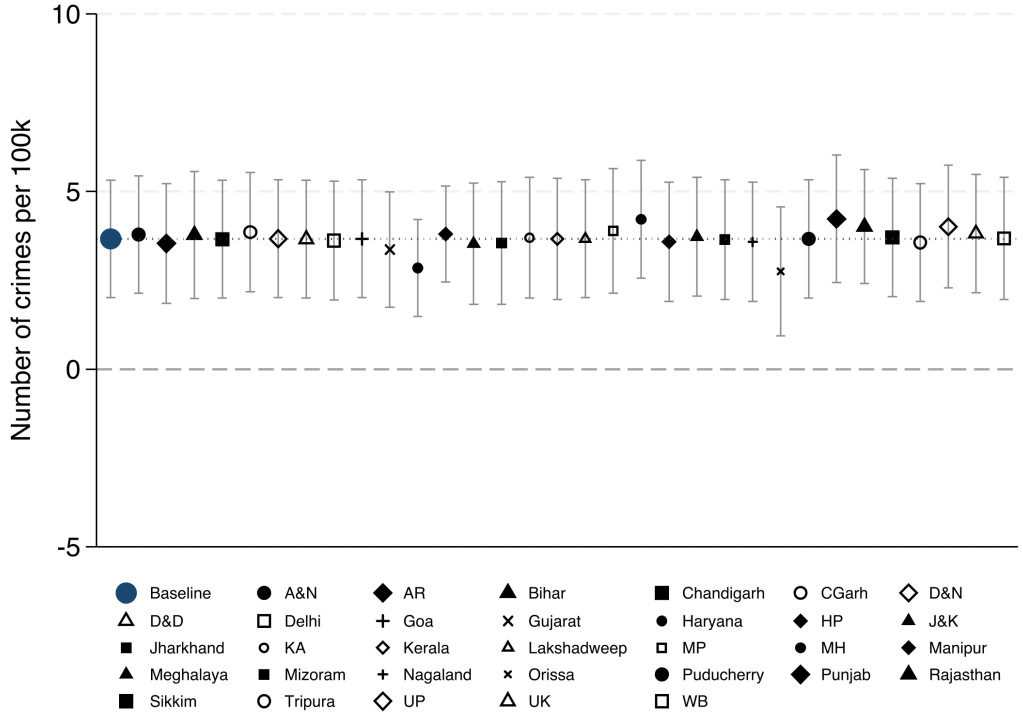
Notes: The table reports estimates from the Callaway–Sant'Anna estimator (Callaway & Sant'Anna, 2021) (never-treated comparison group) as a check on small-cluster inference. Column (1) measures the log of female workers employed directly by factories in state-by-industry (two-digit NIC, manufacturing) cells for 2009–2024, built with ASI weights, with state-by-industry and year fixed effects and the sample of Table 1, column (1). Column (2) measures total crimes against women per 100,000 population (rape, attempt to rape, kidnapping and abduction of women, insult to modesty, and cruelty by husband and relatives) with district and year fixed effects. "Ban Removal" is the average effect over event times 0 to 5. Pre-trend avg. is the mean of the event-time coefficients -5 to -2, relative to the year before reform. Standard errors are a wild cluster bootstrap with 1,000 repetitions, clustered by state, and reported in parentheses; brackets report pointwise 95 percent confidence intervals. Sources: ASI 2009–2024; NCRB 2008–2024; Census of India 2011.

Figure A.7: Sensitivity to Excluding 2020



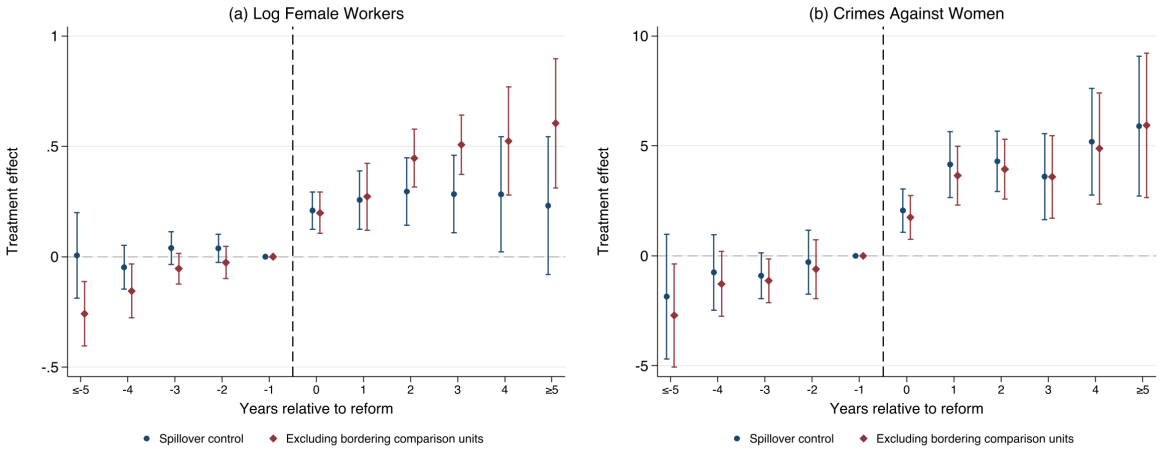
Notes: The figure compares the baseline event studies with estimates that exclude 2020. In panel (a) the outcome is the log of female workers employed directly by factories in state-by-industry (two-digit NIC, manufacturing) cells for 2009–2024, built with ASI weights; in panel (b) it is total crimes against women per 100,000 population in districts (rape, attempt to rape, kidnapping and abduction of women, insult to modesty, and cruelty by husband and relatives). The model uses the interaction-weighted estimator of Sun and Abraham (2021) with state-by-industry and industry-by-year fixed effects in panel (a) and district and year fixed effects in panel (b). Event times outside $[-5, +5]$ are pooled into the endpoints. Bars are 95 percent confidence intervals from standard errors clustered by state. Sources: ASI 2009–2024; NCRB 2008–2024; Census of India 2011.

Figure A.8: Recorded Crime against Women: Omitting One State at a Time



Notes: The figure reports the average effect of the removal of the night ban dropping one state at a time. The outcome measures the total crimes against women per 100,000 (rape, attempt to rape, kidnapping and abduction of women, insult to modesty, and cruelty by husband and relatives). Each estimate omits one state and reports the average post-reform effect over the baseline event-time bins. Standard errors are clustered by state. Sources: NCRB 2008–2024, Census of India 2011.

Figure A.9: Spillovers and SUTVA: Robustness of the Crime Effect



Notes: The figure reports event studies under two checks for spillovers to comparison units. *Spillover control* adds an indicator for comparison units bordering a reforming state after the first neighboring reform. *Excluding bordering comparison units* drops these units. In panel (a) the outcome is the log of female workers employed directly by factories in state-by-industry (two-digit NIC, manufacturing) cells for 2009–2024, built with ASI weights; in panel (b) it is total crimes against women per 100,000 population in districts (rape, attempt to rape, kidnapping and abduction of women, insult to modesty, and cruelty by husband and relatives). In panel (b) the units are districts and adjacency is queen contiguity of 2011 Census districts. In panel (a) the units are states, and a comparison state borders a reform if any of its districts does; excluding bordering states leaves the north-eastern states, Andaman and Nicobar Islands, Daman and Diu, Lakshadweep, and Puducherry as comparison units. The model uses the interaction-weighted estimator of Sun and Abraham (2021) with state-by-industry and industry-by-year fixed effects in panel (a) and district and year fixed effects in panel (b). Event times outside $[-5, +5]$ are pooled into the endpoints. Bars are 95 percent confidence intervals from standard errors clustered by state. Sources: ASI 2009–2024; NCRB 2008–2024; Census of India 2011.

Table A.9: Robustness to Including Assam and to Shops and Establishments Reforms

	Include Assam (1)	Control for S&E reform (2)	Drop S&E reformers among controls (3)
Ban Removal	3.840***	3.520***	4.037***
	(0.893)	(0.960)	(0.843)
Pre-trend avg.	-1.582	-0.317	-1.296*
	(1.161)	(1.089)	(0.685)
Mean dep. var.	18.398	16.343	16.343
Observations	9,723	9,359	8,560

Notes: The table reports the average post-reform effect of the ban removal on total crimes against women per 100,000 population (rape, attempt to rape, kidnapping and abduction of women, insult to modesty, and cruelty by husband and relatives) from interaction-weighted event studies (Sun & Abraham, 2021). Column (1) adds Assam, treated from 2016, excluding its years from 2022 because of a break in NCRB crime registration. Column (2) adds an indicator equal to one from the year a state amended its Shops and Establishments Act to allow women's night work, with amendment dates from Kaur et al. (2026). Column (3) excludes the comparison states that amended their Shops and Establishments Act during the sample period (Kerala and Rajasthan). Models include district and year fixed effects. Pre-trend avg. is the mean of the event-time coefficients -5 to -2, relative to the year before reform. Standard errors are clustered by state and reported in parentheses. Asterisks denote $p < 0.10$ (*), $p < 0.05$ (**), and $p < 0.01$ (***). Sources: NCRB 2008–2024, Census of India 2011, Kaur et al. (2026).